

THE COWLES COMMISSION IN CHICAGO, 1939-1955

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Discussion Paper No. 225, October, 1985

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9/17/85

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Acknowledgements

The author is indebted to the Alfred P. Sloan Foundation and to the Graduate School of the University of Minnesota for financial aid. This permitted visits with quite a few old Cowlespeople, reproduction of documents, and some reduction in teaching commitments. The many who responded with information and suggestions cannot be blamed for the shortcomings of the paper. Faculty and staff at the Cowles Foundation were particularly helpful. Dori Clifton, Business Manager, and Karlee Gifford, Librarian, were always resourceful in locating people and documents.

Michael Intriligator and Leonid Hurwicz furnished perceptive comments on an earlier draft. Intriligator also obtained my access to the Marschak archives at UCLA. Wendy Williamson, the librarian at the Jacob C. Schmookler Library at the University of Minnesota cheerfully and efficiently handled lots of (sometimes vague) requests for reference materials and produced neat and timely drafts from very trying scratchpaper. Appropriate parts of my correspondence and some copies of documents will be placed in a Cowles Commission archive at the Cowles Foundation, Yale University.

1. Introduction

The Cowles Commission for Research in Economics was founded in Colorado Springs, Colorado in 1932 and moved to the University of Chicago in 1939. In 1955, the Commission was reorganized as the Cowles Foundation for Research in Economics and moved to its present home at Yale University.

The founder, Alfred Cowles, president of an investment counseling firm, had become strongly interested in economic research while studying the generally unimpressive records of stock market forecasters [Christ (1952), p. 12, 13; Cowles (1933)]. He was referred by friends to Harold T. Davis, Professor of Mathematics at Indiana University and a summer visitor in Colorado Springs. Besides offering computing advice, Davis put Cowles in touch with Irving Fisher and Charles F. Roos who had recently (1930) joined with others to organize the Econometric Society.

Ensuing discussions also involved other economic statisticians. Arrangements that the Society would sponsor a research commission financed mainly by Cowles and that Cowles would underwrite the cost of publishing a Society journal, Econometrica, resulted. An Advisory Council for the Cowles Commission was appointed from the Econometric Society. The members were Fisher, Ragnar Frisch, Arthur L. Bowley, Wesley C. Mitchell, and Carl Snyder. The Commission and the Society shared administrative offices until separated at Yale in 1955.

Carl Christ (1952) has written an excellent account of these developments and the subsequent history of the Commission to 1952. The

Colorado years (1932-9) saw preparation of the first five Cowles Commission Monographs , publication of an influential statistics textbook by Davis and W.C. Nelson, and a series of stimulating summer conferences. Each summer starting in 1935, leaders of the emerging econometrics profession from various locations in United States and Europe joined the Cowles staff in several weeks of discussion of research problems and results.

When the Commission's first Director of Research, Charles F. Roos, resigned in 1937, the search for an outstanding successor was complicated by the remoteness of Colorado Springs from other principal centers of econometric activity. Ground transportation was very expensive of one's time in the 1930's. The possibility of relocating at a major university was considered and mutually promising arrangements with the University of Chicago were concluded by 1939.

Remaining chronology is interesting and is well covered by Christ (to 1952) and by a series of Cowles Commission annual reports starting in 1940. These are available in a number of economics libraries and a complete set is kept at the Cowles Foundation.

The purpose of the present paper is to review ideas developed at the Commission in the Chicago period and to briefly relate these to general economic and econometric thinking. Chronology and personalities will occasionally enter but, except for this brief introduction, will not be emphasized. Attention will primarily be on work of the years 1943-55 encompassing the directorships of Jacob Marschak (until 1948) and Tjalling C. Koopmans.

Important work was done from 1939 to 1943, but there were frequent interruptions - leaves to do war work, organizational shifts, etc. (see Christ (1952) pp. 22-5). Theodore Yntema, Director of Research, was called to important assignments with the Defense Commission in 1940 and the War Shipping Administration in 1942. Later in 1942, Yntema resigned to organize studies of post-war employment problems for the Committee on Economic Development.

A crucial turn came with Marschak's appointment as Professor in the Economics Department and Director of Research of the Cowles Commission early in 1943. Knowing Jacob (or Jascha) Marschak even briefly quickly revealed that any kind of group in which he was included would have to be a lively group. Fortunately, the inquisitive, unpretentious, perceptive, and persistent yet cordial tone that characterized Marschak has been partially preserved in biographical materials, e.g. [Hirshleifer and Marschak (1978), Arrow (1978a, b) (1979), Klein (1978), Koopmans (1978), Radner (1978)].

There is no doubt that the Cowles Commission under another director would have become a research institution of first rank and made important contributions. Many favorable circumstances existed. The high intellectual goals and achievements of the Economics Department [Reder, (1982)] and the University [Murphy and Bruckner (1976)] were world renowned and the Department was still ascending. The University was a citadel of freedom with intolerance of nonsense one of the few proscriptions. Of course, nonsense and innovation are sometimes hard to distinguish but the Chicago effort was, and is, sincere.

Recruiting opportunities were unprecedented because outstanding European scholars came to the United States as refugees or were attracted by the belief that America would be the most effective place to work, e.g. see Weintraub (1983), pp. 5-21. Another distinct advantage was that Alfred Cowles had relocated in Chicago. His continuing deep interest in the work, continued financial support, and policy of noninterference except when called upon were vital to the stability of the Commission. His advice was needed on several occasions and he sometimes played a key role in explaining the nature of the Commission to others.

By 1942, Cowles' financial support was supplemented by modest grants from the Social Science Research Committee of the University of Chicago, the Rockefeller Foundation, and the National Bureau of Economic Research. Free use of University facilities was an important additional contribution. Significant part-time appointments of Chicago faculty had also been made - in addition to Yntema there were Joel Dean, H. Gregg Lewis, Jacob Mosak, and Oscar Lange.

While a splendid opportunity existed, there were limitations. Total financial resources from the several sources were not large; the Commission was unable to offer faculty status to Research Associates until 1949; living in the University area was difficult; thefts and assaults were common. There is some disagreement among participants and observers regarding the extent to which predispositions in the Department of Economics at Chicago may have been a handicap to the Cowles Commission. I cannot be very helpful on this.

The achievements of both the Commission and the Department belie

the possibility that either was an overwhelming handicap to the other. On the other hand, extensive collaboration did not develop. Fortunately, there was extensive cross attendance at seminars, and sharp differences of judgment sometimes emerged between members of the Economics Department, between members of the Cowles Commission, or between someone primarily associated with the Department and someone primarily associated with the Commission. I recall (my period at Chicago was 1949-53) two distinctive features of the last-mentioned. They tended to repeatedly occur with respect to certain topics (e.g. realism as a possibly desirable property of models, involuntary nature of some economic circumstances) and efforts at resolution did not generally proceed as automatically or smoothly as in the other cases. However, I found senior people in both groups quite anxious to furnish constructive leads when approached with problems in their domains. My conclusion is that each group benefited quite a lot from the presence and activities of the other. Speculation as to why there wasn't closer cooperation must be left to others if it is, indeed, worth pursuing.

However highly one assesses the circumstances that existed at the Commission in 1943, it is hard to see how anyone could have used them better than Marschak did. He quickly provided an eminently effective organizational framework, performed astounding recruiting magic, and cultivated a spirit of free and imaginative yet dogged inquiry.

Organizational matters included starting a discussion paper series, a reprint series (Cowles Commission New Series Papers), and continuation of monographs. An efficient secretarial staff was

organized and semi-regular staff meetings and seminars arranged. Staff meetings tended to be weekly or bi-weekly. Work in progress, sometimes barely started, was presented and then generally discussed. Announcements were circulated to the Economics Department and a list of interested persons on campus and in the Chicago area. Seminars, about once a month, usually involved more nearly finished work, were slightly more formal (but still about 45 minutes reserved for general discussion), and were more widely announced (posters). Circulation of discussion papers preceded the presentations at both kinds of meetings. About half the seminar speakers were Cowles staff members; others were visitors and faculty from Chicago and nearby universities.

Such appurtenances are more common now, but even by current standards the Commission's meetings had a special flavor.

"The first impression of a Cowles...[staff meeting]... was that everyone was talking at once, and each in a different language. The impression was not wholly incorrect. Maintaining order at the...[meetings]...was no mean task, and when Franco [Modigliani] (or others) got hold of the chalk, it was not easily wrested from him, even by the loudest attempts at interruption. But the accents may have been more a help than a hindrance to understanding, for when several speakers were proceeding simultaneously, by holding tight to the fact that you were trying to listen to the Austrian accent, you could sometimes single it out from the Polish, the Italian, the Norwegian, the Greek, the Dutch, or the

middle American. As impressive as the cacophony was the intellectual level of the discussion, and most impressive of all, the fact that everyone, in the midst of the sharpest disagreements, remained warm friends."¹

"The staff meetings were models of constructive intellectual violence, a style very much encouraged by Marschak, who insisted, by example primarily, on complete equality: a graduate student could contradict a senior scholar with impunity and encouragement. On one occasion, when I was presenting some of my results on social choice, a friend of mine, Norman Kaplan (a graduate student in economics, not part of the Cowles group; later a well-known Soviet specialist) was present. He was appalled at the freedom with which some graduate student assured Marschak that he was all wrong. I was so accustomed to this behavior that I hadn't even noticed it; but I began to appreciate our freedom through Norm's eyes. (Of course, Hurwicz, Rubin, and I needed little encouragement for our freewheeling ways,)"²

Utter chaos was, at times narrowly, avoided primarily by the judicious application of what is still sometimes called the "Cowles Commission Rule,"³ During his initial presentation a speaker could be interrupted only by "clarifying" questions. These were permitted in order to guard against the possibility that some listeners might gain very little from a presentation because some definitions or explanations underlying later material were not clear to them. A clarifying

question was expected to fault neither the speaker nor the listener and neutral wording was usually achieved.

Some pretty long speeches were occasionally delivered under the guise of clarifying questions. The chairman, whose rulings were unquestioned, inevitably narrowed his concept of what was clarifying if substantial time threatened to be lost. After the presentation, unrestricted questions and comments flowed and often resulted in subgroups appropriating pieces of the blackboard for pursuit of particular interests. As noted above, differing accents sometimes helped preserve demarcations between subgroups. Chalk of various colors also helped when blackboard territories overlapped. Not all sessions were so spirited, but the fraction must have been close to half.

Recruiting during Marschak's directorship may be judged by the list of appointees⁴ - T.W. Anderson, W.H. Andrews, Kenneth Arrow, Herman Chernoff, Gershon Cooper, Jean Bronfenbrenner Crockett, Barend De Vries, Evsey Domar, Meyer Girschik, Trygve Haavelmo, George Katona, Lawrence Klein, Tjalling Koopmans, Roy Leipnik, Franco Modigliani, Don Patinkin, Herman Rubin, Sam Schurr, Herbert Simon, and William Simpson. Leonid Hurwicz and Jacob Mosak had been appointed by Yntema and continued. Marschak had become acquainted with several of the appointees in 1940-42 through a small weekend econometric seminar that he conducted in New York while teaching at the New School for Social Research.

In mid-1948, Marschak resigned as Director of Research to return to full time research and teaching. Tjalling C. Koopmans⁵ became

Director. In essential professional qualities Koopmans and Marschak were so alike that the transition was hardly noticeable. Koopmans seemed to lay somewhat more stress on rigor, mathematical technique, and the need to solidify advances by producing orderly expositions as promptly as feasible. Marschak, at any given time, seemed likely to be more comfortable juggling a somewhat larger and more diverse collection of ideas, partial developments, and stimulating professional contacts. Minor differences in emphasis may well have increased their joint effectiveness.

Quality appointments continued - Stephen Allen, Martin Beckmann, Carl Christ, Gerard Debreu, Nathan Divinsky, Kirk Fox, John Gurland, Arnold Harberger, I.N. Herstein, Clifford Hildreth, William Hood, H.S. Houthakker, C.B. McGuire, Harry Markowitz, Roy Radner, Stanley Reiter, Morton Slater, James Templeton and Daniel Waterman. The range of activities established by Marschak continued to flourish under Koopmans and a small gap in the publication of monographs was quickly filled.

William B. Simpson was made Assistant Director of Research of the Cowles Commission in 1948. With steadily increasing administrative responsibilities, the post was changed to Executive Director in 1951 and Simpson held this position until 1953. Concurrently, he served the Econometric Society as Secretary (1948-52), Managing Editor of Econometrica (1949-51) and as Co-Editor (1951-53). The economics profession and the Econometric Society owe Simpson a substantial debt for valuable services in these capacities at a very high cost in interruption of his own research program.⁶

Some differences on procedural matters developed between Simpson and others of the Cowles faculty in 1953. These led to his resignation. Rosson L. Cardwell was appointed Executive Director and served very capably, particularly with the inevitable complications of the transfer to Yale. The post was not continued at the Cowles Foundation.

The work of the Commission is reviewed in the next three sections under the headings, Simultaneous Equations (Section 2), Activity Analysis (3), and Related Topics (4). A final section (5) contains some supplementary observations.

In reporting the Commission's work I have generally not tried to distinguish those cases in which part of the work was done at other locations and/or with joint sponsorship. Several Research Associates were classed as Consultants after leaving the Commission and returned periodically to offer seminars and participate in discussions. Others spent summers at other locations (the Rand Corporation in several instances) pursuing essentially the same interests as at the Commission. For instance, Haavelmo's key paper (1944) on the probability approach was partly written at Harvard and Arrow returned from Rand in the fall of 1948 with the essentials of his treatise on social choice (1951a) already developed. Pursuit of such minor historical matters would have been an overwhelming diversion. Everything reported has a strong Cowles Commission component and, in most cases, is as identifiably Cowles as any research pursued at one location, but drawing on activities of a larger intellectual community. Anyone interested in such matters would find the above-mentioned Annual Reports a pretty complete source.

Two circumstances explain the fact that econometric work is reviewed more intensively than mathematical economics. During much of the period reported, new econometric developments were being pursued mostly at Cowles. Mathematical economics was more widely studied (see Arrow, 1983, p.5). The second is that, at the time of writing, it seemed to the author that recent accounts of important developments in mathematical economics were more comprehensive. No judgement of relative importance is intended.

Footnotes

¹Memo from Herbert A. Simon, August 2, 1982.

Franco Modigliani came to the University of Chicago in September, 1948 as Research Associate in the Cowles Commission and a Post-doctoral Fellow in the Department of Economics. He moved to the University of Illinois in November, but continued as a research consultant to the Commission until 1955.

²Letter from Kenneth J. Arrow, November 24, 1982.

³Also sometimes called the "Marschak Rule" from its use at the Interdisciplinary Colloquium on Mathematics in the Behavioral Sciences, UCLA.

⁴This list does not include visitors, student fellows not engaged in Commission research, or consultants who served very briefly. For a more complete listing with biographical sketches see Christ (1952), pp. 110-150.

⁵For biographical notes and a review of Koopman's contribution, see Werin (1976).

⁶See the partial biography in Christ (1952), p. 143.

2. Simultaneous Equations

2.1 The Probability Approach

To draw statistical inferences from an observation y a researcher assumes that y is a realization (or drawing of a possible value) of a random object, say Y , governed by a probability measure P_Y . P_Y is unknown but assumed to belong to a collection \mathcal{P} of probability measures. \mathcal{P} is called the statistical model or specification. Typically y is a vector or matrix of numerical observations and Y is a corresponding array of random variables.

An inference traditionally consists of selecting a particular measure $\bar{P}(y)$ or a subset of \mathcal{P} , say $\bar{\mathcal{P}}(y)$ that is in some sense favored by the observation y . $\bar{P}(y)$ could be the measure corresponding to maximum likelihood estimates of unknown parameters of P_Y . $\bar{\mathcal{P}}(y)$ might be the result of applying a set valued function to y . One chooses in advance a function that has a high probability of including P_Y .¹ Much of classical theoretical statistics consists of proposing and analyzing criteria for saying that a particular measure or subset might reasonably be regarded as favored.

Statistical inference has been useful in many sciences and in applying science to practical problems, e.g. see Mosteller et al (1972). People at the Cowles Commission believed that statistical inference could be very useful in economics and were concerned to find good procedures for making economic applications. It was believed that, in many studies, models different from those that had already appeared

in the statistical literature would be needed. Econometricians and theoretical statisticians would have to extend statistical inference to new models.

Not everyone agreed. There was quite a bit of resistance to regarding economic data as observations of random objects and there were feelings that economic interrelations were too complex to be covered by a tractable model.² The first objection, sometimes phrased that probability does not apply to economic phenomena, drew support from Knight's (1939) distinction between risk and uncertainty.

Such issues are not fully resolved. However, a number of considerations have led to several decades of development and deployment of econometrics with statistical inference the basic approach. The arguments advanced at the Cowles Commission and elsewhere during the 1940's and 50's had considerable impact. The redevelopment of personal probability by Savage (1954) and others (see Rubin 1949a,b, Arrow 1951c, Chernoff 1954) made a probabilistic framework for economic analysis more plausible to many. Greater familiarity of economists with the basics of statistical inference made comparisons with less formal approaches possible and statistical inference had a strong appeal.

The initial modelling suggestions of the Cowles Commission have been extended and new departures have emerged (e.g. see Fair, 1984; Griliches and Intriligator, 1982, 1984; Fienberg and Zellner, 1975). With additional aid from the computer explosion and greater data availability, the collection of tractable models available to applied econometricians has vastly expanded.

In practice statistical inference is almost always applied through

parametric models. Each contemplated probability measure is uniquely determined by a finite set of numbers called parameters.³ Let θ be a k -dimensional real vector representing the k parameters of a particular model. Then the model can be written

$$\mathcal{P} = \{P_{Y|\theta} : \theta \in \Theta\}$$

where Θ is the set of all parameter combinations regarded as possible and $P_{Y|\theta}$ is the measure determined by the particular parameter combination θ . Statistical inference then consists of using the observation y to make meaningful assertions about θ . Appropriate methods for doing so depend critically on stochastic properties of the family \mathcal{P} .

The 1937 volume by Koopmans was one of the earliest examples of the derivation of statistical methods for a probabilistic model that had been particularly suggested for economic investigations. An error in variables regression model was analyzed via maximum likelihood. Haavelmo (1944), provided the first reasonably comprehensive rationale for probabilistic models in econometrics. Problems of model building were discussed along with a sketch of how statistical analysis could be developed in a quite general setting. Other early papers which emphasized the probability approach were Haavelmo (1943), Hurwicz (1944) and Marschak's introduction to Cowles Commission Monograph No. 10 (1950).

In seeking to develop useful probability models for econometric applications, Cowles Commission workers were quickly led to systems of economic relations. Economic theory was naturally regarded as the principal guide. Bringing economic and statistical theory jointly to bear on empirical analysis was a stimulating challenge. Firm and household optimization theories, Marshallian partial equilibrium theory, Walrasian general equilibrium theory, and Keynesian theory

all indicated that, except for special cases, economic variables were determined by the joint actions of several or many sets of participants (consumers, producers, investors, etc.) in the relevant economic sector. Even if an investigator wanted to concentrate on one group of participants, it would usually be necessary to consider interactions with others.

There was also precedent in prior econometric work. Since before the First World War, econometricians had sometimes considered two-equation supply and demand models - e.g. Lenoir (1913); Lehfeldt (1914). Mary S. Morgan (1981) has written an excellent review of these early studies, about 1913 to about 1940. The models were not probabilistic. Investigators typically plotted or examined algebraically price-quantity data making very special assumption about shifts in two variable supply and demand curves. The "cobweb theorem" was formulated by H.L. Moore (1914) and elaborated by others, particularly Henry Schultz (1938). Frisch's algebraic analysis (1933) was based on assumptions about the realized values of descriptive statistics - sample means, variances, covariances - of explicitly included shift variables.

Frisch (1934) also developed bunch maps to check on the possibility of additional relations among variables when only one equation had been specified. Tinbergen (1939) had analyzed a several equation macro model of the United States economy.

As multi-equation probability models were formulated and studied, it became desirable to refine several distinctions and concepts that had been vaguely noted in earlier literature. Variables whose values were considered to be determined by the system under consideration were called endogenous, those whose values could be regarded as determined outside the system were called exogenous.

"In determining which variables are set aside as exogenous, two main principles are implicitly or explicitly applied in economic literature. They might be described as the departmental principle and the causal principle. The departmental principle treats as exogenous those variables which are wholly or partly outside the scope of economics, like weather and climate, earthquakes, population, technological change, political events. The causal principle, which does not always lead to the same result, regards as exogenous those variables which influence the remaining (endogenous) variables but are not influenced thereby.

The causal principle is often used also if it applies only approximately, that is, if the influence of the endogenous variables on those treated as exogenous is presumed to be small. For instance, in explaining the level of employment in a country which has only a small share in world trade, the shifts in the schedules of foreign demand for its exports and of foreign supply of its imports are sometimes treated as exogenous in a first approximation. Another example is found in the formation of quantity and price of a consumers' good that attracts only a small fraction of consumers' expenditure. In such cases, consumers' income is often taken as an exogenous variable, operating at the demand side, although of course consumers' income itself depends on the demand for all commodities." [Koopmans (1950), pp. 393-47.

In time series, lagged values of both endogenous and exogenous variables are usually appropriate. Under certain conditions both

lagged endogenous variables and exogenous variables (lagged and contemporaneous) are called predetermined variables and can be treated similarly for limited statistical purposes (see Koopmans (1950), pp. 402-5).

A property called identification was recognized as basic. It is the subject of many subsequent inquiries (see Hsiao (1983)) and is briefly reviewed here in Subsection 2.2, pp.

It has been widely recognized that many problems of econometric model construction are shared by other sciences mainly dependent on non-experimental data. See Marschak (1949a), Goldberger (1972), Goldberger and Duncan (1973), Greenberger, et al (1976), Blalock (1971). If applicable controlled experiments were typically available, controlled variables could be made exogenous. Careful designs could balance statistical efficiency and relevance to real-world situations. Experiments could be directed toward most-needed information.

Several types of probability models were considered at the Commission. The linear model sketched below was most intensively studied and was used in most Cowles related applications of the period. It is still frequently applied and is still the subject of interesting theoretical research. The static or contemporaneous version of the linear simultaneous equations model may be written

$$\sum_{h=1}^G \beta_{gh} y_{ht} + \sum_{k=1}^K \alpha_{gk} x_{kt} = u_{gt} \quad \begin{matrix} g=1..G \\ t=1..T \end{matrix} \quad (2.1.1)$$

representing the joint determination of G endogenous variables y_{ht} in T observations or trials from the values taken, at each trial, by K observed exogenous variables x_{kt} and G unobserved random disturbances u_{gt} .

In matrix notation

$$\begin{matrix} \beta y & + & \alpha x & = & u \\ GxG & GxT & GxK & KxT & GxT \end{matrix} \quad (2.1.2)$$

Unless β is nonsingular there are infinitely many solutions for y_t at each trial contrary to the assumption that the observed y 's are determined by the system; so β is assumed to be nonsingular. Each of the G equations is intended to approximate the behavior of a particular group of participants (e.g. consumers, producers, investors) in the economic sector under study.

