# Cowles in the History of Economic Thought

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## Introduction

The topic of this paper immediately raises a serious methodological question: In what sense can we isolate the contribution of any individual or institution in the development of economic analysis? This is but one example of a fundamental logical problem that applies to the study of all history, that is, the difficulty of the counterfactual. For when you ask, "What is the influence of A (an event, an individual, an idea) on subsequent history?," you mean to ask what would have happened had A not been there. There is no immediately apparent way to proceed to answer that question. Every now and then historians debate the meaning of interpretation; in recent years the so-called new economic history has been filled with controversy over just such issues.

Suppose the Cowles Commission and Foundation had not existed; what would be the difference in the present state of economic analysis? This is the ideal question; but it is clearly unanswerable. Cowles is not and was not a group isolated from the mainstream of economics, and its contributions are today inextricably mingled with other currents. Influences flowed into it from the worlds of economics and statistics, or at least from selected parts of them, and in turn ideas and achievements circulated from Cowles to the common pools of economic knowledge.

In trying to identify the importance of Cowles, we are not entirely bereft of meaningful data. What people at Cowles did at anyone moment is identifiable. We have the papers they published; we can find out whether the concepts in them had their genesis at Cowles or at some earlier intellectual abode of the author. This sort of study has its place and may be most of what is achievable. But no research institute is an island entire of itself (It would not be appropriate to continue the quotation; it should apply to "joyous spells as well as holy knells.")

Cowles indeed has been an institution with a frequently changing population. The average stay of a scholar has been only a few years, though there is of course a whole distribution of residence times. I was at Cowles for a little over two years, Gerard Debreu spent eleven years, and Tjalling Koopmans forty-one years. Individual scholars come from elsewhere, bringing something to Cowles, and they leave carrying a bit of the Cowles heritage with them. The mobility that is optimal for extending the influence of Cowles certainly makes it difficult to measure Cowles's importance.

Even apart from mobility of scholars, there is the flow of ideas and concepts to and from an institution like Cowles. The topics studied at Cowles originated elsewhere. Some were endogenous to economics and emerged from previous discussions in the economics literature. General equilibrium theory, for example, might be viewed as the result of some economists' attempts to think about what exists in the literature and to improve on it.

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Other topics originated outside economics. A good example is the interest in nuclear energy. This first appeared during Cowles's Chicago period in the project organized by Sam Schurr and Jacob Marschak, which was the first serious attempt to study the economics of nuclear energy. I do not know how much it influenced policy, but it was certainly the first analysis of the subject that made any sense. It is hard now to believe the statements made at that time by leading thinkers, such as the then president of the University of Chicago, that with "atomic energy" (as it was then called), no one would have to work more than an hour or two a day, so that the really imminent problem was how to handle leisure! It took a relatively straightforward analysis by Schurr, Marschak, Herbert Simon, and others to demonstrate that under the most optimistic premises the effects on the economy were marginal. With the recurrence of interest in energy economics, the work of Tjalling Koopmans and William Nordhaus and other work on closely related environmental issues have been significant on Cowles's recent agenda.

Having seen two examples of external influences on Cowles's research, one from within economics and one from outside, one might ask whether Cowles itself opened any brand new fields of inquiry. Obviously, there are problems of definition and classification in such a question, but the answer has to be, probably no. It IS not very clear, however, whether there are many new fields of inquiry in economics at all. When one reads the historical background, economics appears to be a remarkably conservative field. Today, for example, we have animated arguments between the rational expectations school and neo-Keynesianism or Tobinism or whatever label is to be attached to the current versions of disequilibrium economics. But however they are labeled, these are the same questions that Malthus and Ricardo debated.

I am not suggesting that there have not been great improvements. But although the analyses have changed greatly, the questions remain relatively constant. In fact, at few times in the history of economic thought have there been radical innovations. When they occur, they tend to be subverted after a period of time and brought back to the mainstream. Take the striking case of Keynes's general theory. As soon as scholars at Cowles and elsewhere began to work on it, they developed the theory in terms of individual rational behavior. Keynes's bold severing of the connection with rational behavior was undermined by the intellectual need to understand behavior, which we interpret as explaining it in rational terms. Indeed, this search did supply new concepts, firmer foundations, and more empirically correct interpretations, especially in the explanation of consumption.

