Economics & the Real World

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Preface

> [It is my]. . .belief that in the present period economics as a practical art is ahead of economics as a science. At this stage most of us prefer the advising of government economic policies entrusted to the experienced intuitive economist.
>
> Koopmans, *Three Essays*

My work on this subject was provoked by certain aspects of my experience over the last 40 years. I learned that the economic analysis that is actually useful for guiding government decisions is fairly basic but highly effective when supplemented by good judgement derived from experience of how human beings and institutions react. I also found, as have many others, that in the last generation many young economists, fresh out of the university, are ill-equipped to confront the economic problems of the real world. The brightest of these, if they wish to be effective, learn to discard as useless or misleading much of what they have been rewarded in the university for learning. Some even become inclined to go too far and discard all they have learned as useless baggage.

The second main element stimulating this work was the failure of the attempt by Dr Virgilio Barco, one of the ablest of the World Bank's executive directors, to get the Bank to abandon spurious precision — for example, showing a country's GNP per capita to the nearest dollar when it was obvious that the margin of error in calculating GNP was very large. (The highly useful World Bank's *World Tables* is full of examples of this spurious precision. In the USA, the Bureau of the Census reports that the 1970 population census had an error of around 2½ per cent. In the *World Tables*, Ethiopia's population is given to the nearest hundred, with a relative error of 0.0002 per cent, or 10,000 times more precise than the US census even though Ethiopia has never had a complete census!)

Beginning with the presidential address of Wassily Leontief to the American Economic Association in 1970 (Leontief, 1971), several other presidents of economists' societies in the United Kingdom and the United States have over the years expressed dissatisfaction with the current relationship of economics to reality and reinforced me in my belief that research on this subject was overdue. It also became clear that it was useless to expect economists to change the common practice of the profession without their having a theoretical justification and practical guide for the change. Since no one else has appeared willing to undertake the task I have done so.

I have tried to make the paper as intelligible as possible in the hope that it will be read by non-economists who are consumers of economists' memoranda as well as by economists themselves. I have tried to avoid economists' jargon and technical language as much as I could in the text, but put in parenthetical remarks when fuller communication with economists seemed to demand it.

Some may find this paper particularly hard to read not because its content is difficult but because it requires that the reader examine and question assumptions and premises that are most often taken completely for granted and hence not articulated at all. William James observed that there is no pain like the pain of a new idea. But this is only if the new idea is accepted and, by so doing, one has accepted the insecurity of realizing that one's model of reality has been inadequate or defective. A common refuge is to protect one's existing model of the world and to reject the new idea in rage. But economists, one likes to think, are more open-minded. In any case, many of the individual points made in the paper are not original. The originality mostly stems from putting together the various insights developed by others over the years. When these are assembled, the landscape looks considerably different — or to vary the metaphor, as in the poem about the six blind men and the elephant — each touching a different part had a different picture of the whole animal — when all the reports are in, what we have is a phenomenon of major importance.

I am acutely aware that this work is still very much work-in-progress. A great deal more could be done in further refining the

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1

Introduction

> . . .we must not look for the same degree of accuracy in all subjects; we must be content in each class of subjects with accuracy of such a kind as the subject-matter allows, and to such an extent as is proper to the inquiry.
>
> Aristotle, *Nicomachean Ethics*

> We have sought to justify our economic concepts in terms of considerations that are appropriate to the natural sciences; not observing that what economics tries to do . . . is essentially different.
>
> John Hicks, *Wealth and Welfare*

The objectives of this book are to explore how closely economics relates to reality, and what this means for handling economic data, economic analysis and policy. In physics, quantum mechanics has demonstrated that there is an insuperable limit beyond which it is impossible in principle to know precisely both the exact position and the momentum of a simple elementary particle. In mathematics, Goedel proved that no axiomatic system could be complete. Economics is also subject to limitative results and the limits to precise knowledge of an economic situation or problem are approached rapidly. The mesh of the net that economists can weave to catch economic reality is much coarser than that of the natural scientists in their realm.

