HISTORY OF THE COWLES COMMISSION 1932–1952

BY CARL F. CHRIST*

I. *The founding of the Cowles Commission*
II. *The early years in Colorado: 1932–1937*
III. *The later years in Colorado: 1937–1939*
IV. *The move to Chicago: 1939*
V. *The early years at Chicago: 1940–1942*
VI. *Simultaneous developments: 1943–1948*
VII. *Economic theory revisited: 1948–1952*
VIII. *Looking back and looking forward*

I. The founding of the Cowles Commission

The Cowles Commission for Research in Economics was founded in 1932. Alfred Cowles, president of Cowles and Company, an investment counseling firm in Colorado Springs, Colorado, initiated some inquiries into the accuracy of professional stock market forecasters over the period 1928–1932. This aroused his interest in fundamental economic research, which led him to offer his financial support toward the establishment of the Cowles Commission and to bear a significant share of the burden each year. Fortunately at the outset he encountered Harold T. Davis, a professor of mathematics at Indiana University whose interests included mathematical economics and statistics. Davis was to become an important figure in the

* The source material for this sketch consisted of published and unpublished records of the Cowles Commission, and even more important, the personal recollections of several of its leading members, past and present, with whom I was able to talk. I wish to acknowledge the generous assistance provided by these men. They are Alfred Cowles, Harold T. Davis, Charles F. Roos, Dickson H. Leavens, Theodore O. Yntema, Jacob Marschak, Tjalling C. Koopmans, and William B. Simpson. In addition, Ragnar Frisch gave helpful comments on preliminary drafts.

ALFRED COWLES

HAROLD T. DAVIS

founding of the Cowles Commission and in its progressive development through the years. Also important was the new Econometric Society, not yet two years old, which had been organized in 1930 by Irving Fisher of Yale University, Ragnar Frisch of the University of Oslo, Charles F. Roos of Cornell University, and others.

As early as 1912, while Fisher was vice-president of the American Association for the Advancement of Science, he had attempted to organize a society to promote research in quantitative and mathematical economics. Wesley C. Mitchell, Henry L. Moore, and a few others had been interested but they were too few, and for the time being nothing came of their vision.

In the spring of 1928, Frisch was in the United States under a Rockefeller Foundation grant. At Princeton University he met Roos, then a young mathematician who was a Fellow of the National Research Council and secretary of the rejuvenated Section K (economics, sociology, and statistics) of the American Association for the Advancement of Science. They found themselves in agreement that there was a need for bringing economics, mathematics, and statistics closer together for work in what has come to be called econometrics. Frisch had corresponded with several economists about the possibility of founding a society with this objective. He and Roos decided to solicit Fisher's support in organizing such a society, and Frisch went to see Fisher at Yale. In April, 1928, all three met at Fisher's home in New Haven for a weekend to discuss the idea further. Fisher, mindful of the outcome of his earlier attempt, was pessimistic. At length he said that if Frisch and Roos could name one hundred people in the world who would join a society established for the encouragement of econometric work and the exchange of econometric papers, he would become an enthusiastic partner in organizing such a society. They were very happy with this response, thinking that it would be a simple matter to list a hundred interested people. At first, the list virtually wrote itself, but then the going got harder, and after three days they had to give up with about seventy likely prospects. Fisher looked over their list and suggested about a dozen additional names. He was quite surprised that they had found so many, and he agreed that eighty justified going ahead. The three men drafted a letter of invitation to membership in the proposed society together with a request for the names of others who might be interested. The response to the invitation was excellent and nearly eighty more names were suggested.

The American Economic Association, the American Statistical Association, and Section K of the American Association for the Advancement of Science were to hold their first joint meeting in Cleveland, Ohio in December, 1930. Early in that year Fisher, Frisch (who was again in the United States, this time as a visiting professor at Yale), and Roos issued invitations to an organization meeting of the Econometric Society to be held in Cleveland on December 29, 1930. Twelve Americans and four Europeans attended. Joseph A. Schumpeter, then professor at the University of Bonn, was elected chairman of the meeting, and such was his enthusiasm that he himself made the motion whereby the new society was founded. Fisher was elected the first president and nine others were elected to the Council. A tentative constitution proposed by Frisch was adopted in principle, and was then phrased by a committee consisting of Frisch, Frederick C. Mills, and Roos. The constitution as amended appears with annotations in *Econometrica* for January, 1952, together with the names of those attending the organization meeting and of all officers and Council members since the beginning. The first meeting of the Society after its organization was held in September, 1931, at the University of Lausanne, Switzerland. The second meeting was held in Washington, D. C. in December, 1931, together with the meetings of other social science societies. During the year 1931 the Council elected 173 persons to charter membership, including all those who attended the organization meeting in Cleveland and all who attended the Lausanne meeting.

While the Econometric Society was being formed, Cowles was publishing the investment service of Cowles and Company in Colorado Springs. He became interested in comparing his forecasts with those of other professional forecasters, and in checking afterwards to see whether an investor would have done well to follow their advice. After the stock market crash in 1929 and the subsequent long decline of security prices amid optimistic predictions by many investment services, he began to feel that most of the forecasters were just guessing, himself included. In fact in 1931, he discontinued his forecasting service, explaining in his investment letter that he did not know enough about the forces that govern the fluctuations of business and the stock market. He further stated that he was going to try to find out more about these matters through research of his own before making any further forecasts.

In the summer of 1931, Cowles discussed his problems with a friend, Charles H. Boissevain, a Dutch biochemist with mathematical training who was head of the Colorado Foundation for Research in Tuberculosis. Boissevain thought that multiple-correlation analysis might be an effective tool for economic research and suggested to Cowles that he consult Davis, professor of mathematics at Indiana University, who spent his summers in Colorado Springs. Shortly thereafter Davis received a telephone call from Cowles, whose first question was whether it was possible to compute the multiple-correlation coefficient in a problem involving twenty-four variables. Davis replied that he didn't know why anyone would want to compute such a correlation coefficient, but a new method of performing such computations with the aid of Hollerith (IBM) punch-card computing machines had recently become available and he thought he could carry out the desired computations. Cowles enlisted Davis' services and acquired a Hollerith computer, and together he and Davis set to work finding correlation coefficients.

On becoming acquainted with the problems on which Cowles was working, Davis suggested to Cowles that he associate himself with the newly-formed Econometric Society because there he would find the men who by training and interest could be most helpful to him. Davis added that Cowles might enlist their aid on a continuing basis and advance his cause still further. He could offer to finance the publication of a journal for them and set up a research organization under their auspices with the resources and freedom for econometric research and publication. Cowles felt that this would be an effective way of securing first-class talent to work on the problems in which he had become interested.

Accordingly he wrote to Fisher, who was president of the Econometric Society and an old friend of his father and uncle from their undergraduate days at Yale, to propose the two projects. Fisher was delighted, for the Econometric Society was severely limited in scope by its poverty. During the first two years of its existence, 1931 and 1932, its activities consisted chiefly of small meetings at which papers were read and discussed. Because dues were very low, the Society simply could not afford more ambitious activities. Against this background, Cowles' proposal seemed like a godsend. In his excitement Fisher telephoned Roos, then permanent Secretary of the American Association for the Advancement of Science, to read him the letter. Roos was equally delighted; it seemed too good to be true. He asked whether Fisher thought it was a crank letter, and Fisher—fortunately—replied that he thought it was not. They invited Cowles to come East to discuss the matter and he accepted their invitation promptly. The three men met at Fisher's home in New Haven on a weekend in October, 1931. Cowles proposed starting with a budget of about $12,000 per year with larger amounts to follow if the venture met with success. Fisher and Roos, having satisfied themselves that Cowles meant what he said and was genuinely interested in econometric research, were enthusiastic about the scheme.

At about the same time Davis arranged with Thornton C. Fry, a mathematician with the Bell Telephone Laboratories and a charter member of the Econometric Society, to acquaint Cowles with several other members of the Society. A few days later, Cowles and Fry met in New York with Donald R. Belcher, chief statistician of the American Telephone and Telegraph Company; J. W. Glover, president of the Teachers' Insurance and Annuity Association of America; Harold Hotelling of Columbia University; and Walter A. Shewhart, statistician of the Bell Telephone Laboratories. Cowles discussed his proposal with them, and they assured him that in their judgment the Society would welcome the support as a "most fortunate opportunity."

Fisher and Roos, as president and secretary of the Econometric Society respectively, wrote a letter to the other members of the Council outlining Cowles' proposal and recommending its acceptance. The replies were for the most part favorable. However, some of the members of the Council became alarmed lest the Society's good name be harmed by its implication in a venture with a man who was willing to spend a considerable sum of money in order to accomplish they knew not what purposes of his own. The English and European members held a special meeting to discuss the question. They decided that they could not give their approval until a representative had come to the United States to meet Cowles and find out what his motives were. They chose Frisch as their representative and informed Fisher and Roos of their position. Fisher and Roos wrote as tactfully to Cowles as they could, explaining that his proposal was an important matter that might well have a profound effect on the future of the Society, and that the English and European members of the Council felt they would be able to give fuller support if Frisch were to come as their representative to talk with Cowles about the type of organization he had in mind. Cowles was favorably impressed by this cautious approach and responded by inviting Frisch to come as his guest to Colorado Springs. Frisch came and stayed for a week. As the two discussed the project from every viewpoint, Frisch became perfectly satisfied, as were Fisher and Roos, that Cowles was sincerely interested in econometric research. Before returning to Norway, Frisch met briefly with Fisher and Roos, and the three of them wrote a new letter to the Council of the

Econometric Society, reporting on Frisch's visit with Cowles and recommending that they accept the proposal.

This time, in January, 1932, the Council accepted. The agreement was as follows: Cowles would set up a research organization in Colorado Springs to be known as the Cowles Commission for Research in Economics; the Econometric Society would sponsor the Cowles Commission; the Cowles Commission would be guided by an Advisory Council appointed by and from the Econometric Society; and Cowles would underwrite the cost of publishing a journal for the Society. The earlier apprehensions of some of the Society's Council members, though understandable, turned out to be quite unfounded. Indeed, Cowles' generosity, farsightedness, fairness, and objectivity enabled the Cowles Commission to become established in its first few years as a responsible research organization, and have been among the most important sustaining factors in the achievements of the Cowles Commission ever since. Accordingly the Advisory Council was to play a progressively less active part in its affairs.

At first, however, even before the Cowles Commission was formally founded, the Advisory Council did take an active part in supervising its activities. The members of the Advisory Council, appointed in February of 1932 by the Council of the Society, were Fisher; Frisch; Arthur L. Bowley, professor at the London School of Economics; Mitchell, director of the National Bureau for Economic Research; and Carl Snyder of the Federal Reserve Bank of New York. They held their first meeting with Roos in Syracuse, New York, at the summer sessions of the Econometric Society in 1932; all members were present except Bowley. At this meeting it was decided that the first major project of the Cowles Commission should be the construction of indexes of stock prices, earnings, and dividends in the United States with proper adjustments for stock splits, rights, recapitalizations, etc., since any subsequent analytical work on the security market would require more adequate indexes than were then available.

On the 9th of September, 1932, the Cowles Commission for Research in Economics was formally chartered as a not-for-profit corporation in the State of Colorado. The original Articles of Incorporation 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."

Alfred Cowles was elected by the trustees as president of the Cowles Commission. A research laboratory and library were set up in the Mining Exchange Building in Colorado Springs, and good relations were established with the economics department of nearby Colorado College, of which Cowles was a trustee. In addition to Cowles, the initial research staff consisted of Davis, who was in charge of the statistical work; Frisch, who was a nonresident consultant; William F. C. Nelson, who was an economist; and Forrest Danson, who was a statistician. The latter two had been with Cowles in his investment firm before 1932. This group began work on the stock market indexes. Davis and Nelson collaborated in writing a textbook on statistics for economists based on the adaptation of a manuscript previously prepared by Davis. The initial budget was approximately $12,000 per year.

There remains one thread to be picked up. In February of 1932, after the Council of the Society had accepted Cowles' offer to underwrite the founding of a journal, the name *Econometrica* was chosen, Frisch was elected editor-in-chief, Nelson was chosen as assistant editor, and Cowles was chosen as circulation manager and also as treasurer of the Society. Upon its incorporation in September, 1932, the Cowles Commission became host to the archives of the Society, and the offices of the two organizations have been together ever since.