With this background of caution about the difficulty of my task, I want to turn to a more specific discussion of Cowles's place in the history of economic thought. I will talk of precursors and successors. It is easier to discuss the former than the latter for when a concept is really successful, it spreads everywhere. I must warn you, then, of some bias in the following accounts towards identifying the influences on Cowles as opposed to measuring the impact of Cowles's scholars on economics.

The research undertaken at Cowles has addressed a great many topics, of which I will examine only four. These are key issues, and they also serve to illustrate different relations between Cowles and the general history of thought. The four topics are: the estimation of complete models of the economy, the area of programming and general equilibrium theory (although one might question its unity), the economics of uncertainty and information, and the field of intertemporal choice (of

the first importance, although the number of publications is small). I shall treat the first two in some detail, the latter two more briefly. We shall see that for some topics Cowles as an institution played a unique role in the profession as Cowles was, for at least some period, essentially the universe in which the discourse took place. In work on the others, Cowles played an important part but always in a two-way interaction with other scholars in the economics community.

## The Estimation of Complete Models of the Economy

The first topic shows Cowles in its most distinctive role, with the clearest separation from the outside world. Although this work was not started at Cowles, there was a period of four or five years when essentially all the relevant work on both the theory and the practice of estimating large econometric models was done there. We may compare the development of this topic to that of a river valley. A number of streams come together into a single river, which later branches out. But there is some length in which all the activity flows in a single channel. In estimating complete models of the economy, that channel was Cowles. That exclusive role does not characterize, I believe, any other subject of large-scale Cowles activity.

There are two central aspects to the estimation of complete models. One is the idea that a meaningful economic model should be estimated using real-world data, and the second is that the model should satisfy the logical need for a complete system. The second question raises a methodologically interesting problem of ascribing influence — in this case, especially to the work of Jan Tinbergen — as we shall see. One possible point of view is that the need for a complete system is so obvious that no one can be given credit for recognizing it. If there are a number of variables, no one can be predicted except by having a complete system. Of course, systems may decompose, so that a complete system may have a smaller self-contained system embedded within it. For example, consider classical economics as exemplified by Ricardo. The central model had a complete system in which prices were the only variables. The classical economists therefore saw no need to discuss quantities and, in particular, they did not recognize as elementary a concept as a demand function.

Had they thought about it, Ricardo and others would have recognized that even if they were right in asserting that prices could be determined in a complete system not involving quantities, there is a larger complete system in which quantities can also be determined. Indeed, John Stuart Mill did take this additional step, by adding demand functions, although he was not careful enough to ensure consistency in the larger system.

The very large literature on business cycles before 1930 contained analyses of many single relations, specifying, for example, consumption as a function of other variables, or prices and output as functions of money supply. To someone like Irving Fisher, the need for a complete system was so clear that it was given little explicit attention. But many authors did not have the idea of a complete system firmly in mind, for they drew inferences about the existence of cycles from single relations.

There is a very interesting interchange between Ragnar Frisch and John Maurice Clark in the *Journal of Political Economy* for 1931 and 1932. Clark, who may be largely forgotten today but was a major figure of that time, had been one of those who advanced the acceleration principle as

an explanation of cyclical fluctuations. Frisch argued forcibly and persuasively that this inference could not be drawn; the existence of cyclical fluctuations emerges from the complete system. Frisch's paper was essentially a nonmathematical version of his classic, "Propagation Problems and Impulse Problems in Dynamic Economics," which appeared the next year in the Festschrift for Gustav Cassel. This presents, as far as I know, the first specified complete dynamic system.

As far as I can ascertain, the first complete model to be estimated was Tinbergen's for the Netherlands economy, published in 1937. Although the model had been formulated before the publication of Keynes's *General Theory*, it was essentially a Keynesian model. While there were some twenty equations in all, the key equation was one in which Tinbergen set consumption equal to wages plus a fixed share of profits. He had no difficulty understanding all the properties of complete systems, including the role of identities, and he introduced much of the terminology we use to this day.

Tinbergen's statistical tool was ordinary regression analysis. In this he was typical of the emerging econometric school; their statistical outlook derived from the English school and most especially from R.A. Fisher. What was implicit even in the work of earlier statisticians, such as Karl Pearson, was explicit in Fisher: the necessity of formulating an explicit statistical model of the phenomena being studied to derive the statistics needed to estimate the model parameters. To Fisher, this meant using the method of maximum likelihood, by which he derived regression analysis, Student's t-test, the analysis of variance, and the many other offspring of his fertile mind.

