

# Some Developments in Economic Theory Since 1940: An Eyewitness Account

Kenneth J. Arrow

Department of Economics, Stanford University, Stanford, California 94305;  
email: arrow@stanford.edu

Annu. Rev. Econ. 2009. 1:1–16

First published online as a Review in Advance on June 8, 2009

The *Annual Review of Economics* is online at [econ.annualreviews.org](http://econ.annualreviews.org)

This article's doi:  
[10.1146/annurev.economics.050708.143405](https://doi.org/10.1146/annurev.economics.050708.143405)

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1941-1383/09/0904-0001\$20.00

## Key Words

econometrics, general equilibrium, uncertainty, dynamics, information

## Abstract

Any psychologist who has studied eyewitness accounts knows first of all how unreliable they are. I therefore submit this informal account of some developments in economic theory without research, just as a set of recollections. It is also not an autobiography, nor a systematic account of my own work. Rather, I consider primarily those developments in economic theory that have had both general interest in the field and special concern for me. For example, as social choice theory is still a specialized field, I am not going to discuss it at all. Further, as it has turned out, I emphasize the developments of technique, although to some extent I refer to some of the underlying visions of the economy to which they are applied. To evaluate my eyewitness testimony, I give the reader some idea of my background and, in particular, the path that led me to be an economist and that has influenced my work and my perception of the development of economics in general. (As my friend Paul David keeps on reminding me and the rest of the world, all development is path-dependent.) I then follow up with four major aspects of economic research in the last 60 years, the period of my scholarly activity. One, econometric methodology and practice, is of such fundamental importance that it cannot go unnoticed, although I played no role in it. With the other three, general equilibrium, dynamic processes, and uncertainty and information, I was more intimately involved.

## 1. SOME PERSONAL BACKGROUND

I do have to say a few words as to how I came to study economics, as this background plays some role in my range of interests, and also something of the state of education in economics at that time. I was fully exposed to the anxieties induced by the Great Depression, which affected my family acutely, and economic security was a strong goal. I had a variety of intellectual interests: mathematics, history, and logic; I found the first dominant and majored in it at the College of the City of New York, but I took quite a few courses in other subjects, even economics. The question was, how could I make a living out of mathematics? Having an intuitive feeling for the importance of flexibility, later a research theme of mine, I found and pursued three lines: high school teacher of mathematics, statistician, and actuary. The first eventually failed simply because there were no vacancies. I took courses in statistics and learned a lot about graphic presentation; there was one course given in the mathematics department by someone who knew little of the subject but knew excellent references. I read on my own and was fascinated by the papers of R.A. Fisher and, above all, those of J. Neyman and E.S. Pearson. I also took some of the examinations of the Actuarial Society, on basic mathematics (not deep but tricky), and somehow read some of the material on subjects like moral hazard and adverse selection, knowledge that turned out to be very useful to myself and to economic analysis many years later. I also had a summer job as an actuarial clerk (sheer accident; I was looking for any job and passed by an insurance company, so I simply walked in and asked if they could use someone), which taught me a good deal about pricing.

When I graduated in 1940, I really had no clear employment, so I decided to go to graduate school to study mathematical statistics. There were in fact few places to study statistics; one was at Columbia, where Harold Hotelling taught. My family could not afford any support, so going to Columbia meant I could live at home. There was no department of statistics and no degree in it, but there was a listing of courses called Statistics. They were given by Hotelling and an assistant paid for, not by Columbia but by the Carnegie Corporation. The “assistant” when I was there was Abraham Wald, an incredible stroke of good fortune. I naively enrolled for graduate study in the Mathematics Department, as the closest match to statistics. When I wanted to apply for a scholarship for the following year, Hotelling made it plain that mathematics departments had no interest in statistics, that his appointment was in Economics, and that the chances were much better there. I changed my department accordingly. When I have told this story to former students, they have been shocked that I had followed economic incentives.

Hotelling did give a course called “mathematical economics,” which I took, in my first term. I was fascinated, although in retrospect it was very limited, being confined to the theories of the firm and of the consumer with many commodities. I became very expert at manipulating bordered Hessians, an ability for which I have found little use. All the important things that Hotelling had done in economics, on exhaustible resources, welfare theorems, or depreciation, I had to learn from my own reading, not from his course.

The statistics courses given by Hotelling and Wald were superb. We were at the frontiers very quickly, to which indeed they were major contributors.

The teaching and organization of the Economics Department were polar opposites. I was in fact exposed to the usual courses and requirements for only one year, 1941–1942. The reader may find it hard to believe, but there was no course in price theory. Its place was taken by a course in the history of economic thought. I do not know exactly how this came

about, but I would guess that it was the influence of the best-known member of the department, Wesley Clair Mitchell. Mitchell was a pioneer in emphasizing the accumulation of data rather than theory of any kind and was the founder of the National Bureau of Economic Research. To him the great economic problem was the presence of business cycles; at that moment of history, this emphasis did not seem misplaced. Mitchell usually taught the course in the history of economic thought, but he was on leave that year. He was replaced by John Maurice Clark, certainly an interesting thinker, an excellent writer, and an incredibly boring lecturer.