The first issue of *Econometrica* appeared in January, 1933. It contained an editorial by Frisch, an introductory article by Schumpeter, summaries of previous meetings of the

Society, and papers by René Roy, Shewhart, Jan Tinbergen, John B. Canning, and James Harvey Rogers. In introducing Econometrics to the reader, Schumpeter wrote:

"We do not impose any credo—scientific or otherwise—and we *have* no common credo beyond holding: first, that economics is a science, and secondly, that this science has one very important quantitative aspect. . . .

What we want to create is, first, a forum for econometric endeavor of all kinds wide enough to give ample scope to all possible views about our problems. . . . On this forum, which we think of as international, we want secondly to create a spirit and a habit of cooperation among men of different types of mind by means of discussions of concrete problems of a quantitative and, as far as may be, numerical character. . . . Confronted with clear-cut questions, most of us will, we hope, be found to be ready to accept the only competent judgment on, and the only relevant criterion of, scientific method, that is the judgment or criterion of the result. . . . Theoretic and 'factual' research will of themselves find their right proportions, and we may not unreasonably expect to agree in the end on the right kind of theory and the right kind of fact and the methods of treating them, not postulating anything about them by program, but evolving them, let us hope, by positive achievement."

That is the story of the founding of the Cowles Commission, the Econometric Society, and the journal *Econometrica*.

II. The early years in Colorado: 1932–1937

The first Cowles Commission product to attract widespread attention, both from businessmen and from professional economists, and still one of the best known of its publications, was a paper by Cowles entitled "Can Stock Market Forecasters Forecast?," published in *Econometrica* in July, 1933. A three-word abstract of this paper runs as follows: "It is doubtful." As mentioned earlier, Cowles had begun to suspect that many forecasters had no real skill and were in effect simply guessing, and he set out to test this hypothesis. He charted the weekly individual stock purchase recommendations of sixteen established financial services from 1928 to 1932, and found that if an investor had followed all of them, with equal initial amounts of capital allotted to each purchase of a stock, he would have come out making about one and a half per cent per year less than if he had invested in the stock market as a whole. He also calculated that if sixteen *random* series of weekly predictions were made, there was at least an even chance that one of them would lead to just as good results as the most profitable service actually did. He then checked the common stock investment records of twenty large fire insurance companies for the same period, and found that on the average they fell behind the market by slightly more than one per cent per year, while the best record among them was only slightly better than that of the best financial service. He then charted the forecasts of stock market level made by twenty-four financial publications from 1928 to 1932 and found that if an investor had followed all of them, again with equal amounts of initial capital allotted to each, he would have fallen behind the market average by about four per cent per year. Finally, he found that when twenty-four series of random forecasts were made by drawing cards from an appropriate deck, the best series of random forecasts was just as good as the best series of actual forecasts, while the worst series of random forecasts was better than any of the six worst series of actual forecasts. Far from refuting the hypothesis that stock market forecasters were operating according to chance rather than skill or insight, these results were quite consistent with it, except that the poorer actual forecasts seemed to be worse than would be expected on the basis of chance alone. The study pointed strongly to the need for more reliable knowledge upon which to base economic forecasts.

In the summer of 1934, Roos was research director of the National Recovery Administration of the United States government. He was beginning to think about where he would go next, for his opinion of the NRA's usefulness was very low, and he was in the process of writing a report for the NRA recommending that the Act under which it was created be allowed quietly to expire without renewal in 1935. He wrote to Cowles for a reference for a prospective new employer, and Cowles replied by offering him a position as the first director of research of the Cowles Commission. Upon hearing of this opportunity, Colorado College offered him a professorship in econometrics. He accepted the two positions, taking up his duties in September, 1934.

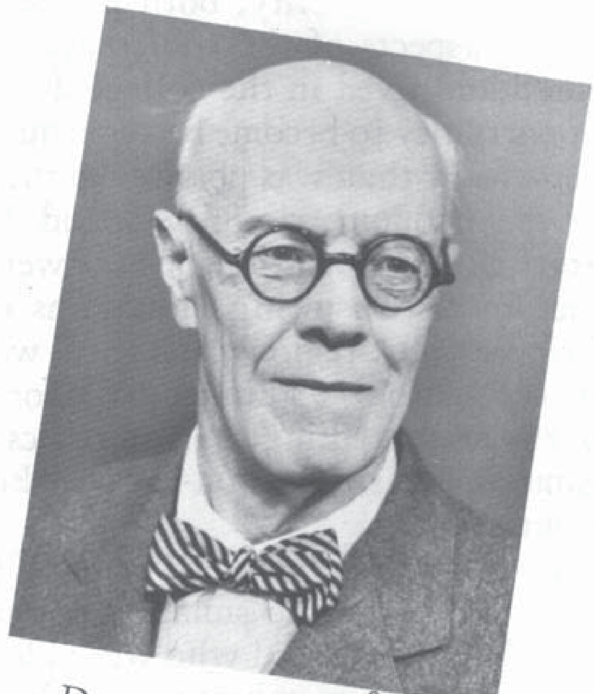
During Roos' term as director of research, the Cowles Commission published three volumes, including the first two in its series of monographs. The year 1934 saw the publication of Monograph 1, *Dynamic Economics*, a series of essays by Roos which he had completed before he came to the Cowles Commission. Next, in 1935, was the aforementioned textbook, *Elements of Statistics*, by Davis and Nelson. Third, in 1937, was Monograph 2, *NRA Economic Planning*, by Roos. This was an enlarged version of the adverse NRA report that he had prepared as NRA research director, which the NRA had declined to publish. (Roos writes that in 1935, after the Supreme Court declared the National Industrial Recovery Act unconstitutional, House Speaker Rayburn and Senate President Garner requested and received manuscript copies of the report. Thus, it may well have contributed to the willingness of Congress to let the NRA idea die with no legislative attempt to revive it.) In preparation were three other monographs: the stock market indexes, by Cowles and other members of the staff; a study of the monetary use of silver, by Dickson H. Leavens, a new staff member; and a study of methods of analysis of economic time series, by Davis.

One of the most significant contributions of the Cowles Commission in its early days was made through its summer conferences, of which the first was in 1935. In June of that year, the Econometric Society had held a meeting at Colorado College and for various reasons several economists and statisticians remained in Colorado Springs for the following week, including Davis, Hotelling, August Loesch, Isadore Lubin, Shewhart, and Snyder. Roos suggested that they have a few informal meetings, at which anyone who wished might discuss his current research work and invite help on the problems involved. The others took up the idea and during the week ten informal papers were presented, with considerably more free and substantive discussion than would have been possible in a larger and more formal group listening to finished work. This short session was such a success that Cowles and Roos decided to plan a similar conference for the following summer. They wrote up the proceedings and sent them out as part of an invitation to the second conference to be held at Colorado College in the summer of 1936. They invited thirty-two people to speak, in the hope of getting about ten acceptances, and were surprised when they received twenty, including three from staff members. As a result, the conference was extended to five weeks. The proceedings were written up, and published by Colorado College. Among the participants who were not connected with the Cowles Commission were Irving Fisher, R. A. Fisher of University College, London, Corrado Gini of the University of Rome, Shewhart, and E. J. Working of the University of Illinois.

The setting of Colorado Springs was conducive to a pleasant informality, both in the econometric and recreational aspects of the conferences. Because the group was small and lived in the college dormitories, people had the opportunity to become better acquainted professionally and personally than was possible in the more crowded meetings of the Econometric Society and American Economic and Statistical Associations. There were hikes, drives, and picnics in the inspiring mountains of the Pike's Peak and Cripple Creek regions, and there was even a play, presented by the visitors at a dinner given for the staff, in which some of the more striking characteristics of the staff and speakers came in for some good-natured but pointed and amusing scrutiny.

Four new people joined the staff while Roos was director of research. In the summer of 1935, Herbert E. Jones, a gifted young invalid who was able to work only part time, became a staff member; in 1936 he was appointed as a Fellow of the Cowles Commission. His training was in mathematics and in engineering. He worked on the stock index project and on problems of fitting equations to time series.

CHARLES ROOS



Stone Photo

DICKSON H. LEAVENS

Leavens, a former member of the statistics research staff of the Harvard Business School, first became acquainted with the Cowles Commission through the summer conference of 1936, at which he presented a paper on gold and silver. Nelson, who had been assistant editor of *Econometrica*, had died suddenly in May, 1936, and Cowles was looking for a man to take over the editorial work of the journal and the Cowles Commission, manage the office, and share in the research work. When Leavens was summoned to Cowles' office toward the end of the conference, just after the performance of the play, he must have wondered uneasily whether it had given offense, for he had written it himself. However, if he had any apprehensions they vanished, for what Cowles wanted was to ask whether he knew any graduates of the Harvard Business School who might be interested in the opening at the Cowles Commission, or, for that matter, whether he would be interested in it himself. He was, and accordingly moved to Colorado Springs in September, 1936. His devoted and unceasing attention to the many administrative aspects of the Cowles Commission's activities up to his retirement in 1947, was an essential factor in the Commission's effectiveness.

Gerhard Tintner of Vienna accepted an invitation to become a Fellow of the Cowles Commission in 1936. Edward N. Chapman of Colorado Springs also joined the staff that year.

At the end of January, 1937, Roos resigned as research director of the Cowles Commission to become research director of the Mercer-Allied Corporation in New York. Shortly thereafter he embarked in business for himself with a new economic forecasting agency, The Econometric Institute, which he heads today.

III. The later years in Colorado: 1937–1939

Upon Roos' departure, Davis took a leave of absence from Indiana University to become acting director of research of the Cowles Commission. He held this position from February until September, 1937, at which time he left to become professor of mathematics at Northwestern University. He continued to spend his summers with the Cowles Commission for several years.

The summer conference of 1937 was used partly as a recruiting ground for a new director of research, and among the prospects invited were Frisch; Jacob Marschak, then director of the Institute of Statistics at Oxford University; and Theodore O. Yntema, then professor in the School of Business at the University of Chicago. None of them was inclined to accept the position, however, for Colorado Springs was too isolated from the large academic centers to be attractive. For the next two years, until September, 1939, there was no official director of research. However, both Roos and Davis met with Cowles from time to time to advise him on the research program.

During this period, the third and fourth monographs were published, and two more were in process. Monograph 3, *Common-Stock Indexes*, by Cowles and Associates, appeared in August, 1938, and a second edition was published in 1939. It presents the results of the extensive gathering and compiling of data on the stock market, and contains indexes of prices with adjustments for rights, splits, dividends, etc., and of yield expectations, yields, dividends, earnings-price ratios, and earnings for a large group of common stocks comprising ninety to a hundred per cent of the value of all those listed on the New York Stock Exchange, 1871–1939 and for sixty-eight subgroups of these stocks. Monograph 4, *Silver Money*, by Leavens, appeared in March, 1939. It traces the history of the monetary use of silver and analyzes recent developments in some detail. Davis continued to work on his time-series analysis, and Tintner proceeded with another monograph on time series, although he left the Cowles Commission in September, 1937, to join the faculty of Iowa State College.

The summer conferences continued vigorously during the Colorado period. The proceedings after 1936 were published by the Cowles Commission itself, under the editorship of Leavens. Among the participants in 1937–1939 who were not previously connected with the Cowles Commission were R. G. D. Allen of the London School of Economics, Louis Bean and Mordecai Ezekiel of the U. S. Department of Agriculture, Fry, Trygve Haavelmo of the University of Oslo, A. P. Lerner of the London School of Economics, Francis McIntyre of Stanford University, Horst Mendershausen of the University of Geneva, Rogers of Yale University, René Roy of the University of Paris, Henry Schultz of the University of Chicago, Abraham Wald of Vienna, Holbrook Working of Stanford University, and (as already mentioned) Frisch, Marschak, and Yntema. Roy came in 1938 as the official representative of the Government of the French Republic in honor of the one hundredth anniversary of the publication of Coúrnot's pioneering *Récherches*. Acting on a suggestion by Davis, the Cowles Commission had invited the French Government to send a representative. This invitation was accepted and Roy was designated.

Additional staff changes were as follows. McIntyre came to the Cowles Commission and the faculty of Colorado College in the fall of 1937, by way of the University of Chicago, Stanford University, and the 1937 summer conference. Two new Fellows were appointed for the year 1938–1939, Mendershausen and Wald. Both had contributed to the 1938 summer conference.