It was natural for econometricians to adopt the tool of regression analysis when trying to estimate relations among variables. That tool had already been used by Gauss and Legendre to smooth astronomical and geodetic observations, and it had been given new life (and the name) by biometricians from Francis Galton through Karl Pearson and others. Tinbergen therefore used regression analysis as the natural tool. He was, candidly, not very reflective on the choice of the dependent variable. He was, however, very concerned with the structural significance of the equations being fitted, and he was very insistent that each equation represent what he called a direct relationship. A variable, such as consumption, should be related only to its proximate causes as suggested by economic theory. Thus consumption was to be related to its direct cause, income. Tinbergen did no (conceptualize the modeling enterprise in terms of simultaneous equations; he did not ask whether the relation he found really represented the determination of income by consumption. Nevertheless, his choice of dependent variable certainly reflected a common-sense viewpoint and could not be described as completely arbitrary.

It was Ragnar Frisch who was more specifically concerned with the statistical problems that arise when the observed variables are determined by a complete system. His model has been characterized as a descriptive model rather than a specification to which inferential procedures could be applied. It would be more accurate to say that the model was specified only in a rough-and-ready way. He assumed a nearly exact relation among unobserved variates, each of which is observed with error, and he made some quite specific statistical assumptions; for example, the errors in variables were assumed to be independent of each other and of the systematic parts of the variables. But his presentation was unclear. He did not use his assumptions systematically to derive estimates or tests of hypotheses, and he did not discuss the estimation of the complete system but only of single relations.

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Tjalling Koopmans, in his 1936 doctoral dissertation, presented a much more precise model in which he combined Frisch's ideas with regression analysis. There were shocks, but the variables were also subject to error. If the covariance matrix of the errors was known ( in particular, if the errors of observation of different variables were independent and the variances of the errors known), then estimates of the regression coefficients could be found. If the errors were independent but their variances unknown, the estimates could be shown to lie within a generalized triangle, but nothing further could be inferred.

As the theoretical modeling efforts developed and grew more sophisticated, the depression of the 1930s had its impact on the choice of problems that those tools should be used to study. The Depression led, not surprisingly, to the belief among economists that unemployment was a serious problem. Economists had not yet arrived at the doctrine some currently espouse that all unemployment is voluntary, the result of errors and misperceptions. In response to the observed crisis, the League of Nations mounted a major study in two parts. Gottfried Haberler wrote an excellent critical survey of underinvestment, underconsumption, monetary investment, and other then-current theories of the business cycle. Much of this literature, as I have suggested before, advanced hypotheses about single equations rather than attempting complete explanations. Jan Tinbergen was commissioned to develop statistical analyses of economic fluctuations, which eventually emerged as two volumes.

In principle, Tinbergen was to test the alternative hypotheses studied by Haberler. The execution did not fully satisfy this criterion; many of the hypotheses were not testable, and Tinbergen ignored others. On the other hand, Tinbergen tried to do more. He responded to Frisch's concerns. The margins of the book are filled with bunch-maps that attempt to test Frisch's concerns about the reliability of the statistical fits when there are errors in the observed variables.

How influential was Tinbergen's work, in particular on the Cowles Commission in its Chicago period? One point of view might be that what Tinbergen did was so obvious that it cannot be ascribed any independent significance. All he did was to assemble a number of relations suggested by the literature, fit them to the best data he could find, and use the complete system to analyze, for example, the effects of alternative policies. The relevant Cowles literature contains very few references to Tinbergen. In fact, that literature contains very few references at all, and only cursory ones at that, to the work of anyone outside the Cowles circle. The reason is clear. There was such a discontinuity in both statistical methodology and model building that external references would be only of historical curiosity. The sheer volume of the mathematical work and the rigor and precision of the structure in the Cowles approach dominated the choice of citations. The style was derived from mathematics: citations are for reference to something used, not for historical acknowledgment.

But it would be a mistake to infer that Tinbergen's work was not influential, in spite of the lack of references to it. It created the prototype of the next step forward; it was the work that had to be carried out in better form. To speak of myself for a moment, one of my first attempts at a doctoral dissertation was a redoing of the Tinbergen model. It was foolish; I had no idea of the amount of work involved. But I made notes as to the improvement of this equation and that. My guess is that improvement of Tinbergen's work was a widespread dream; it was at Cowles that it was achieved.

