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Limits to the Uses of Mathematics in Economics

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This paper will be included in a monograph on _Mathematics and the Social Sciences_ to be published by the American Academy of Political and Social Science. The volume will deal with Economics, Sociology and Political Science. There will be, for each of these, one paper on the uses of mathematics and one on the limitations.

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by Oskar Morgenstern

I. Meaning of the Question

While some of the profoundest insights the human mind has achieved are best stated in negative form, it is exceedingly dangerous to discuss limits in a categorical manner. Such insights are that there can be no perpetuum mobile, that the speed of light cannot be exceeded, that the circle cannot be squared by using ruler and compass only, that similarly an angle cannot be tri-sected, etc. Each one of these statements is the culmination of great intellectual effort. All are based on centuries of work and either on massive empirical evidence or on the development of new mathematics or both. Though stated negatively, these and other discoveries are positive achievements and great contributions to human knowledge. All involve mathematical reasoning, some are, indeed, in the field of pure mathematics, which abounds in statements of prohibitions and impossibilities.

Now it does follow from this observation that it is a worthy attempt to try to formulate some of our knowledge by negations. The above mentioned are, as far as we can see, categorical, i.e., should one of them ever be reversed, a fundamental upheaval in our body of scientific knowledge would be the consequence. Should it ever be possible to show that the speed of an object can go beyond the present known constant value of the speed of light, relativity theory would fall and with it most of modern physics. Similarly with the laws of thermodynamics, expressed in the statement about the impossibility of a perpetuum mobile. As for the mathematical impossibilities the situation is such that probably no-one expects ever to see these theorems negated. In general, in mathematics we had been conditioned to assume that a problem can either be solved or not solved, where the latter may mean: not _yet_ solved, or not solvable in principle (with given means).

Yet Kurt Gödel's great discovery of 1931 has shattered this belief. He has proved, broadly speaking, that there exist, under certain conditions, theorems in mathematics of which it can be proved that they can neither be proved nor disproved. Their truth is undecidable within a given system. Thus the situation is, indeed, very complicated and must be seen as such.

In the light of these preliminaries the question of the limits of mathematics in economics must be approached cautiously. One is inclined to dismiss the question a limine. The reason would be that looking at past uses of mathematics in economics, one can see limitations whose nature can be described to some degree. The limitations arose mostly because a faulty economic model was set up and analyzed mathematically or because mathematics was simply used in an inadequate if not even incompetent manner. But it is virtually impossible to infer from earlier mistakes that mathematics is useless or if it has now certain uses, that their limits can be foreseen at present. I am greatly tempted to abandon the entire matter with the remark that the proof of the cake is in the eating which, applied to the present issue, means that there is no other sensible approach but to let mathematics be used to the fullest extent and observe results. In other words let the point (if it exists) be discovered by evolution, the point from whereon there is no further room for more mathematics in economics. If looked upon the question in this manner — which is clearly unassailable — then nothing can be said, except that I would add that no such point exists, since both mathematics and economics will always undergo great changes and developments. These will, in fact, to a high degree be intertwined, an illustration of which will be provided below (Section 7). We would have to foresee the entire future development of mathematics and economics in order to give meaning to this question. None of these two fields has arrived at the state where nothing more can be said. It would simply be absurd to follow this line of thought.

The question is also imprecisely stated; it is itself too limited. What mathematical uses are meant? Where does mathematics begin? End? Is arithmetic considered, or calculus, or topology? Some of these parts of mathematics are quite separate from each other and if one part is no longer useful, perhaps another one is or — most important — perhaps altogether new mathematics has to be invented in order to cope with the manifold forms of economic problems. If this be the case — as I believe history will show — then nothing at all can be said until these developments have occurred and consequently nothing can be said about limitations. This view gives, however, some clue already about a certain kind of limitation of uses of mathematics: it is not likely that the cause of economics will be significantly advanced by relying on the mathematics already "on hand." Though this latter kind has happened on rare occasions in physics, there is no guarantee that there will be a repetition for physics or the social sciences. On the contrary, all signs point in the opposite direction.

