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ON MATHEMATICAL THINKING IN ECONOMICS

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The interest of scholars in the application of mathematics to the social sciences is particularly lively at the present time. This is especially true in economics, as well as in such allied disciplines as econometrics and economic statistics. And yet, critical works by economists on methodological problems remain rare, as witness the fact that Professor Milton Friedman, in his *Essays in Positive Economics* (1953), based many of his ideas on John Neville Keynes' volume of 1891.[1]

The application of mathematics to the human sciences, and particularly to economics, rests on the implicit assumption that mathematics is an appropriate tool for all the sciences. The widespread adoption of this assumption stemmed from the brilliant successes achieved by the use of mathematics in physics, consisting mainly of accurate predictions of events taking place under controlled conditions. It is often overlooked, however (particularly by non-physicists), that these successes scarcely imply that every type of prediction of events can be attained by mathematics. In fact, events can only be predicted when the physicist can reduce and simplify them so as to correspond with a mathematical formula, capable of a calculable numerical solution.

Suppose, for example, that one tries to predict the movement of water contained in a glass when subjected to oscillations of any given magnitude. To answer this problem, we would first need to demonstrate that the system of differential equations involved in the problem has one and only one solution. Otherwise we could not discover a numerical solution. Such a solution, in our example, would only be possible if (a) the oscillations to which the glass is subjected are so slight as to allow the system of differential equations describing the movement of water to be *linear*, *and* if (b) the shape of the glass allows actual numerical calculation of the solution.

Devising a system of mathematical relations governing a phenomenon has no scientific or predictive use until a numerical solution can be obtained, i.e., until the theorems of existence and the methods of approximate numerical calculation have been established. Such a solution may only be considered satisfactory when it predicts successfully in experimental tests. Only in that case may the system of mathematical relations from which the solution derives be considered efficient.

Such is the essential role of mathematics in the science of physics. The example of physics therefore suggests that, in order to apply mathematics to a science, certain preconditions must be fulfilled, conditions which may or may not be the same as the prerequisites for applying mathematics to physics.

It would be quite meaningless to rest content with vague categorical statements that mathematics may, or may not, be properly applied to economics. We must inquire in greater de-

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tail into the titles of legitimacy, into the advantages and disadvantages, of such mathematical instruments as numerical utility theory or analysis of indifference curves, of cost curves or of various maximum and minimum constructions. To answer these questions, it is necessary to examine the often inarticulated goals that mathematical economists have in view. If we examine the works of mathematical economists, and consider that these applications to economics have been suggested by purported analogies to physics (as is revealed by such generally adopted terminology as "statics", "dynamics", "equilibrium", etc.) then we may conclude that their goals are the description of phenomena and the successful prediction of events. In this respect, their ends are similar to those of the logical economists, with whom they join against those who deny altogether the possibility of a scientific economic theory and who reduce economics to a simple history of economic phenomena. But while the prediction employed by the logical economist is *qualitative* in nature, the predictions of a mathematical economist must, to be justified in the manner of physics, be *quantitative* and numerical. And in a scientific theory, the validity of its description of phenomena must depend on the accuracy of the predictions which it allows.

We must now consider how the events to be predicted present themselves in physics and in economics respectively, and what is the proper method for their prediction. In most cases, the empirical determination of events in physics is performed by applying the classical procedure for measuring magnitudes. Such measurement makes it possible to select an array of numbers which fully represent the event considered. Each measure of magnitudes postulates a rigid scheme of operations the result of which must never depend upon the time or place of the operation, or on the opinions and interests of the operator himself or of anyone else. Therefore, this measurement of a given magnitude is always repeatable with identical results, and in this sense one can say that the measurement of a magnitude is *objective*. The empirical operation of repeated measuring, in fact, itself implies identical results, modified only by small variations according to the well-known "theory of errors", which we can set aside for the purposes of our inquiry.

In order to measure a magnitude, we must first choose a *unit* of measurement homogeneous with the magnitude itself, and for which we can postulate invariability, at least as far as the measurement is concerned. We must be able to verify for the magnitudes in question the formal properties of the equalities (reflexive, symmetric, and transitive), the inequalities (irreflexive, asymmetric, and transitive), and the trichotomy (given two terms *a* and *b*, *a* must be equal to, less than, or greater than, *b*). We must define a method of constructing whole multiples and submultiples of the unit of measurement, which is generally, at least in classical physics, a real number or a multiple of real numbers.

