The lack of progress in economics

from A.S. Eichner

It is possible for economics to become a science — if only the discipline would abandon its present practice of eschewing empirical tests of its theories in favour of 'formal' ones.

OF all the subfields within biology, the social sciences — economics among them — have made the least progress in developing a scientifically valid body of theory. This lack of progress has been attributed to the unpredictability of human behaviour, to the complexity and lack of permanence of social phenomena and to the difficulty of carrying out controlled experiments. At least insofar as economics is concerned, a different explanation can be offered — one which, if true, holds out greater hope that the principal obstacle to economics becoming a science can eventually be overcome.

An examination of economics as a discipline reveals that it is based on an epistemology, or method of establishing the validity of its knowledge claims, that runs counter to the norms of science. The prevalent view among economists is that formal (mathematical) proofs are not just helpful or indeed even necessary but also sufficient — and thus that empirical proofs can be dispensed with altogether. The result has been the development of a body of economic theory which is based entirely on axiomatic reasoning, without empirical support for the key propositions upon which the theory rests (P. Wiles, p. 67 in ref. 1). In contrast, those disciplines which have become sciences have done so because of the insistence that any knowledge claims, especially those which constitute the theoretical core of the discipline meet certain empirical tests.

Utility

The core of economic theory consists of four theoretical constructs which serve as the micro foundation for the dominant paradigm (A.S.E., p. 210 in ref. 1). These four theoretical constructs — indifference curves, isoquants, positively sloped supply curves and marginal physical product of capital curves — are derived from two more fundamental relationships. One is a 'utility' function representing the utility, or satisfaction, derived from the use of a given assortment of goods, x_1, x_2, x_3, . . . , x_n. This function is usually assumed to take the following general form:

$$U = U(x_1, x_2, x_3, \ldots, x_n)$$

Certain 'first-order' conditions are then posited, namely, that the first derivative of the utility derived from any one of these goods relative to that obtained from another is negative (the 'law' of diminishing marginal utility). From this first-order condition, a set of indifference curves is then derived representing the various combinations of goods which individuals consider to be of equal utility, along with a set of negatively sloped demand curves for each of those same goods indicating how the quanitity demanded varies as the price of one good relative to another changes[2].

However, no evidence that such a utility function actually exists has ever been adduced — let alone any evidence that the 'first-order' conditions are satisfied[3]. All that is known is that households (consisting of one or more individuals) purchase goods of different types in varying quantities, with no means presently available to economists for determining the utility or satisfaction derived therefrom. Both the existence of the utility function and the postulated first-order conditions are said to derive from the logic of rational choice, based on the assumption that each individual seeks to 'maximize' his or her utility while having only a limited amount of income to spend (thereby facing a 'budget constraint'). Since the utility which the individual supposedly seeks to maximize as the necessary corollary to making a 'rational' choice cannot be observed and measured, the argument is tautological, without substantive content.

Production

The other basic relationship on which the prevailing microeconomic theory rests is a production function representing the different quantities of inputs, z_1, z_2, z_3, . . . , z_n, required to produce the output of the economic system. This function is usually assumed to take the following general form:

$$O = O(z_1, z_2, z_3, \ldots, z_n)$$

with various first-order conditions again posited, in this case that the first derivative of the change in the quantity of one input required relative to the change in the quantity of another input, holding output constant, is negative (the 'law' of diminishing returns). From these first-order conditions are then derived (1) a set of isoquants, representing the different combinations of any two inputs which can be used to produce the same amount of output; (2) a set of positively sloped supply curves, indicating how the quantity supplied varies with the price being charged, and (3) a marginal physical product curve for 'capital' (which is assumed to be one of the inputs).

The quantity of output produced, both by each industry and by the system as a whole, can be directly observed and measured. The production function, as usually specified by economists, is not therefore subject to the same criticism as the utility function. However, the capital which is assumed to be a homogenous physical input cannot be directly observed and measured, and thus the marginal physical product curve of capital, upon which the most widely accepted theory of income distribution is based, is no less metaphysical a theoretical construct (pp. 200-201, ref. 1).