IV. The move to Chicago: 1939

Colorado Springs had many advantages as a location for the Cowles Commission, but its geographical position with respect to other centers of economic and statistical research was certainly not one of them. This disadvantage was underlined by the failure to secure as director of research any of the three men who ranked as first choice when Roos departed in 1937. For the next two years, there was continually in the background the question of whether the Cowles Commission ought to move to a more suitable location. Several universities showed interest in providing it with a new home, including California at Los Angeles, Yale, and Northwestern, but no definite arrangements were made. Then in January, 1939, the issue was forced by the death of Cowles' father; as a result it became necessary for Cowles to make his headquarters in Chicago. Northwestern thus became a clear favorite over U. C. L. A. and Yale, but in the end the Cowles Commission did not move to any of these universities.

Schultz had begun to build a strong tradition in mathematical economics and econometrics at the University of Chicago. Then his work was cut short by his death in an automobile accident in November, 1938, and the tradition was left without its most vigorous figure. Thus the University was in a position from which the possibility of adopting a group such as the Cowles Commission appeared particularly attractive. Likewise, the University was an ideal environment for the Cowles Commission, so much so that Cowles decided to see what could be done. Through Laird Bell, his family's attorney and a trustee (now chairman of the trustees) of the University, he met Robert M. Hutchins, then President of the University, and discussed the idea with him. In the early spring of 1939, Cowles and Hutchins worked out a loose and informal relationship between the Cowles Commission and the University, under which the University provided a suite of four offices rent free on the fourth floor of the Social Science Research Building overlooking the Midway; Cowles Commission staff members were granted certain University privileges; Yntema, professor in the School of Business, became director of research of the Cowles Commission; and Jacob Viner, professor of economics, became the sixth member of the Advisory Council of the Cowles Commission. The move was made in September, 1939, the Econometric Society's offices coming along too. On September 29, 1939, the Cowles Commission was chartered as a not-for-profit corporation in the State of Illinois, and the Colorado corporation was dissolved soon afterward.

Several members of the University faculty became part-time staff members of the Cowles Commission when it came to Chicago: Joel Dean of the School of Business, and H. Gregg Lewis, Jacob Mosak, and Oscar Lange of the Economics Department. The Colorado staff came to Chicago with five exceptions: McIntyre took a leave of absence to teach at Stanford and later resigned to accept a position at Indiana; Wald took a position at Columbia where he later became professor of mathematical statistics; Mendershausen joined the faculty of Colorado College; and Chapman and Jones also remained behind in Colorado Springs. Chapman has since devoted himself chiefly to medical research and public health work. Jones died in 1942, when his long illness ended what had promised to be a brilliant career. A memorial note to him in the 1941 Annual Report of the Cowles Commission contains these words:

> "During his brief period of active participation in the work of the Cowles Commission, Herbert Jones made a number of significant contributions to statistical and econometric science. Trained in electrical engineering and equipped with an excellent understanding of fundamental mathematics, he brought to bear upon the problems of the Commission a keen and analytical point of view. His breadth of interest is readily observed from the variety and difficulty of the studies which he made. . . .
>
> In all of these studies Herbert Jones proved himself to be a young man with an exceptional imagination and an analytical power far beyond the average. Perhaps there is no higher encomium possible than to repeat what was said about the remarkable English mathematician, W. K. Clifford, who died very young: 'If he had lived we might have known something.' "

V. The early years at Chicago: 1940–1942

During 1940–1942, the fifth and sixth monographs were published. Monograph 5, *The Variate Difference Method*, by Tintner, appeared in February, 1940. It analyzes the successive differences of time series, (i.e., the year-to-year differences between adjacent numbers in a series, then the differences between adjacent differences, etc.) and uses them as the basis of a method for deciding whether one should use a straight line, or a quadratic, or a cubic, etc.

for fitting the trend of a particular time series. Monograph 6, *Analysis of Economic Time Series*, by Davis, appeared in December, 1941. It is a survey of many methods available for dealing with time series, with applications to economic situations.

Staff changes during this period were relatively few. John H. Smith, a statistician in the School of Business, joined the staff in September, 1941, and stayed until the summer of 1942 when he went to the Bureau of Labor Statistics. Leonid Hurwicz joined the staff in January, 1942, as an assistant to Lange. Aside from Cowles and Davis, Hurwicz is the only present member of the Cowles Commission staff whose association with it dates back ten years.

A summer conference was held in Colorado Springs in 1940, even though the Cowles Commission had moved to Chicago by then. Among the new participants were W. E. Deming, senior mathematician of the U. S. Census Bureau; E. A. Goldenweiser, chairman of the Board of Governors of the Federal Reserve System; Wassily Leontief of Harvard University; and Paul A. Samuelson of the Massachusetts Institute of Technology. However, with the library and computing equipment no longer available to the participants of the conference, with the added expense of moving the staff to Colorado Springs, and with the interruption of work in the office at Chicago, it was not thought worth while to continue the summer conferences.

The Annual Report of the Cowles Commission for 1940 begins with the words, "Among economic problems none is more important than unemployment of labor and other resources." It continues, "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." There was something almost prophetic in the last statement, for the compelling reasons presented themselves within a year in the form of war. As a result, the Cowles Commission turned early in 1942 to a study of wartime price control and rationing, with a view to appraising the possible policies with respect to them and the administrative devices that might be used for implementing them. Three parts of the study were planned: a theoretical analysis of the problem, an analysis of pertinent available statistical material on prices and wages, and a series of personal interviews with both buyers and sellers to learn about their actual behavior under price controls and rationing. The project was undertaken jointly with the Committee on Price Determination. It was organized under the auspices of the Price Conference of the National Bureau of Economic Research and was under the direction of Yntema and Hurwicz. A monograph was expected to be published by 1944.

Until the Cowles Commission came to Chicago, all of its funds had been provided by Cowles and his family. In 1940, another source of funds made its appearance. The Social Science Research Committee of the University of Chicago made grants so that several members of the Cowles Commission staff might employ research assistants; Melvin Reder, now associate professor of economics at Stanford University, was one of the first of these assistants. In 1942, the Cowles Commission received its first grant from the Rockefeller Foundation, for conducting the price control study. The National Bureau also contributed to the expenses of the price control study.

By 1942, something like half of the staff of the Cowles Commission had been drawn into work directly or indirectly connected with the war effort. Dean was on leave as director of gasoline rationing in the OPA in Washington. Hurwicz was teaching mathematics and statistics in the U. S. Army Signal Corps and Meteorology training programs at the Illinois Institute of Technology and the University of Chicago. Mosak was on leave as head of a section of the research department of the OPA in Washington. Yntema had been on leave in Washington as head of economics and statistics in the division of industrial materials of the Defense Commission in 1940, and was on part-time leave during the spring and summer of 1942 as a consultant to the War Shipping Administration.

Schneider Photo

JACOB MARSCHAK

Moffett Photo

THEODORE O. YNTEMA

LEONID HURWICZ

In November, 1942, Yntema resigned as director of research of the Cowles Commission and took a leave of absence, to become research director of the newly created Committee on Economic Development and organize a study of conditions favorable and unfavorable to full employment after the war.

VI. Simultaneous developments: 1943–1948

In 1939, Marschak left Oxford University to come to the United States as professor of economics at the New School for Social Research in New York. Just before taking up his duties there, he renewed his acquaintance with the Cowles Commission at the summer conference of 1939. In 1942, when Yntema made known his intention to resign, Cowles for the second time offered Marschak the position of director of research of the Cowles Commission.

At the same time, the University of Chicago was again without a senior mathematical economist, for Lange had taken a leave of absence to become visting professor at Columbia for the years 1942–1944 and it was uncertain whether he would return. Accordingly the University offered Marschak a professorship in economics. He accepted, and came to the University and the Cowles Commission at the beginning of 1943.

During Marschak's term as director of research, which extended until July, 1948, the Cowles Commission underwent fundamental changes in several directions. The administrative structure of its relationship to the University of Chicago was reorganized. Its financial support became more extensive and more diversified. There were many changes in its staff, including the arrivals of several of the present staff. The means of making its members and their work known to the University community and to other economists and statisticians were extended. Its research program underwent a major reorientation that determined its essential character for the second ten years of its life. These developments will now be discussed in turn.

The University of Chicago handled its funds for research in the social sciences through its Social Science Research Committee. Applications for funds from outside sources such as the Rockefeller Foundation were also channeled through this committee. In the summer of 1943, to help secure better co-ordination of the research programs and fund-raising activities of the various departments and affiliates of the University, the Cowles Commission replaced the dormant Advisory Council by a new and more active Advisory Committee representing various aspects of the University's interest in the work of the Cowles Commission. The Advisory Committee originally was composed of Walter Bartky (mathematics), Garfield V. Cox (business), Simeon E. Leland (economics, chairman), Theodore W. Schultz and Viner (economics), and Louis Wirth (Social Science Research Committee). Later Neil H. Jacoby (business) and Philip Hauser (Social Science Research Committee) joined it as replacements for some of the others. It periodically received and discussed the progress reports of the research director. It also reviewed the Cowles Commission's applications to the Rockefeller Foundation for grants-in-aid. In addition to grants received from Cowles and his family, the Rockefeller Foundation, and the Social Science Research Committee of the University, the Cowles Commission also received indirect aid through the payment of Marschak's salary (and later Koopmans' as well) by the University, and through grants from the Guggenheim Foundation, the Rockefeller Foundation, and the Social Science Research Council to persons working at the Cowles Commission as guests on problems related to its program.

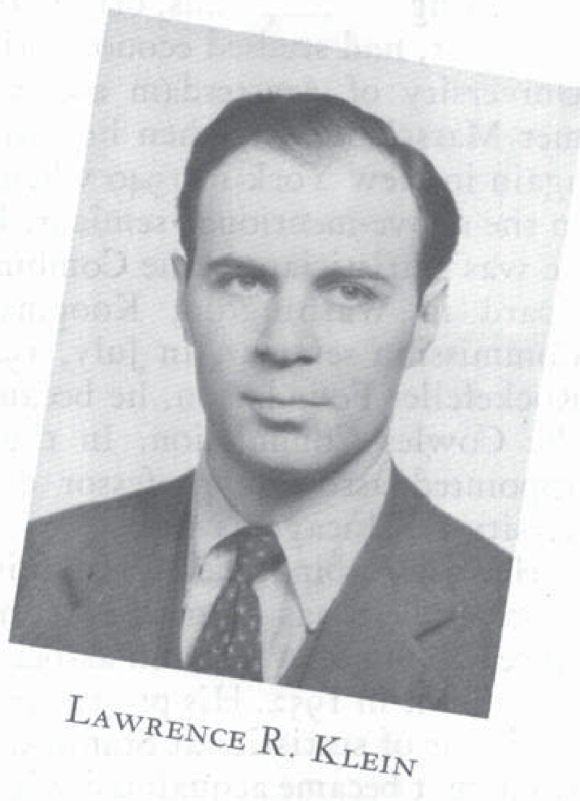
There were many changes in the staff during the five and a half years from 1943 to the middle of 1948; it is impractical to detail them all here. Those staff members and guests who came during this period and are no longer associated with the Cowles Commission are mentioned in connection with the research program a little further on, or in the biographical sketches.

Haavelmo had been a student and collaborator of Frisch at the University Institute of Economics in Oslo and the University of Oslo, and had worked at several American universities under fellowships of the American-Scandinavian and Rockefeller Foundations. From 1942 till the end of 1945, he was attached to Norwegian government agencies in New York and Washington. He participated with Marschak, Wald, and others in a small econometrics seminar that met regularly in New York on weekends during 1940–1942. Marschak asked Haavelmo to become a research associate of the Cowles Commission in July, 1943. He accepted although he was not always in residence. He moved to Chicago in January, 1946, where he also became a member of the Agricultural Economics Research Group in the Department of Economics. He has been associated with the Cowles Commission as a research consultant (a new category of staff members, comprising those active but not in residence) ever since his return to Norway in March, 1947. He is now professor of economics at the University of Oslo.

Tjalling C. Koopmans, originally trained as a theoretical physicist, had studied econometrics with Tinbergen at the University of Amsterdam and with Frisch in Oslo. He met Marschak first when he visited Oxford in 1938, and again in New York in 1940 when he became a participant in the above-mentioned seminar. In December, 1943, while he was statistician to the Combined Shipping Adjustment Board in Washington, Koopmans addressed a Cowles Commission seminar. In July, 1944, under a grant of the Rockefeller Foundation, he became a research associate of the Cowles Commission. In the spring of 1946, he was appointed associate professor of economics at the University of Chicago as well.