There was a significant conference called at Cambridge, England, in 1938 to discuss the draft of Tinbergen's League of Nations study. Ragnar Frisch was not actually present at the conference, but he contributed his famous memorandum on statistical versus theoretical relations in economic macrodynamics. The emphasis was very much, as might be expected, on dynamic relationships. Although the paper was meant to have implications for the interpretation of statistical relations, the core of the piece concerned deterministic systems. His argument followed these lines: Suppose the observed data are in fact the solution of a simultaneous system of linear difference equations. The solution is then a combination of exponential and trigonometric functions. Suppose further that a lag structure of fixed length is specified for one of the difference equations. The solution has to satisfy this equation. We must necessarily be able to find one equation with the specified number of lags such that the observed variables satisfy it. Frisch asked whether another equation of the same form might also be satisfied by the solution. If not, the original equation was called a coflux equation. If the solution did satisfy another equation of the same lag form, it was called a superflux relation. (This terminology was born and died with this paper.)

Frisch made the important point that only the coflux relations can be estimated. In a deterministic system, this will be obvious. When random terms are added, the lack of uniqueness will be less obvious. But dividing zero by zero, which is clearly impossible, will be replaced by dividing one random element with mean zero by another; the impossibility will not be obvious, but the ratio will have a very broad distribution.

The historical significance of Frisch's paper has to be judged with care. History in general, and history of thought in particular, is always written from the perspective of hindsight. The historian looks at today's ideas and asks whether and how clearly earlier scholars anticipated them. There are always dangers of either under- or overinterpretation. One can elevate a sneeze into a deep anticipation or, if too precise and fussy, always find the earlier statement to be unclear and obscure. My reading of Frisch's paper is that he was indeed groping for the concept of identification but only identification by specification of lag structure. He did introduce a very important point as to the aim of empirical study; what we want to estimate are structural or, as he called them, "autonomous" relations. To be sure, the idea had been implicit earlier. In particular, Tinbergen's emphasis on direct relations had the same purpose, as he pointed out in his reply to Frisch. Frisch's fundamental point, that the only autonomous relations are the coflux relations, contains the essence of the identifiability concept.

This is not the first time that identification appears in the literature. Elmer Working's much-cited paper of 1927 raised the same question about the statistical estimation of demand curves. But Frisch's analysis was certainly placed in a more general context. The next step was the famous doctoral dissertation of Trygve Haavelmo. Since Haavelmo was Frisch's student, there was certainly some interaction between them. From later references, though, it is clear that Frisch was not entirely pleased, since Haavelmo shifted the basis of discussion to a pure shock model. Haavelmo put forth the idea of finding the maximum likelihood estimates of simultaneous equations. Some preliminary ideas already appeared in a paper of his in 1940, which discussed the problem of testing business cycle theories by examining the observed relationships and so was very closely related to Frisch's 1938 memorandum.

Haavelmo developed his ideas further in a brief paper in 1943; his full dissertation was published in 1944 as a supplement to *Econometrica*. The identification concept is still cloudy there but it is more clearly adumbrated. An accompanying paper by Henry B. Mann and Abraham Wald is basically just a proof of the consistency of regression when the independent variables are lagged endogenous variables and the disturbances are serially uncorrelated. (I say "just," but I do not mean to imply that the analysis was not very difficult indeed.) At the end of the Mann–Wald paper, there is a discussion of the transformation of what amount to estimates of the reduced form back into estimates of the structural parameters, and the question of uniqueness arises.

It was therefore a major step for Tjalling Koopmans and his associates to state once and for all what the identification problem is and to give the order and rank criteria for identification in linear systems. Haavelmo, building on Frisch and Tinbergen, had stated a program; Koopmans, Herman Rubin, R.B. Leipnik, Theodore W. Anderson, Jr., Leonid Hurwicz, and others carried out the program as far as statistical methodology goes. At the same time, Lawrence R. Klein pursued the Tinbergen program according to the new statistical methods and their implications and according to the macroeconomic concepts adapted from Keynes but already mixed with microeconomic theory.

