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ECONOMIC THEORY AND MATHEMATICS—AN APPRAISAL

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It has been correctly said that mathematical economics is flying high these days. So I come, not to praise mathematics, but rather to slightly debunk its use in economics. I do so out of tenderness for the subject, since I firmly believe in the virtues of understatement and lack of pretension.

I realize that this is a session on methodology. Hence, I must face some basic questions as to the nature of mathematics and of its application. What I have to say on this subject is really very simple—perhaps too brief and simple. The time that I save by brief disposal of the weighty philosophical and epistemological issues of methodology I can put to good use in discussing the tactical and pedagogical issues—or what you might even call the Freudian problems that the mathematical and nonmathematical student of economics must face.

The Strict Equivalence of Mathematical Symbols and Literary Words. On the title page of my *Foundations of Economic Analysis,* I quoted the only speech that the great Willard Gibbs was supposed ever to have made before the Yale Faculty. As professors do at such meetings, they were hotly arguing the question of required subjects: Should certain students be required to take languages or mathematics? Each man had his opinion of the relative worth of these disparate subjects. Finally Gibbs, who was not a loquacious man, got up and made a four-word speech: "Mathematics is a language."

I have only one objection to that statement. I wish he had made it 25 per cent shorter—so as to read as follows: "Mathematics *is* language." Now I mean this entirely literally. In principle, mathematics cannot be worse than prose in economic theory; in principle, it certainly cannot be better than prose. For in deepest logic—and leaving out all tactical and pedagogical questions—the two media are strictly identical.

Irving Fisher put this very well in his great doctoral thesis, written exactly sixty years ago. As slightly improved by my late teacher, Joseph Schumpeter, Fisher's statement was: "There is no place you can go by railroad that you cannot go afoot." And I might add, "Vice versa!"

I do not think we should make too much of the fact that in recent years a number of universities have permitted their graduate students to substitute a reading knowledge of mathematics for a reading knowledge of one foreign language. For after all we run our universities on the principle that Satan will find work for idle hands to do; and the fact that we may permit a student to choose between ROTC and elementary badminton does not mean that these two subjects are methodologically identical. And besides, we all know just what a euphemism the expression "a graduate student's reading knowledge" really is.

Induction and Deduction. Every science is based squarely on induction—on observation of empirical facts. This is true even of the very imperfect sciences, which have none of the good luck of astronomy and classical physics. This is true of meteorology, of medicine, of economics, of biology, and of a number of other fields that have achieved only modest success in their study of reality. It used to be thought that running parallel with induction there runs an equally important process called "Deduction"—spelled with a capital *D*. Indeed, certain misguided methodologists carried their enthusiasm for the latter to such extremes that they regarded Deduction as in some sense overshadowing mere pedestrian induction.

Now science is only one small part of man's activity—a part that is today given great honorific status, but which I should like to strip of all honorific status for purposes of this discussion. However, to the extent that we do agree to talk about what is ordinarily called science—and not about poetry or theology or something else—it is clear that deduction has the modest linguistic role of translating certain empirical hypotheses into their "logical equivalents." To a really good man, whose IQ is 300 standard deviations above the average, all syllogistic problems of deduction are so obvious and take place so quickly that he is scarcely aware of their existence. Now I believe that I am uttering a correct statement—in fact, it is the only irrefutable and empty truth that I shall waste your time in uttering—when I say that not everybody, nor even half of everybody, can have an IQ 300 standard deviations above the mean. So there is for all of us a psychological problem of making correct deductions. That is why pencils have erasers and electronic calculators have bells and gongs.