Even if different values of t refer to different time periods, such a model is regarded as static so long as no equations involve lagged values of the endogenous variables and the disturbance vector at one time is assumed to be statistically independent of the disturbance vector at any other time. x may contain lagged values of exogenous variables without disturbing the statistical properties of the system.

Since β is nonsingular, one may write

$$\begin{aligned} y &= -\beta^{-1} \alpha x + \beta^{-1} u \\ &= \pi x + v \end{aligned} \quad (2.1.3)$$

explicitly showing y as a function of x and u or x and $v = \beta^{-1} u$. (2.1.3) is called the reduced form and (2.1.2) is the structural form of the system. A particular coefficient π_{gk} of the reduced form tells how the endogenous variable y_{gt} responds to a unit change in the exogenous variable x_{kt} for any trial t when all interacting structural equations are taken into account.

In the normal static linear model (NLM), u is assumed to be a realization of a $G \times T$ random matrix U with independently and identically

distributed columns $U_{[t]} \stackrel{d}{=} N(0, \Sigma)$. This implies that y is a drawing from a random matrix Y with independently distributed columns $Y_{[t]} \stackrel{d}{=} N(-\beta^{-1} \alpha x_{[t]}, \beta^{-1} \Sigma \beta^{-1'})$. x is non-random and independent of U . Thus the set \mathcal{P} of possible distributions of Y is specified and one may consider statistical inferences regarding elements or subsets of \mathcal{P} . An economist analyzing the model hopes to connect such inferences to the economic parameters β, α, Σ . Establishing connections is called identification and is discussed in the next subsection.

The static linear model (LM) differs from NLM in that normality of U and Y is not assumed. In this case a parameter point (β, α, Σ) does not specify a unique distribution of Y but a family of distributions with the same first and second moments. The statistical model \mathcal{P} is enlarged and only partly parametrized. Inferences for LM are based largely on analogies with NLM and, as expected, are less precise. This is briefly discussed in Subsection 2.3, pp. below.

When elements of different columns of U are permitted to be correlated, they are called serially dependent and the model will be designated LMU. If t represents time, LMU is a dynamic model.

If t represents time and the vector of endogenous variables $y_{[t]}$ depends on values taken by all or some of these variables in earlier time periods, the system (2.1.2) must be altered to include these earlier (lagged) values. Write

$$\beta y + \sum_{j=1}^J \beta^{(j)} y^{(j)} + \alpha x = u \quad (2.1.4)$$

where

$$y^{(j)} = \begin{pmatrix} y_{1,1-j} & y_{1,2-j+1} \cdots y_{1,T-j} \\ \vdots & \\ y_{G,1-j} & y_{G,2-j+1} \cdots y_{G,T-j} \end{pmatrix}$$

$$\beta^{(j)} = \begin{pmatrix} \beta^{(j)11} & \beta^{(j)12} & \cdots & \beta^{(j)1G} \\ \vdots & & & \\ \beta^{(j)G1} & \beta^{(j)G2} & \cdots & \beta^{(j)GG} \end{pmatrix} \quad j = 1 \dots J.$$

Let $y^{(j)}[t]$ be the t th column of $y^{(j)}$. Note that $y^{(j)}[t-k] = y^{(j+k)}[t]$ $j+k \leq J$. The model is abbreviated LMY. Since $y_{g,t-j}$ $j > 0$ depends only on disturbances and exogenous variables prior to t , these lagged endogenous variables are stochastically independent of the current disturbances and, for several purposes, may be treated like exogenous variables (see Koopmans, 1950). In an LMY model the exogenous and lagged endogenous variables are sometimes grouped together as predetermined variables and denoted by a common symbol, say z . One has

$$\beta y + \gamma z = u \quad (2.1.5)$$

for the structural form and

$$y = -\beta^{-1}\gamma z + \beta^{-1}u, \quad (2.1.6)$$

for the reduced form where

$$\gamma = (\beta_{(1)} \cdots \beta_{(J)} \alpha) \quad \text{and}$$

$$z = \begin{pmatrix} y_{(1)} \\ \vdots \\ y_{(J)} \\ x \end{pmatrix}.$$

For some aspects of statistical analysis, as distinct from practical interpretation, LM and LMY may be treated alike.

If a system has both lagged endogenous variables and serially dependent disturbances, it will be called a doubly dynamic linear model and designated DLM. If LMU, LMY, or DLM is assumed to have normally distributed disturbances the respective designations will be NLMU, NLMY, or NDLM.

For either DLM or LMY (or either normal counterpart), another statement of the system is of interest. The final form (see Theil and Boot (1962), Intriligator (1983)) expresses endogenous variables for $t=1..T$ as functions of the exogenous variables, and disturbances for periods $1..T$, and the "initial values" of the endogenous variables for periods $0, -1, \dots, -(J-1)$. For a model with one period lag ($J=1$), the reduced form is

$$\begin{aligned} y &= -\beta^{-1} \beta_{(1)} y_{(1)} + \beta^{-1} \alpha x + \beta^{-1} u \\ &= \pi_{(1)} y_{(1)} + \pi x + v. \end{aligned}$$

The equation determining the current endogenous variables at time t is

$$y_{[t]} = \pi_{(1)} y_{(1)[t]} + \pi x_{[t]} + v_{[t]} \quad t=1..T. \quad (2.1.7)$$

Omitting the lower square brackets and letting $y_{(j)[t]} = y_{t-j}$, one may make successive substitutions

$$\begin{aligned} y_t &= \pi_{(1)} [\pi_{(1)} y_{t-2} + \pi x_{t-1} + v_{t-1}] + \pi x_t + v_t \\ &\quad \vdots \\ &= \pi_{(1)}^t y_0 + \sum_{j=0}^{t-1} \pi_{(1)}^j \pi x_{t-j} + \sum_{j=0}^t \pi_{(1)}^j v_{t-j} \end{aligned} \quad (2.1.8)$$

When lagged endogenous variables are present, a unit change in an exogenous variable at a particular date generally affects the endogenous variables of that and subsequent dates. Let $\pi^*(t, j) = \pi_{(1)}^j \pi$. Then a

unit change in $x_{k,t-j}$ changes y_t by $\pi^*(t,j)_{[k]}$ where the subscript indicates the k th column of the GxK matrix and when intermediate changes in y_{t-j} on y_t are taken into account. Although the final form was not studied at the Cowles Commission, its existence for dynamic models was postulated by Koopmans (1953), p. 36. It is a natural dynamic extension of the static reduced form. Expressing each dated value of an endogenous variable as a linear function of dated exogenous variables and disturbances, it is convenient, among other things, for connecting assumptions about distributions of disturbances with consequent distributions of endogenous variables.

The above linear models are frequently called error-in-equation or shock⁴ models characterized by the fact that randomness is introduced through a stochastic variable introduced into each equation. If the model allows for random errors in observed values of the variables it is called an error-in-variables model or sometimes just an error model. If both kinds of random components are included it is a shock-error model. Some results for shock-error models are noted in Subsection 2.2.5 below. At the Cowles Commission they were studied by Anderson and Hurwicz (1947, 1948).

Section 2.1 Footnotes

- ¹Confidence regions are the principal example, see Pfanzagl (1978).
- ²For discussion of these points see Sections 4 and 6 of Koopmans (1937).
- ³There are also partially parametrized models in which a point in the parameter space corresponds to a set of probability measures. See pp.
- ⁴This term seems to have originated in Marschak (1950), see pp. 18-21 for a discussion of various random components that should, in principle, be considered.

2.2 Identification

2.2.1 Abstract Identification

Let Ψ be a function from a set \mathcal{S} onto a set \mathcal{B} .

$$\Psi : \mathcal{S} \rightarrow \mathcal{B}$$

The problem of identification of \mathcal{S} with respect to \mathcal{B} via Ψ is that of determining something of the behavior of Ψ^{-1} which is defined on \mathcal{B} by $\Psi^{-1}(b) = \{g \in \mathcal{S} : \Psi(g) = b\}$. In general, Ψ^{-1} is a correspondence from \mathcal{B} to \mathcal{S} or a function from \mathcal{B} to the set of all subsets (the power set) of \mathcal{S} .¹ Write

$$\begin{aligned} \Psi^{-1} : \mathcal{B} \rightarrow \mathcal{S} & \quad \text{or} \\ \Psi^{-1} : \mathcal{B} \rightarrow \pi(\mathcal{S}) \end{aligned}$$

where \rightarrow indicates a correspondence and π indicates the power set.

The following definitions will be used.

1. A subset G of \mathcal{S} is identified with respect to \mathcal{B} via Ψ iff $\Psi^{-1}(\Psi(G)) = G$.² Hereafter, the phrases "with respect to" and "via" will usually be omitted when the function and its range are clear from the context.

2. A subset G is completely identified iff $\Psi^{-1}(\Psi(g)) = \{g\} \forall g \in G$. $\{g\}$, called a singleton, denotes the subset of \mathcal{S} whose only element is g . Alternatively, one may say that G is completely identified iff G is identified and Ψ is 1 to 1 on G .

3. A collection $(G_\delta)_{\delta \in D}$ of subsets of \mathcal{S} is [completely] identified iff each element G_δ $\delta \in D$ is [completely] identified.

Let \mathcal{J} be the family of identified subsets of \mathcal{S} and \mathcal{J}^* the completely identified family. The following remarks are immediate.³

(i) $\mathcal{J}^* \subset \mathcal{J}$

(ii) $\mathcal{S} \in \mathcal{J}$, $\emptyset \in \mathcal{J}^*$

- (iii) $G \in \mathcal{J} \Rightarrow G^c \notin \mathcal{J}$. G^c denotes the complement.
- (iv) If (G_δ) is an identified collection, then all unions of sets in (G_δ) are identified.
- (v) If G is completely identified then every subset of G is completely identified.

As a fairly general example suppose it is known or assumed that some treatment g known to be from a set \mathcal{J} has been applied to a specified object. Suppose each treatment is known to produce a particular result $b = \Psi(g)$. An interested party can learn something of the result and wishes to infer something about the treatment.

If the party is particularly interested in the possibility that the actual treatment was in a set G and G is identified, then any evidence that $b \in \Psi(G)$ immediately weighs equally in favor of the proposition, $g \in G$. In particular, if it becomes known for sure that $b \in \Psi(G)$ then it is also known that $g \in G$. If G is not identified then $\Psi^{-1}(\Psi(G)) - G$ is not empty and could have occurred even if $b \in \Psi(G)$. Thus evidence that $b \in \Psi(G)$ must be qualified in making judgements about $g \in G$ when G is not identified.

The simplest case occurs when Ψ is 1 to 1. Then \mathcal{J} is completely identified (Definition 2), and every subset of \mathcal{J} is completely identified (Remark v). Ψ^{-1} is then a function from \mathcal{B} to \mathcal{J} . In this case if one learns that some result b occurred, it follows that the specific treatment $g = \Psi^{-1}(b)$ must have been applied.

If $\Psi(g_1) = \Psi(g_2)$, treatments g_1 and g_2 lead to the same result and no evidence about results will distinguish them. They might be called Ψ -equivalent. Note that $\Psi^{-1}(\Psi(g))$ is the set of all treatments Ψ -equivalent to g and that $\Psi^{-1}(\Psi(G))$ is the set of all treatments Ψ -equivalent to some

member of G . If $\{g\}$ is identified and $b = \psi(g)$, one may say that b identifies g , i.e., learning b enables one to conclude g .

Problems that have the above logical structure occur often in ordinary life and in various lines of investigation. Does the condition of the corpse indicate the cause of the demise? Does the way the motor fails indicate the internal maladjustment? Does a deep color indicate the fruit has ripened?

2.2.2 Identification in Probability Models

In the abstract, identification does not necessarily involve probability. Before probability models were common in econometrics, what is now called identification was sometimes encountered in deterministic models - Morgan (1981), Wold (1969). Use of deterministic models by econometricians continued in order to conveniently clarify some aspects of identification before introducing randomness - Marschak (1950), Simon (1952). To avoid undue length and repetition, such examples are not elaborated in the present paper. One could interpret the models considered here as deterministic by assuming that the random variables included are degenerate, taking their mean values with probability one.

Scientists are interested in explaining observed phenomena. In probabilistic analyses what is explained is not the precise observation but a probability measure or set of measures according to which the observation is reasonable.

The scientist consults his basic theory, and any additional information he decides can appropriately be brought to bear, and constructs a substantive model. The substantive model may be viewed as a collection of explanations for possible distributions of the random object whose realization

is observed and whose possible probability laws comprise the statistical model. In most econometric literature the explanations are called structures. Each structure determines a unique probability measure. This defines a function from the set of possible structures (the substantive model or specification) onto the set of possible measures (the statistical model or specification). Write

$$\Psi : \mathcal{S} \rightarrow \mathcal{P} \quad (2.2.1)$$

The identification problem concerns the precision with which inferences about the underlying measure P (the subscript Y will usually be omitted) can be translated into inferences about the underlying structure s - i.e. identification is a name for some properties of $\Psi^{-1} : \mathcal{P} \rightarrow \pi(\mathcal{S})$ where $\pi(\mathcal{S})$ is the power set of \mathcal{S} .

Clearly the definitions and remarks of the previous subsection can be applied by substituting $s, \bar{\mathcal{S}}, \mathcal{S}$ for g, G, \mathcal{G} in the domain of Ψ and $P, \bar{\mathcal{P}}, \mathcal{P}$ for b, B, \mathcal{B} in the range.

If the subset $\bar{\mathcal{S}}$ of structures is identified with respect to \mathcal{P} via Ψ , then a statistical test of the hypothesis $H_0 : s \in \bar{\mathcal{S}}$ is equivalent to testing $H_0^* : P \in \Psi(\bar{\mathcal{S}})$ since $s \in \bar{\mathcal{S}} \Leftrightarrow \Psi(s) \in \Psi(\bar{\mathcal{S}})$. If $\bar{\mathcal{S}}$ is not identified there is no equivalent test in the \mathcal{P} -space.

In the Chicago period literature, aspects of structural identification were sometimes elucidated by considering a hypothetical fortunate investigator whose observation completely determines the underlying probability measure.⁴ He knows P and then considers what can be said about s . $s \in \Psi^{-1}(P)$ is what can be said. If \mathcal{S} is completely identified then s becomes known also. Otherwise, $\Psi^{-1}(P)$ may contain several or many Ψ -equivalent structures.

In the probability model context, Ψ -equivalence is called observational equivalence. The concept was introduced in Haavelmo (1943) and developed in Haavelmo (1944); Koopmans, Rubin and Leipnik (1950); and Marschak (1953).

Note that, by Definition 1, p. a singleton $\{s\}$ is identified iff the substantive model contains no other structure observationally equivalent to s .

If a subset of structures \bar{S} consists of all structures having a given property - e.g. an inelastic demand function for a certain commodity, a given value for a particular parameter, or random terms belonging to a particular family - then the property is said to be identified or not according to whether or not the subset is identified.

If \bar{S} is identified, then learning P enables the researcher to either assert $s \in \bar{S}$ or to assert $s \in \bar{S}^c$. In fact, to make the appropriate assertion, the researcher does not have to learn P precisely, but must know whether $P \in \Psi(\bar{S})$ or $P \in \Psi(\bar{S}^c)$.

The above approach is a mild modification of that developed by Hurwicz (1950a) and employed by Koopmans and Reiersol (1950). Their discussions are mainly in terms of the model S whereas it has seemed convenient here to emphasize the role of the function Ψ . Their statistical models are derived from functions of possible distributions of latent (unobserved) variables. This is usual in econometrics but such a format isn't always needed - e.g. Bowden (1973), Rothenberg (1971). There are also minor differences in terminology. For example, an identified structure as defined in this section would be called "uniquely identified" in Hurwicz and "identifiable" by Koopmans and Reiersol.

2.2.3 Parametric Models

As noted earlier, models in statistics and econometrics are nearly always formulated in parametric form. Each element s of the substantive model is associated with a parameter set λ which is usually an M -tuple of real numbers $\lambda = (\lambda_1 \dots \lambda_M)$; λ_m is called the m^{th} structural parameter. If the parametrization is full - each λ stands for just one structure - it doesn't logically matter whether one works with the set of possible structures S or the set of possible parameter vectors, say Λ . For several purposes parametric representation is more convenient. If the Λ - parametrization is full, one has

$$S = \{s_\lambda : \lambda \in \Lambda \subset \mathbb{R}^M\} \quad \lambda \neq \lambda^* \Rightarrow s_\lambda \neq s_{\lambda^*} .$$

If Λ provides just a partial parametrization, there are at least some parameter vectors that correspond to more than one structure. Let \bar{S}_λ be the set of structures that corresponds to λ .

$$S = \bigcup_{\lambda \in \Lambda} \bar{S}_\lambda .$$

In practice one of the most useful notions is the identification of a particular structural parameter.

Def. The m th structural parameter is said to be identified with respect to \mathcal{P} iff λ_m is constant over observationally equivalent structures.

In symbols, λ_m is identified iff

$$\forall P \in \mathcal{P} \quad s_\lambda, s_{\lambda^*} \in \Psi^{-1}(P) \Rightarrow \lambda_m = \lambda_m^* .$$

This means that determining the distribution P would also determine the value of λ_m .

Let $\Lambda_{(m)}$ represent the admissible values of λ_m . Formally

$$\Lambda_{(m)} = \{w \in R : \exists \lambda \in \Lambda \exists \lambda_m = w\}.$$

Now let Θ provide a parametrization for \mathcal{P} . If full,

$$\mathcal{P} = \{P_\theta : \theta \in \Theta \subset R^N\} \quad \theta = (\theta_1 \dots \theta_N) \quad \theta \neq \theta^* \Rightarrow P_\theta \neq P_{\theta^*}.$$

If partial,

$$\mathcal{P} = \bigcup_{\theta \in \Theta} \bar{\mathcal{P}}_\theta \quad \bar{\mathcal{P}}_\theta = \{P \in \mathcal{P} : P \text{ is indexed by } \theta\}.$$

Let $\varphi : \Lambda \rightarrow \Theta$ be a function onto Θ . By Def. 1, p. 1 a subset $\bar{\Lambda}$ of Λ is identified with respect to Θ via φ iff $\varphi^{-1}(\varphi(\bar{\Lambda})) = \bar{\Lambda}$. The other definitions and remarks of Subsection 2.2.1 also apply. λ and λ^* are φ -equivalent iff $\varphi(\lambda) = \varphi(\lambda^*)$.

Def. The m th structural parameter is said to be identified with respect to Θ iff λ_m is constant over φ -equivalent structures.

If λ_m is identified with respect to Θ then determining the parameter set Θ of the distribution would also determine the value of λ_m .

In the literature to date, investigators commonly specify that first and second moments exist for the observed random object. If, in addition, a normal distribution is specified, parametrization in terms of first and second moments is full. If the family of distributions is left unspecified, such parametrization is partial.

In the non-normal case, identifiability with respect to a parameter space Θ which consists of first and second moments is sometimes called identifiability in the wide sense and two structures that imply equal first and second moments are called observationally equivalent in the

wide sense, Hsiao (1983), p. 7. Thus observational equivalence in the wide sense is the same as φ -equivalence when points in the parameter space determine means and variances but not the form of the distribution.

If Λ is a subset of R^M (or more generally a topological space) and for a given $\lambda \in \Lambda$ there is a neighborhood of λ , η_λ such that $\varphi^{-1}(\varphi(\lambda)) \cap \eta_\lambda = \{\lambda\}$, then Λ is said to be locally identified and $\theta = \varphi(\lambda)$ will be said to locally identify λ . Identification as used in the present paper is sometimes called global identification.

2.2.4 Linear Shock Models with Linear Constraints

Much of the statistical theory constructed at the Cowles Commission applied to the linear models LM, LMY and their normal counterparts described in Subsection 2.1, pp. . For the most part, the results pertaining to identification also apply to LMU and NLMU but not to DLM and NDLM.

Emphasis on these models did not reflect high confidence that they would always prove adequate in practice.⁵ Rather, getting desired statistical results for these relatively simple models proved a difficult and gradual process. For the most part more complicated models just had to wait. It was hoped that experience with applications of the models then being developed along with continued theoretical work would steadily expand the repertoires of practitioners. This has been happening, in part by continued progress along lines suggested by the Commission's work

and in part by innovations. See Hsiao (1983), Fisher (1966).

In the remainder of this subsection, the principal Cowles Commission results on linear models with linear constraints are cited. Some are stated in a slightly more general form than in the original publications. It is convenient to start with a formulation that includes (N)LM, (N)LMU and then cite an argument of Koopmans, Rubin and Leipnik (1950) that (N)LMY is also covered. Some results involving nonlinear restrictions and other models will be briefly noted in the next subsection.

Recall that the structural or substantive form of a linear shock model is indicated

$$\beta y + \alpha x = u \quad \text{or} \quad (2.2.4.1)$$

$$\sum_{i=1}^G \beta_{gi} y_{it} + \sum_{k=1}^K \alpha_{gk} x_{kt} = u_{gt} \quad g = 1..G, \quad t = 1..T.$$

y_{it} is the observed value of the i th jointly dependent variable in the t th trial or time period. x_{kt} is the observed value of the k th exogenous variable at trial t . u_{gt} is the realized but unobserved value of a random disturbance.

β_{gi} and α_{gk} $g, i = 1..G, k = 1..K$ are behavioral coefficients. The interpretation of the structure as determining y from x and u requires that the $G \times G$ matrix β be nonsingular. Multiplying both sides of the matrix equality by β inverse yields the reduced form

$$y = -\beta^{-1} \alpha x + \beta^{-1} u = \pi x + v \quad (2.2.4.2)$$

$$\pi = -\beta^{-1} \alpha, \quad v = \beta^{-1} u. \quad (2.2.4.3)$$

Equations like (2.2.4.1) to (2.2.4.3) also connect the underlying random matrices Y, U, V of which y, u, v are realizations.

$$\beta Y + \alpha x = U \quad (2.2.4.1^*)$$

$$Y = -\beta^{-1}\alpha x + \beta^{-1}U = \pi x + V \quad (2.2.4.2^*)$$

$$V = \beta^{-1}U \quad (2.2.4.3^*)$$

The statistical model is a set \mathcal{P} of possible distributions of Y . Since Y is an array of random variables, each possible distribution can be denoted by its corresponding distribution function F_Y .

If it is understood that the components are to be combined as in (2.2.4.1*) then a structure may be indicated $s = (\beta, \alpha, F_U)$ where F_U is a specific distribution function (with finite mean) for U . s then determines a specific F_Y via

$$\Psi(s) = \{F_Y : F_{Y-\pi x} = F_{\beta^{-1}U}, \quad \pi = -\beta^{-1}\alpha\} \quad (2.2.4.4.)$$

Throughout, the distribution function for any random matrix A will be denoted F_A . The last two equalities of (2.2.4.4) follow from (2.2.4.2*), (2.2.4.3*). To apply the function Ψ , one would determine $F_{\beta^{-1}U}$ from F_U and then F_Y from $F_{(Y-\pi x)}$.