The department felt that it had to have price theory as a required subject on the oral comprehensive examination, even if it did not offer a course. To this end, a student was supposed to arrange with a member of the department a list of books and articles to be read and to be examined on. The hot topic in price theory at that time was imperfect competition; I proposed the books of Edward Chamberlin (1933) and Joan Robinson (1933) and some subsequent papers, and they were accepted by Clark. This work was indeed what later became non-zero-sum game theory.

Mitchell ordinarily gave a seminar in business cycles, which, to him, was indeed the most important economic problem. Because he was on leave, the seminar was given by his chief deputy at the National Bureau of Economic Research, Arthur F. Burns (who later, of course, became the Chairman of the Federal Reserve System). As I was primarily interested in statistics and was taking courses in economics just to meet requirements, I felt that this was an opportunity to learn what a very differently oriented tradition had to offer. The National Bureau approach was primarily the accumulation of data with the view that understanding would emerge. There was a statistical methodology, later published by Burns & Mitchell (1946). The method was about as anti-theoretical to that I had learned in my mathematical statistics courses as was conceivable. R.A. Fisher, Hotelling, or Wald would start with a model in which certain parameters were unknown. The aim was to make observations and use them to draw inferences about the unknown parameters. Burns and Mitchell had a description from which, even in principle, it would be impossible to draw any causal inferences; at most, there might be some kind of prediction about leads and lags, and even for that purpose, their method seemed to me to throw away most of the relevant information. When Burns and Mitchell finally published their work, it was subjected to a withering review article by Tjalling Koopmans (1947).

Apart from reading Burns and Mitchell in the manuscript stage, Burns's course centered on the reading of Joseph Schumpeter's *Business Cycles* (1939). The latter attempted to show that the innovation process whose importance he had done so much to emphasize was also responsible for business cycles. Among other implications, he claimed to find three kinds of cycles of various lengths, one nested within the other. Burns was concerned with denying this empirical argument. The implications for growth, which now are taken to be of dominant interest, were given no stress.

Remember, this is a course on business cycles in 1941–1942. Perhaps the reader will think of Sherlock Holmes's calling Inspector Gregory's attention to "the curious incident of the dog in the night-time," in the story "The Silver Blaze." "'The dog did nothing in the night-time.' 'That was the curious incident,' remarked Sherlock Holmes." The dog in question is, of course, Keynes; *The General Theory* (1936) went unmentioned.

I must add that, despite my antipathy to Burns's statistical methodology and his lack of attention to the most important current topic in economic thought relevant to Burns's

chief concern, economic fluctuations, I found him one of the most brilliant and knowledgeable economists I have ever met.

I do not want to leave the impression that graduate education in economics at Columbia at that period was a total loss. The courses in public finance, by Robert Haig and Carl Shoup, were very solid, and Carter Goodrich's course notes for American economic history were gems (his oral examination was the most innovative and educational that I have ever experienced).

The content of graduate education in economics at Columbia was far from typical. In fact, graduate education then differed across universities much more than it does today. Price theory was a serious subject at Harvard (Wassily Leontief) and Chicago (Jacob Viner). Alvin Hansen at Harvard was bringing Keynes's doctrines to the United States and attracting as students the leading macroeconomists of the postwar period. Edward Mason at Harvard was also the leader in industrial organization and again trained many of the next generation. Chicago was already the leader in stressing laissez-faire doctrines, as well as emphasizing the quantity theory of money, and this was well before Milton Friedman and George Stigler joined the faculty.

## 2. ECONOMETRICS

Perhaps no development in economics since World War II has been as important as the steady use of formal statistical models. The Econometric Society had been founded in 1933, largely due to the drive of the great Norwegian economist Ragnar Frisch, with the support of Irving Fisher and Joseph Schumpeter (and the financial support of Alfred R. Cowles III). It was devoted to the development and use of formal statistical models, mathematically expressed, in the analysis of economic data. The first steps had been the use of existing regression methods, applied in economics indeed not long after their applications in evolutionary biology. Although demand and supply curves had been fitted intermittently, a much bolder attempt was that begun by Jan Tinbergen in the 1930s to develop and fit complete systems of equations in order to grasp the nature of business cycles (see Tinbergen 1939).