Herman Rubin joined the staff in July, 1944, as a research assistant. He has been an intermittent staff member ever since, becoming a research associate in 1946 and a research consultant in 1952. His present position is that of assistant professor of statistics at Stanford University. Lawrence R. Klein first became acquainted with the Cowles Commission at a meeting of the Econometric Society in Cleveland in

TRYVE HAAVELMO



LAWRENCE R. KLEIN

September, 1944. He was a research associate from November, 1944, until July, 1947, and has recently become a research consultant. He is at present a research associate both of the National Bureau of Economic Research and of the Survey Research Center at the University of Michigan. Theodore W. Anderson, Jr. was a research associate from November, 1945, to September, 1946, when he became a research consultant. He is now an associate professor of mathematical statistics at Columbia University, and editor of the *Annals of Mathematical Statistics*. Kenneth J. Arrow was a research associate from April, 1947 to July, 1949, and assistant professor of economics at the University of Chicago from October, 1948 to July, 1949. He is now a research consultant, and an associate professor of economics and statistics at Stanford University. Herman Chernoff joined the staff as a research assistant in July, 1947, and was a research associate from the spring of 1948 until September, 1949. Among his other activities was the supervision of the Cowles Commission computing staff. He is at present a research consultant, and an associate professor at Stanford University. Herbert Simon, who is professor of administration and chairman of the Department of Industrial Management at Carnegie Institute of Technology, became a research consultant in April, 1947, while he was professor and chairman of the Department of Political Science at Illinois Institute of Technology.

During the early years, the Cowles Commission made itself known through the summer conferences and through its monographs, of which six had appeared when Marschak became director of research in 1943. With the summer conferences gone, and the publication of a monograph being something less than an everyday event, the output of materials failed signally to indicate the amount or quality of research being performed. From the beginning, the staff members had published numerous articles in professional journals each year, and had presented papers at meetings of the professional societies. Marschak introduced two new devices to help bring the light out from under the bushel. First, beginning in the summer of

1943, was a series of seminars on various topics in econometrics, presented every three or four weeks at the University of Chicago by staff members or by various visitors to the Cowles Commission. The seminars were soon promoted to a regular bi-weekly schedule (except in summer) and are still conducted today.

Second, beginning late in 1943, the Cowles Commission Papers, New Series were initiated. These are reprints of the more important published articles of staff members, bound in paper either singly or in groups of two or three related articles. They are distributed free of charge to a selected list of interested people and to others who write and request individual copies. By the middle of 1948, New Series Papers Nos. 1–27 had been prepared and distributed.

During 1943–1948 the seventh, eighth, and ninth monographs were published. Monograph 7, *General-Equilibrium Theory in International Trade*, by Mosak, appeared in 1944. It extends the modern theory of general equilibrium and comparative statics, as formulated by J. R. Hicks, Allen, and others, to the field of international trade. Monograph 8, *Price Flexibility and Employment*, by Lange, also appeared in 1944. It first extends the modern theory of general equilibrium to treat money as distinct from other goods, and then analyzes the roles of price flexibility and of substitution between money and other goods in the determination of the level of employment. Monograph 9, *Price Control and Business*, describing the results of the price control study initiated in 1942, was published in 1945. Its author was George Katona, whose journalistic and research experience in both economics and psychology well equipped him to write it. He was a research associate of the Cowles Commission and in charge of the study from January, 1943, until it was finished at the end of 1944. He is currently the Program Director of the Survey Research Center and professor of economics and psychology at the University of Michigan.

The reorientation which Marschak and his new staff wrought in the Cowles Commission's research program is sketched in the following passage from the Annual Report for 1943, the first year during which Marschak was director of research (the passage refers to three new studies that were started during the year):

"The method of the studies . . . is conditioned by the following four characteristics of economic data and economic theory: (a) the theory is a system of simultaneous equations, not a single equation; (b) some or all of these equations include "random" terms, reflecting the influence of numerous erratic causes in addition to the few "systematic" ones; (c) many data are given in the form of time series, subsequent events being dependent on preceding ones; (d) many published data refer to aggregates rather than to single individuals. The statistical tools developed for application in the older empirical sciences are not always adequate to meet all these conditions, and much new mathematical work is needed. To develop and improve suitable methods seems, at the present state of our knowledge, at least as important as to obtain immediate results. Accordingly, the Commission has planned the publication of studies on the general theory of economic measurements. . . . It is planned to continue these methodological studies systematically. The available results of mathematical analysis are currently applied and tried out in econometric investigations; conversely, new situations arising in the course of practical work present new problems to the mathematician. It is intended to make this hand-in-hand work the basis of the Commission's activities."

The four characteristics referred to were central to the new program of the Cowles Commission. Accordingly, brief remarks on each one follow.

"The theory is a system of simultaneous equations, not a single equation." Consider for example the theory of the determination of the market price of a commodity. The quantity demanded by buyers depends on the price, rising when the price falls. The quantity offered by sellers also depends on price, falling when price falls. And the price adjusts itself in response to the higgling and bargaining of the market place, being driven up if there is excess demand and down if there is excess supply, until the quantities demanded and supplied are the same. Here is exhibited a system of three equations: the demand equation, the supply equation, and the price adjustment equation. There are three variables: quantity demanded, quantity supplied, and price.

It is important to notice that *none* of the three equations by itself can specify the level of even *one* of the three variables, but together the system specifies the level of all three. It is also important to notice that the number of variables to be explained by the theory is the same as the number of equations, i.e., three. These are called *endogenous* variables. There may be other variables (e.g., an excise tax) whose values are not specified by the theory but are assumed to be determined independently of the relationships described by the theory. Such variables, taken as a starting point of the explanation offered by the theory, are called *exogenous* variables. Exogenous variables are typically used to represent changes in policy or in the underlying economic environment which can effect the operation of the economic relationships described by the theory.

There do exist simple economic theories that consist of one equation only (such as a theory that relates the amount of tax receipts that will be collected under a given revenue act to the national income). But in the most interesting and important problems, the theory that is relevant is typically a system of several simultaneous equations.

"Some or all of these equations contain 'random' terms, reflecting the influence of numerous erratic causes in addition to the few 'systematic' ones." Except for equations expressing definitions, which of course must hold exactly, no one has ever found a numerical theory, i.e. an equation, that fits the relevant facts exactly. Many equations have been found that fit the relevant facts approximately, with errors or deviations that are sometimes positive and sometimes negative. This is true of the "exact" sciences as well as of economics, the chief difference being that in economics the deviations are usually not negligibly small. Accordingly, it is convenient to introduce them explicitly into each equation (except definitions) in the form of an extra term added on at the end, the value of which changes with each observation so that the equation always remains exactly true. It is assumed that the values of the deviations in any equation are determined as if by chance, as if drawn at random from a large jar containing tags with a number stamped on each one, some negative and some positive. Such variables are called "random" or "stochastic" variables. The use of random variables here is quite realistic, even though it may appear to be somewhat artificial. Each of the major factors bearing on a particular economic relationship is presumably accounted for explicitly by a separate variable, so that only the minor factors are left to be thrown into the random term, and the cumulative effect of a large number of small unrelated causes almost always acts as if it really were random.

"Many data are given in the form of time series, subsequent events being dependent on the preceding ones." The vast majority of available statistics on prices, wages, production, income and its components, inventories, etc., are published in chronological series covering successive weeks or months or years; such series are known as time series. They are important because the economy never starts with a clean slate but is always conditioned by what has happened before, and because many theories attempt to explain economic behavior through time. In addition, cross-section data, pertaining to different families, firms, industries, regions, etc. at a single point of time, are provided by surveys and censuses. The methods of treating the two types of data are essentially the same; however, there are some differences which will not be discussed here.

"Many published data refer to aggregates rather than to single individuals." This is clear from an examination of the national income statistics or almost any other familiar economic data. It becomes important when it is realized that most economic theory pertains to individual firms or families, and that there is accordingly a gap to be bridged if the published data are to be used systematically to test economic theory.

It should be noted that all of the four characteristics of economic theory and data were well-known for many years before the Cowles Commission began its research. Walras explicitly introduced systems of simultaneous equations into economics in 1874. Although the concept of a random variable did not appear as such in economic theory until fairly recently, it nevertheless lurks at the base of all attempts to fit equations to data—in fact, the theory of correlation and regression was built upon it in the last quarter of the nineteenth century. Time series and aggregated data have been with us for centuries. It cannot be said that the Cowles Commission contributed to the discovery of any of these important characteristics.

What *can* be said is that the men who were members of the Cowles Commission staff in the early years of Marschak's directorship, realizing that traditional statistical methods are by design unsuited to problems involving systems of simultaneous equations with random components, were among the first to devise new methods that are more suitable. Frisch had published a paper in the *Nordic Statistical Journal* in 1929 entitled "Correlation and Scatter in Statistical Variables" and an amplified version in 1934 entitled *Statistical Confluence Analysis by Means of Complete Regression Systems*, in which he foresaw and dealt with some of the difficulties that arise in regression and correlation analysis where "multicollinearity" exists, i.e., where there are other equations connecting the variables in question, in addition to the equation being studied. Then in January, 1943, in *Econometrica* there appeared Haavelmo's ground-breaking paper, "The Statistical Implications of a System of Simultaneous Equations." Although this paper is scarcely twelve pages long, it contains the beginning of some of the Cowles Commission's most important subsequent research. In particular, Haavelmo considered a three-equation theory of national income, and examined the consequences of fitting one of its equations to observed time-series data by means of the traditional "least-squares" method of regression analysis. (The term "least-squares" is used because this method selects as the best-fitting line the one that produces the smallest sum of squared deviations of the observed points from the line.)

The statistical problem in fitting a line to a set of observed points is that of deriving from observed data estimates of the numerical values of the unknown constants (parameters, as econometricians call them) in the equation of the line. For example, in Haavelmo's consumption equation, the parameters to be estimated are α and β, and the equation is: national consumption expenditure in any year equals α times national income in that year, plus β, plus a random disturbance. Because of the random components, it is impossible to find the *exact* values of the parameters. However, a very powerful general method of estimating unknown parameters, called the maximum-likelihood method, was devised by R. A. Fisher just after the turn of the century. It is difficult to describe clearly in nontechnical language, but it can be likened to the principle of trying to decide a question by considering which alternative, if true, would be most likely to have produced the evidence at hand. If Cassio and Desdemona were not in fact lovers, the chance of her handkerchief coming into his possession would be smaller than if they were; hence on the evidence of the handkerchief, their guilt is more likely than their innocence. (This ironic example illustrates the importance of making sure that the events in question are random and the importance of taking into account all available additional information, such as Desdemona's veracity as opposed to Iago's.) The maximum-likelihood estimate of a parameter is that value of the parameter which, among all possible values, would be most likely to give rise to the data actually observed. The merit of the maximum-likelihood method is that in a wide class of cases, judged by three accepted statistical criteria (known as unbiasedness in large samples, consistency, and efficiency), it yields the best possible large-sample estimates of unknown parameters.

It so happens that if an equation satisfying certain conditions is not a member of a system of equations, the maximum-likelihood estimates of its parameters are identical with the least-squares estimates; thus the least-squares estimates are the best estimates for such an equation. This had been known for a long time before the Cowles Commission came on the scene. Now to return to Haavelmo: he showed how to calculate the maximum-likelihood estimates of the parameters of his consumption equation and showed that they are *different* from the least-squares estimates precisely because the equation *is* a member of a system of simultaneous equations. Thus, the least-squares estimates are not generally the best estimates for an equation that belongs to a system. In fact, Haavelmo showed later that they are biased and inconsistent.

Before one attempts to calculate estimates of the parameters in a system of equations, or in a *model* (as economists say), it is well to pause and ask whether the observed data can really convey any information about the parameters of the model in question. Consider, for example, a two-equation model containing a demand and a supply curve for a commodity, each relating market price to quantity sold, with no other variables. The very best that can be expected from the data here is that they will locate exactly the intersection point of the two curves (random fluctuations would mean that the location could only be estimated). But even if the intersection is exactly located, no light is thrown on the shapes and directions of the curves, because there are infinitely many pairs of curves relating price to quantity that *could* intersect at the observed point. In this case, there is more than one set of parameters consistent with the observations and hence the observations cannot determine the values of the parameters.