The substantive model is indicated

$$S = \{(\beta, \alpha, F_U) : |\beta| \neq 0, (\beta, \alpha) \in \mathcal{B}, F_U \in \mathcal{F}\} \quad (2.2.4.5)$$

where \mathcal{F} is a set of admissible distribution functions and \mathcal{B} a set of admissible pairs of coefficient matrices. At this stage $EU = 0$ is the only requirement that will be imposed via \mathcal{F} . Stricter provisions will be considered later. In the models of this subsection, admissible coefficients β are defined by a set of linear restrictions.

Write the statistical model

$$\mathcal{P} = \{F_Y : \exists s \in S \exists F_{(Y-\pi x)} = F_{\beta^{-1}U}, \quad \pi = -\beta^{-1}\alpha\}.$$

$\Psi: S \rightarrow \mathcal{P}$ is defined by (2.2.4.4) above. To consider identification via Ψ it is useful to look at observationally equivalent structures. Recall that two structures $s_1, s_2 \in S$ are observationally equivalent if they determine the same distribution of Y , that is, if $\Psi(s_1) = \Psi(s_2)$.

Proposition 2.2.4.1. In a linear shock model as given by (2.2.4.5); $s_1 = (\beta_1, \alpha_1, F_{U_1}^{(1)})$ and $s_2 = (\beta_2, \alpha_2, F_{U_2}^{(2)})$ are observationally equivalent if $\exists |H| \neq 0$ $\beta_2 = H\beta_1, \alpha_2 = H\alpha_1, F_{U_2}^{(2)} = F_{HU_1}^{(1)}$. The converse is true if x is of full column rank.

This proposition is essentially the same as Theorem 2.1.3.5, p. 16 of Koopmans, Rubin and Leipnik (1950).

In the remainder of this subsection, $\rho(x) = K$ is assumed. Proposition 2.2.4.1 allows many observationally equivalent structures. To get sharper results, it is necessary to use narrower restrictions on coefficients and/or F_U . In most econometric work, linear constraints on coefficients have been emphasized. These are sometimes classified as normalizing restrictions and substantive restrictions.

Normalizing restrictions arise from the fact that any of the G behavioral equations can be multiplied (on both sides) by any nonzero constant without changing the solution sets (values of y consistent with given values of x, u) or the economic meaning. For instance if the elements of $\beta_{(g)}, \alpha_{(g)}$, the g th rows of β and α , are doubled and the disturbance is considered a random drawing of a random variable $2U_g$ instead of U_g , the basic relation is not changed. σ_{gg} would be multiplied by a factor of four; $\sigma_{gh}, h \neq g$ would be doubled. The implied behavior is not changed, only a detail of the investigator's way of expressing it.⁶

This redundancy is commonly removed by setting the diagonal elements of β equal to one. This may involve reordering equations and/or coefficients to place coefficients strongly believed to be nonzero in diagonal positions. For a specific nonsingular β this can always be done. Observe that $\beta_{gg} = 1 \quad g=1..G$ is a special set of G linear restrictions on the behavioral coefficients β, α .

The more important restrictions, substantive restrictions arising from the investigator's beliefs about the economic sector being examined, also frequently take the form of linear equations involving the behavioral coefficients. For reasons of tractability and widespread applicability, models involving such restrictions were emphasized in the early literature.

If both normalizing and substantive restrictions take the form of linear equations in the behavioral coefficients, then $(\beta, \alpha) \in \mathcal{B}$ can be replaced by

$$\vec{\beta}B + \vec{\alpha}A = d \quad (2.2.4.6)$$

in (2.2.4.5). $\vec{\beta} = (\beta_{(1)} \beta_{(2)} \dots \beta_{(G)})$ is a row vector consisting of the rows of β written in order. $\vec{\alpha}$ is a similar elongation of α . B, A, d are $G \times r$, $K \times r$, and $1 \times r$ matrices of known constants.

For given $(\beta, \alpha, F_U) \in \mathcal{S}$, the observationally equivalent structures are of the form $(H\beta, H\alpha, F_{HU})$ where H is non-singular and must satisfy

$$(\vec{H}\vec{\beta})B + (\vec{H}\vec{\alpha})A = d. \quad (2.2.4.7)$$

$(\vec{H}\vec{\beta}) = (H_{(1)}\beta \dots H_{(G)}\beta) = \vec{H}(I \otimes \beta)$ where $I \otimes \beta$ is the Kronecker product

(Theil (1983), p. 16) $I \otimes \beta = \begin{pmatrix} \beta & 0 & \dots & 0 \\ 0 & \beta & \dots & 0 \\ \dots & \dots & \dots & \dots \\ 0 & 0 & \dots & \beta \end{pmatrix}$. $\vec{H} = (H_{(1)} \dots H_{(G)})$. Similarly

$(\vec{H}\vec{\alpha}) = (H_{(1)}\alpha \dots H_{(G)}\alpha) = \vec{H}(I \otimes \alpha)$. One may rewrite (2.2.4.7) as

$$\vec{H}((I \otimes \beta)B + (I \otimes \alpha)A) = d, \quad (2.2.4.8)$$

a set of G^2 linear equations in the G^2 elements of H . It is clear that one solution to (2.2.4.8) is $h_{gg} = 1$, $g=1..G$; $h_{gk} = 0$ $g \neq k$. For this solution $H=I$, $H\beta = \beta$, etc. If this is the only solution then (β, α, F_U) is identified because there are no distinct observationally equivalent structures. The above solution is unique iff the rank of the coefficient matrix, $((I \otimes \beta)B + (I \otimes \alpha)A)$, is G^2 . Since the matrix has G^2 rows and r columns, identification requires at least G^2 restrictions.⁷ Summarizing,

Proposition 2.2.4.2. A linear structure (β, α, F_U) is identified by linear restrictions on the structural coefficients iff the restriction matrix $((I \otimes \beta)B + (I \otimes \alpha)A)$ is of rank G^2 .

A number of propositions are simpler to state and derive if the notation is condensed. Let $(\beta \alpha) = v$, $\begin{pmatrix} y \\ x \end{pmatrix} = w$. Then a matrix representation of the model is

$$vw = u. \quad (2.2.4.9)$$

The restrictions are stated $\vec{v}C = d$ where $\vec{v} = (v_{(1)} \dots v_{(G)})$ is an elongation by rows of v and C is a rearrangement of the rows of $\begin{pmatrix} A \\ B \end{pmatrix}$. Proposition 2.2.4.2 is stated in this notation by replacing (β, α, F_U) with (v, F_U) and $((I \otimes \beta)B + (I \otimes \alpha)A)$ with $(I \otimes v)C$. For brevity, $(I \otimes v)$ will be written I^v .

An investigator is sometimes interested in the identification of a particular behavioral coefficient or linear combination of coefficients (e.g. the sum of elasticities of inputs in a Cobb-Douglas production function). Consider the linear combination $\vec{v}\xi$ where ξ is a known nonzero $G(G+K)$ vector.

$\vec{v}\xi$ is identified iff its value is constant over observationally equivalent structures, namely those obtained by admissible non-singular linear

transformations H of the structural equations. Admissibility means that H must satisfy $\vec{H} I^U C = d$.

Proposition 2.2.4.3. The linear combination $\vec{U} \xi$ of structural coefficients of a linear model with restrictions $\vec{H} I^U C = d$ is identified iff

$$\rho(I^U C \ I^U \xi) = \rho(I^U C).$$

Proposition 2.2.4.3 says that $\vec{U} \xi$ is identified for a given \vec{U} if that \vec{U} , in combination with the restrictions, causes the rank condition to be satisfied. It is worth noting that if the stronger rank condition $\rho(C \ \xi) = \rho(C)$ is satisfied, then $\vec{U} \xi$ is not only identified for all admissible \vec{U} , but its value can be determined from the imposed linear conditions. It is therefore a property of the model and not dependent on sample observations or statistical inferences.

The special case of identification of a particular structural coefficient is obtained by letting $\xi = e_i$, a $G(G+K)$ vector with one in the i th position and zeros elsewhere. Any subset of coefficients may be considered by inspecting them one at a time. Propositions 2.2.4.2 and 2.2.4.3 are modest extensions of those which appeared in the Cowles literature and may be found in the above form in Hsiao (1983). They are included because they seem to help bridge a gap between the initial very general discussion and the specific Chicago-period theorems which follow.

Of considerable historical interest and still frequently relevant to empirical studies is the possibility of identifying the coefficients in a chosen structural equation using only the restrictions that pertain exclusively to that equation - any restrictions involving other equations are ignored.

In early applications of systems of equations, this aspect of identification was particularly important because the application of single equation methods of estimation and testing depended on such identification. Full information methods (see Subsection 2.3) on desk calculators were too expensive of both time and money for all but a few empirical studies. Also, an investigator primarily interested in a particular relation may have more confidence in his specification relating to that equation. Fortunately, these considerations, especially the first, are seldom dominant in current research.

If the restrictions to be considered affect only the g th equation they may be written

$$v_{(g)} C^{(g)} = d^{(g)}$$

where $v_{(g)}$ is the g th row of v and $C^{(g)}$ is $(G+K) \times r_g$. For given v the restrictions must hold for all admissible transformations $H_{(g)} v$ of the g th equation, so $H_{(g)}$ must satisfy

$$H_{(g)} v C^{(g)} = d^{(g)}$$

$H_{(g)} = e_g$ is known to be a solution, unique if $\rho(v C^{(g)}) = G$. Since $e_g v = v_{(g)}$,

Proposition 2.2.4.4. In the linear model $vw = u$, the g th equation is identified by linear restrictions $v_{(g)} C^{(g)} = d^{(g)}$ iff $\rho(v C^{(g)}) = G$.

The most common forms of linear restrictions are normalizing restrictions already discussed and exclusion restrictions. The latter assert that certain variables do not enter (have coefficients equal to zero) in certain equations. For example, the price of a raw material used in producing a commodity might well enter the supply equation for that commodity but not the demand equation.

If the restrictions on relation g consist of a typical normalization, $\beta_{gg} = 1$ or equivalently $\nu_{gg} = 1$, and some exclusions, $\nu_{gi_j} = 0$, $j = 1..r_g - 1$, then the relevant $G \times r_g$ matrix $\nu C^{(g)}$ consists of r_g columns of ν - the g th column and the $r_g - 1$ columns $\nu_{[1]} \dots \nu_{[r_g - 1]}$ that are constrained to have zeros as their g th elements. For this special case write $\nu C^{(g)} = \nu^{(g)}$. Note that $d^{(g)} = e_g$. Thus, using Proposition 2.2.4.4,

Proposition 2.2.4.5. In the linear model $\nu w = u$, the g th equation is identified by a typical normalization, $\beta_{gg} = 1$, and $r_g - 1$ exclusions iff $\rho(\nu^{(g)}) = G$ where $\nu^{(g)}$ is the submatrix of ν consisting of the g th column and the $r_g - 1$ columns whose g th elements are required to be zero.

For each of the necessary and sufficient rank conditions in Propositions 2.2.4.2, 2.2.4.4, and 2.2.4.5 there is clearly a corresponding necessary but not sufficient condition that there be enough constraints so that the critical matrix has enough columns to permit the needed rank. Thus to identify a particular ν (2.2.4.2) there must be at least G^2 constraints while to identify a particular equation (2.2.2.4 and 2.2.2.5) there must be at least G restrictions. These necessary conditions are called order conditions and are typically all that follow from the specification of the model. The rank conditions depend on values of unknown parameters so their status is typically unknown. However, an investigator can often form a pretty good judgement by inspecting the relevant matrix in a particular case and by performing a statistical test for identification (Rubin and Anderson (1949, pp. 60-1); Koopmans and Reiersøl (1950, pp. 16972); Subsection 2.3 below, pp.).

To relate the above propositions to the specific models that have been frequently applied, observe that the specification $F_{ij} \in \mathcal{F}$ has been left very general. The propositions thus apply directly to models LM, NLM, LMU, NLMU. It turns out that distinctions among these models are more important for estimation than for identification (Subsection 2.3).

What have not been included thus far are models involving lagged endogenous variables. These have the form (see pp.)

$$\beta_0 y + \sum_{j=1}^J \beta_j y^{(j)} + \alpha x = u \quad (2.2.4.10)$$

where $y^{(j)}$ is a $G \times T$ matrix of values of endogenous variables with a lag of j time units, i.e. $y^{(j)}_{gt} = y_{g(t-j)}$ $g=1..G$, $t=1..T$, $j=1..J$. J is the maximum lag with which any endogenous variable appears. β_j is a $G \times G$ matrix of coefficients of endogenous variables with lag j . As noted earlier, it is not necessary to distinguish current and lagged values of exogenous variables.

Following the study by Mann and Wald (1943) of maximum likelihood estimation of LMY; Koopmans, Rubin, and Leipnik (1950) showed⁸ that if the disturbances of (2.2.4.10) are serially independent ($U_{[s]}$ independent of $U_{[t]}$ for $s \neq t$) then lagged endogenous variables can be grouped with exogenous variables as predetermined variables and treated like exogenous variables for purposes of identification. The model is rewritten

$$\beta y + \gamma z = u \quad (2.2.4.11)$$

where $\beta_0 = \beta$, $(\beta_1 \dots \beta_J \alpha) = \gamma$, $\begin{pmatrix} y^{(1)} \\ \vdots \\ y^{(J)} \\ x \end{pmatrix} = z$.

The propositions derived above can then be applied substituting γ for α and z for x in the statements of the propositions. The arguments for this extension depend strongly on serial independence so the stated propositions do not apply to models of types DLM and NDLM. A different approach to these models may be found in Hsiao (1980).

The remainder of this subsection considers identification in parametric form for NLM and identification in the wide sense for LM. The arguments and conclusions can be applied to NLMY and LMY by substituting γ for α and z for x .

Compare NLM and LM. For NLM, the class \mathcal{F} of admissible distribution functions restricts the disturbances to the normal family. Since $EU = 0$ and the columns of U are independently and identically distributed, F_U is then determined by the covariance matrix Σ of a column of U . A structure can be indicated by $\lambda = (\beta, \alpha, \Sigma)$ instead of $s = (\beta, \alpha, F_U)$.

The set of admissible substantive or structural parameter points is

$$\Lambda = \{(\beta, \alpha, \Sigma) : |\beta| \neq 0, \Sigma \text{ is positive semi definite}\}. \quad (2.2.4.12)$$

From the reduced form $Y = \pi x + V$ $V = \beta^{-1}U$ one sees that Y is a normal random matrix with independent columns having common covariance $\beta^{-1}\Sigma\beta^{-1'}$ and $EY = \pi x$. Since x is known, the statistical model may be parametrized by

$$\Theta = \{(\pi, \Omega) : \Omega \text{ is positive semi definite}\}.$$

Since any $\lambda \in \Lambda$ determines a specific structure and any $\theta \in \Theta$ determines a specific F_Y , both parameterizations are full. The function $\varphi : \Lambda \rightarrow \Theta$ giving a statistical parameter $\theta = (\pi, \Omega)$ for each structural parameter $\lambda = (\beta, \alpha, \Sigma)$ is defined by $\pi = -\beta^{-1}\alpha$, $\Omega = \beta^{-1}\Sigma\beta^{-1'}$.

By Proposition 2.2.4.1, two structures $s_1 = (\beta_1, \alpha_1, F_{U_1}^{(1)})$ and $s_2 = (\beta_2, \alpha_2, F_{U_2}^{(2)})$ are observationally equivalent iff \exists nonsingular H such

that ① $\beta_2 = H\beta_1$, ② $\alpha_2 = H\alpha_1$, ③ $F_{U_2}^{(2)} = F_{HU_1}^{(1)}$. With normality, the last equality holds iff ④ $\Sigma^{(2)} = H\Sigma^{(1)}H'$. Thus λ may be said to be φ -equivalent to λ_2 iff ①, ②, and ④ hold. Since both Λ and Θ are full parametrizations, φ -equivalence and observational equivalence coincide.

For NLM, Λ and Θ are natural full parametrizations. For LM, these parametrizations have been commonly used even though they are partial. In principle, a fuller parametrization (involving other properties of the distributions of U and Y , perhaps higher moments) should be more informative in some circumstances. An appropriate way to proceed is not obvious and this line of thought has not been developed.

When Λ and Θ as defined above are applied to LM models then each structural parameter point $\lambda = (\beta, \alpha, \Sigma)$ corresponds to a subset of structures. For the structural model one has

$$S = \{(\beta, \alpha, F_U) : |\beta| \neq 0, EU = 0, U_{[s]} \text{ ind } U_{[t]} \text{ } s \neq t, \text{Cov } U_{[t]} = \text{Cov } U_{[1]} \text{ } t = 2..T\}$$

When S is parametrized by Λ , each parameter point $\lambda = (\beta, \alpha, \Sigma)$ corresponds to a set of structures, say \tilde{S}_λ with fixed β, α combined with all $F_U \text{ } EU = 0, \text{Cov } U_{[t]} = \Sigma, U_{[t]} \text{ ind } U_{[s]} \text{ for } s \neq t$. Similarly, a statistical parameter point (π, Ω) corresponds to a set $\tilde{\rho}_\theta$ of distributions of Y . The notion of identification applies to both the function $\Psi: S \rightarrow \rho$ and the function $\varphi: \Lambda \rightarrow \Theta$. It is the latter that has been treated in the literature on models with disturbances that are not necessarily normal.

The development of Propositions 2.2.4.1 to 2.2.4.5 clearly applies as stated to identification via Ψ and with minor rewording of 2.2.4.1 to identification via φ .

Proposition 2.2.4.1*. In LM, $\lambda_1 = (\beta_1, \alpha_1, \Sigma_1)$ and $\lambda_2 = (\beta_2, \alpha_2, \Sigma_2)$ are observationally equivalent in the wide sense iff $\exists |H| \neq 0$ $\beta_2 = H\beta_1$, $\alpha_2 = H\alpha_1$, $\Sigma_2 = H\Sigma_1 H'$. (note that $\rho(x) = K$ was assumed earlier)

Statements of Propositions 2.2.4.2 to 2.2.4.5 involve only the coefficients and restrictions and thus apply to both identification of s via Ψ , and identification of λ via φ . It may be noted that the parameter point λ is φ -identified iff the subset \bar{S}_λ is Ψ -identified. Propositions 2.2.4.2, 2.2.4.3, 2.2.4.5 and 2.2.4.6 have counterparts stated in terms of π rather than β, α . These may be found in Hsiao (1983), pp. 234-40 and Richmond (1974). One example is utilized below in Subsection 2.2.3, p. . 2.2.4.2 and 2.2.4.3 are due to Wegge (1965).

The basic results for parameters of one equation with homogenous restrictions (except for the normalization restriction) were developed in Koopmans (1949) and Koopmans, Rubin and Leipnik (1950). Fisher (1959) extended the results to nonhomogenous restrictions and summarized work to date in Fisher (1966).

2.2.5 Other Results

The basic identification and inference (Subsection 2.3) results obtained at the Commission for linear shock models with linear constraints facilitated early empirical trials of the simultaneous equations approach. At the same time starts were made on theoretical analyses using other models and constraints. The other Cowles identification results are noted below.

Wald (1950) considered identification of a particular structural parameter in an LM or LMY model subject to nonlinear restrictions on the parameters. He obtained necessary and sufficient conditions for local identification and also for identification in the sense that structures satisfying the restrictions and observationally equivalent to a given structure should include only a finite number of values for the parameter in question. His conditions however depend on quite complicated functions of the initial parameter point and, to the author's knowledge, have not been applied.

Koopmans, Rubin, and Leipnik (1950), in addition to developing conditions for identification via homogenous linear restrictions on one equation, discussed the need for and problems associated with using other forms of prior information. Inequalities, bilinear restrictions on coefficients from different equations, and restrictions on covariances of disturbances were considered. Another paper from Monograph 10, Koopmans (1950b) was not directed primarily toward identification problems, but is clearly relevant.

As the title, "When is an Equation System Complete for Statistical Purposes?", suggests, the aim is to aid model-building by helping an applied worker decide how large a system of equations is needed to furnish a reasonably comprehensive analysis of an economic sector. The principal result is that, if a structural system of (not necessarily linear) equations can be divided into two parts such that

- (1) The n equations of the initial system are consistent and functionally independent and determine unique values of n current endogenous variables for given values of the predetermined variables and the unobserved random disturbances.

(2) The n_1 equations of the first part of the system include n_1 current endogenous variables which do not appear in the second part.

(3) The unobserved random disturbances which appear in the first part are statistically independent of those which appear in the second part.

(4) The functions relating disturbances to current endogenous variables have a nonsingular Jacobian

then the likelihood function factors into two parts corresponding to the two parts of the system of equations. These can be maximized separately to obtain maximum likelihood estimates of identified structural parameters. Since the disturbances in the second part of the system are independent of the n_1 current endogenous variables that do not appear in the second part, these current endogenous variables may be treated as exogenous for identifying structural parameters in the second part.

When a system has been divided into two parts, one may make further divisions if Conditions (1) - (4) hold for either or both parts. For linear systems, the recursive form introduced by Bentzel and Wold (1946) and block diagonal models discussed by Fisher (1963, 1966) are special cases. Although Koopman's argument can be simplified his article is the first to the author's knowledge that clearly brought out the governing statistical property for exogenous variables namely, independence of the unobserved random disturbances.

Subsequent to the 1945 conference, Olav Reiersol of the University of Oslo, spent a year (1949-50) as a Research Fellow of the Commission working on error in variables models and factor analysis. Reiersol (1950a) includes a review of previous work on errors in variables and finds conditions for identification in two types of single equation error models. Anderson and Hurwicz (1948, 1949) provided a general formulation of

problems arising in simultaneous error and shock-error models and found identifying conditions for some special cases. Results for several of the special cases were later included in a more general result of Gerachi (1976).

Identification in factor analysis was analyzed by Reiersol (1950b) and an example was developed by Koopmans and Reiersol (1950). This work increased the interest of many social scientists in the statistical and modeling developments taking place in econometrics. With the definition by Herbert Simon (1950, 1952) of a concept of causality closely related to exogeneity and identification, exchanges between economists and other social scientists were facilitated and areas of common interest have steadily grown. See Goldberger (1972) and Goldberger and Duncan (1973).

Thus far, the justification offered for interest in identification and structural analysis has been the scientist's desire to have intellectually satisfactory and empirically reliable explanations for observed phenomena. Since science advances by constructing successively better explanations, this is an adequate rationale. However, there is another very important reason which is often more immediate in applications. Hurwicz (1950b) explained that forecasts based on (approximate) knowledge of the distribution of observed random variables are logically valid only so long as the underlying structure remains constant. If structural change is anticipated before the forecast events take place, then a logically valid forecast must be based on (approximate) knowledge of the existing underlying structure and some way of anticipating (approximately) the structural changes. This was sharpened in a later presentation (Hurwicz, 1962). Assuming an investigator can specify a domain of possible structural changes of interest, it was shown how the structural properties for which identification is needed depend on that domain.