I suppose this is what Alfred Marshall must have had in mind when he followed John Stuart Mill in speaking of the dangers involved in *long* chains of logical reasoning. Marshall treated such chains as if their truth content was subject to radioactive decay and leakage—at the end of n propositions only half the truth was left, at the end of a chain of $2n$ propositions, only half of half the truth remained, and so forth in a geometric multiplier series converging to zero truth. Obviously, in making such a statement, Marshall was describing a property of that biological biped or computing machine called *homo sapiens;* for he certainly could not be describing a property of logical implication. Actually, if proposition A correctly implies proposition B, and B correctly implies proposition C, and so forth all the way to Z, then it is necessarily true that A implies Z in every sense that it implies B. There can be no leakage of truth at any stage of a valid deductive syllogism. All such syllogisms are mere translations of the type, "A rose is a rose is a rose."

All this is pretty well understood when it comes to logical processes of the form: Socrates is a man. All men are mortal. Therefore, Socrates is mortal. What is not always so clearly understood is that a literary statement of this type has its complete equivalent in the symbolism of mathematical logic. If we write it out in such symbolism, we may save paper and ink; we may even make it easier for a seventeen-year-old freshman to arrive at the answer to complex questions of the type: "Is Robinson, who smokes cigarettes and is a non-self shaver, a fascist or is it Jones?" But nonetheless, the mathematical symbolism can be replaced by words. I should hate to put six monkeys in the British Museum and wait until they had typed out in words the equivalent of the mathematical formulas involved in Whitehead and Russell's *Mathematical Principia*. But if we were to wait long enough, it could be done.

The Case of Neoclassical Distribution. Similarly, in economics. The cornerstone of the simplest and most fundamental theory of production and distribution—that of Walras and J. B. Clark—is Euler's theorem on homogeneous functions. Now it is doubtful that Clark—who rather boasted of his mathematical innocence—had ever heard of Euler. Certainly, he cannot have known what is meant by a homogeneous function. But nonetheless, in Clark's theory, there is the implicit assumption that scale does not count; that what does count is the proportions in which the factors combine; and that it does not matter which of the factors of production is the hiring factor and which the hired. If we correctly interpret the implication of all this, we see that Clark—just as he was talking prose and knowing it—was talking the mathematics of homogeneous functions and not knowing it.

I have often heard Clark criticized for not worrying more about the exhaustion-of-the-product problem. He seems never to have worried whether rent, computed as a triangular residual, would be numerically equal—down to the very last decimal place—to rent calculated as a rectangle of marginal product. Like King Canute, he seems simply to have instructed his draftsman to draw the areas so as to be equal.

As I say, Clark has often been criticized for not going into this problem of exhaustion of the product. I myself have joined in such criticism. But I now think differently—at least from the present standpoint of the nature of true logical deductive implication as distinct from the human psychological problem of perceiving truth and cramming it into the heads of one's students or readers. Even if Euler had never lived to perceive his theorem, even if Wicksell, Walras, and Wicksteed had not applied it to economic theory, Clark's doctrine is in the clear. His assumptions of constant-returns-to-scale and viable free-entry ensure for him that total revenue of each competitive firm will be exactly equal to total cost. And with this settled in the realm of cost and demand curves, there is no need for a textbook writer in some later chapter of his book dealing with production to suddenly become assailed by doubts about the "adding-up problem of exhaustion-of-the product."

Now let me linger on this case for a moment. Economists have carefully compared Wicksteed's and Clark's treatment of this problem in order to show that mathematics is certainly not inferior to words in handling such an important element of distribution theory.

What is not so clear is the answer to the reverse question: Is not literary economics, by its very nature, inferior to mathematics in handling such a complex quantitative issue. As one eminent mathematical economist put it to me: "Euler's theorem is absolutely basic to the simplest neoclassical theory of imputation. Yet without mathematics, you simply cannot give a rigorous proof of Euler's theorem."

Now I must concede that the economics literature does abound with false proofs of Euler's theorem on homogeneous functions. But what I cannot admit—unless I am willing to recant on all that I have been saying about the logical identity of words and symbols—I simply cannot admit that a rigorous literary proof of Euler's theorem is in principle impossible.

