TRAINS OF THOUGHT

by Harry M. Markowitz*

My essay will be concerned principally with some philosophical views I have held for much of my life. After recounting the sources (for me) and the nature of these views, I will conclude with some brief personal reflections.

These philosophical views are on a few related topics. My views on any one topic did not spring instantly to mind, but were the results of a train of thought to which I would return many times over weeks, months and years. It was also important to me that the train of thought on one topic did not contradict that on another.

What Do We Know?

The first topic to occupy me, among those reviewed here, concerned what do we know and how do we know it. Until I was thirteen or fourteen I read comic books and "The Shadow" mystery magazines. then I read (I cannot remember why) Darwin's *Origin of Species*. I was especially fascinated with how Darwin marshalled his facts, argued his case and considered possible objections. Subsequently I read popular accounts of physics and astronomy, from the high school library, and original accounts by philosophers, purchased from wonderful big, old, musty used book stores then in downtown Chicago.

The philosopher who impressed me most, who became "my" philosopher, was David Hume. He argued that even though we release a ball a thousand times and each time it falls to the floor, we are not thereby provided proof with certainty that the ball will drop when released a thousand-and-first time. On reflection, one modification to Hume's views seemed necessary. Hume spoke in terms of cause and effect. I release the ball, then it drops to the floor. I eat a substance with a particular appearance and then I feel nourished. I see or do A and then B occurs. At least it always has; but there is no necessary proof that it will. The reason that Hume's view needs modification—really amplification—is that science does not merely catalog cause and effect. Rather it develops theories, what I would now refer to as models, sometimes mistakenly thought to be inevitable universal laws.

Consider the ball once more. What do we mean that it will fall down if I release it? Which way is down if I stand in Australia? Or in space a thousand miles from the earth? The "universal truth" which was observed over and over—as any eighteenth or nineteenth century physicist would tell you—is that the ball attracts, and is attracted to, each other object by a force which is proportional to the product of their masses and inversely proportional to the square of their distance. But this universal law of gravity did not hold universally. In particular it failed to accurately explain the path of the planet Mercury. Einstein's general theory of relativity presents a quite different model of phenomena which the older Newtonian model failed to explain as well as phenomena which the latter has succeeded in explaining. But, as Hume tells us, the fact that the theory of relativity had succeeded in all instances in the past does not prove that it will continue to do so in the future; and, as Einstein himself said, in this case we would have to seek a new theory. For a statement of this view, amply illustrated, see Einstein and Infeld, *The Evolution of Physics*.

Some readers may feel that Hume's views may be true in principle, but of little applicability to Economics or Finance. But if the reader has attended Economics seminars for a few years, he or she can probably supply examples of empirical economic relationships which held in one decade and not in the next; or which held for the preceding twenty years, but not last year.

Better still, the reader should try the exercise which Descartes undertakes in his first meditation, to distinguish between what is known and

*Marvin Speiser Distinguished Professor of Finance and Economics, and the 1990 winner of the Nobel Memorial Prize in economic science, Baruch College. Consultant, Daiwa Securities Trust Co.

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what is conjecture; in the present instance, however, seeking this distinction in financial or economic matters. The reader might reflect on the fact that much of our information on economic and financial matters come from newspaper, radio and television accounts. But we know from our experience as teachers or students that even good, college level students have difficulty in relating more than 80 or 90 percent of any material correctly. This, in itself, is a source of a 10 or 20 percent error rate. In addition, we frequently learn from later accounts of events that earlier accounts were falsified. Even if all participants in events tried to inform reporters accurately, we know that participants and witnesses see and believe different things; and who knows who is correct.

Thus our primary facts delivered to us by newspapers, radio and television are the output of a process full of noise. In addition, we know that different people receiving essentially the same primary facts can fit these into radically different belief structures. Again, who knows who is correct?

Further, databases have errors, programs have bugs; most facts are brought to mind from our memories, and you know how faulty memories can be. Sometimes we dream, and then anything can happen. Perhaps now is a dream. I do not assert that everything we believe is wrong; rather, that much we take as fact is only hypothesis.