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¹These terms are explained in many references on elementary properties of sets and relations. For example, Green and Heller (1982), Debreu (1959), Royden (1968). One might alternatively say that identification is concerned with the properties of the composition $\Psi^{-1} \circ \Psi$.

²Since $G \subset \Psi^{-1}(\Psi(G))$ by definition of Ψ^{-1} , an equivalent condition would be $\Psi^{-1}(\Psi(G)) \subset G$. In some contexts one might also want to define, "G is identified with respect to B" to mean " $\Psi^{-1}(\Psi(G) \cap B) \subset G$ " where B is a subset of \mathcal{B} . This notion is not needed in the present discussion.

³(iv) is perhaps a little less immediate. Suppose $(G_\delta)_{\delta \in D} \subset \mathcal{J}$. Let $G^* = \bigcup_{\delta \in A \subset D} G_\delta$. Must show $\Psi^{-1} \circ \Psi(G^*) = G^*$. Clearly $G^* \subset \Psi^{-1} \circ \Psi(G^*)$. Suppose $g \in \Psi^{-1} \circ \Psi(G^*)$. Then $\Psi(g) \in \Psi(G^*)$ which means $\Psi(g) \in \Psi(G_\lambda)$ for some $\lambda \in A$. Since G_λ is identified one has $g \in G_\lambda \subset G^*$. Therefore $\Psi^{-1} \circ \Psi(G^*) \subset G^*$.

It is interesting that (iii), (iv) imply that \mathcal{J} is a σ -field and (ii), (iii), (iv) imply that \mathcal{J} is a topology in which open and closed sets coincide.

⁴Although I have no actual experience to cite, it is clearly not an illogical case, Suppose ρ were the uniform distributions with range $\frac{1}{2}$ and mean equal to an integer. Then a sample of one would reveal the distribution. In less restrictive cases, access to an appropriate sample of unlimited size might be regarded as equivalent to knowledge of P.

⁵For preliminary examinations of other models and discussions of needed work see Hurwicz (1950c); Rubin (1950); Anderson and Hurwicz (1947).

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⁶ Another way of looking at the redundancy is to note that the structural system $D\beta y + D\alpha x = Du$ has the same interpretation as 2.2.4.1 provided the matrix D is diagonal and nonsingular.

⁷ It is assumed throughout that the restrictions imposed are consistent and functionally independent. An inconsistency or redundancy would require reformulation of the model.

⁸ Both papers treated a slightly less general specification than here, but the results are easily extended.

2.3 Estimation and Testing

2.3.1 Estimates

Most of the Cowles Commission results on estimation and testing refer to asymptotic behavior of statistics used with the linear shock models subject to linear constraints introduced in Section 2.1. Maximum likelihood estimators were developed for models with normally distributed disturbances. Information on the probability distribution of these estimators and other statistics of interest was sought. In most cases, useful information could be obtained only for asymptotic (large sample) distributions. These provided some basis for judging the accuracy of estimates and for developing asymptotically valid statistical tests. The need to rely on asymptotic results was recognized as a real difficulty.

For models with disturbances of unspecified distributional form, the situation was still more trying. What was done was to borrow the statistics (functions of observations) obtained in the normal case and see what could be determined about their distributions when normality was relaxed. Again, some large sample results were obtained which could be used to some extent to judge estimators and construct tests. These estimators and other statistics for non normal models are called quasi-maximum likelihood. Improvements, particularly results for moderate sized samples have developed only gradually since the Commission period. Accounts of these are in Hausman (1983) and Phillips (1983).

Several key contributions were made in 1940 to 1943 before such work was undertaken at the Commission. At Marschak's initiative, when he was at the New School for Social Research, a seminar on econometric methods

and empirical studies met about every three weeks at the Columbus Circle or Hillside facilities of the National Bureau of Economic Research in New York.¹ Frequent participants included Sidney Alexander, Franz Alt, William Andrews, Haavelmo, Carl Kaysen, Koopmans, Wassily Leontieff, Franco Modigliani, Albert Neisser, Paul Samuelson, Joseph Schumpeter, and Abraham Wald. The New York seminars started early in 1940 and continued until mid-1943 when Marschak moved to the Commission where he was able to continue and strengthen these activities.

Besides mutual education and suggestions, three vital papers emerged from the New York sessions. Haavelmo (1943), "On the Statistical Implications of a System of Simultaneous Equations," contained the basic ideas which, as elaborated in Haavelmo (1944), may have been as influential as any in the history of econometrics. Mann and Wald (1943), "On the Statistical Treatment of Stochastic Difference Equations," extended several important results of classical likelihood theory to sets of equations containing lagged endogenous variables. This opened the way to further theoretical work and to time series applications in which some statistical properties could be recognized. Marschak and Andrews (1944), "Random Simultaneous Equations and the Theory of Production" was the first empirical application of the approach articulated by Haavelmo.

Early in 1945 a conference consisting of papers and discussions by fifteen (Marschak, 1950 , p. 2) of the leading U.S. statisticians and econometricians was held at the Commission. Most of the papers later appeared in Cowles Commission Monograph 10. This and subsequent Cowles publications provided an initial development of statistical inference (estimation and testing) for multi-equation econometric models and

stimulated others to join in studying applications, extensions, and alternatives.

The Cowles work was mainly directed toward structural parameters, but the reduced form is also of interest and it is convenient to start with some statistical aspects of the reduced form. Recall that

$$y = \pi x + v$$

represents the reduced form equations and that the structural parameter space $\Lambda = (\beta, \alpha, \Sigma)$ is mapped onto the reduced form parameter space $\Theta = (\pi, \Omega)$ by a function $\varphi: \Lambda \rightarrow \Theta$. Since no prior restraints are imposed on Σ in the models of Section 2.1, the restriction $\varphi^*: (\beta, \alpha) \rightarrow \pi$ of φ to the structural coefficients is of primary interest. This discussion will first concentrate on coefficient estimation; corresponding estimators for Σ, Ω will then be noted.

If π is unrestricted² by the linear constraints on (β, α) , it is natural to regard least-squares as a possible way to estimate. Least squares estimates are then the same as maximum likelihood (ML) estimates if the model is normal (see Hurwicz (1950c), p. 293) and are the same as quasi-maximum likelihood estimates (QML) for non-normal models. In normal models least squares estimates have the extended Markov property of being best unbiased (Anderson, 1962) and in the non-normal case, they have the traditional Markov property of being best linear unbiased.

Let π be unrestricted and $\hat{\pi}$ the least squares, also ML or QML, estimates. Then $\varphi^{*-1}(\hat{\pi}) \subset \Lambda$ will have a unique value for each identified structural coefficient, β_{gh} or α_{gk} . These values are the ML(QML) estimates of the identified structural coefficients and are also called indirect least squares estimates. M.A. Girshick was the first to note the possibility of

estimating by indirect least squares. For unidentified coefficients, $\varphi^{*-1}(\hat{\pi})$ is multivalued but does determine, in effect, estimate, some linear combinations of the structural coefficients and estimates of these linear combinations are sometimes of interest.

Although the restrictions being considered are linear on β , α they are nonlinear on π (e.g. exclusion restrictions on β , α restrict the rank of submatrices of π) and, except for simple special cases, it is difficult to directly estimate π with the restrictions taken into account. If all of the structural coefficients are identified it is typically simpler to estimate β , α by one of the approaches discussed below and then estimate reduced form coefficients by

$$\hat{\pi} = -\hat{\beta}^{-1}\hat{\alpha}.$$

If π is restricted but some structural coefficients are not identified (for example, if some structural equations are not identified and others are over identified) then one possibility is to ignore the restrictions when estimating π . By ordinary least squares one still obtains consistent estimates. The alternative is to get better estimates at higher cost by imposing the difficult nonlinear restrictions.³

Regarding structural coefficients, if π is unrestricted then the indirect least squares estimates of identified parameters are ML for NLM, NLMY and QML for LM, LMY. If π is restricted it is simpler to use the likelihood function in terms of structural parameters, see Koopmans, Rubin, and Leipnik (1950), Sec. 4. However obtained, the ML estimates for NLM and NLMY are consistent, asymptotically normal, and asymptotically efficient. The QML estimates for LM and LMY are consistent and asymptotically normal. For NLM the results are a very minor extension of traditional maximum likelihood theory. Results for the other cases involved more difficult

extensions of then existing statistical theory, first by Mann and Wald (1943) for a particular case of LMY and then generalized by Koopmans, Rubin, and Leipnik (1950) and by Chernoff and Rubin (1953).

In the versions of NLMU and LMU in which the disturbances are related through a linear stationary autoregressive process, it followed from the linear regression results of Cochran and Orcutt (1949) that least squares estimates of the reduced form are consistent and unbiased. There is, however, loss of efficiency in not taking account of the autoregressive structure and in neglecting restrictions on π that might be implied by the structural model. When π is restricted, these inefficiencies are shared by any structural estimates obtained by indirect least squares.

Rubin (1948) and Chernoff and Rubin (1950) showed that, under mild stability assumptions, ML estimates for NLMU and QML estimates for MLU would be consistent and, under somewhat stronger conditions (Chernoff and Rubin, pp. 207-9) would be asymptotically normal and asymptotically efficient. Rubin (1950) also showed that consistency could be retained in some cases involving instability.

For LM, LMY and their normal counterparts, variances and covariances could be estimated by

$$\hat{\Sigma} = (\hat{\beta}y + \hat{\gamma}z)(\hat{\beta}y + \hat{\gamma}z)'$$

$$\hat{\Omega} = (y - \hat{\pi}z)(y - \hat{\pi}z)'$$

once coefficient estimates had been obtained. For NLMU, LMU the estimates would depend on the precise relations assumed among the disturbances.

The above results enabled applied econometricians to estimate structural parameters with some notion of the statistical status of the results. There were two major drawbacks. Knowing only asymptotic properties when almost

all samples were of modest size was not satisfying, and computations were difficult and expensive.⁴ As noted earlier, progress toward finite sample statistical theory has been slow and is still highly incomplete. Advances in computer technology have substantially ameliorated the calculation problem. Before this came about, the development of limited information or subsystem methods at Cowles and the introduction of two and three stage least squares by Theil (1953) and Zellner and Theil (1962) were quite important and these methods are still frequently used. Basmann (1957) independently developed two stage least squares, calling it generalized classical linear estimation.

Koopmans, Rubin and Leipnik (1950) sketched two methods for computing estimates of all of the identified structural parameters of a complete system. These were elaborated by Chernoff and Divinsky (1953). General methods applicable to this problem were reviewed by Crockett and Chernoff (1955). An early version of the methods had been presented at the 1945 Chicago conference and it was recognized that they were generally very burdensome, frequently prohibitive. T.W. Anderson, who came to the Commission in 1945, became interested in how a single equation of a linear stochastic system might reasonably be estimated. In collaboration with Rubin, he developed Limited Information Maximum Likelihood and Limited Information Quasi-Maximum Likelihood procedures for estimating parameters of a single structural equation.

The latter procedures may be viewed as replacing the investigator's model with a proxy model involving fewer constraints. Only the linear constraints on coefficients of the equation of primary interest are retained. This typically means that parameters of other equations are not identified. If constraints on the primary equation are sufficient for identification of that equation, then maximization of the likelihood function for the proxy

model yields consistent estimates of parameters of the primary equation.

Anderson and Rubin (1949, 1950) showed that such estimates are consistent in a variety of circumstances. They are consistent for LM, LMY, and their normal counterparts and for NLMU, LMU under mild stability conditions. They remain consistent under mild stability and convergence conditions if some of the exogenous variables in non-primary equations are omitted from the calculations; if some or all of the non-primary equations are nonlinear; and if the disturbances for successive observations are not identically distributed. Under somewhat stronger conditions than required for consistency, estimates of the structural coefficients of the primary equation are asymptotically normal and their covariances can be consistently estimated. Limited information estimates of a chosen structural equation can generally be calculated much more simply than those which utilize all constraints. Appropriate procedures were described by Anderson and Rubin.

The limited information idea was extended by Chernoff and Rubin (1950) to subsystems of structural models. Again, one analyzes a proxy model which includes only part of the constraints of the initial model. One retains constraints that apply to several equations of primary interest. One then estimates coefficients in the primary equations by maximizing the likelihood function for the normal version of the proxy model. If normality is not assumed, the estimates are regarded as quasi maximum likelihood subsystem estimates. As in the single equation case, consistency of estimates is retained for a variety of circumstances under mild stability and convergence assumptions, while somewhat stronger assumptions imply asymptotic normality. As one expects, the computation burden is in between that of full information estimation and that of single equation estimation. One

interesting circumstance treated by Chernoff and Rubin (1953, pp. 204-6) which draws on Anderson and Hurwicz (1947) is a special class of shock-error models. If the limited information subsystem procedure is applied to a linear model in which there are errors of observation in some (possibly, all) of the current endogenous variables and/or some (possibly all) of the predetermined variables not in the subsystem, the resulting estimates remain consistent provided the errors of observation are stochastically independent of each other and of the disturbances and do not cause any of the necessary convergence assumptions to be violated. If there are similarly well behaved errors of observation in predetermined variables appearing in the subsystem, one can still obtain consistent estimates by treating these variables as current endogenous provided this does not destroy identifiability.

2.3.2 Tests

Cowles Commission results on probability distributions of estimators can be used in several ways to construct approximate large sample tests for a variety of linear hypotheses regarding structural coefficients. When the limiting distribution is normal one can test simple hypotheses about the estimated coefficients using a multivariate normal distribution with mean equal to the hypothesized value and covariance matrix equal to the asymptotic covariance with unknown parameters replaced by their ML estimates. This procedure clearly extends to hypotheses about subsets of the coefficients and linear combinations of coefficients (general linear hypotheses).

In connection with their work on limited information single equation estimation, Rubin and Anderson (1949, 1950) also developed a number of useful tests. For NLM, they showed (1949, pp. 60-61) that a

valid small sample test of a simple hypothesis⁵ on coefficients of the primary equation (or on coefficients of the endogenous variable) could be based on the familiar F distribution. For the other models (except DLM, NDLM), it was shown that if the model imposes more linear constraints on the primary equation than the minimal G needed for identification, an asymptotically valid test of the constraints is available. It is based on the fact that -2 times the log of the likelihood ratio in these cases is distributed as chi square with degrees of freedom equal to the excess number of constraints. This is frequently called the test for over identification of the primary equation. Koopmans and Reiersøl (1950, pp. 169-72) discussed the general testing of particular properties (called characteristics or specifications in the article cited) of structural models.

If one estimates the reduced form of NLM or LM by least squares, it is natural to test general linear hypotheses with the usual F-ratio. The test is exact for NLM and frequently a good approximation for LM. If π is a priori unrestricted there is no reason to consider other possibilities. When π is restricted a problem arises. The F tests are still valid, but it is possible that a test based on ML estimates or a likelihood ratio statistic would be more powerful.

Analogous situations exist for other models. Single equation methods for estimation and testing are available for the reduced form (e.g. Maddala, 1977, Ch. 12). So long as π is unrestricted, these are clearly appropriate. If π is restricted one must balance the simplicity of single equation tests

against the possible loss of power due to neglecting restrictions. Unfortunately comparisons are difficult because the distributions of test statistics are generally unknown when constraints on coefficients are recognized.

The tests sketched so far are sometimes called tests of internal consistency. They bring evidence furnished by a particular statistical sample to bear on questions of interest to the investigator. The questions are formulated as assertions about the underlying structure - particular specifications that are part of the model or specifications that go beyond the model. They are the null hypotheses of traditional statistical testing. For various reasons, it is important that evidence furnished by theoretically comparable data outside the sample which has been analyzed also be brought to bear as promptly as is feasible. This is called prediction testing. Although the basic notion can be applied to data other than time series, these applications predominate and language relevant to time series is used here.

Every finite sample has accidental peculiarities - features that are not part of the general process that generated the observations. Both the investigator and the economics community need help in judging the extent to which conclusions which look convincing in an initial analysis seem to apply to wider circumstances - to be characteristic of a general process. The danger that initial conclusions reflect special properties of a particular sample is heightened when, as is common, investigators experiment with several models before choosing one to analyze more fully.⁶

Using a fitted model to predict future values of endogenous variables and comparing observed errors with their estimated distribution essentially tests the hypothesis that the chosen model is valid for both the sample period and the test period. Rejection of this hypothesis by large observed

errors leads to consideration of alternative models for the sample period and/or the possibility of a structural change between periods.

Another purpose of prediction testing is to try to judge the potential value of the estimated structure for short run forecasting. Errors from the fitted structure are compared with those from naive models, e.g. see Christ (1951), or less formal forecasts of experts. If comparisons are carefully formulated, a model that does badly in such comparisons is suspect. However, since naive models do not contain explanations of the phenomena investigated and explanations underlying informal forecasting may be hard to communicate, a formal model may have value in developing objective explanations even if its short run forecasting value is not great.

Just how these considerations can best be translated into useful prediction tests was, and is, a subject of some controversy. Its importance was emphasized at an early stage of the applied work at Cowles. The most ambitious product at that time was Christ's (1951) test of a revision of Klein's inter-war model. Christ's formulation of these problems and the accompanying discussions of Friedman, Klein, Moore and Tinbergen illustrate the significance and complexity of this line of thought.

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¹See Koopmans (1978). This account has been supplemented by conversations with some of the participants.

²This means the reduced form coefficient space is R^{GK} . In linear models with constraints specific to particular structural equations, this will be true if no equation is over-identified.

³Throughout the simultaneous equations literature it has been taken for granted that failure to impose valid restrictions on estimated parameters necessarily involves loss of quality of the resulting estimates. This seems intuitively true, but I am not aware of a general proof. Examples tend to confirm the general feeling. One was furnished by Allen (1950), another is the demonstration that full information (or three stage least squares) furnishes better estimates in the generalized variance sense than limited information (or two stage least squares), Theil (1971), Ch. 10. Also, it can readily be shown that neglect of linear restraints on coefficients in the linear regression model increases the generalized variance. With modern computers and methods, costs of imposing restrictions are much less. See Amemiya (1983), Section 5.

⁴Some light on the circumstances of the times is cast by the minutes of discussions of papers by the Cowles staff presented August 23-24, 1946 during and after meetings in Ithaca, New York of the Institute for Mathematical Statistics. These appear in the Appendix, pp. 73-80. I am indebted to P.C.B. Phillips and Karlee Gifford for locating these minutes.

⁵Their discussion is in terms of confidence regions but these can, of course, be converted to tests of simple hypotheses. The result was independently obtained by Bartlett (1948).

⁶Such experimentation is clearly of value. Failure to report it adequately is still too prevalent in applied statistics.

2.4 Applications

2.4.1 Illustrative Applications

As the procedures described in preceding subsections developed, there was naturally much interest in making empirical applications. It was recognized that there were many difficult problems of model building and statistical inference to be resolved before one would be able to view econometric results with the same confidence as, say, the results of a well planned biological experiment. However, other sources of information (introspection, testimony of participants and observers, casual empiricism) on economic relations are also fraught with uncertainty so there was, and is, a strong incentive to extract the best possible indications from econometric studies.

Interpretations of empirical results are qualified by possibilities of sampling errors and specification errors, the latter due to defects in the models used. It was hoped that successive reformulations based on further thought, discussions, and statistical tests (especially predictions) would gradually reduce exposure to specification error and make straightforward statistical interpretations of results valid. Together with improvements in quality and quantity of data available, this would enable econometrics to increasingly contribute to practical decision making. In this sense, all of the applied results achieved by Cowles at Chicago can be labeled preliminary.

The earliest applications, however, were still more tentative and were designed to illustrate aspects of simultaneous equations models in order to get a start on the modeling, testing, re-examining and remodeling sequence that was visualized as the heart of empirical work.

The first application, Marschak and Andrews (1944) appeared when study of statistical aspects of simultaneous equations models had just started. The authors formulated a general model of production decisions based on static equilibrium assumptions. The model was used as a basis for interpreting results of a number of earlier empirical studies. The general structure of the Marschak-Andrews model appeared in a number of later production studies.

Haavelmo (1947a) compared estimates of the marginal propensity to consume, and the associated multiplier, from two alternative models. The first was essentially a one equation model with investment treated as exogenous. The other was a two equation model in which an endogenous component of investment was a function of "gross disposable income" (Haavelmo, p. 114). Structural estimates for the latter model were obtained by indirect least squares. Joint confidence regions were displayed for propensity to consume and propensity to invest as well as for the reduced form coefficients.

Also in 1947, Girshick and Haavelmo analyzed a five equation model of demand and supply for food. One equation contained just one current endogenous variable. Hence it could be, and was, consistently estimated by least squares. Another equation was just order-identified (see Subsection 2.2., p.) and was estimated by indirect least squares. The other three equations were over identified by the usual order condition and were estimated by the Anderson-Rubin limited information single equation method.

The model analyzed was plausible, but the emphasis in the Girshick Haavelmo paper was clearly on helping others understand and possibly use the simultaneous equations approach. Rationale for simultaneous

equations, the contemporary state of statistical theory, and issues involved in formulating the particular model used were all discussed in some detail. Computations were shown and explained in a step-by-step fashion. In a subsequent paper by Haavelmo (1947b) the estimates were used as a basis for discussing relationships between the general economy and the agricultural sector.

2.4.2 Klein Models

Although the division is not sharp, it seems reasonable to say that with the publication of Klein's "The Use of Econometrics as a Guide to Economic Policy," (1947), there was a shift in emphasis from illustration and exposition of principles to the attempt to construct models of relevance to current decision making. By now, that emphasis has been pursued by Klein for thirty-seven years and has contributed more to the evolution of the current thriving econometrics industry than has any other line of development, see Ball (1981).

Klein joined the Commission in the fall of 1944 with considerable knowledge of the start that had been made in model building and in the adaptation of statistical inference to econometric problems. His major professor at MIT (1942-44), Paul Samuelson, had attended most of Marschak's New York seminars and later Cowles conferences. Samuelson's reports of ongoing work and his discussions of the basic problems greatly stimulated and guided an already live interest on Klein's part. In his 1947 paper, Klein starts with models that are mild elaborations of Haavelmo's first model. From a statistical point of view, these are one-equation models but the economic interpretation involves several equations. Model II (p. 122) may be written -

$$c = \alpha_0 + \alpha_1 y + \alpha_2 y_{-1} + \alpha_3 m_{-1} + u$$

$$g = c + w + h$$

$$y = g - t \quad \text{where}$$

c represents consumer expenditures; y is disposable income; y_{-1} is lagged y ; m_{-1} is lagged cash balance; g is gross national product; w is gross private capital formation; h is government expenditures; t represents taxes; u is a random disturbance. All of the variables are in real dollars per capita.

c , g , and y are regarded as endogenous; w , h , t as exogenous; y_{-1} and m_{-1} as predetermined. Since y and g are known functions of e and some exogenous variables, there is just one functionally independent endogenous variable. The unknown coefficients can be estimated by least squares using g , y or c as the dependent variable. Klein's estimates for

$$y = \frac{\alpha_0}{1 - \alpha_1} + \frac{\alpha_2}{1 - \alpha_1} y_{-1} + \frac{1}{1 - \alpha_1} (w + h - t) + \frac{\alpha_3}{1 - \alpha_1} m + \frac{1}{1 - \alpha_1} u$$

are

$$\hat{y} = 186 + .30y_{-1} + 2.36(w + h - t) + .13m$$

using annual data, 1922-41. The estimated parameters were used to make conditional forecasts of GNP and disposable income for fiscal 1947. Alternative forecasts were associated with alternative levels of consumer prices and of investment.