From the postulates fixed for the magnitudes there is derived a law of proportionality between the magnitudes and the numbers which measure them. Physics is essentially founded upon magnitudes; it discovers mutual relationships between magnitudes and their variations, and it therefore fits into the universe of mathematical discourse of which the theory of magnitudes (particularly the theory of real numbers) is a part. The compelling nature of the mathematical method in physics appears to be correlated with the regularity and repeatability of observed physical events.

In the realm of economics, a series of events considered to be objects of prediction, appear to be represented by the numerical *prices of* goods in a monetary market economy. Beside these prices, such other real numbers come into play as (in the case of "quantifiable" goods) the number of quantities of goods sold at a particular price. Therefore, in economics as in physics, arrays of numbers may appear to represent given events (e.g., the exchange of given goods at given times and places.)

Moreover, the numbers representing the quantities of goods exchanged may be determined by the methods of measuring magnitudes or by the ordinary rules of using cardinal numbers. As far as price is concerned, the analogy with physics has induced some writers to consider price as the measure of value or of the utilities of goods, and therefore to advance to an even closer analogy between physics and economics. Let us, therefore, consider some aspects of the determination of the prices of goods in the market in relation to what we have previously noted about the measurement of magnitudes.

In the determination of price on the market, we cannot postulate a rigid scheme of operations of the operator. Furthermore, the process of establishing market price, being dependent upon the opinions and interests of buyers and sellers, can clearly *not* be independent of the time and place at which prices are set. The market price of a given good will therefore not necessarily be the same from one observation to another, varying as it clearly does according to historical times and places, and according to the opinions and interests of the relevant operators. We may in this sense declare that the operation of establishing a market price is poly-subjective and non-repeatable. This operation requires: (a) a commodity or service the price of which is to be established, and which is generally, but not necessarily, quantifiable according to the rules of the theory of magnitudes; (b) money, i.e., a numerable set of elements all equivalent to each other, but not homogeneous with the service or commodity to be purchased, and in terms of which the setting of prices takes place; and (c) an historical environment (the market), and at least two operators (one buyer and one seller) who transact the exchange. No law of proportionality can be said to exist between the quantities of exchanged goods (where the goods are quantifiable) and the amount of money for which they are exchanged. (In fact, it happens that prices for large quantities of goods are usually different from the prices of small quantities of the same goods.) Moreover, knowledge of the above-mentioned elements (a), (b), and (c), even when complete, is not sufficient to allow prediction of the price. For we lack an efficient scheme for predicting the behavior of any operator based on his opinions; and we lack also an efficient method of connecting the behavior of any single operator to the behavior of others, and therefore of predicting changes in the various opinions and actions in the process of establishing prices.

We may, therefore, conclude that there is a profound difference between the operation of measuring and that of establishing prices, and that it is unjustifiable to consider the prices of given goods as the measure of the values or utilities of those goods. These differences are connected with the fact that a price in an exchange can only be established if the value assigned by the buyer to the goods he buys is greater than the value to him of the money he pays, *and*, correlatively, if the value to the seller of the goods that he sells is smaller than the value to him of the money he receives in payment. If this double inequality of values did not exist, neither of the two operators would find it profitable to make the transaction, and no exchange would take place—or price be established. Thus, the operation of arriving at prices on the market cannot be considered as also determining a measurement of the values of the exchanged goods. And furthermore, we cannot consider these values to be matters of objective judgment. A judgment of values appears to be defined as a demonstrable inference from observed choices: commodity A has a greater value than commodity B, when commodity A is chosen before commodity B. An exchange of goods between the two operators may, then, only take place when the choice of one is contrary to the choice of the other. It is therefore obvious that judgments of values—understood as assigned orderings of values of the exchanged goods—will depend essentially upon the particular individual who makes the valuations. If one speaks of a "value", one must therefore indicate what operator is doing the *valuing*; values must never be spoken of as absolute, i.e., without referring to an operator. What we have said may be briefly summarized by stating that it makes no sense to speak of an "objective value".