Hotelling had been President of the Econometric Society and invited me, as a student, to join. *Econometrica* quickly became my main source of information about developments in economics. Paul Samuelson's papers were, of course, very important on the theoretical side, but the developments in statistical method were then equally important to me. It occurred to me that regression analysis had been developed for single equations and that statistical inference for a complete system might have some new points. However, I quickly learned that this point had occurred to others. A number of the European refugee scholars then living in New York had formed a seminar on mathematical economics and econometrics within the framework of the National Bureau of Economic Research; the leader was Jacob Marschak, then a professor at the New School for Social Research. At one meeting in the spring of 1942, a new approach to statistical inference in simultaneous-equations systems was presented by a Norwegian scholar caught in the United States by the German invasion of Norway, Trygve Haavelmo. It was disarmingly presented, as an application of Neyman-Pearson theory to the problem, and I did not understand the full extent to which new concepts were implied. There was present a tall, very thin Dutch econometrician, identified as Tjalling Koopmans, who, alone of those present, seemed to grasp the full novelty. I already knew his name because he had published a paper on the

distribution of the serial correlation coefficient (Koopmans 1942), which Hotelling had regarded as a major unsolved problem, and an important step in time-series analysis. But, more than anyone else, he recognized the implications of Haavelmo's proposals.

Koopmans continued his development of simultaneous-equations estimation along with others stimulated and led by him. His institutional setting was the Cowles Commission for Research in Economics. This research organization, founded by Cowles, the financial supporter of the Econometric Society and an investment manager, had started in Colorado but was housed at the University of Chicago beginning in 1939. In 1943, Marschak was invited to join the department at Chicago and to be Director of the Commission. The next year Koopmans joined him in the department and at the Commission and assembled an extraordinary staff of statisticians and econometricians: Theodore Anderson Jr., Haavelmo, Leonid Hurwicz, Roy Leipnik, and Herman Rubin. Within less than two years, the basic concept of identification was clearly articulated and estimation methods were developed; with further improvements leading to more flexible methods of estimating single equations of a complete system, the results were published in Koopmans (1950). Lawrence Klein joined the Cowles Commission to apply these methods to a complete macroeconomic model (Klein 1950), inaugurating the era of large-scale econometric models.

I was first in touch with the Cowles Commission and learned of its work in 1946, and I joined their staff in 1947, remaining there for a little over two years. Although my interests developed differently, I was immersed in these statistical developments.

To be sure, the use of econometric methods has gone in directions different than those envisaged by the Cowles pioneers. There is little emphasis on estimating complete systems. But the underlying importance of identification and its frequent implementation through the use of instrumental variables are now standard (for a survey, see Angrist & Krueger 2001, which traces pre-Cowles uses, some, but not all, of which were known to and cited in the Cowles literature).

The wide diffusion of econometrics depended not only on methodological innovation, but also on two other factors, the great increase in the collection of available data and the revolution in computing. I have no special knowledge of the first, but I do have one anecdote about the second. In my courses in statistics, Hotelling had expressed his concern about the barrier to the use of statistical methods caused by the difficulty of inverting matrices. He proposed various tricks, but the Gauss-Doolittle method (as it was called), i.e., successive elimination of variables, was the standard. Having had occasion to fit regressions, usually with eight variables as part of my work in the Army when using statistical methods for weather forecasting, I can testify with pain to the difficulty; inverting an  $8 \times 8$  normal matrix took me about eight hours (and, to anticipate the reader's reaction, I performed this task very well). Hotelling had mentioned an idea current at the Bell Laboratories, the research arm of the then-dominant telephone carrier, American Telephone and Telegraph, that, logically, diodes could replace relays and so add and multiply at much higher speeds. During World War II, it became publicly known that an electronic computer, with the acoustic ENIAC, had been built for the U.S. Navy and was being used to compute ballistic tables. Just before the end of my military service, one of the creators of the ENIAC, John Mauchly, came to my office at Langley Field in Virginia. The computation for the Navy was almost complete, and he was looking for new work. Somehow he learned that I might be a good customer. I was awaiting discharge (hostilities had ceased), had no authority to give him a contract, and had no intention of starting

a new project, but I was eager to learn ENIAC's capability. When I described the number of additions, multiplications, and divisions needed, he told me that ENIAC could do my eight-hour job in five minutes. I had learned from my parents always to distrust salesmen, so I called a naval weather officer I knew to check. He confirmed the numbers but added that they applied only when ENIAC was running; it was down 80% of the time. Still, I figured, that meant 25 minutes on the average, a large gain indeed, and I assumed that reliability problems could be solved.

It was not for another decade that the availability of computer time and ease of programming permitted considerable use of computers in econometrics, but thereafter computational time ceased to be a significant factor, not only in estimation but also in deriving the distributions of the statistics.

### 3. GENERAL EQUILIBRIUM

The central question in most of my work in economics has regarded, one way or another, the concept of general competitive equilibrium and its range of applicability, including the limits on that range. Hotelling's course provided the microfoundations, in studying the behavior of firms and of consumers under competitive conditions with many commodities, but the role of the markets in drawing the individual actors together was taken for granted.

