

Amartya Sen

Economic Methodology: Heterogeneity and Relevance

THIS IS AN ISSUE-RAISING, QUESTION-MONGERING PAPER. MANY MORE questions will be asked than will, in fact, be answered. The intention is to propose a broad-based critique of contemporary economic methodology starting off from the recognition of the deep-seated *heterogeneity* of the subject matter of economics.

There is a good deal of discontent about the methods and traditions in vogue in contemporary economic theory. I shall argue that while parts of that disquiet are well grounded, they have to be separated from others that seem to take insufficient note of the particular nature of the exercises under attack. In particular, some reproaches are based on an inadequate recognition of the diversity of motivations and concerns underlying different types of economic theory.

In reading the literature, I am sometimes reminded of an old story concerning two estranged brothers—one a general and the other a bishop—who meet after many years in a desolate railway station and find their old dislikes revived. The bishop asks the general: “Assistant Station Master, tell me when is the next train to London?” The general replies to the bishop: “Madam, in your condition, do you think it is safe to travel?” Our ability to misrepresent or misunderstand what others are up to is quite striking. As a consequence oddly inappropriate broadsides can come mixed with pertinent and penetrating criticisms. The need for sorting out is, thus, quite central. To some extent, this is what

ORIGINALLY PUBLISHED IN SOCIAL RESEARCH VOL. 56, NO. 2 (SUMMER 1989)

will be attempted in this paper, though given the vast varieties of methodologies used and the criticisms made, the ground I shall be able to cover will, of necessity, be relatively tiny.

THE HETEROGENEITY OF ECONOMICS

There are many different types of problems with which economics as a subject is concerned. This simple and homely thought is worth stating only because the diversities are so often overlooked, insisting on a narrow approach confined to addressing a particular category of problems, ignoring or denying the legitimacy of others. For example, prediction is undoubtedly a major exercise in economics, but an overconcentration on this one function can lead to undue neglect of other exercises—for example, doing economic *evaluation* or providing adequate *description*.

The narrowness of that prediction-centered approach can be illustrated by considering Milton Friedman's otherwise illuminating contribution to "the methodology of positive economics" in which he proposes that "realism" may have to be judged "by seeing whether the theory works, *which means* whether it yields sufficiently accurate predictions" (Friedman, 1953: 15). The ability of a theory to yield sufficiently accurate predictions is undoubtedly important, and Friedman does indeed throw much light on this aspect of the problem. But there are many other worthy economic exercises that cannot possibly be judged in this light (Sen, 1982; Hicks, 1983). For example, descriptive statements such as "unemployment has increased" or "inflation is slowing down" call for some way of assessing the reality of what is occurring. They are primarily concerned with reflecting adequately what *has* happened rather than with foretelling what *will* happen or what *would have* happened had the circumstances been different. Similarly, evaluative statements involving valuation and assessment may depart from purely predictive exercises and the methodology related to that exercise. Whether or not one accepts John Hicks's powerful argument to the effect that economics is "a discipline, not a science" (Hicks, 1983), we have to acknowledge that there is much more to economics than predictive efficiency.

At the very least, the subject of economics includes three diverse, though interrelated, exercises: (1) predicting the future and causally explaining past events, (2) choosing appropriate descriptions of states and events in the past and the present, and (3) providing normative evaluation of states, institutions, and policies. Predictive economics is directly concerned only with the first of these exercises. The “methodology of economics” has to admit enough diversity to be able to deal with the other classes of problems as well.

These different exercises are not, of course, independent of each other, and there are clear links between these problems. The interdependence of the issues must be recognized without confounding the distinctions between them and without ignoring their claims to specific attention.

While description, prediction, and evaluation may be the main elementary exercises with which the subject of economics is concerned, there are more complex (and, in some ways, more derivative) problems, too, such as using economic arguments for political advocacy (Myrdal, 1953; 1958) or seeing “the rhetoric of economics” as an object of direct importance, in addition to its instrumental roles in means of communication (McClosky, 1983, 1985). The diversity of the discipline of economics has to be kept firmly in view to achieve an adequate grip on the methodological issues underlying the subject.