Some of these difficulties are abated in Crusoe economics, where there is only one operator in society, and therefore all choices are made by him, so that the ordering of values is determined by Crusoe's choices only. However, even in this case, the possibility of introducing a mathematical theory of values or of utility seems doubtful. If we put a given operator before a set of goods or a set of bundles of goods, in order to construct the concept of abstract utility, we must know that the operator, whenever confronted with bundles of goods, is always able to give one and only one of three judgments: (1) Bundle A is preferable to bundle B; (2) bundle B is preferable to bundle A; or (3) bundles A and B are equally preferable—that is, the operator is indifferent (*not* uncertain) between the two bundles of goods. This, however, is an empirical assumption, which need not necessarily be valid in practice. For the operator may be *uncertain* as to his preferences for the goods, and these uncertainties will be the more numerous the more the operator's judgment involves his entire economic behavior. But if the operator, who corresponds to the measuring instrument in our analogy with physics, is uncertain, then so must be the economist, who corresponds to the physicist using the instrument. The economist will be in the position of a physicist who does not know if the measuring instrument he is using is in working order.

Nor is uncertainty the only difficulty we face in constructing the concept of utility. For another prerequisite of such construction is that the operator must behave according to the formal rules of the equalities and inequalities; otherwise we could not construct a proper ordering of the utilities. One great difficulty is that the operator's choices between commodity A and commodity B, between commodity B and commodity C, and between commodities A and C, necessarily take place at three different times. The three concrete choices, therefore, may happen to be the results of three *different* sets of value-orderings. In fact, all the choices made during the lapse of time between the first and the last of the aforementioned choices, will modify the economic situation of the operator, and may therefore lead him to modify his opinion of the economic situation and hence to change his value-orderings. And his choices will depend not only upon the goods which the operator is considering, but also upon his opinions of his situation at the moment of his choice. Therefore, the operator's choices could not only defy the transitivity rule, but also could never reveal judgment of preference *independent* of the particular moment in which the judgment occurred. We must here refer again to what we have said about the problem of inadequacy of instruments of measurement.

Setting aside the foregoing considerations, we may now continue to examine the process by which some mathematical economists, starting from the aforementioned theory of choices, mathematically construct a numerical utility, while others formulate the so-called "theory of indifference curves".

If we assume that the economic operator will behave according to the necessary rules (and we have seen that this cannot be demonstrated, and is strongly open to question), it seems possible to take the goods and bundles of goods which the operator has judged to be equally preferable (i.e., indifferent), and to make subsets of them so that all elements within each subset will be equally preferable. It then seems possible to order these subsets by saying that one subset will follow another in which *an* element (and therefore *all the elements) is preferred to an* element (and therefore to *all*) of the other subset. This ordering obviously implies transitivity, which was also implied in the analysis of choice. Some mathematical economists, notably von Neumann and Morgenstern and their followers, after having defined these ordered subsets, construct a concept of abstract utility, by defining as utility the category common to all the elements of one of the subsets. By imposing on the utilities the ordering of the subsets to which they belong, these authors manage to define the set of abstract utilities (relating to the set of goods and bundles of goods being considered) as a properly ordered set. The possibility of constructing a proper ordering for the set of utilities, however, does not imply that this set may be put into reciprocal, ordered correspondence with the set of real numbers or a segment thereof. In fact, nothing has as yet been demonstrated about the type of ordering of the ordered sets of utilities. But here the above-mentioned authors meet the problem by introducing a new set of postulates. They consider two goods or bundles of goods, A and B, with two different corresponding utilities u (A) and u (B), and imagine it possible to consider as a *commodity* chosen by the operators, the possibility of obtaining *either* the commodity A

with probability x, *or*; alternatively, commodity B with probability 1-x. After having postulated this possibility, they assume that for any two goods whatever A and B, where $u(A)$ is less than $u(B)$, and where x may be any real number between 0 and 1 (extremes excluded), the following propositions are valid:

(1) $u(A) \qquad x\,u(A) + (1-x)\,u(B)$

(2) $u(B) \qquad x\,u(A) + (1-x)\,u(B)$

and, moreover, that for any commodity C, there is one x for which it is possible to write:

(3) $x\,u(A) + (1-x)\,u(B)\,u(C)$, if $u(A)\,u(C)\,u(B)$ and, finally, if $u(A)\,u(C)\,u(B)$, that there is one y for which:

(4) $y\,u(A) + (1-y)\,u(B)\,u(C)$.