There is one curious pattern in the development of simultaneous-equations estimation that also occurred in the development of general equilibrium theory and linear programming: a tendency to move from the dynamic to the static. To a considerable extent in Tinbergen, and almost exclusively in Frisch, the explanatory variables were lagged endogenous variables. The Mann–Wald proof of consistency was confined to the same case. It took some time for economists to realize that the arguments were applicable if some of the predetermined variables were exogenous. Indeed, the difficulties of the consistency proof arise mostly from the lagged endogenous variables, and the extension is relatively simple, though not entirely trivial. Similarly, the identification criteria depend on the specification of the predetermined variables, whether they are exogenous or lagged endogenous. The dynamic elements that were prominent in the motivation of the research turned out to be secondary in the final logical structure.

**Programming and General Equilibrium Theory**

In the second major field discussed here, programming and general equilibrium theory, Cowles played an influential role, but it was never the sole channel of intellectual flow. The concept of programming originated outside of economics. There was, however, a relevant history in economics — namely, in the fixed — coefficients model of production-that was not fully exploited. The fixed-coefficients model is very traditional in economics, though even Ricardo had examples of alternative methods of production. Later in the 19th century, the idea of alternative methods of production in the form of the smooth production function was developed by Stuart Wood, John Bates Clark, Léon Walras (explicitly in the later editions of the *Eléments* but clearly suggested even in the first edition of 1874), and Philip Wicksteed. But there were always some economists who held out against the possibilities of complete substitution. Vilfredo Pareto always had great reservations, and Nicholas Georgescu-Roegen in the 1930s was following Pareto by writing about "limitational factors"; instead of smooth isoquants, there could be barriers beyond which substitution was not possible at any level, e.g., a minimum amount of pig iron in the production of steel.

John von Neumann, in a famous paper presented to the Princeton Mathematics Club in 1932, though not published until 1937, had a perfectly clear and general activity analysis model. There were finitely many activities, each with fixed coefficients, but alternative activities for producing the same goods. Activities could even have multiple outputs. What I suggested this idea to him is very unclear. The only economics he seems to have read, to judge from his references, is Gustav Cassel's *Theory of Social Economy*. But Cassel's formulation has only fixed coefficients of production and indeed in a very primitive way; it does not even allow for circular flows. Perhaps the activity analysis formulation of production was simply obvious to a genius like von Neumann.

Later, and independently of both the economics tradition and von Neumann, Marshall Wood and George Dantzig in the Air Force Controller's office, operating under the practical impact of wartime planning, were concerned with time-phased programs; again they formulated the model in terms of alternative linear activities, each with fixed coefficients. (Note that Wood and Dantzig started with dynamic models just as von Neumann had; only later did the more general formulations switch to a static framework.)

Linear activity analysis may be a concept like complete systems; if one thinks hard enough about the problem, one is bound to come to it. There were, after all, two more independent sources, L.V. Kantorovich's work in the Soviet Union, starting about 1939, and Tjalling Koopmans's work on the transportation problem for the Combined Allied Shipping Boards in World War II. Kantorovich gave a characterization of the solution, first for the transportation problem and then for linear programming in general, but not an effective method of solution in general. Koopmans, drawing upon analogies from physics, produced a perfectly constructive solution for the transportation problem.

Perhaps the only common element in the efforts of von Neumann, Wood and Dantzig, Kantorovich, and Koopmans was that each was a mathematician's reaction to practical problems. As we all know, Dantzig's simplex method provided the effective solution for linear programming in general. The reasons for its excellent performance in practice are, however, still somewhat mysterious.

Linear programming and its generalizations were applied very rapidly, in business and in military logistics. Programming methods have also been extensively applied within economics itself, in such areas as economic development and the analysis of specific industries. The great change in computer capabilities at this appropriate moment was crucial, as indeed it was in the development of large econometric models.

In contrast to the multiple origins of linear programming, general equilibrium theory was invented once by Leon Walras. One may ask how anyone could possibly have thought differently. It is indeed just a special case of the need for modeling complete systems. Nevertheless, no one did impose this condition until Walras, and many economists to this day reject it as too complex to serve as a basis for analysis. Walras had a slogan, repeated in different contexts: the system is determinate when the number of equations equals the number of unknowns. It is fortunate for the development of existence theorems for general equilibrium that differential topology was unknown in the early 1950s. If the tools had been available to us, we would simply have written down a few appropriate transversality conditions and then said that Walras was really right all the time.