The investigator surveyed a number of deficiencies in available data and expressed several reservations about the model, particularly the treatment of taxes and investment as exogenous. The model was reformulated with investment disaggregated and several components treated as endogenous. This required recognizing the roles of other variables, e.g., interest

rates, construction costs, housing expenditures. A system which might be called the first Klein model, comprising twelve behavioral relations and four identities resulted. Parameters were estimated by indirect least squares and a provisional range of 177 to 190 billion was forecast for 1947 GNP.

A more thorough examination of these models was reported in Klein (1950), Economic Fluctuations in the United States. Data sources and adjustments were described in careful detail. Assessments of the correspondence between available data and the concepts related by the model were presented. A number of alternative theories and suggested rules of thumb were considered. Some of these re-examinations seem rather casual and some seemed so at the time. For example, in applying Hart-Von Neumann tests (the predecessor of Durbin-Watson tests) of serial correlation in disturbances, the hypothesis of non-serial correlation was accepted without considering power of the test and was accepted even when there were pretty strong indications of serial correlation (see Klein, 1947, pp. 115, 123). Anticipations were considered but anticipated values of a particular variable were always made functions of lagged values of that same variable. The conclusion (Klein, 1950, p. 32), "We could modify the theory of utility maximization by assuming imperfect competition and increasing risk; however, the general form of the equations would not be seriously affected if the additional restrictions were the same as those introduced into the theory of profit maximization.", leaves much unanswered. Detailed criticisms were presented by Evsey Domar (1948). A more optimistic general evaluation by Tinbergen appeared in 1950.

It has to be remembered that this was the pioneering effort in the

formulation of macroeconometric models with specified statistical assumptions. The number of important issues requiring attention was very large. Klein (1950, Preface; 1947, p. 111) clearly regarded his models as steps in a continuing process of formulation, testing, discussion, and reformulation. Issues casually resolved at one stage were typically reconsidered as revised models were constructed.

Klein left the Commission in the spring of 1947 to spend a summer in Ottawa, Canada participating in the construction of the first Canadian econometric model, and then a year in Norway and Sweden working with Haavelmo, Frisch, Wold, Lindahl and others. On his return he worked a year at the National Bureau of Economic Research and then joined the faculty of the University of Michigan in the Economics Department, the Survey Research Center, and the Research Seminar on Quantitative Economics. A main result of his work at Michigan was the Klein-Goldberger (1953) model, a benchmark in econometrics and the point of departure for many of the larger models constructed in the 60's. Klein remained a Consultant of the Cowles Commission through the Chicago period. His occasional visits were helpful to others working on problems of econometrics and economic theory, and provided for some limited interchange on problems raised by his own research.

During his association with the Commission, Klein's models and results were a natural focus for those with special interests in macroeconomics and fiscal-monetary policy. This group was continuing and extending lines of thought in which Walras-Hicks general equilibrium theory and Keynesian behavioral assumptions played central roles. Lange (1945) provided an initial formulation but was then drawn into other

activities (see Patinkin, 1981, pp. 8,9). Subsequent contributions by Marschak (1945, 1949, 1950); Haavelmo (1945, 1950); Klein (1946); Patinkin (1948, 1949a, 1949b); Domar (1949); Malinvaud (1953); and Strotz, McAnulty, and Naines (1953) were of major importance in the development of neo-Keynesian theory. Visitors who contributed to discussions in this area included Gottfried Haberler, R.M. Goodwin, Everett Hagen, and Robert A. Gordon. The Commission's general equilibrium-Keynesian orientations and methodological approaches sometimes produced sharp disagreements with other Chicago economists - see Reder (1982); Friedman (1946); Patinkin (1981), Chapters 7, 10, 11.

The Chicago Cowles group did not enjoy the ready access to policy makers which later developed at the Cowles Foundation under the leadership of James Tobin and William Brainerd. However, there were frequent contacts with Arthur Smithies, then Director of the Budget. Also, among the items found in old Commission files were - an exchange between Klein and Alvin H. Hansen regarding Klein's empirical results and their possible implications; a memorandum prepared by Klein at the request of Albert Hart, then Chief Economist for the Committee on Economic Development; and a communication from Marschak to Senator Robert A. Taft, then Chairman of the Joint Economic Committee.¹

2.4.3 Small Sectors

Starting about 1950, a number of statistical studies were undertaken to estimate parameters of models of small sectors of the economy. These were viewed as complementing work on macro-economic models. There were several kinds of interest in the new studies. The results were of interest in themselves to participants in the markets studied and people

affected thereby. It was possible that findings in one sector would contain hints or reflect on conjectures about other sectors. It was also believed that sector models would contribute to the construction of more detailed and more accurate models of the general economy by taking advantage of the special knowledge of people closely acquainted with the operation of particular sectors.

These potential advantages have been to some extent realized, but not by sustained efforts of the original model builders who presented their results and then pursued other interests. The results influenced later work on both sector and general models but did not give rise to the continuous modeling and testing that characterized Klein's work on macro models.

The most extensive sector study of the period was Hildreth's and Jarrett's (1955) work on the livestock economy in United States. The study was conducted at the Commission in cooperation with the Bureau of Agricultural Economics, United States Department of Agriculture and the Agricultural Economics Research Group, University of Chicago. Five equations of an incomplete model of the livestock economy were specified and estimated by least-squares and limited information. Recall that Chernoff and Rubin (1953) had shown consistency of limited information estimates for an incomplete model, provided enough predetermined variables were used to furnish identification of the estimated equations.

The five equations analyzed consisted of a livestock production relation, three farm decision relations (which could be regarded as deciding how much grain to feed, how much high protein supplement to use, and how to divide the total livestock on hand between sales and

animals retained for future production), and a demand for livestock products relation. The farm decision relations were formulated from a microtheory based on the assumption that producers maximized utility which depended on current withdrawals from the enterprises and productive assets held for future production.

Estimates were based on annual data for 1920 through 1949. Almost all of the estimated coefficients were plausible and the others had high standard errors indicating that implausibility might be due to sampling fluctuation. Limited information estimates were more plausible than least-squares and also led to somewhat better predictions when 1950 data became available. Structural relations also predicted somewhat better than naive models. Some estimates (higher price and income elasticities in the demand relation, complementarity of grains and protein feeds in the production relation) were more consistent with beliefs of experts than previous empirical studies had been.

The principal negative results came from statistical tests. For most equations, the Durbin-Watson (1951) test cast doubt on the assumption of serial independence of disturbances and the Anderson-Rubin (1949) test of overidentifying restrictions cast doubt on the general structure of the model. The latter is a large sample test and its behavior with a combination of 30 observations and an incomplete model was hard to conjecture. It would have made sense to re-estimate allowing for serially correlated disturbances and/or to complete the model. In the 1950's either would have been a very formidable task and current resources were not available. Though revisions were not immediately pursued, the results did influence subsequent research in agricultural economics.

Arnold C. Harberger (1950, 1953) was a Research Assistant during the academic year 1949-50 and a Research Associate, 1954-5. He worked on problems of demand for imports. His first paper was a theoretical derivation of the response of a country's balance of trade to currency depreciation in a linear 2-country model. In the second, estimates were obtained for price elasticities of demand for several categories of United States imports assuming that the demand equation was part of an incompletely specified structural system. The estimates obtained were generally higher than the least-squares estimates that had appeared in the literature. They were of particular interest in suggesting a stronger possibility for stable equilibrium of international trade.

George H. Borts (1952) was a guest member of the Cowles staff for the academic year, 1950-51. He studied production relations for railways, developing a model in which amount of service and various categories of fixed capital were taken as predetermined. The endogenous variables, several kinds of variable inputs, were each determined by an equation linking the input to the service-capital variables. Therefore parameters could be estimated by single equation, least squares methods.² The relations could be used to construct cost functions for various patterns of services demanded and states of capital investment (levels of unadjustable inputs).

A study of the flaxseed and linseed oil markets was undertaken in 1949 by S.G. Allen (1954), then a research assistant at Cowles. He left to join the staff of Stanford Research Institute in 1950 and became a research consultant to the Commission, finishing the study in 1952. A quarterly model that emphasized inter-relations between United States and Argentine production was formulated and was estimated by limited

information methods.

Hendriecke Goris was a guest of the Commission in 1951-2. Her statistical analysis of the American tobacco market was completed in Amsterdam, Goris (1954).

Hendrik S. Houthakker joined the Commission as an Assistant Professor in 1952. He continued demand studies initiated at Oxford (see Houthakker, 1952, Houthakker and Tobin, 1952) and initiated inquiries into commodity futures markets. Lester Telser assisted with the latter work during the 1952-3 academic year. Results were reported in a series of Cowles Commission Discussion Papers (Nos. 2089, 2090, 2091, 2104, 2124) and in later publications of the investigators.

A not very well known paper by Koopmans raises a question that is fundamental to many small sector analyses. The question has not been satisfactorily resolved and is pertinent to several of the investigations noted in this subsection. The question is, "When, in studying a small sector of the economy, can macrovariables such as GNP or aggregate disposable income properly be regarded as exogenous?" It was, and still is, common practice to reason that, since adjustments in the small sector have small effects on, say GNP, the interdependence is necessarily small and the specification error that might be induced by considering GNP to be exogenous is also small. Koopmans argued that smallness of the sector being studied is not sufficient reason to regard macrovariables as exogenous. He produced an example in which the large sample specification error in analyzing arbitrarily small sectors could be expected to be of the same order of magnitude as the specification error in a macro model. Although the example is directed toward single-equation bias the general question clearly arises in choosing exogenous variables for multi-equation models. The

matter certainly deserves further investigation. Koopmans' example was formulated in 1944 and had limited circulation in unpublished form before finally being printed in his collected works. See Koopmans (1970), p. 87.

Section 2.4 Footnotes

¹These materials are included in the section of the Cowles Foundation Archives labeled, " Reflections on the Cowles Commission".

²Zellner's (1962) proposal for related regressions would seem to have been worth considering had it been known at the time.

2.5 Theory and Measurement

Interplay between theory and observations is a central part of the development of any science. Theory furnishes explanations for patterns or properties of the observations and suggests what further observations are needed. Observations or measurements furnish puzzles to be explained by theory and material for testing aspects of theories. Particular scientific investigations may concern theory and observations in various proportions, but it seems unlikely that a useful inquiry can completely exclude either. As a minimum, primarily theoretical studies need some impressions of the measurable world as a guide, while studies devoted primarily to gathering and/or manipulating observations in a preliminary fashion need some rationale for choosing the observations to be included.

Simultaneous equations methods proposed at the Commission were sometimes criticized for demanding too much theory - for requiring that what was not known about an economy or sector be reduced a priori to unknown values of some coefficients in (then almost always linear) difference equations and some parameters of a probability distribution of unobserved disturbances. Sometimes people at the Commission criticized alternative methods as not utilizing economic theory or not being sufficiently articulated with economic theory to provide effective ways to choose among competing theories. Of course, there is nothing wrong with having several approaches being pursued at any given time. If it were not necessary to economize research resources, the profession could be quite cavalier about methodology, letting each investigator follow his or her own conjectures in choosing methods and letting eventual results arbitrate. However, resources are limited and investigators are naturally interested in

making best use of their own time so consultations on probable productivity of various approaches is natural. Certainly lots of freedom is desirable, but decisions as to which proposals receive financial support and which techniques are worth teaching to future researchers furnish additional occasions for studying methodology. Evaluations of methods for fundamental research are necessarily imprecise so judgements rendered must allow considerable latitude.

Early explanations (e.g., Haavelmo, 1944; Marschak, 1950, 1953; Koopmans, 1949; Hurwicz, 1944) of simultaneous equations were offered partly to get reactions and suggestions and partly in the hope of attaining acceptance and support for the approach. In the process, believed disadvantages of alternative approaches (non-probability, vague statistical specifications, inappropriate single equation models, etc) were noted. The most extensive and specific criticism of an alternative was directed by Koopmans (1947) toward the approach being developed by the National Bureau of Economic Research as presented by Burns and Mitchell (1946). A hint of the nature of the criticism is revealed by the titles, "Measuring Business Cycles" by Burns and Mitchell and "Measurement without Theory" by Koopmans.

Koopmans stated the following grounds for reservations about the Burns and Mitchell approach.

(1) "Fuller utilization of the concepts and hypotheses of economic theory as a part of the processes of observation and measurement promises to be a shorter road, perhaps the only possible road, to the understanding of cyclical fluctuations."

(2) "The prediction, within the narrowest attainable limits of error, of the effects of stated hypothetical measures of economic policy.. is actually the most important objective of the analysis of economic fluctuations..... without resort to theory.. conclusions relevant to the

guidance of economic policies, cannot be drawn."

(3) "...any rigorous testing of hypotheses according to modern methods of statistical inference requires a specification of the form of the joint probability distribution of the variables.."

In a commentary, Rutledge Vining (1949) interpreted Koopmans as essentially claiming that the simultaneous equations approach was the only scientific way to study economic fluctuations. He expressed interest in simultaneous equations, believing that the associated methods clearly deserved a place in business cycle analysis but also believing that the NBER approach had promise. He regarded both, but especially simultaneous equations, as sufficiently untried to make a convincing evaluation possible.

Vining expressed doubts that the economic theory being drawn upon by the Commission would prove adequate and he quoted several prominent statisticians (Kendall, Pearson, Yule, see Kendall, 1942) to the effect that there were important roles for statistics other than those based on probabilistic models. He conceded the importance of providing policy guidance, but did not want special relevance to be required at all stages of a scientific program.

In his reply, Koopmans agreed with Vining about social relevance and also that firm judgements of scientific procedures would have to await eventual results. He argued that, meanwhile, some comparisons could usefully be made on the basis of logical clarity and consistency and by analogies to other sciences. He indicated that, in making comparisons, he had been using work at Cowles to illustrate a broader approach in which the basic elements were formulation of a statistical model using the best economic theory the researcher could contrive and then using the model as a basis for empirical exploration.¹

At that time, the simultaneous equations approach was the only example of an explicitly probability based method under current development.² Now there would be frequency domain analysis, multivariate autoregression, and a much more developed simultaneous equations approach. A few comments on post-Commission evolution are included in Section 5, pp.

Efforts to gain acceptance and support for the multi-equation discipline were eminently successful. Most economists who regarded themselves as primarily econometricians found it attractive. It became the core of econometric teaching for about three decades and still occupies a very prominent place. The NBER approach also thrived with a somewhat different clientele. It contributed substantially to development of National Income Accounts, to expansion of the econometric data base, and to the vocabulary for discussing business cycles, first among economists and now in the media as well.

Section 2.5 Footnotes

¹Further contemporary exchanges may be found in Gordon (1949), Koopmans (1949b) and discussions by Haberler, Burns and Angell.

²The variate difference method was an interesting precursor which never gained wide application because of inherent conceptual problems. See Tintner (1940), Haavelmo (1941).

APPENDIX

Minutes of Discussion of Papers on
MULTIVARIATE ANALYSIS FOR NON-EXPERIMENTAL DATA AND RELATED PROBLEMS
OF MATRIX COMPUTATION

Presented by Cowles Commission Staff Members on August 23 - 24, 1946
 During and after the meeting of the Institute for Mathematical Statistics,
 in Ithaca, New York

I. Morning Session of Friday, August 23rd

1. Paper by T. Koopmans, "Statistical Inference in Dynamic Economic Models."
 Discussion.

Wald: If the disturbances u_g have a known non-normal distribution of known form, linear transformation of equations may change this distribution. Knowledge of the form of the distribution can therefore be used in identifying structural equations.

Koopmans: While this is correct as a mathematical statement, one must be careful not to make identification depend essentially on assumptions of which one is not definitely sure. In such cases large sampling variances of estimates may result, and it may be more efficient computationally to allow for such indeterminacy in the form of outright lack of identifiability.

Rubin: If sufficient restrictions on the form of the equations are available, identifiability can be established without recourse to assumptions about the distribution of disturbances.

Tukey suggested the use of methods of estimation which are simpler computationally than maximum likelihood. His suggestion, supplemented later in personal conversation, was as a first step to solve for A_{yx} the equations

$$A_{yy}^{-1} A_{yz} = P_{yz}$$

sacrificing the overidentifying restrictions to make a (unique) solution possible. Then insert

$$\alpha_{yx} z A_{yx} + E_{yx}$$

in the determinant of the second moments of

$$y' - \alpha_{yy}^{-1} \alpha_{yz} z'.$$

(which is the generalized variance in the sense of Wilks) and expand this determinant in powers of the components of E_{yx} , obtaining

$$\text{GenlzdVar}(y' - \alpha_{yy}^{-1} \alpha_{yz} z') = \text{GenlzdVar}(y' - P_{yz} z') + Q(e_{11}, e_{12}, \dots) + O(E_{yx}^3).$$

where Q is a homogeneous quadratic form in the components e_{ij} of E_{yx} . Then neglect the terms of higher order and minimize Q subject to all restrictions.

Wald: We attach too much importance to unbiased estimates. Sometimes biased estimates are better than the best unbiased ones. An example was given by Halmos.

Cochran: Bias is unimportant unless of order comparable to the sampling error. Moreover, in sample surveys, the deliberate use of biased estimates is now quite common, because they are often more accurate than the best available unbiased estimates.

Tukey referred to an estimate used by H.B. Mann to predict whether a State would go Republican ($\alpha=0$) or Democratic ($\alpha=1$). The use of unbiased estimates would here require using an estimate outside the interval $(0, 1)$ which is absurd.

Guttman: Is there any testing of the theory involved, as in factor analysis?

Koopmans: Only overidentifying restrictions, that is, restrictions not essential to identification, are subject to statistical test. In such a test the maintained hypothesis necessarily contains a minimum set of restrictions which is essential for identification.

Anderson: The factor analysis model can be derived from the present model. The factor analysis test is a test of whether or not we did include all relevant predetermined variables z .

2. Paper by T. W. Anderson and H. Rubin, "Estimation of Structural Equations through Linear Transformation of Regression Coefficients." Discussion.

Hotelling: Can the estimates developed by Anderson and Rubin be consistent under incomplete knowledge of the variables entering the complete system?

Anderson: Certain predetermined variables need not be known if enough known

predetermined variables remain. For this to become profitable, it is necessary that the unknown part of the reduced form behave asymptotically like the disturbances.

* * * * *

II. Friday Afternoon - First Session of the Discussion on
Matrix Computation Methods.

Present: T.W. Anderson, Arrow, Blackwell, Boschen, Bowker, Cochran, Curry, Dwyer, Feller, Girshick, Goheen, Guttman, Hotelling, Kao, Koopmans, Mauchly, Miss Rees, Rubin, Satterthwaite, C. Stein, Tukey, Wald.

Koopmans indicated how the Hotelling procedure for iterative inversion of a matrix can be obtained as an application of the Newton method to finding the saddle point of the function

$$\log \det A = \text{tr} AK.$$

He indicated that the application of the same method in the problem of estimating linear structural equations with uncorrelated disturbances, where we maximize

$$\log \det A = \frac{1}{2} \text{tr} AMA',$$

does not have comparable simplicity, because the linear equations that need to be solved with each iteration have coefficients which change from one iteration to another. He indicated a simplified procedure with smaller speed of convergence per iteration, which is computationally simple. In this procedure the direction in which the next approximation is sought is determined first from linear equations with a matrix of coefficients which is constant in iteration. Thereafter a scalar h is determined which indicates how far to go in that direction to reach the next approximation. This scalar can either be made variable in iterations, or kept constant.

Curry inquired about the size of the matrices involved.

Koopmans indicated that the Cowles Commission planned to deal with systems of increasing size from three equations with ten parameters, to, say, twenty

equations with fifty parameters. At the same time the computation problem would be made more complex by relaxing all restrictions on the covariance matrix of disturbances.

Curry mentioned that he had heard matrices of order about 150 had been inverted in geodesy in England. He was uncertain as to whether these were general, or had non-vanishing elements only in certain diagonal rows.

Feller suggested partitioning of matrices, especially if regularities in the pattern of the matrices would permit partitioning into commutative parts.

Koopmans referred to equation (4190) and equation (4191) as an example of the regularities in the matrices whose inversion is needed.

Curry: Have you considered using the gradient for the method of steepest descent?

Koopmans: So far we have not, under the influence of the consideration that the convergence properties of such a method would depend on the arbitrary units of measurement of the variables. The method described in the manuscript has no such arbitrariness.

Curry recommended working with constant h . For a high speed computing machine simplicity of each iteration is more important than high speed of convergence per iteration. He suggested that, of machines available at Aberdeen, the most suitable one for this problem is the IBM Delay Multiplier, which is not yet on the market. There is one in existence at Aberdeen Proving Grounds, and one at the Thomas J. Watson Computing Bureau at Columbia University. The computation is too simple to justify tying up a machine like the ENIAC, because most of the time would be taken up by input and output on cards. The ENIAC has only a limited internal memory of twenty 10-digit numbers.

Tukey suggested that, instead of obtaining a new value of h for each iteration by the von Neumann procedure indicated in the manuscript, one might compute $\log L$ for two values of h and fit a parabola.

Guttman suggested that the value of h which maximizes the first two terms in the Taylor expansion of the likelihood function might not be the best value from the point of view of having a good starting point for the next iteration.

Koopmans indicated that the constant value $h=1$ has an advantage over all other constant values in an important case. The argument in question is contained in pages 422-433 of the manuscript. Only if all relevant characteristic values f are made to coincide by the restrictions, will the variable value $h^{(n)}$ converge to a limit.

Tukey suggested use of values of h which vary over the interval $1/2$ to $3/2$ say, for example alternately $3/4$ and $5/4$, in any case according to a prearranged plan rather than depending on the outcome of each iteration. This would ensure rapid diminution of components corresponding to a wide range of characteristic values.

Mauchly indicated that the electronic machine, to be built by the Institute for Advanced Study, will have a memory of 4000 12-digit numbers (decimal basis). The one to be built by the University of Pennsylvania will have a memory of 1000 10-digit numbers. The Census Bureau will also have a machine of this kind. Magnetic tape will provide flexible control of the machine and easy feeding of numbers and obtaining the results. Numbers on magnetic tape can also be erased or read back.