In fact, I tried a literary proof on my mathematical friend. He quite properly pointed out that it was not rigorous in the way it treated infinitesimals. I fully agree. My argument was heuristic. But I do claim that if my friend and I could spend a week or so talking together, so that I could describe in words the fundamental limit processes involved in the Newton-Leibniz calculus and derivatives, then this problem of lack of rigor could be met. In fact, much more subtle properties of Pfaffian partial differential equations are in principle capable of being stated in basic English. As Professor Leontief has pointed out, the final proof of the identity of mathematics and words is the fact that we teach people mathematics by the use of words, defining each symbol as we go along. It is no accident that the printer of mathematical equations is forced to put commas, periods, and other punctuation in them, for equations are sentences, pure and simple.

Geometry in Relation to Words and Mathematical Analysis. Today when an economic theorist deplores the use of mathematics, he usually speaks up for the virtues of geometrical diagrams as the alternatives. It was not always thus. Seventy years ago, when a man like Cairnes criticized the use of mathematics in economics, probably he meant by the term "mathematics" primarily geometrical diagrams. From the point of view of this lecture, the ancients were more nearly right than the modern critics. Geometry is a branch of mathematics, in exactly the same sense that mathematics is a branch of language. It is easy to understand why a man might have no use at all for economic theory, invoking, instead, a plague on mathematical economics, on diagrammatic textbooks, and on all fine-spun literary theories. It is also easy to understand why some men should want to swallow economic theory in all of its manifestations. But what is not at all clear—except in terms of human frailty—is why a man like Cairnes should be so enamored of literary theory and should then stop short of diagrams and symbols. Or why any modern methodologist should find some virtue in two-dimensional graphs but should draw the line at third or higher dimensions.

I suggest that the reason for such inconsistent methodological views must be found in the psychological and tactical problems which constitute the remaining part of my remarks.

But before leaving the discussion of the logical identity of mathematical symbols and words, I must examine its bearing on a famous utterance of Cairnes. He lived at a time when, as we now know, mathematics was helping bring into birth a great new neoclassical synthesis. Yet Cairnes went so far as to say: "So far as I can see, economic truths are not discoverable through the instrumentality of mathematics. If this view be unsound, there is at hand an easy means of refutation—the production of an economic truth, not before known, which has been thus arrived at." Now this view is the direct opposite of that of Marshall. Marshall in his own way also rather pooh-poohed the use of mathematics. But he regarded it as a way of arriving at truths, but not as a good way of communicating such truths—which is just the opposite of Cairnes's further remarks on the subject.

Well, what are we to think of the crucial experiment proposed by Cairnes? In the first place, he himself was both unable and unwilling to use the mathematical technique; so it might have been possible for us to produce a new truth which Cairnes could never have been capable of recognizing. Indeed, many have cogently argued that Jevons had in fact done so. However, from the methodological viewpoint that I have been expounding, it will be clear that any truth arrived at by way of mathematical manipulation must be translatable into words; and hence, as a matter of logic, could quite possibly have been arrived at by words alone. Reading Cairnes literally, we are not required to produce a truth by mathematics that could not have been proved by words; we are only required to produce one that has not, as a matter of historical fact, been previously produced by words. I suggest that a careful review of the literature since the 1870's will show that a significant part of all truths since arrived at have in fact been the product of theorists who use symbolic techniques. In particular, Walrasian general equilibrium, which is the peak of neoclassical economics, was already enunciated in Walras' first edition of the *Elements* at the time Cairnes was writing.