Central to the whole discussion are the meanings of the words 'accuracy' and 'precision'. In common usage, the meanings often overlap — there is a predisposition to believe that the more precise a statement is, the more accurate it is. For our purposes, it is necessary to make a clear distinction between them. 'Accuracy' will be used to convey the meaning of 'correctness', of 'true value'. 'Precision' will be used to convey the meaning of 'degree of sharpness' by which a thing or concept is specified. For example: on Cape Cod, where the pace of life is unhurried and casual, you may ask a craftsman in June when he will come to repair your fence. If he answers, 'Sometime in the autumn', he is being accurate but not precise. If he answers, 'Ten a.m., October 2', he is being precise but not accurate — it is almost certain that on October 2, the fish will be running and he will be out in his boat. One of the recurring themes that we will find in our discussion is that too often in economics the choice is between being roughly accurate or precisely wrong.

As is well known, Alfred Marshall and John Maynard Keynes, both of whom had had mathematical training, did not believe it was possible to apply exact mathematical methods to economics and, consequently, warned against them. Keynes endorsed Marshall's approach that a pervasive part of economic life cannot be precisely measured. As he stated in his famous footnote in his memorial of Alfred Marshall: economic interpretation in its highest form requires an '. . . amalgam of logic and intuition and the wide knowledge of facts, most of which are not precise. . .' (Keynes, 1925, p. 25).

To clear the ground, I would like first to state what this book is *not* about. It is *not* concerned with the question whether mathematics has any use in economics. I regard this as definitely settled — mathematics is useful as an economic language, as an efficient technique of reasoning and logic, as an essential part of the logico-mathematical framework of economic theory; in short, mathematics is an indispensable tool in economics. (I do not believe, however, that the use of mathematics in economics is justified by the argument that we need the shelter of Mathematics to protect Economics from intruders (Dasgupta, 1968, p. 4).) Mathematics is not, however, a perfect substitute for language — not every communication among human beings can be expressed as well by mathematics as by words. The meaning of a mathematical symbol does not change, once defined, whereas words can flirt with meanings and coquette with relationships. Words can be deliberately ambiguous where relationships are ambiguous and it is desired to leave them so; many a peace treaty if written in maths would never have been signed. One could argue that the whole of constitutional law in countries with written constitutions is essentially involved in the changing meaning of words in new circumstances and new times. Poetry can never be perfectly translated even from one language into another and certainly not into mathematics. In sum, natural language can be more flexible in conveying meaning, it is infinitely richer in vocabulary than mathematics, and frequently it can be more accurate although less precise.

What this book *is* concerned with is the application of theory to economic reality: is the result meaningful when mathematical symbols or words are replaced by numbers that are intended accurately to explain or to forecast the real world? Certainly, one can conceive that every detail of human economic behaviour could be expressed mathematically, but this does not mean that the mathematical expression in question actually can be produced within the body of mathematical theory that now exists or that may ever exist (Arrow, 1951, p. 130). Moreover, it does not follow at all that because mathematics may allow us to describe the general outlines of a system of relationship as algebraic equations we can then always discover the specific numbers to replace the symbols to make the equations fit the particular situation we are concerned with. Hayek has pointed out that the founders of modern mathematical economics had no such illusions. For example, Pareto clearly stated it would be absurd to assume we could ascertain all the data needed for his system of equations describing market equilibrium that would make it possible to calculate the prices and quantities of the commodities and services sold (Hayek, 1975, p. 35).

The discussion in this book is based on the assumption — often falsified in reality — that the mathematics used in economics is not mishandled. Paul Streeten has called attention to the danger that use of mathematics may create a false sense of certainty: there is a temptation to mistake 'validity' for 'truth'. That is, the correct deduction of logical conclusions is mistaken for the discovery of facts about the real world. Another temptation is sub-optimization; that is, to assume that only what is quantifiable is important and to forget the rest. 'The result may be the worst of all possible worlds. . . . rationality about a sub-system can be worse than sub-rationality about the whole system' (Streeten, 1972, p. 371).

Underlying much of the practice of modern economists is a silent premise that the formal properties of the mathematical symbols used in economic models must be isomorphous with the empirical relations they purport to represent in the real world; that is, that the two complex structures — the model and the reality to which it refers — can be mapped onto each other in such a way that to each significant part of one structure there is a corresponding part of the other structure and each of these has the same functional role in their respective structures. A good example of isomorphism is the relationship between the notes shown on a musical score and the sounds of the composition when it is perfectly played. (For economists, perhaps, the best example is the isomorphism between the curves in a Cartesian plane and the corresponding equations in two variables — in mapping from one onto the other all the relevant information is preserved.)