Finally, they assume (5) that the combination of above-mentioned relations satisfies the ordinary rules of the calculus of probabilities.

In this way they conclude that it is possible to let the set, or a segment of the set, of abstract utilities correspond to the set or a segment of the set of real numbers, up to a linear transformation, and they thus manage to determine what they call "numerical utility". But by introducing an equivalence between goods that are already certain, and the mere possibilities (each with an assigned numerical probability) of obtaining alternatively two other goods, they have forced the operator making the choice into the mould of a gambler. This imposed assumption must be treated with extreme caution; for can it be empirically verified for the behavior of the usual economic operator (i.e., the man making the choices from which is constructed the ordered set of utilities)? The assumed operator is both a *speculator* (in the sense meant by Professor Ludwig von Mises); i.e., a man who chooses between goods in order to arrive at his preferred goals, and a *gambler*, i.e., a man who accepts alternatives dependent entirely upon chance. On the other hand, von Neumann, Morgenstern, and their followers assume that the assigned probability of the alternatives is not subjective but statistical, and they therefore imply that the operator is somehow correctly informed about the size of the probability and accepts it as given. But it is not proven that these implications correspond to empirical reality, since the operator may have no trust at all in the probability data which are hypothetically supplied to him. He may thus construct subjective probabilities of his own, like the gambler who bets on the roulette numbers which have not come up at all during the evening. Furthermore, the hypothesis that the probability data must have been previously supplied to the operator to permit him to choose between alternative goods, makes all the more tenuous, if not impossible, the empirical validity of this method of constructing numerical utility.

Finally, the further assumptions, (1 - 5) appear gratuitous; we are not told the conditions under which the decisions of the operator would be compatible with these assumptions. These hypothetical postulates appear to be a surreptitious introduction of the very *possibility* of representing utilities by real numbers, which should have been *deduced from* the postulates themselves.

The authors of this theory seem to have been unaware of these fundamental objections, perhaps because they were too fascinated by the elegance of their theory to pay sufficient attention to the empirical validity of the proposed method for calculating utilities. They have not asked whether their constructs fit the actual behavior of the economic operator. This *insufficient attention to the workings of their* theory prevents us from using the theory to predict the economic choices of individuals. The actual cases studied by the von Neumann - Morgenstern theory are extremely simplified hypotheses that cannot be applied to actual economic problems. If we wanted to follow von Neumann and Morgenstern in their favorite comparison with the problem of measuring temperature, we could say that they have constructed a logically perfect theory of temperature, but have failed to construct any instrument for measuring temperature. The problem of numerical measurement of utility can only be solved when such an instrument has been constructed.

On the other hand, the von Neumann-Morgenstern theory is exempt from many of the naive postulates and logical and mathematical transgressions committed by preceding theories of utility, and it has therefore merited detailed consideration here.

Let us now examine the theory of "indifference curves", which no less than the theory of numerical utility, presupposes what we have dicussed about the theory of choices, including the construction of equally preferable subsets of goods and bundles of goods.

In order to construct the theory of indifference curves (perhaps better termed "indifference classes"), we must construct a space for the set of goods and bundles of goods which we have defined above as the field of possible choices of our hypothetical operator who, as we have repeatedly noted, must correspond to the measuring instruments of physics. Now this very operation of constructing a space is confronted with a serious difficulty: for it has not been proved that the goods forming this set K, by their different quantities and combinations, are all measurable by real numbers. For some goods, the quantity will only be expressed by whole cardinal numbers (e.g., such indivisible goods as automobiles). Other goods will be difficult to express by any numbers, because they can only be conceived as present or not present (e.g., health, life, honor.)

Setting aside this difficulty, and including these goods, a given element of the set K will be represented by: (a) an ordered set of real positive numbers (for which the power, P^1, cannot be established), i.e., a real number for each measurable good, or (b) a further ordered set (for which it is again not possible to establish the power, P^2) of cardinal numbers, i.e., cardinal numbers for each of the indivisible goods, and either 0 or 1 chosen as the number to depict each of the goods that can only be conceived as present or absent. This construct is only valid if we leave out of account all non-quantifiable goods, such as the tang of the air, the respectability of a man, etc.