Jevons, Walras, and Menger each independently arrived at the so-called "theory of subjective value." And I consider it a lucky bonus for my present thesis that Menger did arrive at his formulation without the use of mathematics. But, in all fairness, I should point out that a recent rereading of the excellent English translation of Menger's 1871 work convinces me that it is the least important of the three works cited; and that its relative neglect by modern writers was not simply the result of bad luck or scholarly negligence. I should also add that the important revolution of the 1870's had little really to do with either subjective value and utility or with marginalism; rather it consisted of the perfecting of the general relations of supply and demand. It culminated in Walrasian general equilibrium. And we are forced to agree with Schumpeter's appraisal of Walras as the greatest of theorists—not because he used mathematics, since the methods used are really quite elementary—but because of the key importance of the concept of general equilibrium itself. We may say of Walras what Lagrange ironically said in praise of Newton: "Newton was assuredly the man of genius *par excellence,* but we must agree that he was also the luckiest: one finds only once the system of the world to be established!" And how lucky he was that "in his time the system of the world still remained to be discovered." Substitute "system of equilibrium" for "system of the world" and Walras for Newton and the equation remains valid.

Summary of Basic Methodology. In leaving my discussion of Methodology with a capital M, let me sum up with a few dogmatic statements. All sciences have the common task of describing and summarizing empirical reality. Economics is no exception. There are no separate methodological problems that face the social scientist different in kind from those that face any other scientist. It is true that the social scientist is part of the reality he describes. The same is true of the physical scientist. It is true that the social scientist in observing a phenomenon may change it. The theory of quantum mechanics, with its Heisenberg uncertainty principle, shows that the same is true of the physical scientist making small-scale observations. Similarly, if we enumerate one by one the alleged differences between the social sciences and other sciences, we find no differences in kind.

Finally, it is clear that no a priori empirical truths can exist in any field. If a thing has a priori irrefutable truth, it must be empty of empirical content. It must be regarded as a meaningless proposition in the technical sense of modern philosophy. At the epistemological frontier, there are certain refined difficulties concerning these matters. But at the rough and ready level that concerns the scientist in his everyday work, the above facts are widely recognized by scientists in every discipline. The only exceptions are to be found in certain backwaters of economics, and I shall not here do more than point the finger of scorn at those who carry into the twentieth century ideas that were not very good even in their earlier heyday.

Differences in Convenience of Languages. I now turn to the really interesting part of the subject. What are the conditions under which one choice of language is more convenient than another? If you are a stenographer required to take rapid dictation, there is no doubt that you will prefer shorthand to old-English lettering. No disinterested third party will ever be in doubt as to whether Roman numerals are less convenient than arabic numerals for the solution of problems in commercial arithmetic; and the same goes for a comparison between a decimal system of coinage and that used by the English.

A comparison between a language like French and one like German or English or Chinese is a little more difficult. We might concede that any proposition in one language is translatable into another. But that is not relevant to the psychological question as to whether one language is intrinsically more convenient for a certain purpose than another. We often hear it said that French is a very clear language, and that German is a very opaque one. This is illustrated by the story that Hegel did not really understand his philosophy until he had read the French translation!

I do not know whether there is anything in this or not. It seems to me that Böhm-Bawerk or Wicksell written in German is quite as straightforward as in English; whereas I find Max Weber or Talcott Parsons difficult to understand in any tongue. I suspect that certain cultures develop certain ways of tackling problems. In nineteenth century German economics it was popular and customary to ask about a problem like interest or value: What is the essence of interest or value? After this qualitative question is answered, then the quantitative level of the rate of interest or price-ratio can be settled. Now I happen to think that this is sterile methodology. But I cannot blame it on the German language.

It is interesting, however, that Menger wrote a letter to Walras on this very subject. As reported by Professor Jaffe's interesting article (*Journal of Political Economy,* 1936), Menger said that mathematics was all very well for certain descriptive purposes, but that it did not enable you to get at the essence of a phenomenon. I wish I thought it were true that the language of mathematics had some special faculty of drawing attention away from pseudo problems of qualitative essence. For, unlike Menger, I should consider that a great advantage.