2. Positive Statement of Question

To eliminate some apparent obstacles to the uses of mathematics which are still frequently mentioned, I shall state a few properties of mathematics by implication: There is no limitation because economics deals with psychological entities, sometimes with non-quantitative data, with expectations, or with mixtures of qualitative and quantitative entities in the same argument. Mathematics is not a science of quantities only; it does not require measurement, there is no fundamental difference between a simple addition with integers and that expressed by an integral. Mathematics does not necessarily need symbols other than words which, up to some degree of complication, can adequately express mathematical ideas, state theorems, formulate proofs. Mathematics is not only a deductive science, it also uses (logical) induction for proof.

Thus there is no point looking for alleged limitations of mathematics which derive from a misunderstanding of the character and hence the power of mathematics, on the one hand and on specifics or pecularities of economics on the other. Yet the literature is full of statements against the uses of mathematics based on exactly these and other misunderstandings. Among them is the idea that mathematics could not be used to analyze psychological factors in value theory, that human nature cannot be described mathematically, because it is "too rich" to be represented, that though curves or other graphs may be appropriate, the use of exactly the same information in the form of equations is unwarranted, that there is no infinite divisibility of goods and "hence" no use of differential calculus, that utility is not measurable, that people do not behave rationally, etc. All these "objections" lead to trivially false statements which need no refutation because they reveal nothing about the issue under consideration. They merely show that their proponents do not know what mathematics is about and how a mathematical model of economic phenomena is to be constructed. It is so easy to mistake one's own limitations for those of the method or the subject matter. In more recent literature such views are less frequently encountered, but when they re-appear — as for example in some current criticisms of game theory — they have lost nothing of their inappropriateness. In earlier decades they have successfully contributed to slowing down the development of mathematical economics.

Once the nature and power of mathematics is understood the only possible statement is that as far as we understand mathematics now, there is nothing in the nature of that science which would exclude its decisive use in economics because of some peculiarities of the latter that mathematics could not overcome in principle. If we were to ask today what the limitations of mathematics are in physics, both mathematicians and physicists would be baffled by the question, brush it aside as meaningless and go on with their work. It could not be performed without mathematics being used to an ever increasing degree. The fact that this question is not asked is a sign of the maturity of physics and a consequence of the tremendous success mathematics had in developing that science — nay, indeed, in developing together with physics.

If there is nothing in mathematics that would preclude its use in economics — or more generally in the social sciences — perhaps there is some deep, hidden peculiarity of the latter to make a marriage of the two fields hopeless. Thus we shall have to inquire briefly into the historical development in order to see how the basic question can be given appropriate meaning.

3. Historical Evidence

It would be surprising if the use of mathematics in any new field were spectacularly successful and encompassing from the outset. The late start of economics found a well-developed science of mechanics; indeed, Newton's crowning great work was already completed before the physiocrats began writing. Little wonder, therefore, that the emerging mathematical economists looked at mechanics and analysis (i.e. the differential and integral calculus) as the model and tool with which to approach the economics problem. When they did, professional mathematicians, up to the last 30 years or less, mostly scoffed at the triviality of these mathematical applications, triviality compared to what went on in physics and compared to what could have been said in plain words. In that the mathematicians were partly wrong; instead of taking a stand-off position they should have become interested in the subject matter and seen its great interest for mathematics. This did not happen until more recent times with consequences to be described. So we have the phenomenon of the greatest mathematicians for centuries working hand in glove with astonomers and physicists and vice versa. But apart from D. Bernoulli's aside on utility in discussing the St. Petersburg Paradox (1730) and a brief remark by Leibnitz (1710) until recently no mathematician of the rank of a Laplace, Lagrange, Gauss, Riemann, etc. had concerned himself in any major way with the social sciences. A. A. Cournot, though very important for his work in economics, was compared to these giants, a much lesser light as a mathematician. This in no way should be viewed as a disparagement of the work of men like Thünen, Edgeworth, Fisher, Slutsky, Evans and others. But the fact remains that until very recently in general professional mathematicians have shied away from economics.

A genuine mathematization of economics not being a trivial matter (*a fortiori* the same applies to all social sciences, as shall no longer be separately mentioned in what follows) earlier, non-professional workers, merely "applying" mathematics, could hardly have been expected to produce startling results. And they have not done so. They have mostly translated into formulae what could be said adequately in words; they have not established new theorems of any mathematical consequence, as far as I am aware. That includes all mathematical economics up to the 1930's. This was evident to those professional mathematicians who have looked at mathematical economics and have found it wanting of mathematical interest in two ways: no new theorems, and no new problems they might find challenging. While the former appears to be true, the latter is not true if the economic problem is properly formulated, in which case it offers formidable challenges. Even when the problem is stated as in the systems of Walras and Pareto there were some deeper mathematical issues hidden beneath the — unproven — assertions of the existence of unique and economically meaningful solutions and of the stability of these systems. Mathematicians were not brought into contact with these aspects early or closely enough to give the whole field the stimulus it deserved.