If we could also exclude indivisible goods from the set, then our problem could easily be solved by letting each element of the set K correspond to a point in a P^1- dimension Euclidean space (provided that the power P^1 is finite). But since indivisible goods are indisputably present, we cannot see a way to construct a space for the set K, a space which would have to be topological in order to define indifference classes in that space. This criticism renders invalid the construction of the entire theory. Moreover, if we assumed it possible to construct for K a topological space with a definite whole dimension q, we could not assume it possible to represent the subsets of equally preferable goods in classes of q-1 dimensions. To demonstrate such a possibility, we would have to verify assumptions every bit as restrictive as those needed to construct a numerical utility. We therefore cannot conclude that such hypothetical assumptions would fit the real behavior of the operator—who seems to be burdened with so many complicated and difficult operations. If it were possible to construct indifferent classes, we could then also determine an index of utility that would be constant over an indifferent class, and would increase when passing from one indifference class to a more highly-valued indifference class. Such an index of utility would be considered a measure of utility up to increasing transformations.

Thus, when we attempted to construct a numerical utility, we had to face the grave difficulties involved in choosing between given goods and probable alternative goods. In the present case, we must overcome the no less critical difficulties of constructing a space for the set K and the difficulties of constructing indifference classes.

We may conclude this analysis of utility theory as follows: to obtain a theory of numerical utility, we must construct a topological space for the set K to obtain a theory of indifference classes. And even if we admit such a theory of indifference classes as valid, we must also introduce postulates about choices between given goods and probable goods. Both the theories of indifference classes and of numerical utility rest on mathematical concepts that are essentially different from any validly empirical view of utility. The adherents of the mathematical theory of utility have not justified their substitution of a mathematical construct for the processes occurring in the real world. The utility which we have called "empirical" is that which governs the actual behavior of human operators, with perhaps the undoubtedly rare exception of those who purposely commit themselves to following postulates of the mathematical theory. At best, mathematical utility theory will only permit us to predict the behavior of these peculiar operators.

The mathematical economists, furthermore, have not sufficiently defined the purposes of constructing their theories of utility, and have not adequately explored the semantic problems involved. If we resumed the comparison with physics, we could say that modern mathematical physics is strictly connected with the impressive construction of experimental equipment. On the other hand, this development has compelled physics to move within the narrow grooves imposed by the range of possibilities of that equipment. As a result, physics has only been able to make successful predictions when the physicists have been able to arrange events in advance. In short, physics has proved incapable of predicting with sufficient accuracy such natural phenomena as the courses of rivers, the phenomena of meteorology, of biology, etc. As far as economics is concerned, the attempts to apply mathematics have not yet involved any creation whatever of experimental equipment to evaluate relevant data translatable into mathematical language. If robots to determine such data were one day constructed, we could still only predict the behavior, not of men, but of the robots themselves—unless we wanted to, and could, transform our human subjects into robots. Unless such transformation be the goal of mathematical economics, such mathematical theories merely encourage the illusion that they may someday predict the behavior of individual human beings in the real world.

It could be objected that calculations based on mathematical utility theories allow at least approximate prediction of economic events. But then, the mathematical economist must provide a theory allowing us to *calculate* the approximation by which each forecast will be successful. If they do not do so, they are hardly in the usual position of scientists using mathematical tools.

In the case of numerical utilities, we are confronted with the problem of comparing the choices assumed by the above theories with the choices made by actual individuals in the real world. We also face the problem of determining, according to an elaborated theory of approximation, the minimum percentage of verified cases. Solutions of these problems must precede any attempt to apply the theory.