Baconian and Newtonian Methods. There are many empirical fields where translation into mathematical symbols would seem to have no advantage. Perhaps immunology is one, since I am told not a single cure for disease—vaccination against smallpox, inoculation for diphtheria, use of penicillin and sulpha, and so forth—has been discovered by anything but the crudest empiricism and with sheer accident playing a great role. Here the pedestrian methods of Francis Bacon show up to much greater advantage than do the exalted methods of a Newton. If true, we must simply accept this as a fact. I am sure that many areas of the social sciences and economics are at present in this stage. It is quite possible that many such areas will always continue to be in this stage.

Pareto regarded sociology as being of this type. But curiously enough, he goes on to argue that the chief virtue of mathematics is in its ability to represent complexly interacting and interdependent phenomena. I think we must accept this with a grain of salt. Analogies with complicated interdependent physical systems are valuable if they alert us to the dangers of theories of unilateral causation. But after mathematical notions have performed the function of reminding us that everything depends upon everything else, they may not add very much more—unless some special hypotheses can be made about the facts.

On the other hand, there are areas which over the years have fallen into the hands of the mathematically annointed. Earlier I mentioned the case of symbolic logic. There are still some girls' seminaries where literary logic rules the roost; but no sensible man expects that in the centuries ahead the field of logic will be deloused of mathematics.

Another field is that of physics. Its capture by mathematics is a fact —as solid and irreversible as the second law of thermodynamics itself.

It is dangerous to prophesy. But I suspect that in some small degree the same will hold of the field of economic theory. For a century mathematics knocked at the door. Even today it has no more than a foot in the doorway. But the problems of economic theory—such as the incidence of taxation, the effects of devaluation—are by their nature quantitative questions whose answer depends upon a superposition of many different pieces of quantitative and qualitative information. When we tackle them by words, we are solving the same equations as when we write out those equations.

Now I hold no brief for economic theory. I think the pendulum will always swing between interest in concrete description and attempts to construct abstract summaries of experience, with one decade and tradition giving more emphasis to the one process and another time and place giving emphasis to the other. But I do think that when the pendulum is swinging in favor of theory, there will be kind of a Gresham's law operating whereby the more convenient deductive method will displace the less convenient.

Convenience of Symbols for Deduction. And make no mistake about it. To get to some destinations it matters a great deal whether you go afoot or ride by a train. No wise man studying the motion of a top would voluntarily confine himself to words, forswearing all symbols. Similiarly, no sensible person who had at his command both the techniques of literary argumentation and mathematical manipulation would tackle by words alone a problem like the following: Given that you must confine all taxes to excises on goods or factors, what pattern of excises is optimal for a Robinson Crusoe or for a community subject to prescribed norms?

I could go on and enumerate other problems. But that is not necessary. All you have to do is pick up a copy of any economic journal and turn to the articles on literary economic theory, and you will prove the point a hundred times over.

The convenience of mathematical symbolism for handling certain deductive inferences is, I think, indisputable. It is going too far to say that mathematicians never make mistakes. Like everybody else, they can pull some awful boners. But it is surprising how rare pure mistakes in logic are. Where the really big mistakes are made is in the formulation of premises. Logic is no protection against false hypotheses; or against misinterpretation of reality; or against the formulation of irrelevant hypotheses. I think it is one of the advantages of the mathematical medium—or, strictly speaking, of the mathematician's customary canons of exposition of proof, whether in words or symbols—that we are forced to lay our cards on the table so that all can see our premises. But I must confess that I have heard of card games—in fact I have participated in them myself—where knowingly or unknowingly, we have dealt cards from the bottom of the deck. So there are no absolute checks against human error.

The Human Dilemma. In conclusion, ask yourself what advice you would have to give to a young man who steps into your office with the following surprisingly common story: "I am interested in economic theory. I know little mathematics. And when I look at the journals, I am greatly troubled. Must I give up hopes of being a theorist? Must I learn mathematics? If so, how much? I am already past twenty-one; am I past redemption?"

Now you could answer him the way Marshall more or less advised Schumpeter: forget economic theory. Diminishing returns has set in there. The world is waiting for a thousand important applications.