The originators, having great merit in collecting variables and writing down seemingly plausible equations connecting these variables, were not sufficiently equipped mathematically to solve the problems had they seen them. Instead, they merely asserted the existence of solutions, usually by counting equations and unknowns. Finding their numbers equal made them state, quite wrongly, that this assured the existence of solutions. Ironically, even if they had avoided the mistake of relying on the equality of these numbers — a mistake which it would have been possible to avoid at that time — they probably could not have solved the equations because this required mathematics tools then not available — even when drastic simplifications were made in some underlying assumptions. This task was not accomplished before A. Wald wrote in 1934. If the economists had only stated the problem, seen its difficulty, called for help, mathematicians might have been attracted much earlier to assume the task of inventing the proper mathematics. (This last remark foreshadows observations to be made in greater detail below in Section 7.)

The purpose of mathematical economics is, of course, primarily to advance economics, not to apply mathematics, merely because some mathematics exists and one has learned that science to some extent. Nor is it to find new mathematics theorems *per se*. But it may, and most assuredly will, happen that economics cannot be advanced decisively without proving fundamentally new mathematical theorems. Note, that I say: economics and not "mathematical economics." In other words at some stage of development and at some level of theory abstraction the two may be synonymous.

As far as the use of mathematics in economics is concerned there is an abundance of formulae where such are not needed. They are frequently introduced, one fears, in order to show off. The more difficult the mathematical theorem, the more esoteric the name of the mathematician quoted, the better. Then one is "in." So it happens that statements are proved — laudable by itself, and correctly done — by means of complicated reasoning and use of elaborate machinery, though they can also be proved by elementary means.

This differs from another, truly significant process: sometimes it is exceedingly difficult at first to prove a theorem at all. In the course of time the successful proof may get simplified and one finally arrives, often after decades only, at proofs that are elementary. But such a development represents true, highly significant mathematical progress — always to be hoped for. It occurs in pure mathematics as well as in applied fields; countless examples could be given. So instead of using far outlying mathematical disciplines — pour épater le bourgeois — the economic problems should be treated at their simplest analytical levels, which is no mean task, often involving great intellectual effort.

An illustration of a misdirected effort is offered by the theory of utility. Here the early writers of the 1870's assumed naively that utility was numerical such that certain arithmetical operations could be performed: they assumed a so-called "cardinal utility." Later it became clear that this was a doubtful assumption, that instead only a "greater," "smaller" or "equal" could be stated for two utilities or preferences of the same individual: the so-called "ordinal utility." On the basis of this latter, more convincing, idea an elaborate theory was developed, the "indifference curve" approach. It was mathematically treated and led to a large body of literature to which additions are still being made. The theory is complicated, involved and of some mathematical complexity; or shall I say, clumsiness? In 1944 it was shown by von Neumann and Morgenstern that by taking a more realistic look at the utility problem, especially by realizing that individual preferences have to be stated in the face of uncertain, rather than as formerly, of sure prospects, a number (up to a linear transformation) could be defined for "utility." This numerical property was derived from a set of simple axioms that have found wide acceptance as properly describing the intuitive notion of preference acts. The advantages of being able to deal with utility numerically are considerable: one can now do away with vast amounts of inapplicable and irrelevant analysis, simplify exposition, deal with a more realistic situation and attack problems that remain inaccessible for the more primitive tool of indifference curve analysis. Incidentally, one was also led on to the question of experimental verification and the design of experiments — precisely in an area where the use of exact methods has often been deemed to be impossible. The indifference curve method has not led, though much older, to comparable experimental work. It is by experiments that one hopes to get confirmation and new ideas for better theories. Such interplay, therefore, constitutes a most desirable development.

The significance of this brief remark is that the primary task is to discover the true nature of the underlying economic phenomenon and to concentrate efforts in that direction, instead of stopping short and branching out into the mathematical treatment of an ill-defined and vaguely described situation. The value of possessing a number was at any rate well-known from other sciences. Therefore a great effort to obtain a number would have been justified. The parallel with thermometry is truly significant: the theory of heat could only develop after a physically relevant measurement of the different phenomena of temperature had been designed. Then followed the necessary extensive mathematical treatment, noted for early contributions as significant as Fourier analysis.