When I enrolled as a student in the Economics Department at Columbia (1941), I was assigned a desk in the library stacks near the economics book collection. As is my wont when placed in the neighborhood of books, I immediately started browsing and ran across a work by an economist whose name I had never heard mentioned, J.R. Hicks's *Value and Capital* (1939). As apparently happened to other like-minded economics students (e.g., my good friend, Frank H. Hahn) at that time, it gave me a powerful orientation to economic analysis. It showed how the techniques of static analysis (already familiar to me from Hotelling's course but expressed with more verve and style) could be applied to events unfolding in time. Savings and investment could be analyzed using the same tools. If the reader thinks this is obvious, I recommend that he read the controversies over capital theory, as exemplified by the papers of Frank Knight (1936) and Friedrich von Hayek's book (1941) and try to find out, as I tried then, what in the world were the questions being debated. Hicks had a simple approach: Decisions on commodity consumption and production today are made jointly with consumption and production in the future. Therefore, simply put a time subscript on commodities; use the static formalism with the enlarged commodity space. (To be fair, after reading Hicks, one can understand that Hayek was saying much the same, but I defy anyone to learn that from reading Hayek alone.)

Of course, Hicks's reformulation did not end the difficulties in theories of savings and investment; rather, it enabled one to understand what they were. In brief, as Hicks explained, the problem was the nonexistence of a full set of futures markets. There were a few commodity futures markets, and, more importantly, there were markets for credit. As Hicks emphasized, the nonexistent futures markets were replaced in the calculations of expectations by firms and households, and the resulting supply and demand behavior determined "temporary" equilibrium prices and allocations on the current and existing futures markets. As Hicks showed, one consistent set of expectations was the equilibrium set that would have been obtained if all markets existed (what later became known as "rational expectations").

My enthusiasm for *Value and Capital* did not exclude a critical attitude to many of its details. (Indeed, I regard an uncritical attitude to any work as a sign that it is not very interesting.) On returning from military service, I planned to write a dissertation which would redo *Value and Capital* properly, a very foolish idea. I had two motivations. One was to supply a theoretical model as a basis for econometric estimation.

The other was a strong interest in planning. I would have described myself as a socialist, although one that had a strong belief in the usefulness of markets. Market socialism was a widespread view. Hotelling held it. It had been popularized especially by the works of O. Lange (reprinted in Lipincott 1938) and A.P. Lerner (1946). In the immediate postwar period, the idea of national planning to supplement markets was common in Western Europe, and allocation in effect was treated, in principle, as the solution of a general equilibrium system (although with many simplifications). It appears in retrospect that the planning had little effect (good or bad) on the development of the European economies, but a great deal of intellectual energy was expended (see, e.g., Malinvaud & Bacharach 1967).

There were two particular points I was considering in attempting to improve the Hicks model. One was the role of expectations and, in particular, the importance of uncertainty. Hicks recognized this, but his handling was, as he acknowledged, unsatisfactory. In his treatment, firms or households might be uncertain about future prices, but they acted as if there were a certainty-equivalent price. From what I already understood about behavior under uncertainty, I knew that there were precautionary actions that guarded against uncertainty and that would not be taken under any conditions known for certain (e.g., portfolio diversification), a point already recognized by such economists as Frank Knight, Irving Fisher, and Albert G. Hart (for a survey I wrote at that time, see Arrow 1951a, especially section 2).

The second point was the theory of investment by a multiowner firm. Consider a modern corporation with publicly traded stock; in theory, at least, it has many owners. It is true that they all have the same objective, maximization of the sum of discounted profits. But, in the absence of a full set of futures markets, each one computes the optimal policies according to her expectations of future prices. There is no reason why these expectations could not differ. As a result, the owners (stockholders) might have different views on the optimal policy of the firm. It occurred to me that a natural definition of firm choice would be based on the voting powers of the stockholders. Specifically, investment project A would be preferred to project B if a (share-weighted) majority of the stockholders expected a higher sum of discounted profits from A than from B. It occurred to me that if this is to be a rational theory of the firm, the preference relation so defined had better be transitive (a concept I had learned in an undergraduate course from the great logician Alfred Tarski). It took about 15 minutes of figuring to see that majority voting could easily be intransitive, thus rediscovering what Condorcet had found in 1785, although I did not become aware of this until 1952. At this moment, I regarded this discovery as an obstacle, and put it aside until the problem arose in a different form.

The economic behavior of a multiowner firm, however, was still a problem, and its importance was (independently, of course) stressed much later by Ekern & Wilson (1974). Their paper gave rise to a considerable literature on “stockholder unanimity.” To my mind, the issue has still never been resolved.