This of course is no answer at all. Either the young man disregards your advice, as Schumpeter did. Or he accepts it, and psychologically you have dealt him the cruelest blow of all.

I think a better answer might go somewhat as follows: Some of the most distinguished economic theorists, past and present, have been innocent of mathematics. Some of the most distinguished theorists have known some degree of mathematics. Obviously, you can become a great theorist without knowing mathematics. Yet it is fair to say that you will have to be that much more clever and brilliant.

It happens to be empirically true that if you examine the training and background of all the past great economic theorists, a surprisingly high percentage had, or acquired, at least an intermediate mathematical training. Marshall, Wicksell, Wicksteed, Cassel, and even such literary economists as Nicholson or Malthus provide examples. This is omitting economists like Edgeworth, Cournot, Walras, Pareto, and others who were avowedly mathematical economists.

Moreover, without mathematics you run grave psychological risks. As you grow older, you are sure to resent the method increasingly. Either you will get an inferiority complex and retire from the field of theory or you will get an inferiority complex and become aggressive about your dislike of it. Of course, those are the betting odds and not perfect certainties. The danger is almost greater that you will overrate the method's power for good or evil. You may even become the prey of charlatans who say to you what Euler said to Diderot to get him to leave Catherine the Great's court: "Sir, $(a + b^n)/n = x$, hence God exists; reply!" And, like Diderot, you may slink away in shame. Or reacting against the episode, you may disbelieve the next mathematician who later comes along and gives you a true proof of the existence of the Deity.

In short—your advice will continue—mathematics is neither a necessary nor a sufficient condition for a fruitful career in economic theory. It can be a help. It can certainly be a hindrance, since it is only too easy to convert a good literary economist into a mediocre mathematical economist.

Despite the above advice, it is doubtful that when you check back five years later on that young man he will be very different. Indeed, as I look back over recent years, I am struck by the fact that the species of mathematical economist pure and simple seems to be dying out and becoming extinct. Instead, as one of my older friends complained to me: "These days you can hardly tell a mathematical economist from an ordinary economist." I know the sense in which he meant the remark, but let me reverse its emphasis by concluding with the question: Is that bad?

DISCUSSION

FRITZ MACHLUP: I shall address myself to three issues: the use of mathematics, the role of realism, and the time for synthesis. The first two—mathematics and realism—seem almost antithetical. My comments will probably disappoint both those who want more mathematics and those who want more realism in economic analysis.

I do not deny Professor Samuelson's assertion that mathematics is a language and even one which for some purposes is superior to English or German. But for other purposes it is inferior or even altogether unsuitable. There are things that ought to be said but cannot be said in mathematical language.

Perhaps I underestimate the potentialities of mathematics. It is true that Atlas, the mental giant in the Barnaby series whose memory for names is so very poor, manages to translate such simple names as J. J. O'Malley into algebraic formulas and vice versa. This ability need not be confined to the mental giants of the comic strip. But there are definite limits to translatability into mathematics. Thus far, love letters cannot well be written in mathematics and read with a full appreciation of the romantic feelings of the writer. And I challenge Professor Samuelson to translate into mathematics the paper he has just read to us in English and preserve all its qualities, including its fine humor. And, to come to the real issue, I submit that the basic human attitudes that underlie economic conduct—and must be understood if we are to understand economics—cannot be described and analyzed exclusively in mathematical language.

Professor Samuelson is contemptuous of what he calls the "pseudo problems of qualitative essence." Am I really concerned with a pseudo problem if I wish to get at the essence of a phenomenon or at its merely qualitative aspects? To my mind, it is perfectly good practice, before making statements about anything, to make clear what it is one is talking about; that is, to discuss the essence of the matter. Professor Samuelson makes specific mention of the "sterile" question of the essence of value. I shall not attempt to tell other people in which problems they should be interested and from which problems they should turn away. I for my part continue to be interested and concerned with the problem of the essence of value even if it takes other languages than mathematics to talk about it.