Moreover, there is considerable empirical evidence contradicting the first-order conditions usually assumed in specifying the production function. What the evidence indicates is that the inputs used in the production process must be used in fairly fixed ratios to one another, with a shift from one set of these fixed combinations of inputs to another set occurring only when a new plant embodying the latest technology is constructed. Thus both the isoquants and the positively sloped supply curves usually postulated by economists lack empirical support, at least insofar as the nonagricultural sectors of the economy are concerned (A.S.E., pp 212-214 in ref 1).

Final demand

It is not for lack of an alternative that the present body of microeconomic theory is retained. Starting with the data available from input-output tables, it is possible to derive a separate production function for each industry that does not depend on any of the first-order conditions usually assumed in economic analysis. From the entire set of such production functions, it is then possible to explain both the quantities supplied by each industry and the corresponding set of relative prices, without having to assume separate demand and supply curves that are each a function of price. The values taken by output and price vectors depends on the level of final demand. By incorporating certain elements of what is known as post-Keynesian theory, it is then possible to explain the level of final

The misuse of mathematics in economics*

NEITHER the utility function $U = U(x)$ nor the production function $O = O(z)$ are functions in the mathematical meaning of that term. That is, no rule is provided by which, given $U(x)$, the value of U can actually be computed, and similarly for O and $O(z)$. We may be told that $U(x)$ and $O(z)$ are monotonically increasing and concave functions, but nothing more beyond such general statements. In consequence, there is not enough information to obtain an actual solution . . . By the very statement (or, rather, incomplete statement) of the problem, it is impossible to solve it! . . . This peculiar way of half-posing problems can be traced back to quite early times, but it was given its main impetus by Paul Samuelson in his very influential *Foundations of Economic Analysis* . . .

Why are problems of pure economic theory stated in such a peculiar, incomplete fashion? The reason is not far to seek. The utility function $U(x)$ is not given explicity because it *cannot* be given. It is an artificial construct of the theorist's mind and does not correspond to anything in the real world . . . Exactly the same is true of the production function $O(z)$. It is now more than a quarter of a century since Joan Robinson[9] demonstrated that the quantity of capital, [one of the z's] of that theory, is impossible to define consistently. Nonetheless, this very quantity still appears, with monotonous regularity, . . . in . . . innumerable papers on pure economic theory . . . All of this is *not* the application of mathematics to the economic problems of the real world. Rather, it is the application of highly precise and elaborate mathematics to an entirely imaginary and fanciful economic cloud cuckoo land. □

*** An extract taken from J. Blatt, pp. 169–171 in ref. 1. The nomenclature of the mathematical functions is slightly changed to correspond with the convention used elsewhere in this article.**

demand itself.

The growth of output per worker as one form of technical progress — to the extent it leads to a corresponding rise in real income — can be shown to determine, along with the growth of population, the long-term growth of final demand, while the variation in investment and other forms of discretionary expenditures can be shown to determine the cyclical movements of the economy around that trend line[4,5]. This explanation for what happens at the industry and aggregate level, insofar as quantities supplied and relative prices are concerned, can then be supplemented by various 'behavioural' (as distinct from 'rationalist') models of what happens at the individual firm and household level. In this way, a complete and comprehensive alternative to the prevailing theories in economics can be developed—one that appears better able to meet the empirical tests necessary to establish economics as a scientific discipline[6,7].

Beliefs

The existence of this alternative nonetheless continues to be ignored by most economists, along with the evidence on which it is based, simply because it is not consistent with the four theoretical constructs that constitute the core of ecomonic theory. Thus the prevailing theory in economics is based on an *a priori* set of beliefs rather than on any empirical evidence. In this respect, it is more akin to a religious credo than to any scientific body of knowledge. However, it is not just in refusing to accept the need for empirical proof of its key propositions that the prevailing practice in economics runs counter to scientific norms. The violation of scientific norms also involves the following two fallacies:

(1) Acceptance of two, logically incompatible theories as being equally valid. This 'dualistic' fallacy characterizes not only the 'neoclassical synthesis', which combines the Keynesian macroeconomic theory with an irreconcilable pre-Keynesian microeconomic theory, but also the prevailing theories in 'pure' trade on the one hand and the models of international money and finance on the other.