Curry described that on the IBM Relay Multiplier each iteration would have to be started and results collected by hand. A procedure for matrix multiplication on this machine was described by Alt in Mathematical Tables and Aids to Computation. Cards are fed through in such a way that columns are displaced by one unit at the proper time.

Rubin quoted that the IBM machine at Aberdeen multiplies two 10×10 matrices to six places in nine minutes. Rubin indicated how a forward solution in the Doolittle method can be used to simplify matrix inversion.

Dwyer stated that variations of the method (of obtaining the inverse directly from the forward solution without the reduction of the identity matrix) had been known for some time. He advocated the use of the square root method, rather than the Doolittle method, in this connection when the matrix is symmetric.

Guttman indicated that there was a method to invert a matrix starting from a small minor by successively adding more rows and columns.

Girshick has also worked on this same problem.

* * * * *

III. Saturday Morning - Second Session of Discussion on
Matrix Computation Methods.

Present: The same as Friday Afternoon, plus P.F. Stephan and minus Miss Rees and Mr. Goheen.

Koopmans invited further comments on the question of the best equipment and procedure to deal with the computational problem described in the previous session.

Curry mentioned that before remodeling the Relay Multiplier operated with about twenty counters, without accumulation (after remodeling there are 31 counters, each with 6 places). It multiplies in a fixed length of time and divides in a larger and more variable length of time. Each type of computation requires an appropriate plug board which can be filled. The machine operates at 6000 angle cycles per hour with four multiplications in two cycles. Plug board must be changed by hand.

Koopmans, answering a question of Satterthwaite, indicated that in successive problems the nature of the restrictions, and therewith the number of unknown parameters, could change, but the mathematical character of the computation would remain the same. In answer to a question by Girshick, he mentioned that one supervisor and one to two computers had worked two to three months on an eight equation system, by hand calculating.

Rubin estimated that a ten equation system with fifty unknown parameters by hand computing with an ordinary calculator requires 70 24-hour computer days.

Koopmans mentioned that relaxation of Σ restrictions further increased the work at least three-fold.

Cochran and Hotelling inquired into the number of iterations, and criteria for when it is safe to stop,

Koopmans indicated that so far six to ten iterations had proved sufficient with use of a constant h , and that limits could be derived which state maximum distance to the solution in terms of the results of the n th iteration.

Curry described two types of calculations in one of which many parts can go

on simultaneously, while in the other, prevalent in ballistics, there is a long sequence of operations of which each can only be started after the previous one has been completed. The ENIAC was especially devised for the latter type of computation. He described the iterative procedure for solving a one variable equation

$$f(x) = 0,$$

by considering

$$g(x) = x - \lambda(x) f(x)$$

The Newton method employs

$$\lambda(x) = \frac{1}{f'(x)}$$

but for reasons stated previously, a constant value of λ is preferable with machine calculation.

Satterthwaite asked how much time is necessary to multiply two matrices of order 50.

Rubin estimated this to be 19 hours on the Relay Multiplier, not counting wiring.

Kao mentioned that a matrix multiplier, based on an analogy principle, had been suggested in the Journal of Applied Physics.

Feller, Tukey expressed doubt about the accuracy of analogy machines. A discussion of input problems ensued.

Mauchly: The magnetic tape input principle of electronic computers now being developed is much faster than card feeding presently used with the ENIAC, or punched paper tape used by the Bell Telephone Relay machine.

Curry: The Aiken machine and the MIT Differential Analyzer are tape-controlled, but the input for the former is on cards.

Mauchly: The small memory of the ENIAC can be supplemented by a function table whose two matrix subscripts present in the memory of the machine will lead to a 10-digit value of that matrix element, made available to the machine by the setting of manual switches. It would take about half an hour with six people to set up and check a function table for a problem of the size envisaged, during most of which time the machine

could not work on another job.

Feller mentioned that certain operations on matrices recur in many different fields and problems, like matrix inversion, determination of principal components, etc. If the cost of such standard operations were made nominal by machines, the method of solution of more complex problems could be accordingly adapted. He felt that to entrust the whole of the computation problems under discussion to a machine would require a special study of input of data and instructions, and might therefore be less economical than a procedure in which the most is made of certain mechanised standard operations.

Tukey suggested that once the electronic machine, that can be expected to be available after five years, is used for this type of problem, the machine should provide estimates for a large assortment of alternative sets of restrictions at the same time.

Koopmans pointed out that the analysis of the results of such a calculation would require further development of the statistical theory of multiple choice between more than two alternative hypotheses.

Satterthwaite asked what would be the cost of multiplying two fifty order matrices once the electronic computer now under design is commercially available.

Mauchly estimated the total cost to the user of such a machine at \$50,000, roughly. Or perhaps \$15,000 per year. The calculations required for multiplying two 50-order matrices could be performed in two minutes. Most of the cost of the operation would therefore consist in putting the data onto magnetic tape, and getting the result off magnetic tape.

Stephan: With purely computational problems made so simple in the near future, the main problem is the improvement and extension of economic data.

Girshick had come to the same conclusion in applying the present methods in a study of the demand for food.

Hotelling asked whether use of annual or monthly data was contemplated.

Koopmans: So far annual data had been used. He agreed with Hotelling

that use of shorter periods is desirable. He felt that further development of statistical theory to allow for serially correlated disturbances would be necessary to make this possible and conjectured that once this is done, the most satisfactory procedure would be to treat the disturbances as a stochastic process of a continuous time variable.

* * * * *

T.W. Anderson

T. Koopmans

October 4, 1946

3. Activity Analysis

3.1 Historical Sketch

When Koopmans joined the Cowles Commission in 1944, he had worked for two years as a statistician for the Combined Shipping Adjustment Board, a cooperative British-American group studying merchant shipping problems during World War II. He had made preliminary formulations of several models which later became known as versions of the transportation problem (see Koopmans, 1947b; Koopmans and Reiter, 1951), a problem of routing carriers to achieve a stated objective at lowest cost or to achieve as much as possible with resource limitations. He continued studying these models at Chicago and was visited early in 1947 by George Dantzig (see Dantzig, 1963, 1982) who was a mathematical advisor to the Comptroller of the United States Air Force and had, along with Marshall K. Wood and Murray A. Geisler, formulated models of Air Force procurement and deployment activities. The formal structures of the models were similar.

Dantzig had concluded that quite a few of the problems encountered shared the abstract form of minimization of a linear function of decision variables subject to linear equations and inequalities. He was working on a general

algorithm to solve such problems, later designated linear programming. Dantzig wanted to discuss possible related work by economists and to learn the potential interest of economists in these problems. Koopmans provided enthusiastic assurance of potential importance and discussion of past related work. Dantzig proceeded with the quest for an algorithm and invented the simplex method that summer. Meanwhile, Koopmans prepared his 1947 paper on carrying a given pattern of ocean trade with a minimum of empty ship mileage and widened his discussions with other economists. The known economic background included work from the 1930's by Austrian and German economists who cited the need to require nonnegative prices and rates of production in general equilibrium analysis.¹ The need to explain which goods are marketed and which are free was also discussed. Wald (1936a) and von Neumann (1937, 1945) provided analyses of successively more general models of production which included these considerations. Existence of solutions was established but only under very special economic assumptions. Conditions providing uniqueness were also investigated.

The interindustry relations project by Wassily Leontief (1941, 1949) and his associates had some similarities to the models contemplated by Koopmans and Dantzig. Leontief constructed a linear system showing approximate exchanges of products among American industries when operated at levels

needed to produce various combinations of final products - consumer goods and capital investments. There was no optimization problem, but a way of tracing presumed developments through the economy, provided historical input-output relationships were maintained. In this respect the Leontief model is less flexible than linear programming. It did provide a useful way of summarizing some important aspects of extensive economic data and, because of linearity assumptions, posed some mathematical problems similar to those encountered in linear programming and in game theory.

Though not major pursuits at the Cowles Commission, economic interpretations and implications of Leontief models were studied by Klein (1953), Arrow (1954) and Chipman²(1954).

One of the most stimulating developments in the history of economics, the publication of the Theory of Games by von Neumann and Morgenstern in 1944 was also part of the setting in which programming methods and applications developed. It was quickly noted (Gale, Kuhn and Tucker, 1951; Dantzig, 1951) that a two person zero sum game is mathematically equivalent to a linear programming problem. Ever since, interplay between game theory and programming has been important to the development of each. Early review articles by Hurwicz (1945) and Marschak (1946) notified quite a few economists of the potential impact of this line of inquiry.

Many of the discussions among economists in 1947-8 stimulated lively interest and some led to new research initiatives. In June 1949, Koopmans arranged a 4-day conference at the Cowles Commission where active researchers and interested parties could exchange recent experiences, identify common problems, and speculate about useful directions for future research. Conference papers covered the topics that had developed to that time; several presaged future developments that have become central to economic theory. Others presaged developments that have given economic analysis a clearer and more immediate relevance to the problems of firms and public agencies.

The structure of models used in Air Force planning were explained; initial applications to economic theory and to decisions of individual economic units were presented; relations to game theory and to Leontief models were investigated. The theory of convex sets, the key to mathematical analysis of programming models, was presented as it stood then; Dantzig gave his first published account of the simplex method and other possible approaches to computation were indicated.

Most of the conference papers were included in Cowles Commission Monograph 13, edited by Koopmans and titled "Activity Analysis of Production and Allocation" (1951). The term, "activity analysis" seems to stem from early Air

Force literature. Mathematical programming has been a more common designation. The monograph became a starting point for the exciting mathematical economics revolution of the 1950's and 60's and the topics are highly relevant to the continuation of that revolution today; see Debreu (1983), Weintraub (1983). It was also a guide for many who made empirical applications to particular firms, types of firms, and industries; see Charnes and Cooper (1961). During this period, Cowles work on programming was largely supported by a contract with the Rand Corporation. Throughout the 1950's and early 1960's, many of these problems were under simultaneous investigation at the Cowles Commission (Foundation after 1955) and at Rand. Cowles staff sometimes spent parts of summers at Rand and a conference at either place was likely to attract participants from the other.

One development not reported at the initial Cowles conference and, at that time, unknown to all, or nearly all, American and Western European economists was the work of Russian mathematicians and economists, primarily Leonid V. Kantorovich; see Kantorovich (1976a,b), Johansen (1976).

In 1938, Kantorovich was asked to recommend ways of scheduling operations on a fixed installation of machine tools in the plywood industry. He formulated this as a linear programming problem and found optimum assignments. His method, called "resolving multipliers", was not as

general as the simplex method but did essentially use a Lagrangian formulation and properties of the dual problem which have great theoretical interest. The initial work was reported in a Russian pamphlet in 1939 and reprinted in Management Science, Kantorovich (1960), twenty-one years later. For several years, Kantorovich made other applications to particular plants or enterprises - best use of sowing areas, material cutting, use of particular natural resources, transportation flows. His list resembles early applications in United States. Kantorovich (1958) is a translation of a transportation problem.³

The linear programming problem may be stated⁴
Choose $x = (x_1 \dots x_n)$ to maximize $c'x$ subject to

$$Ax \leq b$$

$$x \geq 0$$

where A is a known $m \times n$ matrix and c , b are known column vectors of, respectively n and m components. $c'x$ is called the objective function or the criterion. A is the constraint matrix and components of b are the constraint constants. Properties of solutions of linear programming problems and their interpretations are elucidated by examining the associated Lagrangian function, saddle point problem, and dual problem.

The saddle point problem is

$$\max_x \min_y L(x,y) = c'x + y'(b-Ax) = c'x + y'b - y'Ax$$

subject to $x \geq 0, y \geq 0$. L is the Lagrangian function. The m components of y are Lagrange multipliers for the original (primal) maximization problem and are decision variables for the dual problem, namely - choose y to minimize $y'b$ subject to $y \geq 0, y'A \geq c$.

A nonnegative vector x is called feasible for the primal problem if $Ax \leq b$ and a nonnegative vector y is called feasible for the dual problem if $y'A \leq c$. If both problems have feasible vectors then both problems have solutions. If \bar{x}, \bar{y} are feasible, then the conditions $(c' - \bar{y}'A)\bar{x} = 0$, $\bar{y}'(b - A\bar{x}) = 0$ are necessary and sufficient for \bar{x}, \bar{y} to be solutions for the primal and dual problems, respectively. The conditions are called complementary slackness conditions. (\bar{x}, \bar{y}) solve the primal and dual problems if and only if they solve the saddle point problem.

People who studied or applied linear programming immediately recognized that satisfactory treatment of many problems would require a more general model. Generalizations in several respects were needed, e.g. multiple objectives, integer programming; but the need to deal with nonlinear criteria and/or restraints was recognized as the most important challenge. At the Cowles Conference, Murray Geisler reported on non-linearities that had appeared in Air Force work and preliminary studies at Rand by H.W. Kuhn and A.W. Tucker were

informally discussed. For a penetrating historical account see Kuhn (1976).

The nonlinear programming problem is to choose $x=(x_1..x_n)$ to maximize a differentiable function $f(x)$ subject to constraints $g(x) \leq b$, $x \geq 0$. $g(x)$ is an m -tuple of differentiable functions $(g_1(x)..g_m(x))$. Again, there is a corresponding saddle point problem

$$\max_x \min_y L(x,y) = f(x) + y'(b-g(x))$$

subject to $x \geq 0$, $y \geq 0$, and L may be interpreted as a Lagrangian function with $y = (y_1..y_m)$ as Lagrange multipliers.

Clearly, a nonnegative pair (x^*,y^*) solve the saddle point problem if $L(x,y^*) \leq L(x^*,y^*) \leq L(x^*,y)$ for all $x,y \geq 0$. Kuhn and Tucker (1951) showed the conditions

$$(1) \quad D_x L(x^*,y^*) \leq 0 \quad x^*{}'D_x L(x^*,y^*) = 0 \quad x^* \geq 0$$

$$(2) \quad D_y L(x^*,y^*) \leq 0 \quad y^*{}'D_y L(x^*,y^*) = 0 \quad y^* \geq 0$$

to be necessary for (x^*,y^*) to solve the saddle value problem and that (1), (2) combined with

$$(3) \quad L(x,y^*) \leq L(x^*,y^*) + (x-x^*){}'D_x L(x^*,y^*) \text{ for all } x \geq 0$$

$$(4) \quad L(x^*,y) \geq L(x^*,y^*) + (y-y^*){}'D_y L(x^*,y^*) \text{ for all } y \geq 0$$

are sufficient. By definition, (3) and (4) are satisfied if $L(x,y^*)$ is a concave function of x and $L(x^*,y)$ is a convex

function of y .

For a vector x^* to solve the nonlinear maximum problem it is sufficient that x^* and some y^* solve the saddle value problem - i.e. conditions (1) through (4) are sufficient. Necessary conditions in the nonlinear case are a little more complicated. If the feasible set $\{x : x \geq 0, g(x) \leq b\}$ has the property of satisfying a suitable "constraint qualification" then a necessary condition for x^* to solve the nonlinear programming problem is that there exist a y^* such that (1), (2) hold. A number of suitable constraint qualifications have been identified (see e.g. Bazaraa and Shetty, 1976) and feasible sets encountered in practice have rarely failed to satisfy one or another. If $f(x)$ is concave and the components of $g(x)$ are convex then suitable constraint qualifications are satisfied and the Kuhn-Tucker conditions become necessary and sufficient.

In several contexts extensions of the basic Kuhn-Tucker (1950,1951) results outlined above are needed. Quite a few have been provided through work at various centers. At the Cowles Commission, Morton Slater (1950) confirmed a Kuhn-Tucker conjecture that differentiability was not needed for equivalence between the saddle point problem and the nonlinear maximum problem. He also provided an alternative constraint qualification that is easier to visualize and to check.

Early work of Arrow and Hurwicz further generalizing Kuhn-Tucker results and showing their relevance to a variety of broader questions in economic theory (e.g. dynamic optimization, existence of efficient market mechanisms) was partially supported by the Commission. As consultants, their work was occasionally reported and discussed. In collaboration with Hirofumi Uzawa, the very important volume, Studies in linear and non-linear programming (Arrow, Hurwicz, Uzawa, 1958) emerged. Of utmost importance in economic applications was Koopmans' (1951b, 1957) linear programming representation of equilibrium of the firm. The way in which a concave production function (convex feasible set) followed from a combination of constant-return activities subject to factor limitations clarified many earlier discussions of the possible existence and role of constant returns. The interpretation of Lagrange multipliers (choice variables in the dual problem) as prices showed clearly how scarce and free factors were determined.

Three quadratic programming algorithms were later formulated by Cowles researchers active during this period. See Markowitz (1956), Hildreth (1957), and Houthakker (1960). The Hildreth algorithm has been extended by Herman and Lent (1976) and Lent and Censor (1980) and found useful in diagnostic radiology.

3.2 Impacts

Mathematical programming had profound and continuing influences on both theoretical and applied economic research. These are generally intertwined with influences from game theory, utility theory, behavior under uncertainty, and the steady extension of economists' mathematical repertoire. It is useful to distinguish studies concerned with decisions in particular firms, markets, and agencies from studies emphasizing mathematical economics broadly, especially general equilibrium theory.

Most of the former could be classed as management science and/or operations research. Exactly how these disciplines overlap with and differ from economics depends on one's own frame of reference, but their rapid development⁵ in the 50's and 60's clearly brought economists into much closer touch with real world decision making. The latter studies were centered on general equilibrium and greatly increased the scope, rigor, and logical clarity of analyses of economic systems. They appear to have potential relevance to vital questions of social and economic organization.

3.2.1 Management

After the papers of the 1949 conference were disseminated, Cowles work on programming management problems became a progressively smaller part of this expanding field.

However, significant contributions continued. Applications to ocean shipping by Koopmans (1947b) and Koopmans and Reiter (1951) were supplemented with studies of railway transport and of highway traffic by Beckmann, McGuire, and Winsten (1956).⁶ Hildreth and Reiter (1951) on crop rotation was followed by Hildreth (1955, 1957) on obtaining supply and demand relations for a farm from data furnished by agricultural experiments.

A pioneering study of inventory strategy by Arrow, Harris and Marschak led to many elaborations and allied investigations. The case of a long lag between order and delivery was treated by Beckmann and Muth (1956). A useful advance in location theory was provided by Beckmann and (Thomas) Marschak (1955). Using linear programming and less restrictive assumptions than earlier literature, they analyzed profit maximizing location decisions of a firm with given plants, and efficient assignments for an economy with given plants and resources. Beckmann (1956) also observed some elementary relations of comparative statics implicit in the usual linear programming assumptions. His results were strengthened by M.J. Bailey (1956). Both papers supplement Simon's (1951) conference discussion of technological change.

A natural and growing aid to managerial research is the application of programming methods in statistical inference, see Arthanari and Dodge (1981). Perhaps the first instance

of such application is Hildreth's (1954) procedure for estimating ordinates of concave functions.

In 1946 the Committee on the Social and Economic Aspects of Atomic Power of the Social Science Research Council sponsored⁷ a study of economic implications of atomic power which was conducted at the Cowles Commission. Co-directors were Marschak and Sam H. Schurr, who had been on the staff of the National Bureau of Economic Research and the Division of Interindustry Economics, U.S. Bureau of Labor Statistics. The final report, Schurr and Marschak (1950), contained key chapters by Simon. Simon, then teaching political science at Illinois Technical Institute, had been following Cowles seminars since 1943 and was strongly influenced by the discussions of identification and by nascent activity analysis. He had analyzed, Simon (1947), the effects of productivity on the shift of population from farms to cities and had developed strong interests in technological change and in organization theory.

In the study, comparative costs of conventional and nuclear generated electricity were estimated. Probable changes in the first affected industries were discussed. Likely impacts on nations, regions and particularly on undeveloped areas were studied. Although formal mathematical programming analysis was just beginning to develop, some of the ideas being discussed proved useful.

Schurr became chief economist of the U.S. Bureau of Mines in 1950 and is presently Deputy Director of the Electric Power Research Institute. In 1982 he received the first International Association of Energy Economists Award for Distinguished Contributions to Energy Economics. About the final report of the Cowles Commission study he writes,⁸

"The book was the first comprehensive economic study of the subject, and served to provide a quantitative framework for evaluating the possible economic impacts of this new energy supply technology. It was widely used and cited for many years."

"The book continues to be useful even today for its approaches to studying the impacts of a new technology. We devised a means for approximating a 'minimum conceivable cost' for nuclear power by borrowing cost factors from existing technologies that were similar enough to set boundaries on future nuclear costs. An important feature of our work involved the use of the minimum cost estimate to study the changes in technology within energy-using industries that might be 'triggered' if nuclear power were to be produced at this cost. We did not confine ourselves to factor substitution within existing technologies that might result from reduced energy costs, but inquired also into the reconceptualization of production processes that might result. This

approach to analyzing the extent to which technological progress could be induced (or facilitated) by low-cost electricity can serve as a model for research that is needed today to understand the economic role of energy."

"In all of this, our purpose was, of course to determine whether the cost of nuclear energy could possibly be low enough to result in significant economic impacts. However, the approaches employed have a continuing usefulness, no matter what the cost prospects for any particular energy supply technology."

Simon writes,⁹ "Our conservative (and correct) predictions of the rather modest economic importance of atomic energy at that time were ignored by the optimists who were looking forward to 'free energy.' Hence, we got the Cassandra treatment."

3.2.2 Equilibrium

Work on equilibrium theory at the Cowles Commission was much more than an outgrowth of activity analysis. Indeed, among the most significant pre-activity analysis contributions to and applications of equilibrium theory were Lange's "Foundations of Welfare Economics" (1942) and Price flexibility and employment (1945), and Patinkin's "The Indeterminacy of Absolute Prices in Classical Economic Theory," (1949). However, activity analysis provided some

direct contributions and the general ferment that accompanied activity analysis seems sufficiently responsible for the notable progress of equilibrium theory in the early fifties to permit discussion from this viewpoint.

Once equilibrium of an economy is defined, the theory is concerned with existence,¹⁰ optimality and stability. Since 1950, John Nash's equilibrium for an n -person game has provided a definition which can be adapted to many contexts.

Important advances generalizing and logically strengthening previous work on existence and optimality were made at, or in association with, the Commission primarily by Debreu, Arrow and Lionel McKenzie. Debreu was in residence, Arrow had gone to Stanford in 1949 but remained a consultant. McKenzie spent 12 months at the Commission in 1949-50. Existence and optimality results of the early fifties paved the way for salient later work on stability by Arrow and Hurwicz (1958) and Arrow, Block and Hurwicz (1959).

Investigations of equilibrium in this period have been sufficiently reviewed¹¹ that a very brief sketch should suffice here.