Those who talk only one language are probably barred from the appreciation and understanding of some problems. On the other hand, a problem is sometimes recognized as a mere pseudo problem when its analysis is translated into other languages. All this, I think, adds strength to the argument for polylinguistic scholarship.

There are those who disparage the use of mathematics in economic theory on the ground that it must result in "too unrealistic" models. This complaint, which in my opinion reveals misunderstanding of the fundamentals of scientific methodology, provides me with a welcome transition to the second issue on which to comment: the call for more realism and the rejection of manifestly unrealistic postulates in economic analysis.

No one has ever suggested that a theory of the individual consumer should start, as a first approximation, with a posulate that the consumer seeks to maximize the consumption of food, let alone bread. From the very beginning a "multitude of ends" has been considered the appropriate assumption for a theory of consumer's choice. However, for the sake of graphical demonstrations by indifference curves, a reduction of the possible choices to just two alternatives has proved handy. Of course, this is terribly unrealistic, as those who cannot comprehend such technical things and need an escape are anxious to emphasize, but everybody has been aware of it.

In the theory of the individual seller of labor services, no one believes it would be fruitful to assume that the worker aims to maximize his pecuniary income. It is obvious that not only the alternative opportunity of leisure or the disutility of effort must be taken into account but also such things as differences in working conditions in different employments or differences in living conditions in different places. However, a good deal of insight into the relationship between income and leisure and about the influence of income deductions on the labor supply can be gained by a model which is deliberately unrealistic in omitting the differences in working and living conditions and in isolating the effects of changes in the net rate of pay.

No one would seriously hold that businessmen have no other goal than money profits. It takes no unusual powers of observation to realize that not all businessmen want to get stomach ulcers, that some of them prefer to take it easy, that most of them dislike sleepless nights worrying about risky deals, that some like to gamble, that many are patriots who will want to avoid doing things which the government says are bad for the country, that several like to be "big shots" running big enterprises and being admired for their position, that many have a pride of workmanship and a feeling for sportsmanship. But it is quite apparent that a good many things could be "seen" by operating a much simplified model in which (besides output of a homogeneous product) only pecuniary costs and revenues enter as variables and the maximization of the difference is the basic hypothesis. Unrealistic? Surely. But whenever one can learn something by a simpler method it would be silly to insist on learning it a more complicated way. The addition of "admittedly realistic" variables into analytical models can be defended only if they significantly modify the results.

This means, of course, that models of different degrees of realism and complexity will be adequate for different problems. Certain problems can be adequately analyzed with the maximization of pecuniary profits as the basic working hypothesis; other problems cannot and require much more complicated models. Professor Boulding recognizes this and does not claim that his "asset preference" assumptions are needed for all problems of the economics of the firm. What he does not say, and in my opinion should say, is that there is not one theory of the firm but that there are many. To expect that one theory of the firm should serve all purposes is almost like expecting one all-purpose theory of man or a panacea that cures all ills from sore throats to broken noses. What in our textbooks has been called the theory of the firm is only a part of static price and distribution theory and does not attempt or pretend to be more. Is it not very unreasonable to complain about its inadequacy for other purposes? A model designed to analyze business decisions about the price and output of some particular product or set of products need not be equipped to analyze decisions about the investment of available funds, about the payments of dividends, about the increase in the power of management, and so forth; and if it were equipped for all these purposes, it would be unnecessarily clumsy. Universal tools which, for example, can work as screw drivers, drills, hammers, pincers, and scissors all at once are neither economical nor efficient. A universal theory of the firm which could explain the output of a given product and also the growth of the firm through agglomerative merger and perhaps also any other kind of business conduct would be an uneconomical and inefficient theory.

If I say that the theory of the firm in price analysis is one thing and the theory of the growth of the firm is another—and that the choice of variables is dictated by the problem at issue and not by what to naïve observers appears to be realistic—I should like to acknowledge the benefit of discussions with Dr. Edith Penrose, of Johns Hopkins University, who has been working on the theory of the growth of the size of the firm.