(2) Construction of models to describe the market behaviour of economic systems which offer systems which differ in their fundamental characteristics from any market economy known to have ever existed and which, for that reason, are merely constructs of the mind unrelated to any external reality. This variant of the 'solipsistic' fallacy characterizes most of the work presently being done on 'general equilibrium models'.

These two misconceptions are in addition, of course, to the 'Cartesian' fallacy which underlies economic theory more generally — that is, the belief that formal proofs alone are sufficient. This Cartesian fallacy is reflected in the mususe which economists make of mathematics (J. Blatt, pp. 166–186 in ref. 1). Rather than serving as an aid to empirical research, mathematics is used to give economics the appearance, but only the appearance, of being a science. Mathematical relationships are posited, as in the case of the utility and production functions specified above, but the unknowns remain precisely that — unknowns. They cannot be given a numerical value either because the parameters are unobservable or because the functions, being insufficiently specified, cannot be solved (see the accompanying quotation from J. Blatt). This misuse of mathematics is justified on the grounds that all that is required is that a proposition not be illogically deduced from whatever assumptions have been made. The proposition itself, along with the assumptions from which it is deduced, need have no empirical validity.

Validation

The fact that the prevailing practice in economics runs so counter to the norms of science suggests what must be done if economics is to ever become a science. Economists must come to accept as binding on themselves the rules which govern scientific work in general. At the very least, they must recognize the need to empirically validate the core body of economic theory along with the need to replace any theoretical constructs which cannot meet that test.

It is doubtful, however, that so radical a change will occur without considerable pressure from outside the discipline. The same mechanisms which, in other fields, lead to the reinforcement of scientific norms—the system of graduate training, the appointment and tenuring of faculty, the refereeing of journal articles and the peer review of research proposals—instead act to preserve the core body of non-scientific theory in economics (A.S.E., pp. 225-235 in ref. 2; P.E. Earl, pp. 90-125 in ref. 1). It may therefore require the censure, or at least the strong protest, of the scientific community to force a change in the prevailing practice among economists. The protest would be immediate if creationists and others falsely claiming to be scientists were being similarly rewarded with grants from the National Science Foundation and with prizes from the Nobel Committee. □

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ɪ, who in 1981 wrote a critique of .shi's evolution theory[17] but who subsequently rather spoilt the effect by being nverted to Imanishi's ideas[4,18]. Perhaps the most striking example comes in the writings of Shoji Ijiri, a Marxist palaeontologist who has taken a special interest in evolutionary theory, but has never discussed the contrary views held by Imanishi. The reason according to Ijiri was that he believed the theoretical concepts were simply "saloon theories from the Kyoto Unversity coffee houses. They were group discussions for aristocrats, who talk about science with their brains and their mouths, in contrast to Ijiri who approaches theory on the basis of practical experience of mud and sweat"[19]. As far as Ijiri was concerned "farmers and aristocrats have no points in common", in short he had completely ignored Imanishi's theory.

Whichever stand is taken the result is the same, there is no public response. There is one further factor, which may be significant. Imanishi states he is not an ecologist, hence ecologists cease to discuss his ecological theories even though his ideas may be central to their own researches. When Imanishi now states he is not a scientist, fellow scientists no longer feel there is any need to discuss his theories, even though they are concerned with the central concepts of their own disciplines. There seems to be a rigid regimentation in the natural sciences and great store is taken of labels that are paraded and rather less consideration of the actual content being displayed. There is a handful of exceptions. Sibatani has conducted a series of criticisms of the state of Japanese science[20], and the natural sciences in particular, from his working home of Australia (although he is now in retirement and settled in Japan again).

Vigorous debate is not part of the cultural tradition in Japan although deep and learned disagreements and discussions are conducted within groupings such as the Kyoto Elite. Dedication to the search for truth will allow discussion among elites. There is the paradox of the elite proclaiming the importance of the group and Imanishi is a classic case in point, and yet his entire life has revealed the attitude of an extreme individualist. Indeed members of the Kyoto Elite are bound together by a single trait, extreme individualism, but who to a man proclaim the primacy of the group *for the rest of Japanese society*. It certainly does not apply to themselves. Originality and innovation flourish in a secret enclave beyond the experience of the ordinary Japanese, condemned, as they are, to the rigid authoritarian feudal society that masquerades as one of the advanced nations of the world.