On top of these considerations, I somehow (I cannot remember how) became aware that the existence of solutions to the system of equations defining a general competitive

equilibrium had been questioned and that my teacher, Abraham Wald, had given some kind of answer. On returning from military service, I asked him about it. He gave the reply, which retrospectively appears rather curious, that it was a difficult problem. I had learned some German in college, but reading papers in that language was sufficiently difficult that I took his answer as license not to pursue it. What later intrigued both Gerard Debreu and myself, when we discussed the matter, was that Wald, in an expository paper (Wald 1936), announced (section II) an existence theorem for equilibrium with pure exchange, which, although somewhat less general than the one we published, seemed to require the same techniques. Wald referred to the use of “subtle methods of modern mathematics,” a phrase that might indeed refer to identifying general equilibrium with the fixed point of a suitable transformation. The paper in which the proof was to appear was never published because of the Anschluss (the German annexation of Austria in 1938). Wald’s interests shifted to mathematical statistics on his migration to the United States; still, one might have supposed that he would use the occasion of my question to publish the paper or at least to describe it to me.

A large influence was the publication of von Neumann & Morgenstern’s (1944) great work on game theory. The normal form was very enlightening, and the minimax theorem fascinating. But zero-sum games were hardly in the spirit of economics, where gains from trade are at the very core of the theory. “Economic behavior” was embodied, in this work, in the theory of cooperative games, a subject whose very meaning is still none too clear.

But if the actual content of the theory was difficult to apply, the techniques were not. What I learned, reinforced by association with the game theorists at the RAND Corporation, was the usefulness of convex set theory in understanding what economic theory was all about. This was reinforced by the emergence of George Dantzig’s linear programming, and its enthusiastic reception and economic interpretation by Koopmans (see Koopmans 1951). The use of separating hyperplanes enabled me to overcome a discomfort I had with welfare economics; equating marginal rates of substitution did not account for corners, cases where households or firms had zero purchases of some good. Interpreting prices as dual to the household’s and firm’s problem completed the link back to individual optimization theory (Arrow 1951b). That these ideas were spreading widely is attested to by the virtually simultaneous and very similar treatment of welfare economics by Debreu (1951).

The role of game theory in economics and much more changed with the publication of Nash’s (1950) brief paper on equilibrium in noncooperative games. Many economists, with some knowledge of imperfect competition theory, recognized that conceptually he had long been anticipated by Cournot (1838). The paper, however, supplied a firm basis by providing an existence theorem, although, as with the minimax theorem, it depended on introducing a wider view of possible actions, the mixed strategies.

But its greatest immediate impact on many of us was not the substance but the method of proof. We became acquainted with fixed-point theorems, specifically, Kakutani’s version with point-to-set mappings, as a way of proving existence. This is one situation where the significance of the originating paper (in this case, Nash’s) was clear, for it was followed up by a number of scholars, working independently. It struck me fairly soon that Nash’s method of proof must be applicable to proving the existence of general competitive equilibrium. When I got down to systematic work, I constructed a somewhat complicated game whose equilibrium was the same as competitive equilibrium. The “players” were the consumers, a set of “anti-consumers” choosing the marginal utilities of income (to minimize the sum of the consumer’s utility and the product of the budget surplus and the



marginal utility of income), the firms, and a “market player” choosing prices to minimize the value of excess demands. There was a technical problem. The competitive game was played over unbounded strategy domains, but that was not serious. I “proved” the existence of equilibrium and circulated the paper.

I then received a manuscript from Gerard Debreu. His formulation of the economy was virtually identical to mine; it showed the common background, particularly in Koopmans’s work on the production structure in activity analysis, but also a very strong similarity of outlook. His proof was different in that it used a game where the strategy domains of some players (the consumers) depended on the strategic choices of others. It then turned out we had both made the same mistake in that we did not guard against the possibility of a discontinuity in consumer demand as income approached zero. Some extra conditions were needed.

This took time. Others, with less meticulous descriptions of the economy, assumed the continuity that we wanted to derive from other postulates. Lionel McKenzie, equally influenced by Nash, wrote the first published proof of existence (McKenzie 1954), followed shortly by Arrow & Debreu (1954).

During this period, I had been brooding about the incorporation of uncertainty into general equilibrium. I had learned in my undergraduate days that probability distributions were taken over some underlying space, that the element was the individual “state of nature.” Assumptions that distributions were normal or could be characterized by two or three moments seemed not to get to the heart of the matter. While I was still in Chicago (to 1949), Leonard J. Savage was developing his axiomatic characterization, which derived beliefs in the form of probability distributions as part of rational behavior under uncertainty. Again, the primitive idea was that of bets on states of nature. It occurred to me, then, that the natural formulation of behavior under uncertainty was to characterize commodities as indexed by the state of nature in which they are to be used. That is, the elementary contract is to deliver one unit of a specified commodity if a specified state of nature occurs. Any other contract could be regarded as a bundle of these elementary contingent contracts. This was completely analogous to Hicks’s characterization of commodities as indexed by time.

I saw lots of issues to be tackled, but my schedule was altered by an exogenous event. With about three months notice, I was invited to a conference on the economics of risk bearing organized by two scholars from the French nationalized electricity industry (Electricité de France), Pierre Massé and Georges Morlat, to be held in a suburb of Paris. As I also had other research obligations, my paper was very short, taking account only of pure exchange in a single period. Further, the conference was held entirely in French, and the paper (Arrow 1953) was translated for the occasion and published in French. (The original English text was finally published in 1964.) Debreu promptly took up the task of completing the analysis, particularly by incorporating production and extending the time horizon to many periods (Debreu 1959, chapter 7).