The possibility of nonexistence was raised during the 1930s in papers by Hans Neisser and Heinrich von Stackelberg, and corresponding attempts to prove existence followed. There is a good account of this period in a paper by E. Roy Weintraub. A crucial suggestion was made by one of my favorite characters, Karl Schlesinger, an amateur economist, a Viennese banker who had received a Ph.D. in economics in 1914 under Böhm-Bawerk. Schlesinger perceived that the difficulties raised by Neisser and by von Stackelberg in Cassel's formulation of general equilibrium theory could be resolved if one added to the usual definitions of equilibrium (equality of supply and demand on all markets for scarce goods) a statement that whether a good is free or scarce is itself an economic matter. More specifically, supply may exceed demand at zero price. Schlesinger felt that a mathematician was needed to prove existence under these assumptions. He approached Karl Menger (not the economist, but his son, a well-known mathematician who later occasionally attended Cowles Commission seminars in Chicago). Menger, in turn, referred Schlesinger to a young unemployed mathematician named Abraham Wald. (Wald was either Romanian or Hungarian, according to one's national outlook. He was born in a town that was Hungarian at the time but later was ceded to Romania. The Hungarians interested in general equilibrium theory like to claim Wald for one of theirs.)

Wald produced a proof of the existence of general equilibrium, which is reviewed in Gerard Debreu's paper in this volume. About twenty years later, under the impact of new work in combinatorial topology and especially its application to John Nash's equilibrium point concept, Lionel McKenzie, Gerard Debreu, and I were independently stimulated to renewed work on existence. Here is one clear case of Robert K. Merton's "multiple discoveries"; the tools were available, the field was ripe, and the existence theorems were going to be proved by someone.

A key step in unifying and diffusing the developments in linear programming and relating them to the theory of general equilibrium was the conference on activity analysis organized by Koopmans and held in 1949. This has been regarded by all those in the field, not only the Cowles group, as a decisive event. The exchange of ideas was crucial, as Dantzig has testified in his reminiscences. The papers at the conference called scholars' attention to each other; they clarified the concepts and laid a firm foundation for future work. The first proof of the validity of the simplex method was among its most important products. For the development of general equilibrium theory, the most important paper was Koopmans's in which he developed the theory of production from linear activity analysis. This synthesized all the previous lines of study — fixed coefficients, circular flows, smooth production functions. It was the first time that the relations between resource limitations and the boundedness of the social production possibility set, on the one hand, and between the convexity of that set and the linearity assumptions about individual activities, on the other, were set forth clearly. These two results were crucial in the proofs of existence.

As the conference symbolized, the strands in the development of programming and related areas crossed the boundary of Cowles, and this pattern persisted in subsequent developments. Existence of equilibrium was studied by McKenzie outside of Cowles, by Debreu at Cowles, and by myself who might be regarded at that stage as half in and half out. If we look at the development of programming, an important step was Philip Wolfe's solution for quadratic programming, where complementarity theory first appeared. A series of developments followed with the formulation of the linear complementarity problem by Dantzig and Richard Cottle and its subsequent use by Carleton Lemke and Richard Howson to find the equilibrium points of bimatrix games. By a

process described in Debreu's paper at this symposium, this led to Herbert Scarf's fundamental contribution of an algorithm for finding the general equilibrium of an economy. The existence theorem had been made genuinely constructive. From then on, the sequence of influences fanned out, both in the development of new algorithms and into the new field of applied general equilibrium theory. John Shoven, John Whalley, and more recently Timothy Kehoe and a number of others have been studying serious practical problems of energy, economic development, taxation, and so forth in a correct and consistent general equilibrium framework.

**Uncertainty and Information**

I have concentrated on two of the four main areas of interaction between Cowles and the general stream of economic thought, the methodology and practice of large econometric models and programming and general equilibrium analysis. I will cover the remaining two topics, uncertainty and information, and intertemporal choice, more briefly.

The history of the economics of uncertainty has been rather episodic. There has been extensive discussion of both foundations and applications. The original paper of Daniel Bernoulli (1738!) not only advanced the expected utility hypothesis but also discussed the demand for insurance. His explanation for the purchase of marine insurance, which is actuarially unfair, was thoroughly modern. It was portfolio diversification; insurance payments were negatively correlated with shipping gains.