Serious consideration of equilibrium started with Léon Walras (1874-7). His well known $2(m+n)$ equation model of supply and demand for m factors and n consumer goods was presumed to uniquely determine the $2(m+n)$ equilibrium price and quantities subject to an arbitrary normalization restriction

on prices. The latter came about because Walras (reasonably in his context) equated producer revenue to consumer expenditure (Walras' Law) meaning that at most $2(m+n)-1$ of his equations were functionally independent. However, utility maximization by consumers and profit maximization by producers implied that any positive multiple of an array of equilibrium prices would also be an equilibrium array and would correspond to the initial equilibrium quantities. Thus, without another restriction, the system could at most determine relative rather than absolute prices. Analysts often visualized an added restriction obtained by designating one commodity the numeraire so its price was unity.¹²

A stellar contribution far ahead of its time, the Walrasian development was basically incomplete in not raising the question of mathematical conditions determining the nature of the solution set for the postulated system of equations. In particular, an empty set would mean non-existence of equilibrium. Nothing in the economic reasoning precluded this possibility. A unique solution with positive prices and quantities was an unsupported assumption.

The economics profession, even its small mathematical branch, was slow to recognize the vital importance of existence and related issues such as the determination of which resources are scarce and therefore command a positive price. Discussion did emerge in the late 1920's among the partici-

pants in Karl Menger's Vienna Colloquium (see Weintraub (1983), pp. 5-17; Arrow and Debreu (1954), pp. 287-9). Karl Schlesinger (1935) produced a modified Walrasian model which included inequalities stating that each resource must be available in at least the quantity needed to produce the contemplated quantities of commodities. It was then possible in principle for the system to select the scarce inputs.

Abraham Wald (1935, 1936) showed that several systems of this type possess solutions. However, some of Wald's economic assumptions are very restrictive. Using set theory and convex analysis suggested by programming studies and an extension by Debreu (1952) of Nash's existence theorem for n -person games, Arrow and Debreu (1954) proved several existence theorems for competitive economies under more plausible assumptions. The latter authors had independently - Arrow (1951), Debreu (1951) - proved more general versions of the basic optimality theorems which state circumstance under which (a) an equilibrium relative to a price system is an (Pareto) optimum and (b) a given optimum can be realized as an equilibrium with respect to a price system.

The existence and optimality results were combined by Debreu (1959), Theory of Value, in one of the most useful and influential theoretical treatises of any period. The brief but accessible (with some work) mathematical foundation and the equally concise economic interpretations have enabled

the volume to serve several purposes besides acquainting readers with an important advance in equilibrium theory. Economists can get an introduction to aspects of mathematics that have become important in several areas. Mathematicians can get a quick but effective introduction to one sector of economic theory. The clear rationale immediately started suggesting further inquiries. The volume has served as a point of departure for many subsequent authors and contributed much to the mathematical economics surge considered as starting in the early fifties and continuing unabated.¹³

The existence proof in Theory of Value is simpler than in Arrow-Debreu. The central theorem using Kakutani's Fixed Point Theorem (1941), was developed independently by David Gale (1955), Hukukane Nikaido (1956), and Debreu (1956). Using a 1953 paper by Arrow, Theory of Value also contains an extended interpretation applying to uncertain environments. There are still troublesome special assumptions - e.g. nonincreasing returns, no externalities, price taking by all firms, adequate initial endowments for consumers. However, enough previous difficulties - e.g. slippery mathematics, strong differentiability, absence of corner maxima, *a priori* designation of free goods - were resolved to give the model increased relevance and to identify directions for further study.

Following Debreu (1952; 1984, pp. 269-70), one may

explain the relevance of fixed points to social equilibrium as follows. Suppose there are n participants (agents) in a society. Let $a = (a_1 \dots a_n)$ be actions chosen by the respective participants with $a_i \in Q_i$, a set of possible actions for the i^{th} participant. Define $Q = Q_1 \times \dots \times Q_n$ and let $\tilde{a}_i = (a_1 \dots a_{i-1}, a_{i+1} \dots a_n)$ be actions of all except the i^{th} participant. Assume that actions available to each participant are dependent on the actions of others, e.g. participant i cannot contract to erect a building unless someone else is willing to contract to have a building erected. Denote by $\alpha_i(\tilde{a}_i)$ the set of available actions for participant i given that others choose \tilde{a}_i . Define

$$\tilde{Q} = \{a \in Q : a_i \in \alpha_i(\tilde{a}_i) \quad i=1..n\}$$

as the set of mutually available actions.

Suppose each participant has a preference system or preorder on Q , the set of possible social actions. For any participant i , this defines a preference preorder on any set of actions $\alpha_i(\tilde{a}_i)$ that might become available. Let $\mu_i(\tilde{a}_i)$ be the maximal subset of $\alpha_i(\tilde{a}_i)$, namely those available actions (elements of $\alpha_i(\tilde{a}_i)$) to which no other available action is preferred. If the μ_i are non-empty, one may define a correspondence (multi-valued function) from \tilde{Q} to itself

$$\mu = (\mu_1, \mu_2, \dots, \mu_n).$$

A social equilibrium is a set of actions such that no participant prefers another action to his present one given the present actions of others. In the above notation, this means an equilibrium is an action $a \in \tilde{Q}$ such that $a \in \mu(a)$, i.e. a fixed point of the correspondence μ . In economic applications, the participants are consumers, producers, and possibly a fictitious price setter. The existence problem is that of finding if plausible assumptions about the preferences of producers and consumers yield sufficient mathematical conditions for a fixed point.

Equilibrium analysis of international trade at the Cowles Commission also pre-dated activity analysis. The principal early publication was Mosak's (1944) monograph based on his doctoral dissertation. In 1953 Reiter used activity analysis to consider effects of trade barriers and the next year McKenzie (1954a, b) published two articles extending the analysis of Frank Graham's model of international exchange. The first developed conditions for a maximal value of world production and the second proved the existence of an equilibrium of world trade and other competitive systems. Though developed independently, the formal structure was similar to Arrow-Debreu and Kakutani's theorem was the main tool of the proof. McKenzie's continued investigations (1955, 1981) involved several extensions of this model. A model extending the analysis to an infinite hori-

zon and emphasizing efficient capital accumulation had been developed by Malinvaud (1953). The infinite horizon extension was important because in most dynamic analyses there is no clear point at which the considerations under study cease to apply.

Section 3 Footnotes

¹See Weintraub (1983), pp. 7-15, Koopmans (1951), pp. 1-4, and Schlesinger (1934).

²John Chipman was a guest of the Commission during the 1951-2 academic year and subsequently participated in occasional seminars and discussions.

³Kantorovich (1976b) cites an early statement of the problem by A. Tolstoy in 1930. Cowles literature refers to a version by Hitchcock (1941).

⁴See Intriligator (1981) for further discussion of linear programming and its extensions and for references to the principal theorems. Applied problems do not necessarily appear initially in the standard form stated, but are easily restated if both criterion and constraints are linear. For example the equality constraint $a'x = b$ is equivalent to the two inequalities $a'x \leq b$ and $-a'x \leq -b$.

⁵As indicators of development Koopmans (1957) observed that the journal, Operations Research was started in 1952, Management Science and Naval Research Logistics Quarterly in 1954. These have been followed by the SIAM Journal on Control and Optimization in 1962, International Journal of Game Theory in 1971, and Mathematics of Operations Research in 1976.

⁶See also Beckmann (1952) and Beckmann, McGuire, and Winsten (1956).

⁷A supplementary grant was received from the Life Insurance Association of America in 1948.

⁸Letter to author, December 8, 1982.

⁹Memo to author, August 2, 1982.

¹⁰Including such basic properties of the set of equilibria as uniqueness, finiteness, discreteness, compactness.

¹¹Arrow (1968), Takayama (1973), Arrow and Intriligator (1982), Debreu (1982, 83, 84), Hildenbrand (1983a), Weintraub (1983).

¹²Theorists who wanted to study a model in which absolute prices were determined added money to the system in one of several ways. See Patinkin (1981), Chapters 5, 6.

¹³For brief notes of subsequent work and references see Debreu (1982, 1984). In (1982) it was noted that the bibliography on existence of equilibrium over the previous 25 years included more than 350 items.

4. Related Topics

4.1 Social Choice

During the 1948-9 academic year, Arrow presented six discussion papers on social choice at Cowles seminars and staff meetings. His basic thesis¹ had been developed at Rand the previous summer following discussions with Olaf Helmer, Abraham Kaplan, David Blackwell, and J.W.T. Youngs. The papers were received with much excitement and with some surprise that essentially scientific reasoning could be effectively applied to a basic question of social ethics. Excitement became widespread with the publication of Arrow's study in the Journal of Political Economy in 1950 and, amplified and slightly revised, as Cowles Commission Monograph 12 in 1951.

Arrow accepted the democratic notion that social choices should appropriately reflect individual preferences and values and noted how utilitarianism and the then current "new" welfare economics were vague and incomplete in important respects. He formulated the problem as that of determining a social ordering among available alternatives, called social states,² from orderings by individuals. In symbols,

$$R = f(\{R_i\})$$

where, for any states x and y , xRy means that state x is socially preferred or indifferent to state y and $xR_i y$ means

that state x is preferred or indifferent to state y by the i^{th} member of the society. $\{R_i\}$ is the collection of preference systems of all individuals; Arrow, 1951, Ch. II, III. The function f , called a social welfare function, maps any collection of individual orderings (technically they are called complete preorders, see Debreu, 1959) into a corresponding social ordering R . If any f were agreed upon and one knew how the individuals would choose, f would tell how society would choose. In this approach the central welfare problem is to find a social welfare function, f , with desirable properties. Arrow proposed several properties as desirable and then showed that no social welfare function could possess all of these properties. The properties were stated in the form of four conditions³ on f . Stating the conditions requires a few additional concepts.

Corresponding to any orderings R , $\{R_i\}$ are a set of strict orderings P , $\{P_i\}$ where

xPy means xRy and y (not R) x

$xP_i y$ means $xR_i y$ and y (not R_i) x

xPy reads, " x is socially preferred to y ." $xP_i y$ reads, " x is preferred to y by individual i ." With any social ordering R , there is associated a social choice function C defined by

$$C(S) = \{x \in S \mid \forall y \in S \ xRy\}$$

where S is any collection of social states. If S consists

of the available states, the realized state must come from $C(S)$ if the choice is to be consistent with R . $C(S)$ might also be defined as those available states to which no other available state is preferred (see Arrow (1963), Ch. II, III).

A social welfare function f is said to be dictatorial if there exists an individual j such that for all states x, y $xP_j y \Leftrightarrow xPy$ regardless of $(\{R_i\})$ $i \neq j$.

Arrow's conditions are

(1) The domain of f is the collection of all sets of logically possible orderings $\{R_i\}$ $i=1 \dots n$.

(2) $xP_i y$ for $i=1 \dots n \Leftrightarrow xPy$.

(3) Let $C(S)$, $C'(S)$ be the choice functions derived from $\{R_i\}$, $\{R'_i\}$. If for all $x, y \in S$ and for $i=1 \dots n$ $xR_i y$ iff $xR'_i y$ then $C(S) = C'(S)$.

(4) f is not dictatorial.

Condition (1), called universality, assures that there will be a social ordering whatever individuals decide about their own orderings. (2) is usually called the weak Pareto principle. The frequently quoted Pareto rule that x is socially preferred to y if no one prefers y and someone prefers x is clearly stronger, so replacing (2) by the latter rule would not make it possible to satisfy all four conditions.

Condition (3) is called independence of irrelevant alternatives. It is the key provision for ruling out interpersonal comparisons of utilities on which the early util-

itarianism of Bentham (1789) depended. (3) says that in each instance of seeking a social choice, individual preferences among the alternatives actually available are the only ones to be taken into account. The non-dictatorship condition, (4), is self-explanatory.

Arrow's rationale for imposing such conditions and his proof of their mutual inconsistency, called the General Possibility Theorem, were received differently by different economists. A few regarded the whole approach as irrelevant, see Arrow (1963), Ch. 8. On the other hand, many economists, and quite a few social scientists and philosophers, regarded the approach and analysis as probably the most significant development to date in welfare economics and a notable contribution to social philosophy. Economists who disagreed with some particular aspect of the work were nevertheless convinced that its appearance marked an enormous gain in perceptive discussion.

Subsequent work in welfare economics, particularly by mathematical economists, exploded and has been largely in the Arrowian framework, involving refinements, extensions, modification of conditions or interpretations, and sometimes consideration of particular kinds of social decisions. The resulting literature has been proficiently reviewed by Amartya K. Sen (1985). Sen's bibliography has over 500 entries of which more than 90% appeared since 1965. The

journal, Social Choice and Welfare, started in 1984, included 36 articles in its first 5 numbers.

In his initial volume, Arrow (1951a) anticipated the general directions of much subsequent study. He examined the consequences of several modifications of conditions, reviewed alternative interpretations, and speculated about ways of trying to resolve outstanding issues. The outstanding issues included most everything except the General Possibility Theorem itself.

With Arrow's departure for Stanford in the summer of 1949 the center of discussions based on his approach also shifted. There remained considerable interest at Cowles, however. Papers emerged by Goodman and Markowitz (1952) and by Hildreth (1953). These include substantially different proposals but are alike in several respects. Both object to Arrow's independence condition (3), show that removing this condition allows considerable leeway, and suggest directions for looking for appealing social welfare functions. Both cite arguments (different in the two papers) of L.J. Savage supporting the idea that independence of irrelevant alternatives as posited might sometimes exclude relevant considerations.

Goodman and Markowitz distinguish considered social states from available states and contend that attitudes toward considered but unavailable states might be expected to

frequently reveal something about how much an individual prefers one available state over another. They observe that sums of rankings of a comprehensive list of considered states would provide a choice function satisfying Arrow's conditions (1), (2), (4). An analogy to Savage's (1951) treatment of choice among acts is noted. If further suggested assumptions, principally that individual rankings exhibit a finite number of "levels of discretion", are imposed then social welfare functions satisfying Arrow (1), (2), (4) are possible and further conditions reflecting judgements of social desiderata may still be imposed. Under some conditions, the sum of ranks of social states becomes the only admissible criterion.

Hildreth also disagrees with the independence condition and considers possible information furnished by contemplating states beyond those available at the time of a particular choice. He notes (1953), fn. 10, p. 90),

"L.J. Savage has made the interesting observation that many of the comparisons of degrees of preferences which people are commonly willing to make contain an implicit assumption that certain corporal states have similar significance for different individuals. Marshall's observation that a clerk with a lower salary will generally choose to walk to business through a heavier rain than a clerk with a higher salary (Marshall [1920, p. 19, p. 95] seems to involve a comparison of this kind. Willingness to risk extreme physical hardships such as hunger, exposure, torture, or death for an objective is almost universally accepted as evidence of the subjective importance of the objective."⁴

Random social states were introduced by Hildreth so willingness to risk dire consequences seemed a natural indicator. If states \tilde{X} , \tilde{Y} are contemplated such that each state provides the same fate (e.g. commodity bundle) for every individual, and the fate under \tilde{X} is unanimously preferred to that under \tilde{Y} , these may be used to construct sum of utility social welfare functions that satisfy Arrow (1), (2), (4) and other suggested conditions; similar treatment for similar individuals being the most interesting.

Hildreth also presents a heuristic argument that if one strictly imposes non-comparability of individual utilities (or preferences), then one cannot expect to order states more completely than the partial order furnished by Pareto. To say $X \succ Y$ when there is a nonempty group α such that $Y P_i X \quad i \in \alpha$ is to say that there must be a group β such that $X P_j Y \quad j \in \beta$ and that the gratifications experienced in β are socially more important than the disappointments in α .

Both the Goodman-Markowitz and the Hildreth papers suggest using techniques learned from Arrow to study kinds of individual utility functions that might be acceptable in a Benthamite utilitarian criterion.

4.2 Decisions Under Uncertainty

As noted earlier, publication of Theory of Games by Von Neumann and Morgenstern in 1944⁵ was pivotal for several branches of economics. Decisions under uncertainty is a prominent example. One may view the ideas advanced by Von Neumann and Morgenstern in the following categories.

(1) The abstract theory of a game. This introduces many useful and suggestive concepts such as pure and mixed strategies, coalition, imputation, solution.

(2) The interpretation of real-world problems, especially economic problems, as games.

(3) Axioms sufficient to guarantee an expected utility representation for games in which the decision-maker attaches probabilities to the circumstances (states of nature) which, together with his decision, will determine the final outcome. Utility functions unique to a positive affine transformation which can be used in such a representation are called Von Neuman-Morgenstern utilities.

All of these aspects inspired lively interest and research that is still burgeoning. Recent reviews of the work in game theory and interpretations are in Shubik (1981), (1983); studies of expected utility are reviewed in Fishburn (1982).

Many economists became aware of game theory through the early review articles by Hurwicz (1945) and Marschak (1946). They explained the abstract concepts and supplemented the examples provided by Von Neumann and Morgenstern. Examples were classical problems of monopsony, oligopsony, duopoly, oligopoly and negotiations for unique commodities - houses, production facilities, etc.

In their later accounts of the economic literature on uncertainty, Arrow (1951c) and Hurwicz (1953b) stressed the fundamental difference between probabilistic and non-probabilistic models. These correspond roughly to Knight's (1921) distinction between risk (probabilistic) and uncertainty (non-probabilistic). Subsequent usage puts both cases under the uncertainty label. For probabilistic models the expected utility representation proposed by Von Neumann and Morgenstern seemed a satisfactory formulation. Choices that maximize expected utility are called Bayesian solutions. However, most economists regarded probabilistic models as generally inappropriate - insurance, weather based ventures, and gambling being the principal exceptions. How to reasonably ascertain relevant probabilities in general was an unanswered question which contributed to the reluctance to accept the Bayesian approach more completely.

In this usage a probabilistic model is one in which all of the relevant unknowns can be assigned probability measures.

If some or all of the unknowns are regarded as non-probabilistic or if they depend on the interactions of distinct individuals the model is non-probabilistic. Von Neumann and Morgenstern had shown how a problem involving both kinds of unknowns could be reduced to a model with only non-probabilistic uncertainties. Arrow reviewed suggestions, primarily Irving Fisher (1906) and Keynes (1921), for interpreting probability in a personalistic way. He found vagueness and other difficulties in these suggestions. His emphasis was then placed on non-probabilistic formulations.

For the latter, Hurwicz discussed the various decision criteria that had been suggested⁶ - minimax (Von Neumann and Morgenstern), maximax (Modigliani), minimax regret (Savage), and weighted averages of the minimum and maximum (of money or utility) associated with each possible decision (Hurwicz 1951c). Finding no basis for general superiority of any one criterion, he conjectured that choice among criteria might be partly a matter of individual preference. This issue is still not fully resolved. Another possibility suggested by Hurwicz (1951c) is to modify the space over which any of the above criteria might be applied. In some of Wald's formulations the space consists of all possible probability distributions on states of nature. Bayesians select a specific distribution. One could also define a class of possible distributions suited to a particular

investigation. This idea has attracted favorable comment but has apparently not been pursued. The initial presentation contains an application to the problem of estimating means in a bivariate normal distribution.

Hurwicz was also concerned with conditions for equivalence between Nash equilibria and Pareto optima for n -person games and anticipated that analysis of decision problems for interacting individuals would have strong implications for the effectiveness of alternative organizational structures. A very active and useful field of inquiry has indeed developed (see Shubik, 1983) along these lines.

Both Arrow and Hurwicz noted that Wald's (1947a,b) adaptation of game theory to the problem of choosing a statistical decision function had the same logical form as many non-probabilistic economic decision problems. Rationale and some results could be interchanged. Arrow, Blackwell, and Girshick (1949) had previously extended Wald's work on sequential decisions and had obtained explicit solutions for several interesting special cases.

Chernoff (1954) contributed a penetrating analysis of the general decision problem. He discussed the problem in a statistical setting, but the considerations raised apply equally well to many problems in economics. The abstract problem posed is that of choosing a strategy or decision d from a set of possible decisions D . The outcome will depend

both on the decision and on circumstances s unknown to the decision maker and outside his control (states of nature). The utility of the outcome to the decision-maker is evaluated by a Von Neumann-Morgenstern utility function, thus one may express utility as a function of decision and state of nature⁷

$$u = \phi(d,s) \quad d \in D, \quad s \in S$$

where S is the set of all possible states.

If the decision maker thinks of the state s as the strategy of an opponent in a constant-sum game then a minimax⁸ solution d^* seems appropriate. By definition

$$d^* = \{d \in D : \min_{s \in S} \phi(d^*,s) \geq \min_{s \in S} \phi(d,s) \forall d \in D\}.$$

If the decision maker is willing to postulate a probability measure on S , then the Bayesian solution \tilde{d} seems appropriate where

$$\tilde{d} = \{d \in D : E\phi(\tilde{d},s) \geq E\phi(d,s) \forall d \in D\}$$

and E is expectation with respect to the postulated probability measure. As noted earlier, the annoying and still not fully resolved problem that perplexed statisticians and mathematical economists in the later 1940's and early 1950's was what to do when neither of these assumptions seemed appropriate.

Using suggestions from the literature to that point and adding proposals of his own, Chernoff formulated a set of 10 desirable requirements for a generally optimal decision function. To simplify mathematical arguments he assumed

that D and S were finite. He showed, under his main postulates,

1. The decision maker gains nothing by considering mixed strategies - i.e. using a random device to indicate the chosen decision.

2. The set of optimal decisions for any problem is unchanged if $u(f,s)$ is replaced by $v(f,s) = u(f,s) + g(s)$ - i.e. utility under some or all states is increased or decreased uniformly over decisions.

3. The only decision function satisfying his requirements is the special Bayesian solution

$$d^* = \{d \in D : \frac{1}{n} \sum_{s \in S} u(d^*, s) \geq \frac{1}{n} \sum_{s \in S} u(d, s) \forall d \in D\}.$$

Chernoff noted that the special Bayesian solution is equivalent to applying Laplace's "principle of insufficient reason" to which many objections had been raised. In particular the solution is very sensitive to the way in which states of nature are classified. Unless there is a natural or canonical classification for the problem at hand, the solution depends on a partly arbitrary choice. He noted that weakening one of his requirements would make any Bayesian solution a possible choice but this again raises the question of the source of the probabilities to be used. Chernoff expressed pessimism that an approach would be found that would be convincing for all cases. Such a conjecture

still seems justified,⁹ but many cases have now been studied with progress in quite a few (see Shubik, *op. cit.*).

In the decade, 1944-54, Von Neumann's and Morgenstern's expected utility representation also received substantial attention; see Strotz (1953).¹⁰ It was of recognized importance in solving the probabilistic models and in assigning utilities to alternative decisions in the mixed cases. To aid in understanding its rationale and scope, a number of alternative sets of axioms to justify the representation were offered along with less formally developed interpretations and examples. At Cowles, Marschak (1950), (1952) developed axioms in terms more familiar to economists and which employed familiar indifference surfaces. He also reexamined a number of historical problems in decision theory terms.