Analogous considerations are suggested by such other devices of mathematical economics as demand curves, cost curves, and the like. Let us consider, for example, how the "cost curve" is defined. A producer is assumed to be making a given, measurable commodity. Let us call q the quantity of this commodity which the producer could eventually supply in a given length of time. $c(q)$ is the "total cost", expressed in a given monetary unit, necessary to producing the quantity q in the unit of time. If we let q vary between zero and q' (which we define as the highest quantity producable by the given, existing, plants), and if we assume to know the cost function $c(q)$, and if we represent the consequent relation on a Cartesian diagram, we then obtain a "cost curve" for a given producer of that commodity. If the total cost $c(q)$ is a function of producing the quantity q, then such a cost curve can be constructed, which means that whenever we put q in the interval between zero and q', one and only one $c(q)$ can be determined. Superficially, this condition seems easily satisfiable. Suppose, however, that we inquire how c has been determined as a function of q. We then begin to confront some difficult obstacles. How is the producer going to arrive at his cost function? If he cannot predict it by *a priori* calculation, he must try to determine it experimentally, by trial and error.

We feel that we can exclude completely the possible existence of a producer so devoted to the requirements of economists that he will perform all the experiments required for construction of the cost curve. On the other hand, we cannot hope for the existence of a firm purposely devised for conducting just such experiments. In fact, such a firm would be useless: for the costs of production will depend on the costs of the particular raw materials, the particular equipment, the special geographical location, the particular employment of man-power, the particular wages paid, the particular interest rate, in each particular firm—not to mention the particular production cycle and administrative and commercial organization of each firm. Therefore, any data drawn from the costs of a given experimental firm will hardly be useful for determining the costs of any other firm.

If, on the other hand, we would rely on *a priori* calculation of costs, we are confronted with problems no less difficult than the ones cited above. In fact, no necessary technological calculation could predict the behavior of given equipment under any new rhythm of production, of any new amortization rate, etc. Neither could such problems as supply or finance be precalculable within any useful approximation.

Thus the problem of the actual construction of the cost curve remains an open question, and one which must be solved if we wish to employ mathematical economics. And while a curve may be constructed for a given set of circumstances, we must then change the curve as soon as circumstances change. If the general set of possible circumstances could be unequivocally represented by a definite set of real numbers, we could try to determine the cost as a function of several variables, $c(p,q,r,s,\ldots\ldots\ldots w,z)$ where $p,q,r,s,\ldots\ldots w,z,$ are the parameters representing the general set of circumstances. But since it has not been proved that the general set of circumstances may be represented by a definite number of parameters, we must conclude that c cannot even be considered a function of several variables including q, much less of q, itself. If there is a correlation between c and q, we cannot simply assume that this correlation is one of functional dependence of *c and q*. Therefore, we cannot legitimately represent $c(q)$ by a curve or set of curves. And, moreover, if the correlation between cost and quantity of production is not functional, then we cannot apply the rules of the differential calculus, so popular among mathematical economists.

It could be objected that construction of cost curves is a working hypothesis legitimated by experimental verification of inferences from this hypothesis. But such verification can never be attempted without previously defining the function represented by the curve, and we must therefore begin by establishing the laws for constructing the curves. This task has not yet been accomplished. Furthermore, as in the case of utility theory, the mathematical concept of *function* is being substituted for the empirical concept of *correlation* without adequate justification. The semantic confusion between correlation and function is probably caused by the fact that the word "function" has, in ordinary and mathematical languages, two different meanings. Furthermore, the rules of employing the word "function" are not comparable in the two languages.

The same confusion has probably reigned between the use of curves by non-mathematical economists (understood as non-quantitative symbols of the trends or movements of economic phenomena), and by mathematical economists (understood as graphical representations of mathematical functions). While the use of curves in the former sense may be justified, if their meaning is so defined, the use of curves by mathematical economists confronts the grave difficulties stemming from confusion of the qualitative trends of phenomena with mathematical functions. This semantic confusion encourages the illusion that mathematical calculation can yield us descriptions of the empirical phenomena. A similar confusion pervades the other types of curves employed by mathematical economists.