This property of a two-variable supply-and-demand model had been commented on by many economists, including E. J. Working, Henry Schultz, and Haavelmo. Koopmans initiated a systematic study of such problems, which he called *identification* problems because the aim is to identify the true values of the parameters among all the possible sets of values consistent with the data and with the known or assumed properties of the model. An equation of a model is declared to be *identifiable* in that model if, given a sufficient (possibly infinite) number of observations of the variables, it would be possible to find one and only one set of parameters for it that are consistent with both the model and the observations. The equations of the simple supply-and-demand model of the preceding paragraph are thus not identifiable.

There are various properties which, if known or assumed a priori about a system, will produce identifiability. For example, if it is known above that the demand curve remains fixed while the supply curve shifts, then all price-quantity observations must lie *on* the demand curve, and the shifts in the supply curve will necessarily trace out the demand curve. Then the demand curve is identifiable, but the supply curve is not. However, for most commodities, the demand curve is not known to be either more or less free from random shifts than the supply curve, so that this kind of a priori information is not typically available.

Information *is* typically available a priori as to what variables appear in each equation; such information is derived from economic theory, from previous observation, or both. For example, we know that most demand curves depend on income as well as price, so that if income rises then larger quantities of goods are demanded at the same price. This is the most frequently available and often the most reliable type of a priori information about theoretical models. Koopmans and Rubin worked out criteria using this type of information to determine the identifiability of equations in linear systems. It is important to note that the determination can be made before any data are observed at all. This is of great importance, for it is futile to try to estimate the parameters in unidentifiable equations. Hurwicz and Wald further clarified the identification problem in logical and mathematical terms.

For several years one part of the Cowles Commission staff, including T. W. Anderson, Meyer A. Girshick, Haavelmo, Hurwicz, Koopmans, R. B. Leipnik, and Rubin, worked on the development of identifiability criteria and of methods for obtaining consistent estimates of the parameters in systems of equations with random elements. In January, 1945, the Cowles Commission held a research conference on the statistical problems arising out of economic theories that are systems of simultaneous equations. The proceedings of this conference were revised and enlarged, becoming Monograph 10, *Statistical Inference in Dynamic Economic Models*, edited by Koopmans.

Contributor revisions, editorial efforts toward uniformity in notation, and exploration of alternative printing techniques all combined to delay the appearance of the monograph until 1950. By that time papers on related topics by several of its contributors had appeared in various journals, and had been included in the Cowles Commission Papers, New Series. It is the most difficult and technical of all the Cowles Commission monographs, but it is one of the most important because it presents the fruits of several years of statistical research in a field where the Cowles Commission has been a pioneer. In reviewing it for the *Review of the International Statistical Institute* in 1950, R. G. D. Allen referred to the 1945 research conference and wrote:

"A comprehensive report of their results has been long promised and eagerly awaited. . . . More than half the volume is taken up with an introductory essay by J. Marschak and the main paper on simultaneous equation systems in dynamic economics, contributed by T. C. Koopmans with the assistance of H. Rubin and R. B. Leipnik. Marschak's introduction leads into the main problems (identification and estimation) of stochastic models by considering the simple case of non-stochastic models, a good expository device. Koopmans' treatment in the main paper, again, is to start with a general survey of problems, before passing to a detailed development, first, of the problem of identification of economic relations and, then, of the derivation of maximum-likelihood estimates of the parameters in the relations. . . . Everyone seriously interested in econometrics should make the effort necessary to read, at least, Marschak's introduction and Koopmans' main contribution. From these, it is evident that the method of simultaneous equations is potentially of great value in dynamic economics, that not all the theoretical problems are yet solved, and that the decisive tests of the method in its applications have still to be made."

While the volume sets a new standard in adapting statistical methods to econometric analysis, further difficulties remain to be overcome in this area. In reviewing it for the *American Economic Review* in 1952, Guy H. Orcutt of Harvard University wrote:

"For all practical purposes, the models dealt with are restricted to linear systems of difference equations. . . . Besides an extensive treatment of the identification problem with respect to such models, estimating their parameters from observational data also represents a problem central to this volume. While greatly impressed with the skill shown in attempting to handle this problem, this reviewer has many misgivings as to the applicability of the results obtained to problems of estimation facing the economist. These misgivings do not stem from any discovery of error in the deductive logical processes carried out, but rather in a failure to accept the premises as being realistic and the large sample characteristics of the estimators as applying to small samples."

In addition to Marschak's introductory survey and the long paper on estimation by Koopmans, Rubin, and Leipnik, there are papers on related problems by R.L. Anderson, T. W. Anderson, Jr., Haavelmo, Hotelling, Hurwicz, Koopmans, H. B. Mann, Rubin, and Wald. T. W. Anderson's contribution is a short summary of a paper by himself and Rubin, published in the *Annals of Mathematical Statistics* in March, 1949, and December, 1950, deriving the so-called "limited-information maximum-likelihood method," a more economical way of obtaining consistent estimates than the "full-information maximum-likelihood method" of Koopmans, Rubin, and Leipnik. It acquired the appellation "limited-information" because it yields estimates of only one or a few of the equations at a time and uses considerably less information (both theoretical and empirical) to get them. It is not as accurate as the full-information method but for reasons of cost it has always been used instead, except in small or simplified systems.

Another part of the staff, including William H. Andrews, Jr., Gershon Cooper, Girshick, Haavelmo, Klein, Marschak, and Don Patinkin, worked on the construction of economic models and the estimation of their parameters by the new methods. The construction of a model, starting from considerations of economic theory, must come before the estimation of its parameters by whatever method: the estimation of the parameters of an equation, i.e., the fitting of a graph to a set of observed points, can only proceed *after* one has decided what variables are to appear in the equation, what the form of the equation is to be (linear, quadratic, exponential, etc.), and what the other equations of the system (if any) are like. Of course the validity of any statistically fitted equation depends heavily on whether the model chosen to begin with is realistic or not (light can be shed on this issue by checking the fitted equation against the same or, even better, against new data). This is why the construction of models is so important in econometrics.

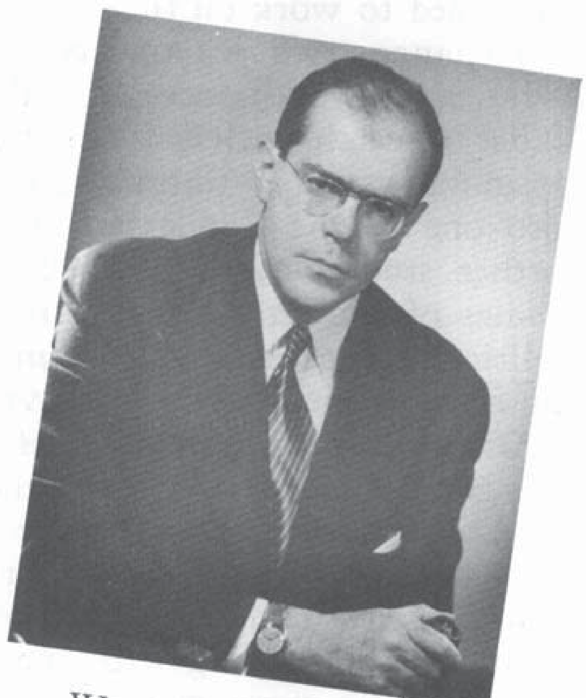
The estimation of the parameters of the models is equally important. Their *numerical* values must be known at least approximately if predictions are to be made about the effects of various possible changes in the economic environment, and such predictions are essential if we are to choose intelligent policies. This applies whether the changes in the economic environment are brought about by deliberate policy action (such as the imposition of an excise tax or a subsidy, or the fixing of quotas on the use of certain materials) or by fortuitous but foreseeable events (examples are shifts in population and the introduction of new products such as nylon and atomic energy). It applies especially if the change, the effect of which is to be predicted, is one with which the economy has had no direct previous experience on which to base predictions. For example, if an excise is applied to a commodity that has previously been tax free, one needs to know certain parameters of both the supply and demand curves for *that commodity* in order to foretell the effect of the tax upon its price and output.

The early results of the Cowles Commission's research in this field appeared in four publications. First was a paper by Marschak and Andrews in *Econometrica* in 1944, estimating the relationship between inputs and outputs in production. Next were two papers in 1947 by Haavelmo and by Girshick and Haavelmo, on the consumption function and the demand for food, published in the *Journal of the American Statistical Association* and *Econometrica*, respectively. Fourth was Monograph 11, *Economic Fluctuations in the United States* by Klein. This was essentially completed in 1948 but additional computational work was undertaken in 1949 and the book did not actually appear until the middle of 1950. It presents three economic models for the American economy, containing from three to fifteen equations, with the estimates of their parameters as obtained by both the least-squares and maximum-likelihood methods. For the large model, the limited-information maximum-likelihood method is used. It represents forward steps in several respects: it contains a good deal of material on the appropriate theoretical procedures for passing from the familiar theory of individual behavior to equations describing entire sectors of the economy; it contains the largest model hitherto fitted by the new techniques; and it presents a statistical test of the hypothesis that the deviations *are* random as assumed, as well as tests of several economic hypotheses.

In 1946, a study of the economic implication of the development of atomic energy was initiated by the Social Science Research Council under the direction of Marschak and Sam H. Schurr. Their coauthors were Simon, who has continued to work on the economic theory of technological change, two economists, E. Boorstein and H. H. Wein, and two engineers, G. Perazich and M. F. Searl. Other research included a study by Koopmans of the optimum use of a transportation system (later to grow into one of the Cowles Commission's major projects), further studies of stock-market forecasting by Cowles, studies of the econometric interpretation of history and politics, and extensive work in preparing and cataloguing mathematical tables by Davis.

Although the informality of the summer conferences was gone, there were still activities on the lighter side at the Cowles Commission. At one party in 1946, a skit was presented portraying the mock trial of Klein on the grave charge of stealing into the Social Science building late at night and finagling with the data for his econometric model. There were many witnesses and clever counsel played by various staff members, and it made delightful entertainment. The record should show, of course, that Klein was acquitted of all wrongdoing.

Tjalling C. Koopmans



William B. Simpson

VII. Economic theory revisited: 1948–1952

In July, 1948, Koopmans and Marschak exchanged places in the Cowles Commission. Koopmans became director of research and also professor of economics in the University of Chicago, while Marschak became the senior research associate and continued as a professor of economics in the University. With the recent growth of the Cowles Commission, which was expected to continue, the sheer weight of administrative work involved in its affairs had become so great that the director of research had relatively little time or energy to devote to the research program and to his own research. Therefore, a new administrative position of assistant director of research was created as part of the arrangement whereby Koopmans became director of research. William B. Simpson, whose acquaintance with the Cowles Commission had begun during his tenure of a Social Science Research Council fellowship for study and research in economics at the University in 1946–1948, was chosen for the new position. He began in May, 1948 and was appointed a research associate at the same time. In September, Simpson was elected secretary of the Econometric Society by its Council. In January, 1949, he became in addition managing editor of *Econometrica*, and then in 1951 co-editor. His great energies in administrative affairs and his dedication to the development of econometrics were to make possible a substantial growth of both the Cowles Commission and the Econometric Society in the subsequent period.

In the fall of 1948, there were further changes in the organizational structure of the Cowles Commission, still preserving its highly valuable academic connection with the University. The Cowles Commission was granted the right to recommend academic rank in the University for its qualified staff members independently of their status in other departments, and thus begin to build up a faculty of its own. This made it easier to attract and hold research workers of high qualifications and attainments. The first appointment was that of Clifford Hildreth, an econometrician, formerly of Iowa State College, who became assistant professsor in the Cowles Commission in January, 1949, and associate professor in July, 1950. Hildreth's appointment was a joint one with the Agricultural Economics Research Group in the Department of Economics in the University where he has the complimentary rank of associate professor. Subsequent appointments went to John Gurland in September, 1949, conferred jointly with the University's Committee on Statistics, Gerard Debreu in June, 1951, and H. S. Houthakker in January, 1952, all as assistant professors. Gurland is a mathematical statistician, formerly an instructor at Harvard. Debreu is a mathematical economist, who joined the Cowles Commission first as research associate in June, 1950, after teaching and doing research at several French institutions and holding a Rockefeller fellowship. Houthakker is an economist, formerly with the Department of Applied Economics, University of Cambridge. Koopmans, Marschak and Simpson (ex officio) are also faculty members of the Cowles Commission. This faculty, now numbering seven men, has gradually emerged as a responsible, self-governing body under the general supervision of the new Executive Committee which was set up in the fall of 1948 in place of the University of Chicago Advisory Committee. The Executive Committee was constituted of the dean of the Division of the Social Sciences (Ralph W. Tyler), the chairman of the Department of Economics (Theodore W. Schultz), the president of the Cowles Commission (Cowles), the director of research (Koopmans), and the assistant director of research (Simpson).