I.N. Herstein of the Cowles Commission and John Milnor of Princeton University collaborated to produce a very elegant and concise derivation of expected utility representation, Herstein and Milnor (1953). From three quite transparent axioms they produced a group of theorems. Several of their theorems had been used as axioms in previous derivations. Their final theorem established the existence of expected utility representation. Several of the concepts and arguments they introduced are still found useful in current treatments of the topic; see Fishburn, *op. cit.* The Herstein-Milnor, Chernoff, and Marschak papers drew upon

unpublished work of Rubin (1949a,b).

Several efforts of the 44-54 decade were directed toward making Von Neumann-Morgenstern utility operational. Friedman and Savage (1948)¹¹ considered whether the behavior of a person who both insured and gambled could be explained by a probabilistic model. (Recall that insurance and gambling were considered among the special cases to which probabilistic models might reasonably apply). They showed that such behavior requires a utility of wealth function with at least two points of inflection. The simplest case consists of concave segments at sufficiently high or low levels of wealth with a convex region at intermediate levels. Markowitz (1952a) showed that certain additional presumptions about behavior implied a more complicated utility function with at least three points of inflection, one located at the decision makers current level of wealth. Markowitz (1952b) also developed the notion that choice in probabilistic models should be confined to efficient points. These are probability distributions of future wealth that offer minimum variance for any achievable expected value. A specific choice results if it is further assumed that utility is a known function of mean and variance, increasing in the former and decreasing in the latter argument.

A major extension of the Von Neumann-Morgenstern results was provided in Foundations of Statistics by

Savage (1954). He provided a set of axioms under which the decision maker's choice in the general decision model corresponds to maximization of expected value of a Von Neumann-Morgenstern utility function with respect to a subjective distribution of possible states. The subjective distribution is kept current by successively applying Bayes Theorem to information acquired by the decision maker. Thus the model becomes probabilistic and, although the problem of finding an appropriate probability distribution is not completely resolved, considerable guidance is furnished.

Savage's work strongly appealed to many economists, statisticians, and other scientists. It motivated an upsurge of research on probabilistic models. Alternative axioms, applications in many areas, methods of elicitation of utilities and subjective probabilities have resulted and are still investigated; see Fishburn (1982), DeGroot (1970), Fienberg and Zellner (1975).

Suppose the outcome of interest is wealth and the feasible distributions of wealth belong to a two parameter family, say (μ_1, μ_2) . If (μ_1, μ_2) can be related to mean and variance (M, V) in a 1-1 fashion, then Markowitz's utility function of mean and variance can be derived and his procedure maximizes expected utility. A particularly simple example appears in Freund (1956). Markowitz's approach has been widely applied in investment, industrial, and agricultural studies.

4.3 Organization Theory, Teams, Decentralization

Economic literature on organization theory expanded greatly in the 1960's and high interest continues. The Cowles Foundation was a major contributor to this development¹² and much of the Foundation's work of that decade stemmed directly from work at the Commission in the early 50's. Research already discussed, particularly activity analysis and decisions under uncertainty, were vital parts of the extended study of organizations.

Abstract theory of organizations applies to many kinds - economies, societies, government agencies, firms, foundations, etc. However, as with activity analysis, one can distinguish investigations primarily directed toward macro units (economies, societies) and those directed primarily toward micro units (firms, plants, clubs). Many intermediate units - industries, markets, interrelated corporations, national and international societies - are hard to classify according to this traditional economic distinction. Roughly speaking, the work of Simon, Marschak, Radner and associates was directed toward micro and intermediate units while that of Lange, Lerner, Arrow, Debreu, Hurwicz was motivated more by macro problems.

Interest in organization theory at Cowles was in part a natural complement of the studies already reported. Inquiry into social choice raised questions about possible procedures

for making choices. Specifying available activities in programming models presumed some knowledge of the decision-making organization under consideration, and inspection of optimal solutions induced curiosity about how these might be approximately achieved in practice. Lange (1938) and Lerner¹³ (1944) had shown how the cost of decision-making in centralized systems could be greatly reduced by utilizing competitive mechanisms and incentives. Arrow and Debreu (1954) noted that existence of a Pareto optimal competitive equilibrium for an economy raised questions about the comparative organizational problems of competitive and more centralized economies. They also noted the need to investigate equilibrium under nonclassical assumptions - indivisibilities, increasing returns, incomplete information.

In applying activity analysis to transportation problems, Koopmans et al. (see Section 3.2.1 above) noted that two facilities, highways and railways, serving the same purpose were at opposite poles with regard to centralization of decision-making.

However, awareness of the need for serious research was clearly heightened by the participation of Herbert Simon. For more than a decade before joining the Commission as a Research Consultant in 1947, Simon had studied and written about organizational problems. His Administrative Behavior (1947) was quickly recognized by political scientists and

students of public administration as providing exciting new insights and guidance for future research. Economists gradually became aware of these ideas as Simon formalized them and related them to economic questions and to the growing decision theory literature; Simon (1952), (1955a,b), (1962), Simon and Holt (1954), Holt, Modigliani and Simon (1955).

In Simon's decision model, called bounded rationality or satisficing, a cooperating group of decision-makers (a single individual in exceptional cases) reviews their substantially shared major objectives and their parcels of incomplete information. Differing views are at least partially reconciled in acceptability constraints or aspiration levels defining what the group will regard as satisfactory achievement in a relevant period. Search procedures are undertaken to find means to achieve the chosen goals. Subgoals that can be pursued with substantial independence are normally identified and allocated to units within the organization. Actions believed to result in realization of the aspiration levels are undertaken when found. The model has been dominant in the field of management science and has influenced important investigations by economists (see Simon, 1979). However, economists generally do not seem to have yet settled on a particular approach to problems of organization.

In 1952, Marschak organized a major project on organization theory with financial assistance from the Office of

Naval Research. Roy Radner and Richard F. Muth were affiliated Research Associates. It was recognized that members of an organization try to find effective cohesive actions despite possible individual differences in (i) initial information and access to additional information, (ii) possibilities for action, and (iii) objectives. After an initial survey of relevant literature and possible lines of attack, Marschak (1954), primary attention was directed toward teams. These are organizations in which disparity of objectives is sufficiently small or has been sufficiently resolved to permit an analyst to assume that a common objective exists. Attention then centers on information, communication, coordination, and the cost in money and effort of alternative patterns of observing, communicating, and deciding. Teams are important in themselves and their study offered a way of exploring some important organizational problems before having to confront the whole array furnished by organizations generally. Simple initial models were formulated, studied, and generalized; Marschak (1955), Radner (1956). The work was continued at the Foundation and culminated in the milestone, The Economic Theory of Teams, Marschak and Radner (1971).

Leonid Hurwicz was a Research Associate at Cowles from 1942-6 and thereafter a Research Consultant while serving on the faculties of Iowa State University, the University of

Illinois, and the University of Minnesota. In Koopmans' absence he served as Acting Director of Research during the 1950-51 academic year.

Hurwicz took an early interest in problems of organization, initially with reference to the firm (Hurwicz, 1951a), and subsequently with reference to economic systems. In two discussion papers (1953) he formulated some of the basic concepts that have come to characterize the adjustment process approach to organization and decentralization of large economic complexes. He also inquired into the mathematical devices that seemed promising for developing theories in this area. The latter consisted partly of extending results that were useful in applying activity analysis (Kuhn and Tucker, 1950, 1951; Slater, 1950) to more general structures - e.g., topological vector spaces and in particular, Banach spaces.

Subsequent work has provided a more complete framework. The analytical problem posed is that of finding efficient or welfare enhancing mechanisms (behavior patterns or adjustment processes) given a collection of possible environments (individual endowments, individual preferences, and technical possibilities) and some properties of relations connecting mechanisms and environments to states. A state is a collection of relevant individual and group experiences¹⁴ - consuming, producing, exchanging, communicating, calculating, etc. Individual preferences are orderings (sometimes par-

tial) of states. The basic aim of such research is to meaningfully characterize and compare alternative social systems.

Emphasis has been on costs of information processing and enforcing organization rules, on decentralization of decision making, and on compatibility of rules with individual and group incentives. The problem of incentives distinguishes this approach from the study of teams for which common preferences provide basically common incentives. Motivation for the work, its place in general economic thought, and main developments to the mid seventies are concisely summarized in Hurwicz, 1973. Later research is included in Thomas Marschak, 1985.

During the academic year 1983-4, the program of the Institute for Mathematics and its Applications of the University of Minnesota (under the direction of Hans Weinberger and George Sell, with economics program coordination by Hurwicz) was largely devoted to problems of mathematical economics, both theoretical and applied. In some respects, its modus operandi was reminiscent of Cowles Commission days. Some of the more than 150 participants (among them Stanley Reiter) spent the whole year, others attended one or more of the ten workshops on specific topics. Its outstanding characteristic was the communication and cooperation between mathematicians and economists. The visible products, in the form of discussion papers, are continuing to appear.

Section 4 Footnotes

¹It was indeed his Ph.D. thesis, the degree being awarded at Columbia with a committee consisting of Albert Hart, Chairman, Abram Bergson, Ernest Nagel and George Stigler.

²In different illustrations social states were visualized somewhat differently - e.g., candidates in an election, distributions of income or bundles of goods in an economy, or more comprehensive descriptions of each individual's social and economic circumstances.

³These are from Arrow (1963). The 1951 conditions were modified to simplify interpretations and to take account of an error in the 1951 theorem shown by Blau (1957).

⁴In the same conversation I asked Savage why he didn't develop his ideas in this area further. His reply, "It's too much like playing God."

⁵Von Neumann's earlier work (1928) contained the central idea but few economists knew of it so the impact on economic thinking came after 1944.

⁶The first three had been included in Arrow's review. Hurwicz's criterion was later generalized by Marschak and Radner (1954) to include all increasing functions of the minimum and maximum of utility associated with each possible decision.

⁷This description corresponds to the normal or strategic form of a game. In the extended form the decision would be decomposed into a sequence sub-decisions corresponding to actions that must be taken at particular times or upon receiving partial bits of information about the state. One of Von Neumann's and Morgenstern's most useful achievements was observing that general properties of games in extended form could effectively be studied in normal form.

⁸In Theory of Games and subsequent statistical literature outcome is evaluated in loss to the individual. In most of economics the outcome is evaluated in utility. When utility is used the term "maximin" would be more appropriate. However "minimax" seems firmly grounded in the literature.

⁹An extension of Chernoff's analysis by Milnor (1954) can also be used to support this pessimism.

¹⁰Robert Strotz, Professor of Economics at Northwestern University, frequently attended Cowles staff meetings and seminars. He became a Commission consultant in 1951 and Managing Editor of *Econometrica* in 1954.

¹¹Milton Friedman was Professor of Economics and L.J. Savage Professor of Statistics at the University of Chicago. Both attended occasional Cowles staff meetings and seminars and their classes were sometimes visited by members of the Commission.

¹²See Manne and Markowitz (1963), McGuire and Radner (1972), Arrow and Hurwicz (1977).

¹³Abba Lerner was Professor of Economics at Roosevelt College in downtown Chicago, 1945 to 1956. He spoke at several Commission Seminars and frequently participated in others.

¹⁴In this literature states are conceived as outcomes, the domain of individual preferences. In much of the individual decision literature, states are circumstances of the decision maker's environment. In the latter sphere states and individual actions combine to determine outcomes.

5. Further Observations

It seems unnecessary to try to write an overall evaluation of the Cowles Commission contribution. This has already been provided by the profession. For example, the 33 people who held research associateships for a year or more in the 1939-55 period have been awarded five Nobel Prizes (Arrow, Koopmans, Simon, Klein, Debreu) and have been elected to eleven memberships in the National Academy of Sciences and to twenty-two presidencies of major professional associations.

A number of desk-top formulations of the period have grown to major economic industries or schools of thought. Several have had deep influence in other social sciences or in statistics. This section provides brief notes (far from comprehensive) on later extensions, adaptations, criticisms, and alternatives.

5.1 Econometrics

The Cowles Commission contribution to econometrics has two main elements. One is the exposition and advocacy of probability models. The other is the development of simultaneous equation models to a useable stage. Subsequent econometric theory and most of econometric practice has employed the probability approach. A large sector of both theory and practice has been in a framework of simultaneous equation models.

A long recognized difficulty in applying simultaneous equations procedures is that a rather narrow *a priori* statistical specification has been necessary to make tractable statistical analysis available. This has pressed econometricians to incorporate doubtful assumptions in their models. Possibilities for misleading results due to misspecifications must therefore be recognized. Such possibilities are present in all statistical applications, not just econometrics. However, in some areas, experience, judgement and theoretical analyses give investigators reason to believe that certain types of misspecification are not seriously misleading and that statistical results are good guides to practical decisions.

In agricultural field-plots for example, common statistical analyses are derived under the assumptions that yields are normally distributed and that treatments specified in the experimental design have been applied exactly. Both are clearly false. A normally distributed yield would have a positive probability of being negative, treatments are applied by humans. However, experience, judgement and calculations of possible effects of misspecification under reasonable assumptions give grounds for high confidence when using the results.

Econometrics has not reached a comparable stage. Some observers doubt that it is possible. Econometricians still

make doubtful assumptions with only guesses about possible consequences. Useful calculations of specification error and bias have been made but these in turn rest on doubtful assumptions. Experience, both formal model testing and comparison of econometric results with knowledge from other sources, has not generally been encouraging.

However, the scope for model builders has steadily widened with further development of econometric theory, with larger and improved data sets, and with spectacular growth in computer technology. Experience in the new environment is quite limited. Fair (1984) reviews current circumstances in the light of past experience and presents careful comparative applications of several macroeconomic models of current interest.

Since the early 1950's such work has proceeded at many locations; the Cowles Foundation has been active as well as the Institute for Mathematical Studies in the Social Sciences at Stanford where T.W. Anderson organized an effective group. Many results can be regarded as natural extensions of Cowles Commission initiatives. Two and three stage least squares and generalized identification have already been mentioned. There has also been substantial progress in instrumental variables (see Sargan (1958)); small sample theory (Phillips (1983), Rothenberg (1984)); nonlinear models (Amemiya (1983)); assumptions about disturbances, e.g. autocorrelation, errors in variables, random coefficients; and treatment of expecta-

tions (Lucas (1975), Sargent (1976), Fair (1978)).

Other developments either represent departures from or major additions to the Commission's lines of thought. The principal contributions of this type are in Time Series, Spectral Analysis, and Bayesian Econometrics. Time series methods¹ represent observed sequences of economic variables as drawings from a stochastic process. The relevant process is assumed to belong to a particular class and the observations are used to draw statistical inferences about further properties of the process - importance of various lags, relations between variables, etc. A primary motivation lies in the belief that typical simultaneous equations models contain damaging erroneous assumptions - e.g. excess exogeneity designations, additive disturbances that are serially independent or have simple autoregressive patterns. Assumptions about a stochastic process generating the observations are regarded as less restrictive and more likely to permit good approximations to the behavior of an economy or sector. If good approximation is established for a process or fairly specific class of prospects, the results have direct bearing on important economic questions and also furnish guides to the choice of models for further exploration. These could be time series models, simultaneous equations models, or some other type. Application of a leading time series technique, vector autoregression, appears in a

classic paper by Sims (1980). It also includes a forceful statement of the case for looking at alternatives to the versions of simultaneous equations that have emerged so far.

Another alternative, spectral analysis, views a sequence of values of a vector of economic variables as a drawing from a stochastic process each component of which is an infinite sum of sinusoidal functions, each with a fixed frequency and random amplitude. Under special assumptions, one can estimate or test various interesting properties of the process. Applications have been mainly to seasonal variation, searches for evidence of cycles, and leads or lags of related series. See Dhrymes (1970), Chapters 9-12.

In principle, Bayesian econometrics offers a direct solution to the problem of having to make doubtful specifications. The analyst is invited to formulate beliefs about unknown aspects, usually values of numerical parameters, of a model in terms of personal probabilities. These beliefs are combined with real-world observations in a fashion prescribed by Bayes Theorem and supported by axiom systems (Fishburn (1982)) which have wide appeal for economists and other scientists. In parametric form, let θ be a specific value for the vector of unknown parameters and $p(\theta)$ be the analyst's personal density or mass function prior to receiving observations to be analyzed. Let $p(x|\theta)$ be the density for possible observations x given the parameter value. If x_0 is observed then

$$p(\theta|x_0) = \frac{p(x_0|\theta)p(\theta)}{p(x_0)}$$

where $p(x_0) = \int_{\Theta} p(x_0|\theta)p(\theta)d\theta$ and Θ is the space of possible parameter values. $p(\theta|x_0)$ is called the posterior distribution of parameter values. For an analyst who accepts the reasoning embodied in Bayesian axioms, $p(\theta|x_0)$ represents beliefs after observing x_0 and this representation may be applied to whatever decision problems are encountered before still newer information emerges. $p(x|\theta) \theta \in \Theta$ is the traditional statistical or econometric model.

The Bayesian approach can be applied with any of the aforementioned types of models - simultaneous equation, time series, spectral. The principle is clear but implementation is usually difficult and concrete procedures have been worked out for only certain combinations of models and prior distributions, see Zellner (1971), Dréze and Richard (1983). These limit the analyst's ability to effectively enter all of his beliefs. However, more scope is available than with non-Bayesian approaches and it may be expected to increase with further research. Of course specification error and bias can still occur if the model $p(x|\theta) \theta \in \Theta$ is badly formulated.

In brief summary, no approach to econometric problems has become firmly established in the sense of justifying high confidence that the statistical results can be directly

applied to practical economic problems. Practical forecasters and decision makers typically revise statistical results before taking action and draw heavily on information that has not been formally analyzed. It is generally agreed that the discipline of building models for empirical analysis has sharpened thinking on many issues and that empirical results have frequently been useful ingredients in weighing real-world alternatives. Fortunately, imaginative inquiry continues on all of these fronts. Cowles Commission initiatives have played a leading role for four decades and, as subsequently extended, still furnish a promising avenue.

From the mid-fifties there has been considerable interchange between econometricians and quantitative analysts in other social sciences, primarily sociologists and psychologists. See Goldberger (1972), Goldberger and Duncan (1973). Sociologists had used path analysis, an approach developed by Sewall Wright (1925, 1934) in the early 1920's. Loosely, one could say that path analysis used simultaneous equation models without explicit probability assumptions. Problems of identification, exogeneity, and joint dependence were clearly recognized but not formulated sufficiently well to permit extensive study of their implications. Psychologists had developed factor analysis. See Lawley and Maxwell (1963). The basic model can be regarded as an error in variables model in which the independent variables are completely unobservable instead of being observable with error. This,

of course, eliminates possibilities of interesting statistical inference unless compensating assumptions are made and these distinguish various versions of factor analysis. A common version can be analyzed by principal components.

Thus the methods in use were closely intertwined logically but practical interchange has proceeded slowly because only a few practitioners were well acquainted with the several lines of development. In the interchange that did take place, other scientists seem to have contributed realization of the need to analyze nonobservables while economists contributed sharper formulation of concepts and development of additional statistical procedures. Simon's characterization of causality has had substantial appeal for social scientists making econometric models and logic more plausible and acceptable, Blalock (1971). Derivations of factor analysis methods were presented by Anderson and Rubin (1956).

5.2 Mathematical Economics

The incorporation of new and previously neglected mathematical methods into economic analysis of the 1950's led to a surge of extended inquiry in the 1960's. Results from both periods are presented in the three volume Handbook of Mathematical Economics edited by Arrow and Intriligator, 1981, 1982, 1985. The Cowles Commission and Foundation played leading roles but the research was not as distinctively Cowles as the probability approach and simultaneous equations in econometrics. Many of the topics studied - e.g. organization, information, public goods, incentives, social choice - have clear relevance to other social sciences. This has induced some useful exchanges and it is hopefully conjectured that mathematical analysis will increasingly prove a unifying force and a stimulus to interdisciplinary study. Still pursued at a highly abstract level, works in these areas are steadily being generalized (uncertainty, nonconvexity, dynamics) and are incorporating features that can be recognized as pertaining to real world problems, e.g., see Laffont (1979). Perhaps applied branches will presently emerge.

The Cowles-related study with widest implications outside economics is Arrow's (1951, 1963) treatise on social choice. It continues and strengthens the long tradition of association between philosophy and economics. It, and the thinking it inspired, have also enriched other social sciences. See Sen (1970, 1985), Hook (1967), Gottinger and

Leinfellner (1978), Rawls (1971).

As noted in Sections 3 and 4 above, this general complex received vital impulses from Von-Neumann and Morgenstern's Theory of Games, 1944, and from the Cowles conference on activity analysis in 1949. The activity analysis conference was also a considerable boost for work in management science.

At Cowles, Markowitz's (1952b, 1959) formulation of efficient decisions under uncertainty as those which minimize the variance of outcome for any attainable expected value found early and widespread application. It has steadily been replaced by maximization of expected utility, but is still a handy way to visualize investment decision problems and is probably a good approximation to expected utility analysis in many cases.

The Cowles Foundation remains a vital center for mathematical economics as well as econometrics. Seminal developments and some of the many linkages to Cowles Commission projects are reviewed in Debreu's 1983 presentation at the 50th Anniversary Conference on Cowles Research held in New Haven. Of particular importance in directing current thinking were - (i) the association of the core of an economy with Edgeworth's contract curve, Shubik (1959) and its generalization by Scarf (1962); (ii) algorithms for computing general economic equilibria, Scarf and Hansen (1973); (iii) stability of equilibrium, Arrow and Hurwicz (1958), Scarf (1960); (iv) discounting and impatience in

dynamic utility theory, Koopmans (1960), Diamond, Koopmans and Williamson (1964), Brown and Lewis (1981); and non-standard analysis, Brown and Robinson (1972).

In addition to the Foundation activities, strong programs in mathematical economics have been established at several universities and additional specialized centers have emerged, several with Cowles antecedents. The Interdisciplinary Colloquium on Mathematics in the Behavioral Sciences was started at UCLA in 1960. It is now called the Jacob Marschak Interdisciplinary Colloquium ... in honor of the founder and is now headed by Michael Intriligator.

The Institute for Mathematical Studies in Social Sciences at Stanford has been directed by Arrow and Kurz. Reiter founded, 1972, and still directs the Center for Mathematical Studies in Economics and Management at Northwestern University. Jacques Dréze, a guest at the Commission in the summer of 1954, is Research Director of the Center for Operations Research and Econometrics, Universite Catholique de Louvain, Belgium.

Perhaps some reasons for the extraordinary productivity of the Cowles Commission in the period are apparent in the introduction to this article. Others appear in biographies of the participants and in memorials presented at the fiftieth anniversary of Cowles research (Debreu, Malinvaud, Solow, Arrow, 1983). Further inquiry into the explanations would be an interesting study in micro organizational behavior.

Section 5 Footnotes

¹ See Granger and Watson (1984) and Geweke (1984). Strictly speaking, simultaneous equations methods applied to variations over time are time series methods. However, as currently used in econometrics, the term denotes certain methods originating in engineering which have been adapted and extended for analysis of economic data.

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