Despite my pride in this accomplishment, there was an inadequacy that Koopmans suggested to me at the time. In effect, I was ascribing uncertainty ultimately to exogenous events. The contracts I called for were contingent on the states of nature, which, in turn, determined the general equilibrium. But, he urged, much uncertainty was endogenous to the economic system. Put as I would understand it today, many contingent contracts are traded, but they are contingencies determined by the equilibrium values of prices and quantities. For example, the value of a common stock depends on the profits of the

corporation (actually, on expectations of such profits). This is a function of input and output prices (under competitive conditions), not of exogenous events. Current events certainly suggest the importance of this viewpoint, but this is not the place to expand on it.

To keep this account in reasonable length, I omit discussion of further work in general equilibrium theory. I had the benefit of working with many collaborators, most notable Leonid Hurwicz and Frank H. Hahn. There was a series of papers on the stability of general competitive equilibrium, mostly but not entirely with Hurwicz, reprinted in Arrow & Hurwicz (1977), and a survey of the entire field, with a number of new results (Arrow & Hahn 1971).

#### 4. UNCERTAINTY AND INFORMATION

Statistics is about information and its best use. Along with uncertainty comes the recognition that it may be reduced by the acquisition of information. Indeed, information is precisely the observation of a random variable, so that subsequent behavior should be and can be based on the distribution of the variables of interest *conditional* on the value of the observed variable. This principle has guided much of my work, although it has not given rise to nearly as coherent a body of analysis as general equilibrium theory.

In the summer of 1948, at the RAND Corporation, Meyer A. Girshick (then a staff member there, later Professor of Statistics at Stanford, unfortunately deceased at an early age) became interested in the foundations of the then-new concept of sequential statistical analysis, and drew David Blackwell and me, both summer visitors, into working with him.

Sequential analysis had been one of the great creations of Abraham Wald; developed during World War II, the ideas were developed in his book (Wald 1947), and its optimality properties extended further by Wald & Wolfowitz (1948). However, the general logic was unclear. On study, one could see that there was a recursive element, in that at each stage there was an optimization problem of the same form. We were able to clarify the underlying logic (Arrow et al. 1949). The usefulness of recursive methods became apparent to me for further uses. Richard Bellman (1957) has stated that this paper suggested to him the fundamental notion of dynamic programming.

The concept of general equilibrium under uncertainty, referred to in Section 3, is one example of the influence of probabilistic thinking. The influence of uncertainty on individual behavior had been recognized by economists, as I note above. Interest in the field was sharpened by the revival of the expected-utility theory of behavior under risk. This had, of course, been first advanced by Daniel Bernoulli (1738!), as a resolution of the St. Petersburg paradox, which was circulating among the probability theorists of the early eighteenth century. But Bernoulli showed a further implication of expected-utility theory, that there was a market for actuarially unfair insurance (he considered marine insurance). It is a small step from this observation to an explanation of portfolio diversification.

There seems to have been no genuine application of the expected-utility theory of individual behavior under risk, even after the rise of neoclassical theory gave central importance to utility-based explanations, before the 1940s. Attention was revived by von Neumann & Morgenstern's derivation (1947, pp. 617–32) in their second edition of the expected-utility theory from more primitive and highly appealing assumptions about rational behavior under risk.

There was considerable resistance; I know that I felt that there must be something wrong with the argument. A good part of the argument was that the newer and more mathematical

thinkers were convinced that utility was an ordinal, not a cardinal concept. After a few years of discussion at conferences and in the published literature, it became clear that there was no contradiction between expected-utility theory and the choice-based concepts of preference that led to the ordinalist position. The discussion had, in my view, the effect of making the expected-utility theory well-known and available for application to specific economic issues.

To me, the greatest stimulation came from the work of James Tobin (1958), which used the expected-utility theory to derive liquidity preference (i.e., the demand for money for other than transaction motives). I gave a course on the economics of uncertainty in 1962, and my attempts to clarify Tobin's work led to a more systematic treatment. In particular, I was led to define the concepts of relative and absolute risk aversion and to hypothesize that the former was increasing and the latter decreasing as wealth increased. Having been invited to give the Yrjö Jahnsson lectures in Helsinki in December 1963, I presented, and published, these results there (Arrow 1965, lecture 2). The concepts of relative and absolute risk aversion were developed independently by John W. Pratt (1964), again one of those multiple discoveries that shows how the time was ripe.

The presence of uncertainty has one very important implication, the possibility and importance of information. If there are two random variables that are not independent and if the payoffs to a range of possible actions are affected by one, then observing the other means that one's behavior should rationally be guided by the *conditional* distribution of the variable of interest given the observed variable. This is what statistical inference is all about; this is also the subject matter of Claude Shannon's (1948) theory of communication, which attracted widespread interest.