While the Cowles Commission faculty was coming into being, numerous other staff changes took place but they are again too numerous to detail. Hence those who came during this period and are no longer with the Cowles Commission are mentioned later on in connection with the research program, or in the biographical sketches. Franco Modigliani, an erstwhile member of the weekend New York econometrics seminar of 1940–1942, became a research associate in September, 1948, while he held a post-doctoral fellowship from the Department of Economics in the University. Shortly thereafter, he resigned to become associate professor at the University of Illinois and director of a research project on expectations and business fluctuations. He is now a research consultant of the Cowles Commission. Stephen G. Allen joined the staff in January, 1949, as a research assistant. He is now a research consultant and a research associate of the Applied Mathematics and Statistics Laboratory at Stanford University. Carl F. Christ became a research associate in September, 1949, after having been an informal member of the staff for the preceding year while holding a Social Science Research Council fellowship. He is now a research consultant, and an assistant professor of political economy at Johns Hopkins University. In September, 1949, William C. Hood became a research associate and a post-doctoral fellow in the Department of Economics in the University of Chicago. He is now a research consultant and an assistant professor of economics at the University of Toronto. Roy Radner became a research assistant in March, 1951, and a research associate in November, 1951. Martin J. Beckmann, I. N. Herstein, and Daniel Waterman became research associates in July, 1951. Herstein was given the complementary rank of assistant professor as well. William J. Dunaway and C. B. McGuire became research assistants in January, 1952. Aryeh Dvoretzky, professor of mathematical statistics at Hebrew University in Jerusalem, Erich Lehmann, associate professor of mathematics at the University of California in Berkeley, and Robert G. Strotz, assistant professor of economics at Northwestern University, became research consultants during 1951–1952.

Although Abraham Wald was a staff member of the Cowles Commission only briefly in 1938, he subsequently participated in several conferences sponsored by the Cowles Commission and contributed extensively to the literature of econometrics with papers in Monograph 10, *Econometrica*, and elsewhere. Therefore it seems appropriate at this point to pause and recall the high points of his career up to its untimely termination in an airplane crash in southern India on December 13, 1950, in which both he and his wife were killed. He was born in Cluj (also known as Clausenburg), Rumania, in 1902. After overcoming great obstacles to his education, he became associated with the University of Vienna in 1925, where he remained until he was dismissed from his position shortly after Hitler's annexation of Austria early in 1938. In July of that year, at Frisch's suggestion, the Cowles Commission offered him a research fellowship, which he accepted. Soon afterward he moved on to Columbia University where he eventually became professor of mathematical statistics. In his fifteen years there he became one of the most distinguished contributors that mathematical statistics and econometrics have known. He solved many problems of the estimation of parameters in statistical models, and his penetrating decision function analysis (see below) is basic to much of the current research in mathematical statistics. The 1952 volume of the *Annals of Mathematical Statistics* has been dedicated to his memory.

In its new status, the Cowles Commission dealt directly with some of the organizations providing its financial support instead of through the University of Chicago, while of course all contracts and applications for funds were first approved by its Executive Committee. Support continued to come from Cowles and his family, from the University in the form of the salaries of the two senior faculty members and the free use of facilities, and from the Rockefeller Foundation in the form of a grant for the project, "Foundations of Rational Economic Policy." Fellowship aid for persons working in econometrics at the Cowles Commission was provided by the Social Science Research Council, the Rockefeller Foundation, the University of Chicago, and various sponsers in Canada and Europe.

In addition, the Life Insurance Association of America made a grant in 1948 to help finance the study begun in 1946 of the economic implications of the development of atomic energy. The Cowles Commission entered into a contract with the RAND Corporation beginning in January, 1949, for the conduct on a cost basis of a research project called "Theory of Resources Allocation." The Cowles Commission entered into another contract with the U. S. Office of Naval Research beginning in July, 1951, for the conduct of a research project known as "Decision-Making Under Uncertainty," also on a cost basis. For the most part, these grants and contracts are for periods of two or three years, and are subject to renewal.

Concurrent with this growth, the Commission gave thought to adjustments in organizational structure which would recognize formally realignments of functions which had developed since 1948 and which would better prepare the organization to meet the problems of the future. Although the plans outlined in the 1950–1951 Report did not all reach fruition in the current year, an initial step was taken by the appointment of an executive director to serve as the chief executive agent of the Commission. Simpson was named to the new post as of July, 1951. The executive director is responsible to the executive committee (of which he is a member) and to the board of trustees, and together with the director of research is responsible for advising those bodies on matters related to the interests, aims, and policies of the Commission.

From 1948 to the present, the research of the Cowles Commission has proceeded along the lines laid out by Marschak with no fundamental changes in philosophy, but with important amplifications and changes in emphasis. In particular, there was a relative shift toward theoretical work to obtain better models preparatory to another phase of empirical work. There was also more concentration on the proper choice of mathematical methods (see below). The central part of the program, including the four projects mentioned in preceding paragraphs, can be described under the headings *actual behavior* and *rational behavior*. The headings *statistical methods*, *mathematical tools*, and *special studies* include ancillary research on analytical tools and several other studies. "Actual behavior" requires no special definition; it is behavior as it occurs in the real world. "Rational behavior," or as it is sometimes called "optimal behavior," is defined as that behavior which best attains the goal (utility, profit, survival, growth, etc.) of the individual or group whose behavior is in question. The study of actual behavior is the attempt to find general laws that describe behavior as it occurs, or would occur, under specified circumstances. The study of rational behavior is the attempt to discover what kind of behavior on the part of an individual or group in specified circumstances would most completely achieve the goals pursued; it presupposes that the goals are known and stated in objective terms, and that their probable achievement or lack of achievement as a result of following a particular pattern of behavior can be discovered. Studies of these two types may be called "descriptive studies" and "prescriptive studies," respectively.

There is a good deal of overlapping between the descriptive and the prescriptive studies for the following reasons. First, in setting up models of actual behavior in a world where monetary and material matters are of great importance, it is convenient and is often a good approximation to reality to assume, as a basis for such models, that individuals and firms do behave rationally. Thus, the assumption of rationality enters into many theories of actual behavior. Second, in order to prescribe what one individual or group should do in order to achieve his or its goals, the economic doctor must know how *other* individuals and/or groups will behave in the future, and in particular how they will respond to the actions of his patient. This requires knowledge about the actual behavior of others, whether it is rational or not. Thus prescriptive studies draw on the results of descriptive studies. Because of this two-way overlapping the distinction must be regarded as an expository device, and it must be remembered that an accepted description or prescription may become inapt if either the prescriptions or descriptions upon which it is based turn out to be incorrect.

The five headings mentioned above will now be discussed.

Actual behavior can be investigated by the techniques outlined in the previous section. Several such studies were undertaken. Andrew W. Marshall tested Klein's fifteen-equation model with its estimated parameters, as presented in Monograph 11, by checking whether it fitted the data for 1945–1946 as well as it did the 1921–1941 data from which its parameters had been estimated. He found that of the twelve random equations (the other three being definitions), only seven could be considered valid in the postwar period; of the remaining five, two were of doubtful validity and three were clearly contradicted by the postwar data. Christ, starting from Marshall's work, revised those equations of Klein's model which did not pass Marshall's tests. He then re-estimated the parameters of the revised model using data for 1921–1947, omitting the war years 1942–1945, and then tested the results against data of 1948, using tests similar to Marshall's and several other tests. He found that the revised and refitted model performed better on extrapolation to 1948 than Klein's had on extrapolation to 1945 and 1946, but it still was not in itself an accurate instrument for prediction. Christ's work, containing also a summary of Marshall's work, appeared in *Conference on Business Cycles*, published in 1951 by the National Bureau of Economic Research. These two studies were among the first to act on the precept that econometric models, like any other theories, must be tested by their performance in making predictions.

Allen worked on equations describing the inventory behavior of firms in the linseed oil industry. Hildreth has used cross-section data from farms in Iowa to estimate the technological relationship between agricultural inputs and outputs. Together with Frank Jarrett, research associate in agricultural economics at the University of Chicago, he has worked on an econometric study of U. S. livestock production. Arnold C. Harberger, now of Johns Hopkins University, set up import-demand equations for the United States and estimated the elasticities of demand for various types of imports and for imports as a whole.

Harry Markowitz, now of the RAND Corporation, studied the financial behavior patterns of open-ended investment trusts and set up equations to describe them; the statistical results are in preparation. George Borts, now of Brown University, constructed a model of the relations between inputs and outputs in the railroad industry, which is different from most industries in that railroads do not control their own outputs, due to the common carrier law; a part of his work appeared in *Econometrica* in January, 1952.

Rational behavior is typically treated in studies that seek to answer questions like this: given an individual or a group, and given the goals of the individual or the goals of the group or its members, and given some kind of environment in which the individual or group operates, what behavior will lead to the most complete achievement of the goals? The Cowles Commission's work in this field springs from three somewhat related origins.

First, Koopmans had been thinking intermittently, ever since his wartime days with the Combined Shipping Adjustment Board, about a systematic way to find the optimum routing plan for empty ships when there are fixed tonnages of cargo per month to go from each port to other ports. The "optimum" routing plan is the one, among all those that deliver the required fixed amounts of goods, for which the required number of ships in service is smallest. Clearly the optimum routing will not send empty ships to any port which is receiving more goods than it is shipping because such a port is already an exporter of empties and it would be wasteful of ships to send any more empties there. Similarly, if New York and Liverpool are exporters of empties and Philadelphia and le Havre are importers of empties, then it would be silly to send empties from New York to le Havre if empties were also going from Liverpool to Philadelphia, because it would take less time for the empties from New York to go to Philadelphia, and for those from Liverpool to go to le Havre. As a result of this shift less shipping time would be used up in empty voyages, so the number of ships required would be decreased. Considerations of this sort indicate a possible, if unsystematic, approach.

In seeking a systematic approach, Koopmans hit upon the principles of an analytic method that was first known as "linear programming" but has now come to be called more accurately "activity analysis of production." It has grown up from several converging sources, including analyses by Wald and von Neumann of the Walrasian general equilibrium theory, discussions in welfare economics, Leontief's interindustry analyses, and the programming activities of government administrators as studied in the U. S. Air Force by G. B. Dantzig and M. K. Wood. It is *similar* to the traditional economic theory of production in that it seeks first to establish the technological relation between inputs and outputs, i.e., to answer questions like these: if the quantities of all inputs and all outputs but one are held at specified levels, what is the maximum quantity of the remaining output that can be produced? Alternatively, if the quantities of all outputs and all inputs but one are to be held at specified levels, what is the minimum quantity of the remaining input that is required? For example, consider cattle raising, where inputs are grazing land, feed, and labor, and outputs are milk and beef (the cattle themselves are purposely not mentioned, as they are a sort of "intermediate product" that is used up in the process of producing the final outputs of milk and beef). A successful analysis of this industry, or of a firm engaged in it, would be able to tell which combinations of land, feed, labor, milk, and beef are *achievable*, and which are not (i.e., one man using one acre of land and one sack of feed per week cannot produce fifty gallons of milk and fifty pounds of beef every day). Further, it would be able to tell how much milk could be gotten from a given combination of land, feed, and labor if the amount of beef to be produced per week was specified, or how much labor would be required to produce specified amounts of milk and beef per week using a given combination of land and feed, etc. A combination of inputs and outputs is said to be *efficient*, if, when that combination is the status quo, it is impossible to increase the rate of any output without at the same time increasing some input or decreasing some other output. Note that for any inefficient combination, there are efficient combinations that are preferable to it in the sense of producing more outputs with the same inputs, or the same outputs with less inputs. Note also that to move from one efficient combination to another, it is necessary to give up some of one commodity (be it output or input) to get more of another; technological "exchange rates" between commodities can be found showing how much of one can be gained by giving up a unit of another. Note finally that not all efficient combinations are rational or optimal. For example, if we should all become vegetarians, then any combination that produced beef at the expense of milk, even if it did so efficiently, would become irrational. As a more realistic example, suppose that certain amounts per week of milk and beef are being produced efficiently; that to consumers, one gallon of milk is worth one pound of beef; and that the combination of land, feed, and labor actually being used to produce a pound of beef would, if turned to dairy cattle, produce two gallons of milk. To continue in this state of affairs is not rational: if beef-raising were curtailed, and dairying were expanded, then for every pound of beef given up *two* extra gallons of milk could be had, and this would represent a net gain because consumers would have been willing to give up a pound of beef for only one gallon of milk. The rational or optimal combination of milk and beef is obtained by increasing milk production and decreasing beef production to the point where no further gain is obtained by continuing the process. This point may be reached either because consumers find that a pound of beef has become more desirable than a gallon of milk now that they have so little of it and so much milk, or because producers find that they cannot get another two gallons of milk from the resources that are freed by producing one less pound of beef now that they are producing more milk and less beef, or because of both. These developments are related to the economic principles of diminishing utility and diminishing returns to changes in input-proportions, respectively.