Probability, Utility and Quadratic Approximations

When I was a student member of the Cowles Commission at the University of Chicago, Karl Brunner and I worked through parts of von Neumann and Morgenstern's *Theory of Games and Economic Behavior*, including the appendix on expected utility. At first I was skeptical of expected utility as a maximum for rational behavior in the face of risk. But a conversation with Herman Rubin when he visited the Cowles Commission and a reading of Marschak's article on expected utility convinced me that this was a plausible maxim. Not long afterward I was convinced in a course by Leonard J. Savage, that one should act under uncertainty as if one assigned probability beliefs to events for which there are no objective probabilities, and should update probability beliefs according to Bayes rule. At first, I considered questions of expected utility and probability beliefs in the context of economic action in the face of risk and uncertainty. After reading F.P. Ramsey's pioneering essay, and further reflecting on Savage's arguments, I decided that the subject was the older one of "what do we know and how do we know it?" As explained above, I previously concluded that models of the world are never known with certainty. But we are more willing to give up some hypotheses than others. I agreed with Ramsey and Savage that degrees of belief should be formalized in terms of the actions of a rational decision maker, i.e., a decision maker who is not omniscient, but makes no mistakes in logic or arithmetic.[1]

Another train of thought began while reading John Burr Williams' *Theory of Investment Value* as background for my Ph.D. dissertation at the University of Chicago. Williams' asserted that the value of a stock should be the present value of its future dividends. But since the future is uncertain, I interpreted this to be the expected value of future dividends. But if one is concerned only with some expected value for each security, one must be concerned only with expected value for the portfolio as a whole. In this case, the investor would place all his funds in a single security—that with maximum expected return; or he or she would be indifferent between any combination of securities, all with maximum expected return, if there were two or more which tied for maximum. In no case would the investor prefer a diversified portfolio to all undiversified portfolios. But common sense, as well as prior examination of Wiesenberger's *Investment Companies* showed that diversification was a common and sensible aspect of investment practice. The reason, obviously, was to reduce risk. Thus the investor was, and should be, concerned with risk and return on the portfolio as a whole.

As a student in the Economics Department it was natural to think of Pareto optimality. More specifically, as a student of Tjalling Koopman's course on Activity Analysis, it was natural to distinguish between efficient and inefficient risk-return combinations; and to draw, for the first time, what is now referred to as the efficient frontier, then with expected return on the horizontal axis. Standard deviation, or variance, came to mind as the measure of risk. I did not know, off hand, the formula for the variance of a linear combination of random variables. This was supplied by a copy of Uspensky's *Introduction to Mathematical Probability* on the library shelf. I was delighted to see that portfolio variance depended on the covariances between securities as well as the variances of the securities held in the portfolio.

I left the University of Chicago for the RAND Corporation in 1951, having completed all but dissertation. At the invitation of James Tobin I spent the 1954–55 academic year at the Cowles Foundation at Yale, on leave from RAND, writing a book that would be published in 1959 as Cowles Foundation Monograph 16, *Portfolio Selection: Efficient Diversification of Investments*. Much of the time during this period was spent writing drafts of chapters explaining the elements of mean—variance analysis. A parallel activity involved attempting to reconcile mean—variance analysis and expected utility theory. Rather than consider the mean and variance of the present value of future dividend, as I first thought after reading J.B. Williams, I now considered a many period game, and assumed that securities were perfectly liquid. Under certain assumptions, each period the rational investor maximizes the expected value of a single period utility function which depends only on end of period wealth, as explained by R. Bellman. It seemed natural then to approximate this single period utility function by a quadratic, and approximate expected utility by the function of mean and variance which results from taking the expected value of the quadratic. This approach was illustrated by a few examples in my 1959 book, and more extensively by Young and Trent (1969), Levy and Markowitz (1979), and others. For most utility functions reported in the literature, and for probability distributions like historical returns on portfolios, the quadratic approximation does quite well.

It is important to distinguish between the assumption that the investor has a quadratic utility function, and the use of quadratic approximation to a given utility function. For example, Levy and Markowitz show that the Arrow and Pratt objection to a quadratic utility function does not apply to an investor who uses a quadratic approximation to a given utility function. In particular, the latter, quadratic approximation maximizer, has exactly the same risk aversion in the small, in the sense of Pratt, as does the expected utility maximizer whose utility function is approximated.

Markowitz 1959 also notes that under other assumptions the single period utility function may depend on state variables in addition to end of period wealth, and that maximizing the expected value of a quadratic approximation to this function of several variables leads to a mean—variance calculation. However, such quadratic approximations to utility functions of several variables were not explored in Markowitz 1959.