Thus, applying this to economics, it is not impossible that, for the basic notion of utility now a number having been established, many more distinctions of different kinds of utility can be made in the future. This would hopefully be followed by corresponding mathematical advances of a theory of utility.

So we see that the limited value of the mathematical treatment of indifference curve analysis does not stem from the use of mathematics but from the limited penetration into the underlying economic problem.

4. The given economic problem

The illustration using the experience with utility has wider significance. It was just noted that in other fields mathematization was inseparable from the development of the corresponding physical theory. We shall explore this occurrence somewhat further.

The invention of calculus occurred simultaneously with the advancement of mechanics and vice versa. This is the most striking and best known instance; there are many others. The lesson is that the economist, whether mathematically oriented or not, has to look first of all at the economic, empirically given problem. He must avoid all those abstractions and generalizations which violate the essence of the reality. He must describe reality by models which are neither too simple (then they are unrealistic) nor too complicated (then they transcend our analytical power). This rule is hard to follow if reality can only be described sensibly by means of a powerful and complex theory when at the same time the logical and mathematical concepts and techniques have to be developed instead of using those that have been successful elsewhere. Thus the eminent Walras and his equally distinguished successor Pareto, identified the economic problem as one in which the firms and consumers in a social economy have to solve ordinary maximum problems. As a consequence, the differential calculus was used to analyze this situation, calculus being the appropriate means for solving a great variety of maximum and minimum problems. According to Walras, Pareto and their followers firms and individuals face fixed conditions (prices, quantities, etc.) and the outcome of their activities depends solely on their own actions (even chance does not intervene!). In technical terms: the individuals are in complete control of all variables in which the strived-for maximum (of utility, profit, growth, etc.) depends. On this assumption rests virtually all of modern economics, whether in the mathematical formulation known as the Lausanne school, or merely cast in words or graphs by others. The resulting theory is one of a general economic equilibrium where forces of demand and supply balance each other, where disturbances bring the economy, by means of appropriate prices and quantities produced, back to the equilibrium. The latter is a condition in which there are neither gains nor losses, in which no individual or firm can improve his position. Note, though, that here a crucial, hidden qualification is made: neither individuals nor firms are allowed to cooperate, to form coalitions, to make side payments, etc. It is precisely this limitation which makes the model fundamentally unrealistic since in economic life cooperation of this kind is of the essence. Walras and his successors thus do not deal with the given economic problems.

This model's lack of correspondence with reality shows up in one basic respect: the fact is that the individuals are <u>not</u> in control of all variables on which the outcome depends. The interference of chance does not matter much,[1] because chance variables can be treated conventionally — i.e. statistically — and the maximum problem could still be salvaged, though now becoming technically much more difficult. But

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[1]However, even chance was not considered in the classical Walrasian model until very recently and quite sketchily, although for it chance would make a large difference. Cf. K. Borch: Equilibrium in a Reinsurance Market, <u>Econometrica</u>, vol. 30, July 1962, p. 424 ff., where this is now fully explored and extended beyond the classical case.

the trouble goes far deeper; it is conceptual: the outcome of anyone's action depends not only on what he does himself, but on the actions of others who sometimes are cooperative, sometimes hostile. The moment even one single variable is <u>not</u> under one's own, but someone else's control there is no way of describing such a situation as a maximum problem, no matter how much the individual may strive for a maximum. Thus the economic problem is conceptually totally different from the way in which it was conceived by Walras — and in fact from all his followers. Economic individuals and firms simply do not face fixed conditions. They act upon each other and for this situation no analogy exists with mechanics as the classical set-up claims. There was no way of formalizing the economic situation until the theory of games of strategy was developed concomitant with the recognition of the nature of the problem. It had not been understood before.