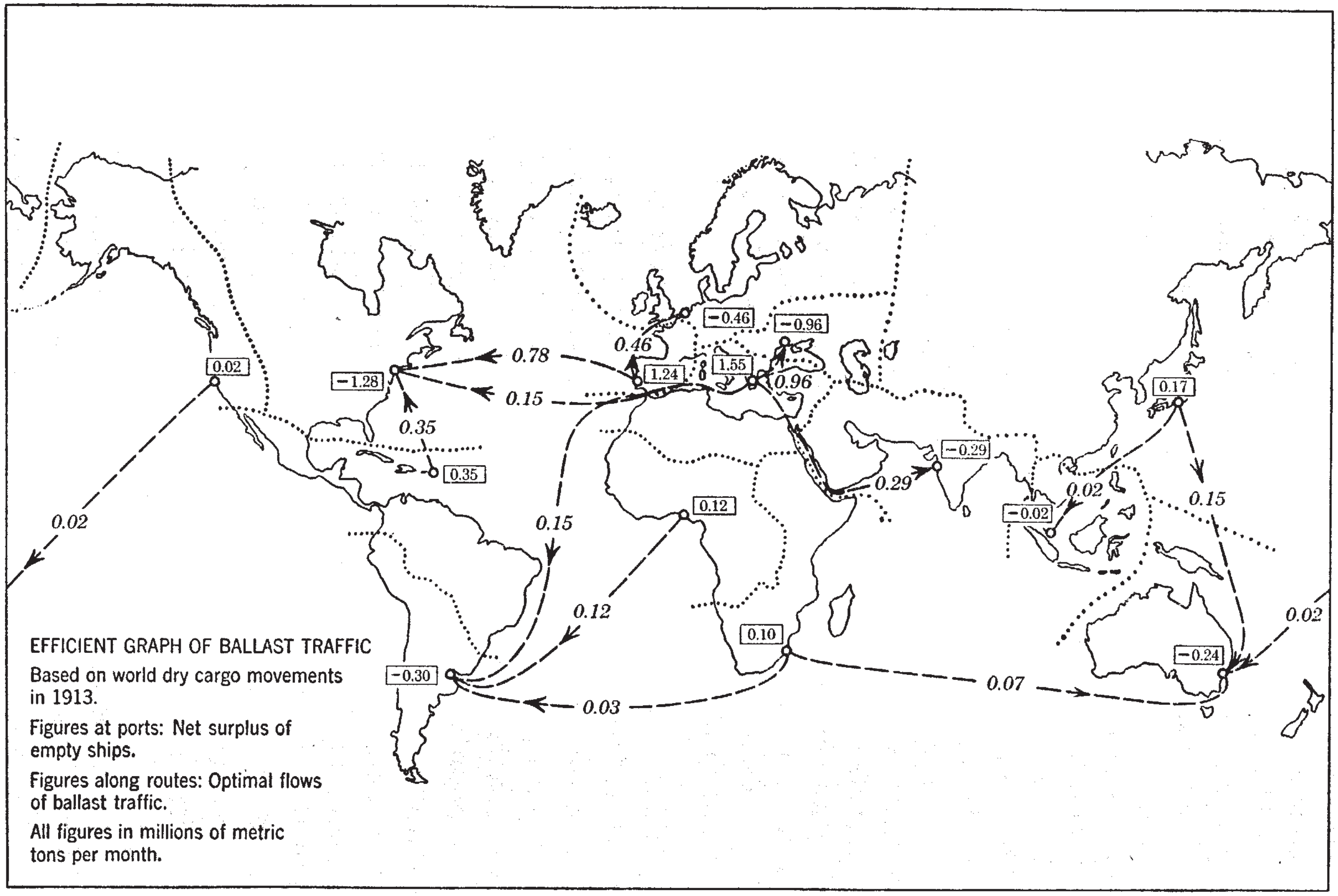
Thus in finding the optimal combinations of inputs and outputs, there are three successive winnowings of all the conceivable combinations. First, the achievable ones are selected and the others are discarded. Second, from the achievable combinations the efficient ones are selected and the others are discarded. (These two steps can be accomplished by means of purely technological knowledge, with no notions of which commodities are highly valued and which ones are not.) Third, from the efficient combinations the optimal one(s) is (are) selected, by considering the relative values to producers and consumers of the inputs and outputs. In these respects, activity analysis is like any other good theory of production.

Activity analysis *differs* from the traditional economic theory of production by being more specific about the technology behind the concepts of achievable and efficient combinations and the substitution of one commodity for another. It regards production as resulting from a number of separate activities, each one of which can be operated on a large or small scale, and each one of which uses certain inputs and produces certain outputs in an assumed fixed proportionality to the scale of operation. Thus any possible combination of scales of operation of the respective activities produces an achievable combination of inputs and outputs. If there is to be any substitution of inputs or outputs for one another in production, it cannot be accomplished *within* any one activity, but must come about through changes in the scales of operation of the various activities, resulting in a partial substitution of some activities for others. In the dairy-beef case above, one might substitute the activity of grazing for that of using feed, and thus effect a substitution of land for feed. Or one might substitute the activity of producing a breed of dairy cattle for that of producing a breed of beef cattle, and so substitute milk for beef.

In the shipping problem the outputs are tonnages delivered from each port to each other port, the input is the use of ships, and there are two separate activities corresponding to the dispatching of ships from each port to each other port, i.e. with and without cargo. The original problem of accomplishing a certain pattern of shipping with the smallest number of ships then is seen to be the problem of finding the efficient combination of flows of empty and loaded ships when the tonnages to be shipped (outputs) are given. It is purely a problem of physical efficiency (even if more than one firm is involved) and not a problem of an economic optimum because there is no question of whether the stated pattern of shipping is worth its cost as compared with other possible patterns. But economic optimum problems can also be handled with this analysis: if the relative values placed upon ships and tonnages shipped on different routes are known, then the rational or optimal shipping plan can be found.

In June, 1949, the Cowles Commission sponsored a conference in Chicago on activity analysis, at which papers were presented by economists, mathematicians, statisticians, and administrators. The greater part of the proceedings of the conference were published in 1951 as Cowles Commission Monograph 13, *Activity Analysis of Production and Allocation* edited by Koopmans. (Monograph 12 will be discussed below.) It begins with a long section on the theory of programming and allocation, followed by a section on application of allocation models. The shipping problem appears in a paper by Koopmans and Stanley Reiter (formerly of the Cowles Commission, now of Stanford University) entitled "A Model of Transportation." Other applications appear in "Development of Dynamic Models for Program Planning" by Wood and Murray A. Geisler of the U. S. Air Force, "On the Choice of a Crop Rotation Plan" by Hildreth and Reiter, "Effects of Technological Change in a Linear Model" by Simon, and "Representation in a Linear Model of Nonlinear Growth Curves in the Aircraft Industry" by Wood. In conclusion there are two shorter sections on mathematical properties of convex sets and problems of computation (see below, under the discussion of mathematical tools).

In reviewing the volume for the *American Economic Review* in 1952, Robert Solow of the Massachusetts Institute


EFFICIENT GRAPH OF BALLAST TRAFFIC
Based on world dry cargo movements in 1913.
Figures at ports: Net surplus of empty ships.
Figures along routes: Optimal flows of ballast traffic.
All figures in millions of metric tons per month.
0.02
−1.28
0.78
0.15
0.35
0.35
0.46
−0.46
1.24
1.55
−0.96
0.96
0.15
0.12
0.12
−0.30
0.03
0.10
0.07
0.29
−0.29
−0.02
0.02
0.17
0.15
−0.24
0.02
0.02

of Technology wrote as follows:

"Like all things good for body and soul, this book is going to hurt. Nevertheless there is no escaping the fact that everyone seriously interested in economic theory ought to keep a stiff upper lip and attempt to read it. This will be no easy task, since mathematical concepts whose use in economics is relatively new abound; . . . Still, by careful selection and constant attention to the *economics* of what is being said, almost everyone can profit.

It must be said at once that many of the general economic results stated in the book are not new; they are, in other forms, already part of the literature of welfare economics and the theory of production. What the new methods offer are first, a clearer insight into the meaning of some established propositions, such as those concerned with the much more than institutional significance of a set of price ratios in the optimal allocations of resources, and second, a framework for formulating many kinds of optimum-problems in such a way that they lend themselves to systematic computation. . . . In sum, the subject matter of this anthology is one of the frontiers of detailed and aggregative economic theory. It deserves a serious try."

Debreu devised a measure for the extent to which a given allocation of resources is efficient, being the smallest fraction of the given input levels (all reduced proportionally) that permits the community to attain through efficient redistribution of outputs the same standard of living for each member as prevailed under inefficient utilization of resources before the cuts in inputs. It appeared in his paper, "The Coefficient of Resource Utilization" in *Econometrica* in July, 1951. Kirk Fox undertook a study of the routing of railroad boxcars in the United States. Markowitz in his study of the behavior of investment trusts is inquiring whether their portfolios are efficient or not, i.e., whether they achieve a minimum risk for their rate of return.

The second source of the Cowles Commission's work on rational behavior lies in von Neumann and Morgenstern's *Theory of Games*, Wald's *Statistical Decision Functions*, and related work. In their book von Neumann and Morgenstern ask questions like these: given a game and its rules, how should a player behave so as to win as much as possible on the average over a large number of plays of the game, and what is the amount that he will win on the average if he so plays? So far the chief applications of the theory as such have been to games (e.g., chess, poker) and to problems in military strategy and some work in the economics of bilateral monopoly. But it soon became evident that many practical situations calling for decisions are very much like a one-person game in which the winnings depend on the decisions of the "player" and on the (perhaps unknown) "state of nature"; it is as if the player had nondiscriminating "Nature" as his "opponent," instead of another player who is hostile and out to win as much from him as possible. With this realization, the formal apparatus of the theory of games was taken over and applied to decision-making in many familiar situations, with a view to finding out what is the rational behavior appropriate to each. For example, suppose a monetary authority such as the Federal Reserve Board were making an estimate of whether the coming year would bring inflation or deflation if no action were taken in order to decide whether to decrease or increase the money supply. This can be regarded as a game played by the Federal Reserve against an economic "Mother Nature" with the Federal Reserve winning if it makes the right decision and losing if it errs. But since the consequences of deflation are most serious from most viewpoints than those of equally severe inflation (with severity measured let us say by the size of the fiscal deficit or surplus required to maintain stability), the Federal Reserve must regard itself as losing more if it prepares erroneously for inflation than if it prepares erroneously for deflation. Therefore it should be willing to run a bigger risk of predicting deflation incorrectly than of predicting inflation incorrectly. Its rational estimate of what the coming year will bring is not the unbiased estimate, but is instead an estimate *biased* somewhat in favor of preparing for deflation. Furthermore, while the range of uncertainty of the estimate presumably can be reduced up to a point if more resources are invested in the estimation process, the optimal extent to which the Federal Reserve should do this is found by balancing the expense against what the expense buys, namely the resulting reduction in the expected loss due to erroneous decisions. Wald's *Statistical Decision Functions*, published in 1950, is a formalization of this approach.