The stress on the importance of information was more thoroughly emphasized to me by Jacob Marschak ("Jascha," as we all knew him), my boss as Research Director of the Cowles Commission in Chicago, and one who became very interested in the economics of information by individuals and by groups ("teams," in his terminology), for whom information not only existed but was transmitted (see Marschak & Radner 1972 for the theory of teams and Marschak 1959 for the applications of Shannon's information measures to microeconomic behavior). Another source, with the greatest influence of all, has been the introduction of search theory to economic choice, the very important paper of George Stigler (1961). Although I have written a significant number of papers inspired by these ideas, they have had little impact.

However, the idea that information was an important consideration in economic analysis was expressed in a paper on the welfare economics of medical care (Arrow 1963), one that has had considerable influence, not only in the original field of application. This was initiated by a request from the Ford Foundation, at that time a major supporter of economic and other social science research. Victor Fuchs was then an officer of the foundation. He thought it would be interesting to have pairs of studies in each of several fields of interest to public policy, each pair consisting of one study by someone engaged in the field and one by a theorist, to provoke the use of theoretical tools in that area. Fuchs invited me to be the theorist considering the economics of medical care. I have always found it difficult to resist challenges like this, and indeed they have been most stimulating. The insurer did not have as much information about a particular case as the physician, so the former could not hold payments to some minimum, and the patient was not in a position to know how well and how devotedly the physician was handling his case.

My own view at the time (and today) is that this situation of *asymmetric information*, as it came to be called, was met by the creation of social institutions rather different from

the market. In any case, I understood the applicability of the terms I had learned in my brief brushes with the actuarial world, moral hazard and adverse selection, now so widely used in economic analysis.

The concept of asymmetric information has become a dominant analytic tool, to my mind, the most important development of economic theory after 1950. The scope of application has gone considerably beyond the medical field, most especially to finance. Much of it comes under the headings of “principal-agent theory” and “mechanism design” (for a useful survey, see Laffont & Martimort 2002).

With the same background and with exposure in my summers at the RAND Corporation to problems of the development of new weapons, I took a similar view to the production of information (i.e., innovation) (Arrow 1962a). Here, the economic meaning of information as a commodity became even clearer. It was scarce and costly to acquire, and it might have value as an input (into treatment or into production of ordinary commodities), but it was not used up in the sense that ordinary commodities are; if one agent uses or transmits information, she still has it.

## 5. DYNAMIC SYSTEMS

In the development of any theory of capital, including inventories or even the holding of money and securities, an adequate theory of behavior over time (optimal or otherwise) is called for. This was as true of operations research, with which I was much involved in the 1950s and 1960s, as it was of economic theory. The formal treatment of future commodities by Hicks did not adequately exploit the fundamentally recursive nature of action over time, a point I learned from the work on sequential analysis with Blackwell and Girshick, mentioned above. The point is that the laws governing motion from one time period to the next are the same over time, although the initial conditions would, of course, be time-varying. The importance of dynamic systems in this sense was recognized in economics in the 1930s, with primary emphasis on the explanation of business cycles; a clear statement of the issues, with strong emphasis on the role of stochastic factors, was the pioneering paper of Frisch (1933), which was followed up empirically by the work of Tinbergen, discussed above.

Curiously, I had brushed up against dynamic analysis on two separate occasions and had not really gone further into their economic implications. One was a master’s thesis in mathematics in which I reviewed the then-existing literature on stochastic processes. The other, more original, was a paper written while serving as a weather officer, in a research unit. The question was posed as to how to use forecasts of winds to minimize the flight time of an airplane. There was a literature on this question, by some very distinguished mathematicians (Ernest Zermelo, Tullio Levi-Civita), but they all assumed a flat Earth. They were using the calculus of variations in a somewhat unusual application. I had to learn the calculus of variations (and also struggle with my limited German, the language in which the papers were written) and succeeded in showing how to vary the heading of the airplane so as to minimize flight time over a spherical Earth (Arrow 1949).

In the summer of 1950, the Office of Naval Research organized a working group on the question of inventory holdings, a major issue with the military establishment. I worked with Jacob Marschak and with the mathematician Theodore Harris, an expert in stochastic processes (Arrow et al. 1951). We quickly realized the existing literature needed a formulation that was both dynamic and stochastic. There were two discrete branches of inventory analysis, one emphasizing dynamic deterministic models marked by increasing

returns and the other planning for a stochastic demand. Our model, which has had a great influence, combined these. We drew from the first branch the idea that the optimal policy would have the so-called  $(s, S)$  form, that is, order when the inventory gets down to a prescribed level, denoted by  $s$ , and the amount ordered was the amount needed to get the total inventory to a fixed level,  $S$ . We found the optimal levels of  $s$  and  $S$ , given the relevant information, that is, the distribution of demands, the fixed cost of ordering as such, the cost per unit ordered, and the loss if a demand could not be met.