The creation of the theory of games brings a clear break in the development of economic thought which touches at the very roots of that science.[1] Now it is clear that the recognition of this conceptual situation demands the use of analytic tools capable of formulating and solving the problem. They may or may not be mathematical. They may or may not be found in our existing knowledge. As it turned out there was no ready-made tool, it had to be forged and it is by necessity mathematical, assuming the form of von Neumann's famous minimax theorem and the further mathematical theory that has been built upon this foundation. The structure of

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[1] Cf. J. von Neumann and O. Morgenstern: <u>Theory of Games and Economic Behavior</u>, Princeton 1944, 3rd Ed. 1953 and the subsequent literature. Most recent: <u>Recent Advances in Game Theory</u>, R. J. Aumann and M. Maschler, Editors, Princeton 1962.

this theory is consequently quite different from the current neo-classical one of general economic equilibrium. Lacking a new specific calculus, one has to fall back essentially on the fundamental, combinatorical elements of mathematical reasoning. Eventually a new calculus may have to be invented or discovered, as specifically suited to economic-social problems as the differential calculus was to classical mechanics.

The new theory also shows clearly a feature that must be expected in any true mathematization: the mathematical theory must first yield the same insights which can be obtained from common sense. Thereafter it will give results which go far beyond common sense, results which common sense could never even guess at. When this happens, some of the new results may be translatable into ordinary language, but for still others this will become impossible. They will remain in mathematical symbolism. When that point has been passed a higher state in the development of a science has been reached. It is only attainable by means of mathematics. Game theory has already entered this phase of development, while for classical or neo-classical theory no such instances appear to exist. This is another indication of what is involved in the successful use of mathematics. When, on the other hand, mathematics is used in developing a model that does not correspond to reality the limits of mathematics show up very clearly. But these are not limits anyone can profitably be interested in, since they are not limits of mathematics but limitations of the model which mathematics is supposed to help in developing. Even the analysis of the Walrasian system can be pushed to high degrees of mathematical finesse — as is indeed the case[1] — but the value of such accom-

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[1]Here we think in particular of the work of G. Debreu: Theory of Value, New York, 1959, where also references are found to the writings of Arrow, Gale, Uzawa, etc.

plishments ultimately depends on the value of the economic content.

5. Intuitive and Axiomatic Theory

The mathematical development of any science culminates in the axiomatic formulation of its contents. The modern axiomatic method has achieved superb success in mathematics itself ever since David Hilbert first axiomatized geometry in the 1890's. The idea of an axiom and of the possibility of deriving deductions from axioms is, of course, much older. Indeed the technique was already used by Euclid. But it is only since Hilbert that a complete understanding of the method was achieved. Since then it has been used extensively, no longer restricted to mathematics. Various parts of physics are now axiomatically formulated — but not all. In this there is a lesson.

The axiomatic method consists of formulating a set of propositions which must fülfill certain conditions. They must in particular be free of contradictions and the deductions derived from them must contain our knowledge of the field and beyond this, hopefully, lead to new insights. If a part of mathematics is axiomatized, the axioms will be mathematical propositions, for example that through two points in a plane there passes only one straight line, etc. If one is dealing with an empirical field, the axioms will be statements about some part of the real world; for example, one may say that the speed of light is constant. In no case have the axioms any superior truth value to that of their implications as these are brought to light in the deductions which can be based on the axioms. The latter are chosen for reasons of convenience, because they are intuitively acceptable and express in agreeable and perhaps esthetically satisfying form some basic knowledge of the field in question. But they are not self-evident truths as the old and now completely superseded view of an "axiom" asserted. The axiomatic method is simply a superb technique for summarizing our knowledge in a given field and for finding further knowledge deductively. This involves inevitably logico-mathematical operations, sometimes of great complexity. If the state of axiomatization of an empirical field has been reached, which is a state of some perfection, mathematics is indispensable.

This last observation makes it clear that the axiomatic method will lend itself especially to those phases or parts of a science where a clear understanding of the basic concepts has been reached. Axiomatics does not burst upon the scene unprepared. There will have been a vast amount of preparatory exploration and thinking, much of it tentative and in parts. Some will have been in mathematical form, some not.

The principal condition for the advancement of economics is still to improve the empirical background of that science. In spite of mountains of figures generated daily by the economy, "our knowledge of the relevant facts of economic life is incomparably smaller than that commanded by physics at the time when the mathematization of that subject was achieved. Indeed, the decisive break which came in physics in the 17th century, specifically in the field of mechanics, was possible only because of previous developments in astronomy. It was backed by several millenia of systematic, scientific, astronomical observation, culminating in an observer of unparalleled caliber, Tycho de Brahe. Nothing of this sort has happened in economics." [1]

This preparatory work leaves abundant room for intuition without which there would be no science. Intuitive theory exists in its own right. It is the step that leads from historical-statistical-experimental research to the first stage of theoretical interpretation. Even in this development extensive use of mathematics may be required — in economics certainly the use of statistics, a discipline in which mathematics has expanded beyond all expectations with no limit being in sight.