Other doubts about mathematical economics concern its use of the maximum and minimum concepts to solve mathematically framed economic problems. In mathematical physics, the formulations of maximum and minimum are the precise and rigorous results of the theories to which they belong, and are not simply a translation of presuppositions from ordinary experience into the language of mathematics. Furthermore, the usages of the terms "maximum" and "minimum" in mathematics and in ordinary language are so different, that the terms cannot simply be used interchangeably in the two cases. In ordinary language, the concepts of maximum and minimum are not only indeterminate, but incapable of determination. For example, such statements as "the most beautiful painting of painter *X*", "the finest woman in Manchester", "the most intelligent economist in America", or, indeed, "the maximum utility" or the "maximum profit", do not actually define the element which seems to be defined. The meaning of the qualifying adjective is also not precisely defined, and it is therefore not always possible to assign an unequivocal meaning to a comparison, or to a relative or absolute superlative. In mathematics on the other hand, the trichotomy postulates the relations of inequality, keeping us from uncertain judgments, while the transitivity of the two inequalities enables us to formulate the problem of the maximum rigorously and to demonstrate the theorems of existence or non-existence of maxima and minima.

It is often claimed that translation of such a concept as the maximum from ordinary into mathematical language, involves an improvement in the logical accuracy of the concept, as well as wider opportunities for its use. But the lack of mathematical precision in ordinary language reflects precisely the behavior of individual human beings in the real world. Furthermore, it has not been proved that economic operators can or will ever change their behavior to suit the requirements of mathematical precision. Translation of the words of ordinary language into mathematics is therefore not necessarily the most suitable way of dealing with the empirical problems of human beings in the real world. We might suspect that translation into mathematical language by itself implies a suggested transformation of human economic operators into virtual robots. But in that case, mathematical economics would not be a science, but rather a set of vaguely-defined normative rules of behavior, the possibility of which has not been demonstrated, and for the sake of goals which have not even been revealed.

We are reminded here of Macauley's comment on utilitarianism: "When we see the action of a man, we know with certainty what he thinks his interest to be. But it is impossible to reason with certainty from what *we* think to be his interest, to his actions." Despite John Stuart Mill's supreme contempt for Macauley, we know that Mill finally admitted the truth of several of Macauley's criticisms.[2]. The problem with which Macauley confronted the utilitarians faces us still, and one wonders whether the mathematical economists of today, in their own contempt for critics, are any better armed against their criticisms than were the utilitarians against the similar structures of Macauley[3].

NOTES

1. John Neville Keynes, *The Scope and Method of Political Economy* (1st Ed., London, 1891; 4th Ed., New York, 1955).
2. See T. Rees, "A Note on Macauley and the Utilitarians", *Political Studies*, IV, no. 3 (October, 1956).
3. In his extremely valuable book, *The Failure of the 'New Economics'* (Princeton, 1959), Henry Hazlitt subjects the use of mathematics by Keynes and other economists, to a series of strictures very similar to those which had been expounded in this paper. For Hazlitt's criticisms of the concept of mathematical function, cf. *ibid.*, pp. 46, 98 ff., and 290 ff. The author characterizes supply and demand curves as mere "analogies, metaphors and visual aids to thought...which should never be confused with realities", and which do not permit any mathematical calculation. *Ibid.*, p. 102. On the pretentious and misleading use of pseudo-mathematical equations in economics, Hazlitt writes: "And if a mathematical equation is not precise, it is worse than worthless; it is a fraud. It gives our results a merely spurious precision. It gives an illusion of knowledge in place of the candid confession of ignorance, vagueness, or uncertainty which is the beginning of wisdom....But we may go much further in our criticism. Even a merely hypothetical equation may be worse than worthless if there is not only no initial evidence that the posited relationship is true, but no way of determining whether it is true. A mathematical statement, to be scientifically useful, must, like a verbal statement, at least be *verifiable*, even when it is not verified. If I say, for example (and am not merely joking), that John's love of Alice varies in an exact and determinable relationship with Mary's love of John, I ought to be able to prove that this is so. I do not prove my statement—in fact, I do not make it a whit more plausible or 'scientific' —if I write, solemnly, let X equal Mary's love of John, and Y equal John's love of Alice, then

$$Y = f(X),$$

and go on triumphantly from there." *Ibid.*, pp. 99 - 101.

Further references suggested by Hazlitt include:

J. E. Cairnes, *The Character and Logical Method of Political Economy* (London, 1875), Preface to 2nd edition, as the most uncompromising classical attack on mathematical economics, and Ludwig von Mises, *Human Action* (New Haven, 1949), pp. 347, 354, and *passim*, for the most uncompromising modern attack. Also cf. George J. Stigler, "The Mathematical Method in Economics", in *Five Lectures on Economic Problems* (London, 1949).