This assumption can be accepted in physics, for example, because of the very way in which physical models are defined and developed. In physics *only* the quantities that can be represented numerically and transformed mathematically are permitted. In fact, physics defines itself as. . .'the science devoted to discovering, developing and refining those aspects of reality that are amenable to mathematical analysis' (Zinman, 1978, p. 28). In economics, isomorphism cannot be tacitly accepted; the relationship between models and reality is a central problem to be examined in each case. This book is largely involved with this theme.

There is an astounding proposition in economics advanced by Milton Friedman and accepted by many economists that what matters in an economic hypothesis is only successful prediction. Further, Friedman argues that not only does it not matter whether the assumptions are unrealistic but even, paradoxically, 'to be important . . . a hypothesis must be descriptively *false* in its assumptions . . .' (1953, p. 15; my emphasis). (Friedman's argument may depend in part on confusing abstraction — i.e., identifying or separating relevant factors from a cloud of irrelevant detail — with the difference between falsehood and truth. That is, since abstraction does not reproduce 'noise' but only 'signal', it must be 'false'.) His discussion demonstrates that he really believes that it is irrelevant whether the assumptions of a theory are a false representation of reality or not. As long as the predictions of a theory appear to be accurate that is all that matters to him.

A most important application of Friedman's approach is his belief that it does not really matter whether it is true or not that all consumers maximize their utility and all businesses maximize their profits, since the economic system functions in such a way that the results come out just *as if* they did try to maximize. Consequently, we do not need to worry what the facts are of behaviour or of motivation in the real world. The basic fallacy here is that nobody knows whether the firms that have survived are truly profit-maximizers or not (Blaug, 1980, pp. 116 – 19). In an industry with increasing returns to scale the existing firms may simply be the ones that entered first and a late-entry profit-maximizer may never have been able to catch up; or the survivors may simply be the ones that had an advantageous power position such as special relationship to a bank or to a government agency, etc. One cannot assume that what exists is the maximizing best of all possible worlds.

Friedman's simple prediction criterion eliminates one of the most important functions of a scientific theory — that is, to provide an understanding of the events and processes that underlie the predicted results. In natural science, it is not sufficient to predict the position of a pointer on a dial, the theory must also explain why the pointer takes up that particular position and only that position. Discovering a correlation is not the same as discovering a causal relationship. Since antiquity the relationship between the moon and the tides has been known, but there was no adequate theory of the tides until Newton's theory of gravitation explained why the tides and their heights occurred just when and how they did. Only when we have adequate theories that explain how the economy works in addition to predicting outcomes can we begin to consider economic policies that can effectively control or change the economy.

The second drawback to Friedman's position is that even if a theory has appeared to provide successful predictions in the past on the basis of false or unrealistic assumptions, one can have no confidence that it will continue to provide successful predictions. It is easy with the help of a calculator and some idle time to come up with obviously nonsensical theories that appear to show a record of successful predictions. For example, David F. Hendry, with tongue in cheek, has demonstrated that cumulative rainfall predicts the rate of inflation in the United Kingdom (Hendry, 1980, pp. 391 – 5). Then there is the Boston Snow Theory: if there is snow on the ground in Boston on Christmas Day, that indicates the following year will be good for the American economy. This is all pseudo-science. In science, merely discovering a series of coincidences linking premises and conclusions does not demonstrate a correct theory. An acceptable theory must have assumptions isomorphic to reality, a clear chain of correct logical or logico-mathematical reasoning leading to conclusions that can be tested for isomorphism to reality; if a single link is broken, the theory fails. If assumptions inconsistent with reality appear to lead to true conclusions, then the chain of logic is wrong, the result is a coincidence, or the conclusions are suspect.

At the purely mathematical stage, an economic model or 'pure' theory can be like a story in science fiction or a game whose rules we make up ourselves. At this stage, our main criteria are only that the rules be logically consistent and the system complete (i.e. internally consistent), whether or not it corresponds in any respect to the real world. While our bodies must function in the real economy, constrained by the limitations and natural laws of reality, our imagination is free to soar above it. But when we must cope with the real economy and our model is needed to solve a real problem, to explain, predict or change reality, then the model must be externally consistent (isomorphic) with the real world: one can no longer assume that we can travel at speeds greater than the speed of light or that our concepts grasp reality more precisely or that our parameters can be measured more accurately than the real world permits (Weizenbaum, 1976, pp. 43 – 4). Econometric models that assume accuracy and precision beyond the margins set by reality have no practical usefulness (other than as games, teaching aids or as kinds of finger exercises) and bear the same relationship to economics and economic policy as scientific fiction has to science — that is, they may require a good deal of imagination and pseudo-scientific calculation but are of no help in coping with the real world.