My study of Imanishi's evolution theory was undertaken during the tenure of a British Council/Monbusho Visiting Professorship held at the Department of Geology and Mineralogy, Kyoto University. Thanks are due to my host Professor T. Kamei, Dr H. Kamiya, Dr T. Ohno and Miss Hatsuko Fujikawa of the Geology Department and Drs M. Tasumi and A. Rossiter of the Zoology Deaprtment for their help and encouragement. Dr Shoji Ijiri of Tokyo provided valuable insights into the radical left of Japanese palaeontology; Dr A. Sibatani of the CSIRO Division of Molecular Biology, Sydney, first introduced me to the works of Imanishi and provided much illumination. Emeritus Professor Kinji Imanishi kindly discussed his ideas with me and informed me that my outline of his theory extracted from my book represented a precise and accurate summary of his ideas. □

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To the defence of economics

from Partha Dasgupta and Frank Hahn

An attack upon the discipline of economics, from within, is repulsed.

A.S. Eichner's article, "The lack of progress in economics" (*Nature* 7 February 1985)[1] is a remarkable exercise in imaginative writing. He has constructed in his mind a theory of what present-day economists are engaged in when they do economics — what Eichner calls "the prevailing practice among economists" — and proceeds to flail against the enterprise because, in Eichner's theory the current practice ". . . is based entirely on axiomatic reasoning, without empirical support for the key propositions upon which the theory rests". Economists reading Eichner's article will have noted at once that his own theory of what other economists do is deeply flawed on this count. But we have no reason for thinking that the majority of *Nature*'s readership, who we take it are non-economists, will recognize this, and in particular, recognize the fact that Eichner's claims are simply false from start to finish. Economics, like other disciplines we imagine, has its share of people who, for whatever reason, cannot grasp what they criticize, and ordinarily we would not respond to such an article as Eichner's. In any case, there is no obvious harm in having some dottiness around. But this piece may cause mischief, for Eichner ends by suggesting that, ". . . it may . . . require the censure, or at least a strong protest, of the scientific community to force a change in the prevailing practice among economists". Since he has carried his frustrations to a multi-discipline journal such as *Nature* it would be improper of economists to let the piece pass by without comment.

We will not engage in a discussion of whether it is possible for economics to become a *science* — the opening summary of Eichner's article — as it is not at all clear what would be meant by a claim that it is not. More to the point, it would have no bearing either way on Eichner's thesis about the intellectual concerns of present-day economists. Eichner begins by claiming "the core of economic theory consists of four theoretical constructs which serve as the micro foundation for the dominant paradigm . . . indifference curves, isoquants, positively-sloped supply curves and marginal physical product of capital curves". Moreover, in Eichner's view "these four theoretical constructs are derived from two fundamental relationships", namely " . . . a utility function representing the utility, or satisfaction, derived from the use of a given assortment of goods . . ." and " . . . a production function representing the different quantities of inputs required to produce the output (*sic*) of the economic system". One of these inputs, so Eichner claims the dominant paradigm requires, is an aggregate measure called "capital", a concept that, in the words of a J. Blatt from whose writings an extract is reprinted alongside Eichner's piece, is impossible to define consistently.

It is difficult to know where to begin. For the economist reader, to quote is to expose. For others this would not be so. Therefore we begin by noting that "the dominant paradigm" in economics, as Eichner calls it, does *not* require and does *not* rely on marginal physical product of (aggregate) capital curves, as a perusal of any leading graduate textbook[2,3] on microeconomic theory would testify, and it is astonishing that Eichner is unaware of this. Nor does it require the third of Eichner's four putative constructs, namely positively sloped supply curves (see for example refs 2 and 3 or the less technical specialized survey by Killingsworth[4], in which the theoretical reasoning behind and the empirical evidence for labour supply schedules that are in part *negatively* sloped are discussed). Indeed, the dominant paradigm recognizes very much that a modern economy is composed, among other things, of a complex set of interdependent markets, so that the supply forthcoming of a commodity depends on current prices, expected future prices of various goods and services, incomes of various categories, and so forth.