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[1] Theory of Games, op.cit. p.4

Theory of this type at first will often be wanting in exactness, in spite of its mathematical appearance.

The difficulties of formulation are, indeed, formidable. It is well-known and has often been said, that formulating a problem is often already the assurance of its solution. Scientific activity consists largely in asking the right kind of question. There comes a point where economists and mathematicians must get together to do precisely this in order to advance our knowledge.

This last remark gives opportunity to point out a frequently found misunderstanding about mathematically formulated theories. It has been stated that "mathematics has no symbols for confused idess." This is not necessarily true. In fact many confused ideas have been put symbolically — in words and formulae — and this activity continues unabated in all fields of human endeavor! Mathematics is no exception when one considers how long it has taken to establish precise notions even of what a rigorous mathematical proof is, not to mention that for generations ideas of the "infinitely small" were allowed to becloud the very foundations of the differential and integral calculus. Even the notion of a "variable" has come under new scrutiny and the common usage of that concept is severely questioned.[1]

Thus the assertion that mathematical economics is not alwyas as clear and transparent as the layman's respect of the use of mathematics would lead him to believe is certainly not unfounded. The non-mathematician must be told in so many words that there is good and bad mathematics, good and bad mathematical writing and presentation, as

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[1]Cf. in particular K. Menger's recent writings, e.g. "The Idea of Variable and Function," Proc. Natl. Acad. Science, vol. 39, 1953, pp. 956-961 and his book: Calculus, A modern Approach, Boston, 1955.

in all other sciences. The appearance of a formula in a paper or book does not automatically clarify a difficult problem or raise that piece to a higher level of value. On the contrary the formula may obfuscate. I have already commented above on the often unnecessary use of mathematical symbols when they contribute nothing of value to the analysis. But that was under the assumption that their use was nevertheless correct, though it meant a superflous burdening of the exposition with mathematical formulae. Here we see that there are sometimes pseudo-applications. But with the beginning germaine penetration of mathematics into economic these occurrences will become less frequent.

6. Hierarchies

The preceding section has shown that there exists the danger of trying to establish a hierarchy among the various branches of economics. Such has, of course, always been the case: sometimes historical research dominated and theory was looked at askance, sometimes the reverse happened. At the present time there is an unmistakable tendency to consider only a contribution to mathematical economics as a contribution of value. Hence a certain craving to press everything into some mathematical form, even if only to embellish an otherwise important piece of research. This preoccupation appears to me to be unnecessary and possibly dangerous. There is no classifying of the value of the different branches along which economics grows. Of course, whenever and wherever a fundamentally new insight is produced, when a new theory is created which orders and interprets vast amounts of empirical material and the results of carefully designed experiments, a great deed has been done. It may outshine for a long time to come the work of the many who have contributed the building stones. This is all in the nature of scientific progress and will never be otherwise. But in no sense is there much meaning in attributing a higher "rank" to some forms of scientific work than to others. Any desire to establish such hierarchies is a sign of immaturity; thus we do not find efforts in that direction in physics.[1] Older philosophers have sometimes attempted to find similar rankings among sciences, but they have invariably run afoul of later, historical developments.

7. Future Developments

We are now ready for some final remarks about the relation between mathematics and economics and therefore the limitations of these two disciplines.

The principal stimulus to mathematics so far has come from the natural sciences. From the measurement of land — geometry — to Archimedes' problems, from Galileo's study of mechanics to Newton's celestial law of gravitation, physics and mathematics have developed together. This chain continues with spectacular success. It is wondrous that the human race can discover laws of nature, that the latter are so narrowly defined and that they can be described by men who in turn are capable of existing only in the narrowest of confines of temperature, light, and composition of water and atmosphere. Man has barely begun to understand the workings of society, the interaction of man with man, state with state. The big problem is whether economic laws can be found — or have already been discovered — comparable in logical quality with those of nature, though they may be different in structure. If such laws exist they will find mathematical formulation. Of this there cannot be the

[1]It is interesting that the Nobel Prize is usually awarded for important experimental work, for the discovery of new "effects," rarely for a new theory. For example, in order to grant the Nobel Prize to Einstein, he had to receive his award for work on Brownian movement and not for the theory of relativity.

slightest doubt. The laws of nature are written in the language of mathematics as already the ancients understood. The laws of society will be written in the same language; this is what the moderns will have to understand.