Perhaps the greatest difficulty economists face in the transition from the ivory-tower of theory to coping with the problems of decision-making in the real economic world is in shifting from the precise numbers and well-behaved models of pure theory to the rough inaccurate data, recalcitrant behaviour and shifting complexities of reality. The transition is particularly difficult because there is little or nothing in economists' training to prepare them for this stark contrast. On the contrary, the conscious or unconscious concept that many economists have of economics as a science comparable to physics leads them to expect that they should be able to explain economic reality with the same precision and predictability that mechanics in physics has been able to attain in explaining and predicting phenomena in the universe. (Georgescu-Roegen's definitive destruction of the mechanics model for economics, 1974, has not yet penetrated much of the profession. See also Hutchison, 1977, Ch. III; Lowe, 1965, pp. 34 – 61; and Sidney Schoeffler's neglected contribution, 1955.)

Even more, there is an unconscious tendency among economists to believe that mathematical rigour *requires* absolute precision in the numbers used. This is not so. John von Neumann, perhaps the greatest mathematician of our time, has specifically dealt with this. He has pointed out that, while in all mathematical problems the answer is required with absolute rigor and absolute reliability, this need not mean that it is also required with absolute precision. In most problems in applied mathematics and mathematical physics the precision that is wanted is quite limited. The data are often not known to better than a few (say 5) per cent, and the result may be satisfactory to even less precision (say 10 per cent). This is compatible with absolute mathematical rigour if the sensitivity of the result to changes in the data as well as the amount of precision of the result are rigorously known (von Neumann, 1963, pp. 324 – 5).

The objective of this book is not only to show that precision in economics is limited but also to attempt to offer a more realistic operational expectation. Quantification and the use of mathematics and statistics in economics are indispensable but it is also essential to recognize clearly that economic reality can be accurately circumscribed only within differing margins of precision. This book is concerned, therefore, with that uneasy frontier-zone where economic theory meets the real world of human beings and institutions. It is concerned with how good a grasp economics can get of that hazy, elusive and changing sector of human life called the economy.

2

Problems of Measurement in General

> When a problem in pure or in applied mathematics is 'solved' by numerical computation, errors, that is, deviations of the numerical 'solution' obtained from the true, rigorous one, are unavoidable. Such a 'solution' is therefore meaningless, unless there is an estimate of the total error in the above sense.
>
> John von Neumann, *Collected Works*, vol.V

Although modern economics has prided itself on its apparent similarity to physics, one of the very first basic lessons taught in beginning physics — that it is essential to understand, to evaluate and to express the degree of accuracy represented by each number used — is generally ignored and neglected in economics. Modern economists insist on quantification but completely overlook the need to understand how much precision is actually attainable in the accuracy of the numbers used as well as the need to express the margin of error present in an economic measurement. Sampling errors for an indifferent (i.e. not hostile) universe are estimated and are usually stated, but the limits of accuracy in most economic estimates that are very rough are usually not stated and sometimes not even acknowledged. Furthermore, economists are not trained to handle this set of problems. Instead, even though quite aware that their data (GNP estimates, costs, prices, rates of return), produced for them by others, are not fully reliable, economists operate with these numbers as though they were precisely accurate to the first or second decimal point.

Norbert Wiener, the noted scientist, observed in this regard that social scientists did not appear to understand the intellectual attitude that underlay the success of mathematical physics. The frequent practice in economics of developing an elaborate model with a relative or total indifference to the methods and possibilities of observing or measuring the elusive quantities concerned is exactly counter to the real spirit of the hard sciences. A true science has to begin with a critical understanding of its quantifiable elements and the means adopted for measuring them (Wiener, 1964, pp. 89–90).

A generation later, Wiener's comment appears still to be fully applicable. Thomas Mayer (1980) has spelled out how econometricians continue to violate the basic requirements of real scientific procedure. For example, it is likely that most econometricians still do not even bother to check that their data are properly copied from their sources through the preparation for the computer to the final printout. Very little attention is paid to the quality of data — data are dumped into a computer without close examination. Students are not taught to keep, nor do practitioners maintain, adequate research records. More prestige is acquired from applying the latest complex techniques to good, bad or indifferent data than in arriving at valid, verifiable and useful results. As a consequence of all this, there is apparently frequent non-replicability of results. Very few attempts are made and published of checks of previous findings of econometric research.