Simulation and Systems Descriptions

In the 1950s Alan S. Manne and I at the RAND Corporation, and others at RAND, UCLA and elsewhere, tried our hand at building industry-wide and multi-industry "activity analysis" models. The first thought was to build models like Leontief's input-output model, except allowing for alternate methods of producing the output of any one industry. Examination of the inverse of a large input-output matrix revealed anomalies that would not be cured by alternate activities, nor by better data. What was required was a more radical departure from the Leontief format; namely, a model in which aggregates of production equipment and aggregates of producer and consumer products were the building blocks of the analysis, as opposed to the Leontief model whose building blocks are "industry capacities" and "interindustry flows". Our reason for departing from the Leontief model, and the results of our collective work, are presented in Cowles Foundation Monograph 18, A.S. Manne and H.M. Markowitz et al., *Studies in Process Analysis: Economy—Wide Production Capabilities*.

Various people provided industry models for this "process analysis" effort. For example, Alan Manne provided a petroleum industry model; Tibor Fabian, a blast furnace, iron and steel industry model; Alan J. Rowe (then at UCLA) and I developed a metal working industries model; etc. As a by-product of this work, I became interested in manufacturing planning and scheduling in the metalworking industries.[2] I soon agreed with those who argued that typical realistic manufacturing planning problems were too complex for analytic solution, or for optimizing algorithms such as linear programming. Simulation techniques were needed for advanced analysis, i.e., to give greater insight than provided by the static analysis of the day. One of the things I did at RAND, after returning form leave at Yale, was to supervise the programming of the computer simulation portion of a large man/machine logistics system simulation. This experience reinforced for me the potential usefulness of simulation techniques and illustrated the difficulty of programming detailed simulation models. These two points had already been illustrated by a previous simulation that had been programmed for me, and large and small simulations programmed by others at RAND. (Programming at the time was done in assembler. FORTRAN was about to make its appearance.)

Not long afterwards I resigned from RAND to accept a tempting offer at the General Electric Computer Department. Soon after I moved from the Computer Department to General Electric's Manufacturing Services where my friend and colleague Alan Rowe was developing a "general purpose" job shop model. It took two or three years for Rowe and one or two programmers to complete the model. Then, when one applied it to a factory other than the one for which it was developed, it turned out to be less "general purpose" than had been hoped. My own theory at the time was to seek "flexibility" rather than generality. This flexibility was to be achieved by building a simulator out of "reusable" FORTRAN modules. The first such General Electric Manufacturing Simulator (GEMS) was built in nine months. This shorter time to program was probably due to the use of FORTRAN rather than assembler language as used in Rowe's job shop simulator. As it turned out, my flexible subroutines were not all that flexible, except for some that performed basic actions such as that of creating or destroying some entity in the simulation (such as a job in the job shop) or inserting an entity into a collection of entities, such as the queue awaiting some resource.

I decided that these basic actions would be more conveniently placed at a programmers disposal by making them part of a programming language rather than leaving them as subroutines as in GEMS. I decided that I would like to develop such a programming language at a place whose mission and environment was like that I had known at RAND. I let my interests to be known to a small number of organizations and, in the end, returned to RAND. Bernard (Bernie) Hausner was assigned to me to implement the new language. Later, Herb Karr was hired as a consultant to write a programming manual. Bernie, Herb and I spent many hours together designing the language which we called SIMSCRIPT (now referred to as SIMSCRIPT I).

Our objective in designing SIMSCRIPT was, insofar as we could, to allow the user to describe the system to be simulated, as distinguished from having the user describe the actions which the computer must take to accomplish the simulation. The status of the system to be simulated was described in terms of *entities* of various *entity types*. Each individual entity was characterized by the *values* of its *attributes*, the *sets* to which it *belonged* and the members of the *sets* it owned. Status changed at points in time called *events*. Subsequently, Ed Russell introduced the notion of a *process* into SIMSCRIPT, which he borrowed from SIMULA, a later simulation programming language. During an event or process, status changes as entities are created or destroyed, attribute values are changed and entities gain and lose set memberships. The SYSTEM as a whole is an entity which can have attributes and own sets. Compound (Cartesian product) entities are also represented.

The SIMSCRIPT programming languages (including the original SIMSCRIPT I and following I.5, II and II.5 versions) have been applied to a wide variety of fields such as manufacturing simulation, from which it evolved, logistics analysis at RAND, and other applications such as to computer systems design, war games, transportation problems and the effects of trading systems on stock price behavior. SIMSCRIPT II.5 continues to have a large number of simulation application users.

The Entity, Attribute and Set (EAS) view of system description has also proven useful for other than simulation programming. For example, the SIMSCRIPT I.5 and SIMSCRIPT II Translators were themselves written in SIMSCRIPT, based on an EAS description of the entities encountered in the translation process.

The SIMSCRIPT II Translator was "bootstrapped" from a SIMSCRIPT I description of the translation process, then recompiled in terms of a SIMSCRIPT II description of its own compilation.