This was not a true dynamic programming solution, in that the form of the policy was given instead of being shown to be optimal. Indeed, Dvoretzky et al. (1952, pp. 194–96) showed by example that, under certain assumptions about costs and distributions, the  $(s, S)$  policy was not optimal. A very plausible sufficient condition for optimality of the  $(s, S)$  policy, involving costs only, was found a few years later by Scarf (1960).

A new research emphasis in postwar economics was that on economic growth. The systematic analysis by explicit dynamic models was made prominent by Solow's (1956) famous paper; as he made clear, he was following up and clarifying earlier work, especially of Roy Harrod and Evsey Domar. He allowed for a wide variety of aggregate production functions and assumed both a fixed savings rate and an exogenously given rate of Hicks-neutral technological growth. Although the basic approach became widespread almost immediately, a number of economists felt both of these last assumptions were limiting. Some stressed the alternative assumption that the savings rate was itself rationally determined, to optimize some function of present and future consumptions. This led to the theory of *optimal economic growth*. Others were concerned that technological change was itself the result of economic and other decisions, not to be taken as exogenously given, leading to *endogenous growth theory*.

Optimal growth theory had in fact already been started, in two (independent) papers with similar roots in the calculus of variations, those of Frank Ramsey (1928) and Hotelling (1931). On the production side, Ramsey introduced the now-standard assumption of an aggregate production function, with output depending on capital (labor being assumed fixed) and divided between investment and consumption. The maximand was the integral of utilities over time (i.e., zero discounting). Hotelling had a very novel idea, making aggregate output a function of both capital and an exhaustible resource. This was the first really thoughtful analysis of exhaustible resources; his maximand, like most of the subsequent literature, is the integral of discounted utilities.

The first important developments of Ramsey's work came in the independent papers of David Cass (1965) and Koopmans (1965). The results were very important, but the methods employed were ad hoc and not easily used for variations and generalizations.

Koopmans (1960) had also given a strong argument that, with an infinite horizon, discounting was essential to avoid clearly paradoxical conclusions. There have been a number of subsequent papers that have given other similar arguments, and I am persuaded completely that it is incoherent to treat all future generations as equivalent to the present and to each other (despite Ramsey's precedent).

All this, like the work on inventory theory cited earlier, was in the spirit of dynamic programming. The formalisms did not explicitly present a set of prices that played a dominant role in allocation. (Actually, they could have easily been derived but were not.) A different, although logically equivalent, formalism, the optimal control theory developed by the Russian mathematician L.S. Pontryagin and his associates (Pontryagin et al. 1962), filled the bill admirably, although they certainly did not have economic

applications in mind. Their methodology enabled immediately a wide variety of dynamic optimization problems to be solved, in the sense of deriving a set of differential equations characterizing the optimal paths.

The Pontryagin theory has certainly had wide repercussions in discussions of dynamic problems, particularly those in which public policy issues (externalities) are at stake. Mordecai Kurz and I soon undertook a major research into the criteria for public investment, making extensive use of these methods (Arrow & Kurz 1970).

The latest set of applications by many, including myself, has been stimulated by the problems of climate change. I have especially learned by working with colleagues such as Karl-Göran Mäler and Partha Dasgupta (Arrow et al. 2003a,b), discussing, for example, extensions to optimal dynamic behavior when there are some nonoptimalities (dynamic second-best policies).

The other development, endogenous growth theory, has also seen a great growth. It necessarily introduces noneconomic factors and economic factors that enter in unusual ways. I was led to an early exemplar of this approach through the concept of “learning by doing.” Industrial engineers had found that, when producing the same commodity, the cost of production fell with cumulative output in a fairly systematic fashion, evidently because there were a number of improvements in production techniques as the result of experience. I suggested that the same model might work at a macroeconomic level (Arrow 1962b). Although I think this point is well accepted, there are many other endogenous factors in technical change; many of them can be found in Aghion & Howitt (1998).

## 6. SOCIAL VALUES IN ECONOMIC BEHAVIOR

I conclude by a speculation as to current and future developments. Economic analysis has tended to be based on individual behavior. There is assumed to be only one set of social institutions, namely, markets. Perhaps governments also matter when dealing with market failures, also called externalities. But clearly social institutions and social interactions among individuals also matter, and perhaps significantly. I note, for example, that two-thirds of public expenditures in the United States (those on health, retirement, and education) are essentially for the production of private goods, yet are accepted and only argued about on the margin. Even the concern about climate change, addressed to the welfare of generations far in the future, must be regarded as a strong social obligation not naturally reducible to individual motivation. It is also easy to identify information channels outside the market and to note other social restrictions on behavior. Network theory has been imported into economics as a tool, although it may not be all that useful or needed.

## DISCLOSURE STATEMENT

The author is not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

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## Errata

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