Neither Jevons nor Marshall ever discussed portfolio diversification, though it would certainly not have been beyond their mathematical powers. Further, both showed awareness of Bernoulli's paper. The widespread acceptance of ordinalism in the 1930s complicated matters; it was a little hard to discuss maximizing expected utility when the utility function itself had no cardinal significance. Jacob Marschak, before he came to Cowles, made some efforts to construct an ordinal theory of choice under uncertainty. He assumed a preference ordering in the space of parameters of probability distributions-in the simplest case, the space of the mean and the variance. He also considered the possibility that preferences might depend on the skewness (the third moment) of the distribution. From this formulation to the analysis of portfolio selection in general is the shortest of steps, but one not taken by Marschak. He derived only a special case of portfolio selection, the demand for money taken as a certain alternative to risky investment.

Marschak later (1949) explored briefly the implications of an alternative view of behavior, the maximin theory of Abraham Wald, for the demand for money. The postwar period, in which this work was done, was one of intensive discussion of foundations for behavior under uncertainty, an outgrowth of the searches by Jerzy Neyman, Egon S. Pearson, Wald, and Leonard J. Savage for foundations for the practice of statistics and by John von Neumann and Oskar Morgenstern for the behavior of individuals in games. To summarize briefly, there was a phase, initiated by Neyman and Pearson and developed more fully by Wald, in which a distinction was drawn between those uncertainties that were representable by probabilities and those that were not. For decision making when probabilities could not be used to represent uncertainties, Wald's criterion was maximization of the minimum possible gain.

The work of von Neumann and Morgenstern and later of Savage restored the confidence of economists in expected utility theory by showing that the theory could be reinterpreted in ordinalist terms, as reflecting only observed behavior satisfying certain additional rationality postulates appropriate to choice under uncertainty. Marschak (1950) and Herstein and Milnor (1953) gave careful expositions that convinced doubters. Inhibitions about use of the expected utility hypothesis were lifted, and more applied research encouraged.

In particular, the theory of portfolio diversification emerged. Dickson H. Leavens, a member of the Cowles Commission staff, who combined research and administrative roles, used probability theory to demonstrate to a practical audience that diversification, while it would not improve the mean, could greatly reduce the spread. He did not use a specific measure of spread but simply exhibited the distributions in an example. (I am indebted to Harry Markowitz for this reference.)

The modern history of the subject of portfolio theory starts, of course, with Harry Markowitz's work at Cowles. Markowitz did not, in fact, use an expected-utility formulation but rather minimized the variance for given mean. This is a problem in parametric quadratic programming; not only can the problem be formulated, but it is explicitly solvable. Subsequently, Tobin derived the mean-variance trade-off as a special case of expected-utility theory and gave renewed vigor to the derivation of the demand for money from risk aversion. At that point, the floodgates were down, and the literature poured forth at Cowles and elsewhere.

Statistical theory can be regarded as an economics of information. The economic aspect, the trade-off between accuracy and sampling cost, had been given some stress by more practical statisticians, particularly Dodge, Romig, Shewhart, and others associated with the Bell Laboratories, and was made explicit by Wald, especially in connection with the development of sequential analysis. But it was Marschak's papers of 1954 and 1955 that made explicit the role of information in economic behavior and organization. Specifically, he considered the economic problems in the acquisition of information and the role of transmission of information from one individual to another in a cooperative organization. It is to these papers that the subsequent explosion in the economics of information, again both at Cowles and elsewhere, can be traced.

**Intertemporal Choice**

Finally, I want to consider a field that has developed almost exclusively at Cowles, which has had little impact, but which I regard as of great importance: intertemporal choice and the necessity of impatience. Understanding these issues is essential for the development of a satisfactory capital theory, which concerns why people save and why they invest. And the motivation of saving and investment is, in turn, a key problem in any modern economy.

That the rate of return is positive has not only factual but also moral implications, since the legitimacy of income from capital is at stake. A number of 19th century writers, such as Nassau Senior, John Stuart Mill, and Alfred Marshall invoked the grounds of abstinence or waiting to explain the need to reward saving. Jevons followed Bentham in postulating explicitly that future utilities count less than present ones, but his discussion of capital does not seem to make use of this insight.

It was Eugen von Böhm-Bawerk who gave the first systematic account (excessively systematic) of capital theory. He presented three "grounds" for the existence of a positive rate of interest. The first is the diminishing marginal utility of income; if the individual expects to be richer in the future and therefore have a lower marginal utility of income, a positive rate of interest is needed to induce savings. The second reason is pure time preference. The third is the positive marginal productivity of capital, which is of a different logical order than the first two. As was later pointed out, if the first two reasons did not operate, the third would not operate either, since capital would be accumulated to the point where its marginal productivity was zero or even less. It is the first two grounds — the diminishing marginal utility of income and pure time preference — that relate to preference orders over present and future goods.