We have some difficulty understanding what precisely Eichner finds objectionable in the idea that firms can, in principle, choose from a variety of techniques to produce a given commodity (the second of Eichner's four constructs). There is now an enormous empirical literature showing that firms do react to altered economic conditions by saving on this resource and relying more on that resource (see for example Berndt and Field[5] for evidence of substantial substitution occasioned by oil price rises). Eichner seems to think there is something phoney about such substitution possibilities among inputs in production (but think of double-glazing as a means of lowering the use of oil and gas, to take literally a homely example), and he seems to suggest (at the bottom of p.427) that there *are* no substitution possibilities among inputs, that there is only one technique — or only a few techniques — for producing a commodity. The dominant paradigm is obviously capable of absorbing such a specification of production possibilities, for it is a *special case* of what Eichner finds otiose.

We have left for last Eichner's most astonishing claim (and that of J. Blatt, whose views are published alongside Eichner's piece), that the theory of consumer choice is founded on the hypothesis that a person's "utility" or "level of satisfaction" can be measured, a hypothesis for which there is no empirical evidence. It is now over fifty years since Sir John Hicks and the late Sir Roy Allen developed empirically refutable propositions regarding consumer demand for goods and services, based on a framework which firmly avoided any such concept as "cardinal utility", or "satisfaction"[6], a framework which formed a cornerstone of two classics in the literature of economics[7,8] and is, if one cares to look for it, to be found in any graduate text on microeconomics. Moreover, the empirical work on consumer demand for various categories of goods and services is simply enormous (see for example Deaton and Muellbauer[9] for a survey and references), and it is incomprehensible to us how Eichner, who we understand has a teaching post a[redacted] the State University of New Jersey, can have missed all of it. The claim that empirical investigations of the implications of microeconomics theorizing are nonexistent is particularly ironic given the extraordinary outburst of publications in the field of applied microeconomics since the Second World War.

How can such remarkable misconceptions be held by a professional of his own discipline? Part of the answer must be simple incomprehension. During the past few decades the subject has developed at a furious rate, largely because it has increasingly attracted talented and well-trained young people, some of the highest ability. The spread in the technical competence amongst economists is very large; by its nature economics is a very technical discipline. It has always been so, and if it did not *appear* to be so it is because earlier economists had adopted a diplomatic style of writing and lulled the reader often into thinking that the analyses were easy. Reading Adam Smith, or David Ricardo, or Karl Marx or Alfred Marshall or John Maynard Keynes is in fact no easy matter. They were grappling with some of the hardest of intellectual problems and were trying to illuminate by abstracting here and simplifying there. For them the matter was harder in addition because they restricted themselves to developing their arguments in spoken language (we will not say *plain* language!) when what they were engaged in was often mathematical reasoning. (The drawback of this approach to communicating with one's readers can be seen from the fact that nearly fifty years after the publication of Keynes' *General Theory* economists are still debating what Keynes really meant.) Today, such inhibitions have disappeared and a greater part of the most interesting advances in theoretical and applied economics are being made by people who write for each other and who get down to business at once, not indulging much by way of preliminaries about the motivation behind their investigations, preliminary chit-chat and so forth. To enter this literature requires formidable technical investment and many economists simply aren't equipped to follow even a fraction of it. There is then, of course, the temptation to criticize what one can't understand. But it does not excuse such economists from reading any of the many semi-technical and non-technical surveys that are regularly published in journals and books.

Economics has few possibilities of controlled experiment. Empirical work must mostly proceed by means of statistical inference on data given by history. Such inference not only requires theory but precise theory. It has been most successful in demand econometrics where the theory of choice caricatured by Eichner has been crucial. But it is a very difficult task and certainly we would not claim that the theory has watertight empirical support. To take another example ask how far high unemployment benefit is an important explanation of unemployment. One needs a pretty comprehensive theory of the economy and of individual choice before one can use such data as there are. The facts do not speak for themselves. Lastly it should be emphasized that a very large number of economists are engaged in empirical research. At the end of this we are left with an interesting question. It seems unlikely that *Nature* would have published an article in which, say, relativity theory had been attacked in way which made it obvious that the author had no glimmer of understanding of what the theory was about. Why is it that in subjects like economics any nonsense can get a hearing? Perhaps sociologists will have an explanation. □

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