Even if unscientific statistical and econometric methods are avoided, errors in economic data will still exist: errors in economic data *are inherent* and unavoidable. Why is this so? First of all, errors are inevitable whenever the transition is made from a purely mathematical model using only symbols to a numerical or econometric model. This is true not only of economic models but of all models. John von Neumann and H. H. Goldstine in their pioneering study (1963) identified four primary sources of errors in all numerical computations. To the extent that an economic decision involves the use of numbers, these sources of errors are therefore also present and need to be taken into account.

First, in applied mathematics the model chosen to represent the underlying problem will represent it only with certain abstractions and simplifications. A mathematical formulation necessarily represents only a more or less explicit theory of some phase of reality; it is not reality itself (von Neumann and Goldstine, 1963, p. 428). In economics, this first step of abstraction from reality may be even greater than that identified by von Neumann and Goldstine — and the resulting inherent errors consequently are greater. In the social sciences it is very easy to formulate a theoretical model where the determination of the optimum statistical model entails mathematical problems that no one yet has been able to solve. There are also models where the problems are soluble in principle by known methods but the computations in practice would take an impossibly long time. In these cases, the procedure is to substitute a mathematically practicable theory, as similar as possible to the desired one (Arrow, 1951, p. 132). In other words, the model used is at second remove from reality — it is a proxy for the unrealizable ideal model, which itself would be an abstraction from reality.

Second, the model may involve parameters, the values of which have to be derived directly or indirectly from observations. These parameters are affected by errors, and these errors cause errors in the results (von Neumann and Goldstine, 1963, p. 482). This comment is deceptively simple: errors in parameters derive not only from the process of measurement of sharply defined individuals or classes but also from the definition of the individuals and classes being measured. As we shall see in the next chapter, the causes of errors in observations are much more prevalent and stronger in economics than in the physical sciences.

Third, the model will in general involve transcendental operations (like integration or differentiation) and implicit definitions (such as solutions of algebraical or transcendental equations). 'In order to be approached by numerical calculation, these have to be replaced by elementary processes (involving only those elementary arithmetical operations which the computer can handle directly) and explicit definitions which correspond to a finite, constructive procedure that resolves itself into a linear sequence of steps.' All these processes are approximative, and so the *strict* mathematical statement we start with is now replaced by 'an *approximate* one' (pp. 482 – 3; emphasis in original).

The fourth source of errors comes from the need to round off numbers. There has to be a cut-off somewhere, that is, a maximum number of places in a number. From this point on these 'noise variables or round off variables' are injected into the computation every time an elementary operation is performed and constitute a source of errors (pp. 484 – 5).

In modern computers there are usually two distinct ways of handling numerical data. In Fortran, for example, numerical data are either in integer numbers or 'floating-point numbers'. Integer numbers are the numbers we customarily use. Floating-point numbers use scientific notation, that is, a number is expressed as a base number (mantissa) times 10 raised to some power (exponent). Instead of writing a number as, say, 430, it could be written as 43×10 or 4.3×10^2 or 0.43×10^3 with the decimal point thus 'floating' as desired. In this way, a number is stored in the computer as a sign bit, a string of bits representing the exponent and a string representing the mantissa. Because the range of floating-point numbers is much greater than that of integers for the same number of bits, most computations work with floating-point mantissa. Floating-point numbers tend to lose accuracy in the course of calculations when the product of two numbers, for example, results in a mantissa with more significant bits than can be retained for storage. In adding numbers, all must have the same exponent, which means shifting the points in any mantissas accordingly, and again some bits of information may have to be eliminated.

These four sources of error are inherent in any numerical model whether in the natural sciences or in economics. Economics has additional problems. Natural scientists learn at the very beginning of their training to be aware of the inevitability of errors and how to deal with them. One of the techniques they learn is 'significant digits', which teaches an appreciation of the degree of accurate precision that a number represents and, consequently, how to avoid meaningless manipulation of 'noise'. Briefly, this technique shows that the degree of precise accuracy in the final outcome of an arithmetical operation is determined by the most imprecise number involved in the operation (see Technical Appendix). Not only are economists not trained how best to cope with errors but economics has much greater problems in this regard than most of the natural sciences. In the next chapter, we shall concentrate on the special factors that make accuracy in the observation and calculation of economic parameters and variables more difficult than similar operations in the physical sciences.