There is every indication that in the future mathematics will receive a tremendous stimulus from the social sciences. There are clear indications of this development at present and it is interesting to note that more and more mathematicians begin to look with interest at economic and social problems. More gifted young people turn to economics, attracted by its new life and outlook, most of it involving a far more significant use of mathematics than has ever been the case.

The new period of mathematical economics is primarily associated with the names of A. Wald and J. von Neumann. The former gave an existence proof for solutions of a modified, but still classical Walrasian system of general economic equilibrium. The latter had an original conception of an expanding economy and found its dynamic equilibrium. Beyond this, the theory of games, based on his fundamental minimax theorem, has created new notions, both conceptually and mathematically significant. Both their works and approaches have been extended.

One of the most noteworthy consequences is that parts of economic theory have suddenly become of new practical interest. Economists are giving advice to firms on business planning, business strategy, inventory control, price policy, investment portfolio management, etc. while formerly they had few chances even of looking into these operations. This greater practical relevance of economics for business is also due to the fact that the objectives which business firms try to reach have recently been formulated in an operationally more meaningful manner. For example, the objective can be expressed as the desire to maximize or optimize some precisely stated mathematical expression.

While the pragmatic character of a theory does not necessarily determine its value, applications are of great importance for confirmation and because the researcher is inevitably exposed to new problems giving rise to new scientific work. All this is furthered by the enormous power of electronic computers which puts into the hands of the mathematical economists a practical device such as could not be dreamed of even a single generation ago.[1] The non-mathematical economist is helpless vis-à-vis this tool. He either has to acquire the necessary skills or he must learn how to cooperate with his mathematically trained colleagues. Both procedures are difficult, but both are necessary. This merely repeats the development in physics where the experimentalist must be able to talk to the mathematical physicist though he need not match him in all his skills and vice versa. In economics the computer will also obliterate the artificial borders between historical and statistical research since time series are of the essence; but being statistical series they require delicate mathematical-statistical analysis in order to yield their information. In short we see that mathematics penetrates more and more aspects of economics and that no limits are in sight where this process may stop.

Indeed, continuously new problems of a mathematical character are generated. For example, this happens in routine numerical operations to which economists would now no longer object. Such are the solving of large systems of equations arising, say, from application of linear programming to a concrete case. There one question is whether the hundreds of thousands of numerical steps, cheerfully carried out by the computer, produce a significant set of numbers in the answer, or

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[1] Cf. O. Morgenstern: Experiment and Large Scale Computation in Economics, in Economic Activity Analysis, O. Morgenstern (Ed.), New York, 1954.

mainly "noise," due to the repeated round-off of numbers which inevitably has to be made. This is a deep problem, still much neglected, which raises a host of new questions.[1] Though they are not particular to economics, the economist has to face up to them and is thus pushed into an additional area of mathematical considerations. Mathematical economics, so far, does not yet contain a trace of it though many large-scale computations are being performed. This problem did not even arise twenty years ago. More problems will follow.

It is clear how this example alone indicates the impossibility of stating any "limits" to the use of mathematics. No one, writing before the advent of computers would have foreseen this situation and the need for economists (and all theoretical scientists) to occupy themselves with these intricate problems. Indeed, there was no need to worry about it in pure mathematics either. The two developments went hand in hand and this, in a minor way, again proves the point of the interaction of man's exploration of the physical world and the development of mathematics.[2] Just as mathematics has profited from having being tied so closely to the physical sciences, mathematics will benefit from becoming deeply involved with the problems of the social world.

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[1]There is also the question of the nature of the data used as inputs. Cf. O. Morgenstern: <u>On the Accuracy of Economic Observations</u>, 2nd Rev. Edit., Princeton 1963.

[2]This is shown nowhere more clearly than in the beautiful paper by J. von Neumann, "The Mathematician," in <u>The Works of the Mind</u>, ed. by R. B. Heywood, Chicago 1947, reprinted in J. von Neumann, <u>Collected Works</u>, vol. I, Oxford and New York, 1961.