The accepted theory of the behavior of a business firm under competitive conditions is based on the assumptions that the firm is concerned only with flows of inputs, outputs, and sales, not with stocks of assets and debts, and that the firm knows its cost curves and demand curves exactly so that it can tell just how much profit it will make from any given level of output. For several years Marschak had been interested in making this theory more realistic by introducing the assumption of uncertainty together with the firm's asset and debt structure—particularly its liquid assets and inventories, because as has long been recognized the most compelling reason why these assets are desirable is the fact of uncertainty about future demand, prices, or other economic factors. His paper entitled "The Role of Liquidity under Complete and Incomplete Information," in the *American Economic Review* for May, 1949, and an earlier paper by Hurwicz, "Theory of the Firm and of Investment," in *Econometrica* for April, 1946, present work in this field.

As a different example, suppose that a firm's daily sales are not exactly known in advance, but are determined as if by being drawn at random from a hat containing numbers; that prices are constant; and that there are certain costs of ordering and storing inventories and certain costs of being caught "out of stock." This is similar to a game between the firm and the market; there is an optimal inventory policy for the firm telling, in terms of the numbers that are in the hat and the various costs, how low the firm should let its stocks get before reordering and how much it should order at a time, so as to minimize the total of all three types of cost. Further complications arise if the numbers in the hat are not known, so that the firm does not even know what its sales will be on the average nor how widely they will fluctuate, or if prices are assumed to be random variables and the firm speculates in inventories. Marschak, Arrow, and T. Harris of the RAND Corporation have published a paper on this work, "Optimal Inventory Policy," in *Econometrica*, for July, 1951. Other work in the area of decision-making under uncertainty, much of it using the decision-function approach, has been done by Arrow, Debreu, Hurwicz, Markowitz, Marschak, Radner, and Erling Sverdrup.

The third source of the Cowles Commission's work on rational behavior lies in welfare economics, and the attempt to deduce from the preferences of individuals a concept of social preference or of the general welfare. Monograph 12, *Social Choice and Individual Values* by Arrow, published in 1951, is addressed to this problem. Arrow assumes that each individual has a consistent value scale which ranks all the possible states of society in the order of his preference, and then he uses symbolic logic to try to deduce from this a social preference scale which has certain reasonable properties. He proves that it is impossible to do so, unless the preferences of the individuals are sufficiently in agreement in the beginning. Further work in this area by Hildreth and Markowitz and also by Leo Goodman is described in the reports on research activities for 1951–52 and 1950–51.

Statistical Methods received further attention during this period with concentration in four areas. The first is an expository monograph (No. 14, *Studies in Econometric Method*) being edited by Koopmans and Hood, to accompany Monograph 10 and offer its conclusions and some new results in more accessible and usable form. The second consists of inquiries into the extent and direction of the bias inherent in the least-squares method of estimating the parameters of econometric models, and more generally, into the consequences of using models that are incorrect. This work has been executed chiefly by Allen, Jean Bronfenbrenner (now of the Department of Commerce), Harberger, and Hurwicz. Some of it will appear in Monograph 14. The third is the application of the decision function approach to the problem of finding the best estimates of the parameters of econometric models, along the lines indicated in the monetary policy illustration mentioned earlier. Hildreth, Hurwicz, Radner, and Sverdrup were engaged in this work. The fourth is the theory of statistical procedures to deal with time series where the successive random terms are interdependent, to which Gurland has made several contributions.

Mathematical tools for handling of problems in activity analysis had to be developed or adapted from branches of mathematics that had been little used in economics previously. The essential mathematical concept in activity analysis is the maximization of a function (value or profit) of several variables (inputs and outputs) when certain *inequalities* among the variables must be satisfied (when input levels are given, output cannot be greater than a certain maximum achievable amount). When the relationships in question are linear, the mathematics of such inequalities turns out to be the theory of convex bodies or sets. Murray Gerstenhaber, Koopmans, and Morton Slater have been chiefly responsible for this work, with Slater extending his research to nonlinear relationships as well.

Special studies includes, principally, *Economic Aspects of Atomic Power* by Schurr and Marschak with contributions by Simon and others, published in 1950. It contains the results of the study mentioned at the end of the previous chapter. It attempts to analyze the effects that the peacetime development of atomic energy may be expected to have on particular industries that use large quantities of either heat or electricity, which are the forms that atomic energy is almost sure to take, and on the underdeveloped areas of the world. The method in brief is to consider the probable costs of atomic versus heat and hydroelectric energy in various industries and geographic areas, and also the probable demands for new energy supplies. This work is closely related to the Cowles Commission's research on technological progress pursued by Simon and Debreu. In reviewing the atomic energy study for the *American Scientist* for January, 1951, Kirtley F. Mather wrote:

"This is in many ways an extraordinarily significant book. It undertakes the unprecedented task of assaying the social consequences of the practical application in human affairs of newly discovered scientific knowledge, even before the technologic problems pertaining to that application have been solved. . . . And it accomplishes this extremely difficult project by teamwork organized on a scale rarely observed even in the most complex research enterprises. Its ingenious methodology provides a pattern for procedures that may well be emulated by research groups concerned with many varied problems in quite different aspects of the broad topic of the social implications of science."

There was levity amid the serious research in this period too. The most notable bit was a skit by some of the junior staff members and others burlesquing the department of economics and its affiliates in songs set to the music of Gilbert and Sullivan's operas and other familiar melodies. The Cowles Commission was featured in this one, to the tune of "The American Patrol" march:

We must be rigorous, we must be rigorous,
We must fulfill our role.
If we hesitate or equivocate,
We won't achieve our goal.
We must investigate our systems complicate
To make our models whole.
Econometrics brings about
Statistical control!

Our esoteric seminars
Bring statisticians by the score.
But try to find economists
Who don't think algebra's a chore.
Oh we must urge you most emphatically
To become inclined mathematically,
So that all that we've developed
May some day be applied!

Its exact authorship is surrounded by a certain degree of obscurity, which perhaps is just as well.

VIII. Looking back and looking forward

Since 1932, the motto of the Cowles Commission has been *Science is Measurement*. It was originally suggested by one of Davis' favorite quotations from the British physicist Lord Kelvin, to the effect that when you can measure what you are speaking about, you know something about it, but when you cannot measure it, your knowledge is of a meager and unsatisfactory kind. Although the motto was inspired by a venerable source, it has been criticized several times by social scientists on the ground that not all science is measurement, even if the term "measurement" is given a very broad meaning. While this is admitted, tradition and the ideals of precision and empiricism are on the side of retaining the original motto. However, in 1952 a partial change was suggested by Clifford Hildreth to make the meaning more appropriate and still to preserve some continuity with the original. The suggestion was accepted, and accordingly the new motto of the Cowles Commission is *Theory and Measurement*. It will be inscribed in the emblem that has carried the motto since the beginning.

In twenty years, a characteristic approach to research has grown up at the Cowles Commission. This is true in a methodological sense, as the preceding pages show. It is also true in an operating sense. The work of the Cowles Commission has been characterized from the beginning by a great deal of discussion and cooperation among staff members. The seminars and New Series Papers have been helpful in this activity, but they are rather in the nature of finished products. Far more important in the working stages are the hectographed "Cowles Commission Discussion Papers." As currently stated on the first page of each, these are "preliminary materials circulated privately to stimulate private discussion and are not ready for critical comment or appraisal in publications." Since 1947 the papers have been numbered consecutively in three series, and in the five years down to June 30, 1952, there have been 143 in economics and 74 in statistics. Recently a separate series has been inaugurated for mathematics papers, which formerly were included in the other series. There have been 14 of these to June 30, 1952. Each is circulated to the staff and guests and to a group of other interested persons in the United States and abroad. Each is typically presented by its author or authors at an informal staff meeting, where it gets a thorough going-over by the staff and guests. Following such criticisms, and written comments received from those too far away to attend the meetings, papers commonly undergo several revisions, usually with further discussions and comments on each, before being ready for publication (of course not all are published). In addition to the staff meetings and discussion papers, there is a still more informal level of communication, that of private conferences and correspondence often extending across the seas.

This cooperative approach to work in progress serves several purposes: authors have the benefit of keen criticism of their research while they are working on it; preliminary results are circulated to other workers in the field more quickly than regular channels of publication permit, a larger community than the resident staff of the Cowles Commission is enabled to participate in its work; and the cross-fertilization of ideas that is so important to research is fostered.

The Cowles Commission has always been partly international in character, even while its home was in Colorado Springs. Particularly since the end of World War II this feature has been enhanced by the regular stream of fellows and other visitors from outside the United States who have come as guests for various periods of time. This international flow goes on in both directions. Several Cowles Commission staff members have traveled in other countries as fellows, lecturers, or consultants from time to time, including Anderson, Arrow, Klein, Koopmans, Marschak, Tintner, and Wald. Other present or former staff members are associated with universities in several countries (Dvoretzky, Haavelmo, Hood, Patinkin, Reiersøl, and Sverdrup).

Some members of the Cowles Commission staff offer or have offered courses in the University of Chicago (in the Department of Economics, the Committee on Statistics, the School of Business) and occasionally elsewhere. This has been the case since the Cowles Commission came to the University in 1939.

The ideas and methods developed by the Cowles Commission and by people working in association with it are being taken up, in a number of instances, by economists in other institutions and in government. Several examples follow. Klein is continuing his studies in constructing models of the United States economy at the Survey Research Center of the University of Michigan. T. M. Brown, D. J.

Daly and others in the Canadian Department of Trade and Commerce in Ottawa have been working since 1947 on a systematic program of constructing econometric models of the Klein type and estimating their parameters for purposes of prediction and policy advising. Brown's paper, "Econometric Research and Forecasting" presented before the Econometric Society in Boston in 1951, is a preliminary account of this work. A second paper of his entitled "Habit Persistence and Lags in Consumer Behaviour," in *Econometrica*, for July, 1952, concerns another part of it.

The limited-information method of estimation, described in Section VI, is being used experimentally in several places, including the U. S. Department of Agriculture under the direction of Karl Fox; a U. S. Treasury project under the direction of Francis M. Boddy of Minnesota; at Iowa State College under the direction of Gerhard Tintner and John Nordin; and at the University of California under the direction of George S. Kuznets.

There has been regular cooperation between the Cowles Commission and a research project on expectations and business fluctuations conducted jointly by the Bureau of Business Research of the University of Illinois and the Public Opinion Research Center of the University of Chicago, under the leadership of Franco Modigliani. Arrow's work on social values has been discussed intensively by a joint seminar of social scientists and mathematicians at the University of Michigan early in 1952. Marschak's work on the theory of teams and organizations, described in the report on research activities in 1951–52, was presented at a colloquium on risk and uncertainty organized by the Centre Nationale de la Recherche Scientifique with the aid of the French Government and the Rockefeller Foundation held at the Institut Henri Poincaré in Paris in May, 1952. Other participants in the colloquium associated formally or informally with the Cowles Commission were Arrow and Edmond Malinvaud, who was a guest of the Cowles Commission in Chicago during 1950–1951. A conference on design of experiments on decision processes, organized by the University of Michigan group, is to be held in the summer of 1952 in Santa Monica, California in which Hildreth, Koopmans, Marschak, and Radner will participate.

The joint work of Arrow, Harris, and Marschak on optimal inventory policy has been taken up and given greater mathematical generality by Dvoretzky, Kiefer, and Wolfowitz on a logistics project of the U. S. Office of Naval Research under the direction of Sebastian Littauer of the Department of Industrial Engineering of Columbia University. This work resulted in a long paper, which appeared in *Econometrica* for April and July, 1952.

Activity analysis, developed both at the Cowles Commission and in the U. S. Air Force in Washington, has been a stimulating element in practical programming studies carried on at Carnegie Institute of Technology. "Blending Aviation Gasolines—A Study in Programming Interdependent Activities in an Integrated Oil Company," by W. W. Cooper, A. Charnes, and B. Mellon, in *Econometrica* for April, 1952, presents the results of their application of activity analysis to the problem of gasoline blending. Melvin Salveson of the University of California at Los Angeles is applying programming methods to problems of production scheduling.

This brings up to date the growth and development of the Cowles Commission. In thinking back over the last twenty years as portrayed in these pages, the reader will realize that much of the work of the Cowles Commission is of an abstract nature, and that many of its fruits are not likely to be reaped in the immediate future. Nevertheless, its work is connected in a very real way with the fundamental problems of a free and democratic society. It is by learning to predict in detail the consequences of *general* economic and social policies that we will be best able as a society to achieve desirable objectives without resort to direct controls over individual economic behavior. In the direction of learning to predict, research like that of the Cowles Commission should continue to yield important dividends in the future.