All subsequent discussions ran in these terms. Irving Fisher was, as always, very clear, but his discussion ran exclusively in terms of two-period models. It is clear that these were regarded as paradigms for models with longer horizons, but he did not squarely confront the problems that arise when the future is much longer (ideally, infinitely longer) than the present. Frank Ramsey seems to be the first to have given an explicit analysis of savings with infinite horizons. But he did not believe in time discounting; he shared with a number of other English economists the idea that discounting is immoral. In his model, then, the pure rate of time preference was zero, and utility was separable over consumption in different time periods. Nevertheless, an optimal path existed.

It is curious that Ramsey's paper on optimal savings and his other economics paper, on optimal taxation, took so long to influence economic analysis. They appeared in a leading journal and were very well written. Their mathematics may have been a little advanced for the day, but even the mathematically sophisticated, such as Harold Hotelling, John R. Hicks, and the early Paul Samuelson do not seem to have been aware of Ramsey's work. Both of these papers were suddenly revived in the 1950s. With regard to the one on optimal savings, Tjalling Koopmans, Christian von Weizsäcker, and others wrote important papers, in which Ramsey's model was reinterpreted for a growing economy, where the steady state of zero marginal productivity of capital was replaced by the golden rule.

These papers on optimal growth did not, however, reexamine the foundations of the theory of intertemporal choice. That task was undertaken separately by Koopmans and continued by Koopmans, Diamond, and Williamson. They raised the general question: Suppose we do not assume additivity over time but instead only posit a general ordering over consumptions streams that go out to infinity. They imposed a number of specific conditions on the ordering, particularly a stationarity condition — that the ordering of programs beginning at any time not depend on the time or upon past consumption — and a continuity condition. Then, as they showed, there must be impatience in the following sense: Start with two consumption programs, and find their utility difference. Then construct two new programs, formed by having the same consumption in the first period and then each of the two previous programs delayed by one time unit. The utility difference between the two new programs must be less than the original utility difference. These papers are, in my judgment, of fundamental importance, but as far as I know they had essentially no follow-up until the paper, again written at Cowles, by Brown and Lewis, who investigated the same issue from a different but related perspective.

One basic issue in all this work is the meaning of continuity in infinite-dimensional spaces. In finite-dimensional commodity spaces, the topology is always Euclidean; any reasonable topology is equivalent to that. In infinite-dimensional spaces, there are different topologies whose plausibility is not so immediately apparent. There is one key assumption, introduced by Koopmans, that of sensitivity, which specifies that consumption in anyone period should matter. This property does not hold for the zero-discount case, in which, effectively, the long-run average is maximized, and no single decision matters. Since intertemporal choice with an infinite horizon is most naturally interpreted as intergenerational choice, using that criterion would permit imposing any sacrifice, no matter how large, on the initial generation if it would lead to an infinite stream of returns, no matter how small. Such a criterion for intergenerational choice is neither moral nor practical.

Hence, if we have both sensitivity and continuity, it must be true that the far distant future cannot count very much. In different ways, this intuition has been formalized by Koopmans and associates and by Brown and Lewis. This is a profoundly important point, recently discussed by some philosophers, especially in the context of disposal of radioactive waste. One view sometimes expressed is that a harm to someone a thousand years hence counts equally with a harm imposed today. Though this formulation puts the nondiscounting view in the most favorable light, I myself believe that the arguments for discounting just given are equally compelling in this case also.

**Concluding Remarks**

In the study of the topics I have covered, the first and fourth are the ones in which Cowles played a unique part; it was, at least for some period, the entire universe in which discourse took place. One of these topics, large econometric models, has had a profound effect on both economic analysis and what perhaps may be termed the engineering side of economics, prediction and policy analysis. Work on the other subject, intertemporal choice theory, has had relatively little application thus far. For the other two topics, programming and general equilibrium theory and the economics of uncertainty and information, Cowles played an important part, but always in two-way interaction with others in the economics community. The channels of influence are subtle and complex, but, however it is analyzed, the Cowles contribution is striking and permanent.

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