How naïve it is to call for a comprehensive all-purpose theory of the firm becomes particularly clear from Mrs. Penrose's finding that the concept of the firm changes as we switch from one problem to another. The firm in conventional price theory is only a very distant relative of the firm in the legal sense, of the firm in any of the sociological senses, and of the firm in any of the other economic senses. All of these are different kinds of animals—or, perhaps I should say, different kinds of models. The concept of the firm may have its criterion in a balance sheet, or in a particular collection of assets, or in the personality of an entrepreneur, or a set of managers, or in a group of persons in control, or in the people who own the equity, or in a corporate name or charter, or probably still other things. Believe me, the "problem of decay and death" of the firm, which Professor Boulding called "mysterious," looks totally different depending on which of the concepts of the firm is adopted—and on the basis of some of the concepts the problem looks not mysterious but meaningless. This is an important conclusion, especially in view of the recent "survival theories" of the firm (Alchian and Enke) that rest on the distinction of successful firms and failures. But I should not anticipate here too many of the findings of Dr. Penrose's critical analysis of these theories.

Some economic theorists have become excessively enamored of the application of biological concepts to economic problems and, I am afraid, Professor

Boulding is among them. "Death and Transfiguration" is a beautiful tone poem; and "The Death of a Salesman" makes an interesting stage play. But before we make much of the "death of a firm" in economic analysis we had better be quite sure how we define the firm and how we ascertain its demise.

If I had to summarize my comments on Professor Boulding's paper in one sentence, I should say this: I agree with his conclusion "that the impact of more realistic theories of the firm on static price analysis is likely to be small," but I am skeptical with regard to his hope that by integrating different models of the firm into a more realistic model we shall progress faster in economic dynamics.

On the problem of integration and realism I should like to add this: It is of course possible that the models of the firm that will prove useful in growth theory will also look more realistic. If they do, this will be so chiefly because of the particular personal experiences of those who pass such judgments, but I do not think there would be any reason to be happy about it. In analysis it is not realism but relevance that counts, as Professor Knight once so cogently demonstrated. As to the eternally popular demand for integration and synthesis, I should like to quote from Professor Boulding's latest book,[1] where he said that "a synthesis of inadequate parts may be worse than no synthesis at all," which he footnoted by the remark that "institutionalism in economics may be regarded as a premature attempt at synthesis of the social sciences, an attempt to synthesize bad economics, bad sociology, and bad anthropology in a medium of subconscious emotional bias." But underlying these remarks is Professor Boulding's conviction, which he apparently shares with the Walrus of *Alice in Wonderland*, that now "the time has come" to talk of many things at once.

It is not so, I submit, that synthesis can wait until the several parts of systematic knowledge are perfected. Probably we do know more now than thirty years ago; but in another thirty years we shall know more than now. The license to synthesize is not granted only upon a certification of maturity. No matter how imperfect our tools, synthesis is always permissible and necessary whenever we deal with specific cases of reality. But synthesis of different fields of knowledge is never called for while we are engaged in developing or formulating general theories. There is no use for a "general synthesis" because we never know just what mixture of bodies of knowledge—what "knowledge mix"—will be best suited for the concrete cases that we may run into. One case may call for a synthesis of social psychology, cultural anthropology, and economics; another for a synthesis of public health, law, and economics; a third may call for a synthesis of criminology, political science, and economics. Hence, we should not scramble our analytical tools before we apply them.

The time to synthesize is when we wish to explain or diagnose particular situations or to predict or control particular events. Reality is complex and no single field of knowledge can suffice for grappling with concrete cases. "Application of theory" means, or should mean, synthesis of the findings of several disciplines. But "synthesis in the abstract" serves no good purpose.

[1] *A Reconstruction of Economics* (Wiley, 1950), p. 5.