When SIMSCRIPT II was designed in the mid 1960s it was planned that it should be a database as well as a simulation language. A database would consist of the EAS description of the entities of the world represented within the database. In other words, the thought was that not only could entities be represented within a simulation, but also "real" entities could be represented within a database in EAS terms. Because of miscellaneous events, not related to the applicability of the EAS worldview, an implementation of the EAS view of database management, bootstrapped from the SIMSCRIPT II translator, was not completed until the work of Malhotra, Markowitz, and Pazel. We argue that the performance of the EAS-E system, including its use in internal IBM applications, prove the technical success of the approach. However, we were not able to persuade IBM to support EAS-E as a product. In part at least this was because IBM had just announced its support for the relational database methodology, after many years of supporting the hierarchical view of IMS. IBM seemed unlikely to be persuaded to change again in the short run. In the long run I was elsewhere; i.e., after building EAS-E at IBM and seeing that it would be a very long process to sell it internally, I was delighted to accept the offer of the Marvin Speiser Chair at Baruch College where I am now located.

The EAS concepts of system description are described in an article on SIMSCRIPT in the *Encyclopedia of Computer Science and Technology*. This includes a proposal for using this view in managing the entities of a computer operating system as well as those encountered in simulation, compilation and database management. The thought is to provide a uniform method of interacting with the entities encountered in computer systems, whether user defined or defined by the developers or the computer system, whether simulated or real, transient or database, etc. I have not succeeded in persuading the software development profession of the desirability of this approach. On the contrary, "object oriented" programming has emerged as the chief contender for the role which I had hoped for EAS programming. It is not only that I speak of "entities" and they speak of "objects". There is also a difference in the paradigms by which my entities and their objects are manipulated.

Perhaps if I took the time to work with object oriented programming with an open mind on a variety of applications I would conclude that it is at least as good as the EAS approach. As it is, the largest item in my queue of things to do, someday, remains to demonstrate the efficacy of the EAS view of system description.

Personal Reflections

Life has afforded me many pleasurable activities, such as enjoying a fine meal, walking with Mrs. Markowitz on new fallen snow on the path through the woods near our home, flying kites with one or more grandchildren, listening to music, especially J.S. Bach, and the like. But no activity sustains my interest as long as does struggling with some technical or philosophical problem, sometimes alone, sometimes with colleagues. From one point of view these struggles may be classified as "work", since I sometimes get paid for such efforts. From another viewpoint they are play—part of a game like chess or amateur cryptography which I have also enjoyed. Often in my work-game, part of the objective is to produce something that someone will use. It is not only that sometimes someone pays me for such creations. It is also the fact that for a long time I have been primarily concerned with the theory of rational action, especially rational action under uncertainty. One measure of one's success in achieving a useful understanding and techniques for rational action is to have theory and techniques tried, accepted and endure. I have also spent time applying theory and techniques. For example, I currently spend half-time as Director of Research of Daiwa's Global Portfolio Research Department (GPRD) which has money management responsibilities in conjunction with other branches of Daiwa Securities. Previously I was President of Arbitrage Management Company. Such alternating between theory and practice is not uncommon among financial theorists. Sharpe, Rosenberg, Roll, Ross, Black, Vasicek, Leland, and Rubinstein are a few of those who are both theoretician and practitioner. Sometimes they develop the theory of practice, and other times the practice of theory. I find that these two activities reinforce each other.

Some economists report that they entered economics to better mankind's state (e.g., see Szenberg, *Their Life Philosophies: Eminent Economists.*) I have never thought it in my power to much improve the human condition generally. Much of human ill is due to violent aggression, political suppression, ancient hatreds and the like. These are not matters I know how to deal with, either from my training as an economist nor with the decision making techniques I have developed. Together with my wife, I try to be a good neighbor, contribute moderately to charities, try to help my children, grandchildren, students and colleagues when I can be of service, and the like. That done, I feel that I have paid my dues and may indulge myself in life's pleasures, including the struggle with interesting problems and questions of philosophy.

Notes

1. The question of what it means to "make no mistake in logic and arithmetic" raises further questions which have been extensively explored, without universal agreement, by generations of mathematicians concerned with the foundations of their own discipline. See for example Kleene, *Introduction to Metamathematics*.
2. Another byproduct of the process analysis research was methods for solving large, sparse systems of equations i.e., large systems with relatively few non-zero elements. See Markowitz, H., "The Elimination Form of the Inverse and Its Application to Linear Programming", *Management Science*, 1957. Variants of these methods are now part of large, production linear programming codes.

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