VERIFIABILITY AND TESTING: SCOPE AND RELEVANCE

The issues of testing and verification are important for many types of economic analyses, since they are concerned with causal relationships and with making predictions—either about the future, or counterfactually about the present or even the past. In the context of this particular part of economics, the failure to formulate the underlying concepts and categories in such a way that they can be examined and used for testing can be quite a major shortcoming. The problematic issues in this area have received a good deal of critical attention recently (Ward, 1972; Latsis, 1976; Gibbard and Varian, 1978; Blaug, 1980; Bell and Kristol, 1981; Boland, 1982; Pitt, 1981; Klamer, 1984;

Marglin, 1984; Wiles and Routh, 1984; Kornai, 1985; McClosky, 1985; Hendry, 1986).

The point has often been made that many economic theories are hard to test, and sometimes they are not testable even in principle. This recognition is frequently seen as the basis of justified skepticism regarding the value of such theories. There is a major issue here, but before the significance of it can be assessed, we need to pause to ask whether verifiability and testability are properties that we would expect all economic theories to have.

The distinctions made in the last section between different exercises within the body of economics become relevant in this context. Evaluative exercises may be based on norms used for assessment, and these norms are, obviously, not open to testing in the same way as causal hypotheses are. There is nothing particularly defeatist in this recognition, since the exercise of normative evaluation is a different discipline from that of making predictions on the basis of causal hypotheses. If someone is worried by the simple fact that parts of economics are not open to testing, then that reflects a failure to understand the nature of economics as a discipline and its internal diversity.

Similarly, some descriptive propositions do not have predictive content, and “testing” would be the wrong operation to seek. One has to approach this particular problem with some caution, since many descriptive propositions about the *present*—and even the *past*—do involve *implicit* predictive content (Friedman, 1953). For example, the claim that a firm is in “equilibrium” cannot be assessed except in terms of what the firm *could have* earned, counterfactually, had it pursued a different policy, and that assessment obviously does involve counterfactual prediction (what *would have* happened had the policy been different). This type of “implicit prediction” is, in fact, involved in many apparently purely descriptive exercises in economics, and here the issue of testing is indeed directly relevant.

However, the existence of “implicit predictions” in many descriptive propositions does not deny the fact that there are many *other* descriptive propositions which are not of this type. For example, a claim that

a country has experienced increased starvation or enhanced inequality may have many problematic features, particularly the selection of the assessment criteria used, but the problems need not arise from the difficulty of testing “implicit predictions” present in these descriptive statements because there may or may not be any such implicit predictions.

There can be considerable argument as to precisely where the lines are to be drawn. What should not, however, be so disputable is the existence of different types of economic problems in some of which the issues of testing and verifiability are quite central, while in others they are either peripheral or quite irrelevant.

COUNTERFACTUALS AND SRAFFA

The importance of “counterfactual” propositions in economics is easy to see.¹ What *would have* happened had something been different is a natural format of inquiry in predictive economics. Counterfactuals can also be relevant even for normative exercises. For example, the idea that someone “should” receive a high reward if he or she causes a great deal of wealth to be created invokes a normative framework in which counterfactuals play an active part.² Similarly, the “claims of need” may be based on an analysis as to what would happen (e.g., what suffering, what misery, what deprivation) but for those claims being met (cf. Meade, 1976; Atkinson, 1983).

However, there are many descriptive problems in which counterfactuals do not have any essential role. Some economists have also tried to explore the extent to which economic analysis can do without counterfactual propositions altogether. Perhaps the most interesting contribution to this exploration came from Piero Sraffa (1960). In a work that is justly famous (and also, arguably, much misunderstood), Sraffa tried to explore whether the relationship between prices, productions, and distributions of income cannot be substantially explored without considering any changes—factual or counterfactual—and without, thus, involving any “marginal” concepts at all (since such concepts take the form of asking what would have happened if something had been one unit more or less).

It is quite remarkable that Sraffa did establish a number of important relationships (e.g., those between the wage rate and the profit rate, and between relative prices and quantities) that could be expected on the basis of certain *given* characteristics (such as the same rate of profits and same wage rates in different enterprises). He did not go into the question as to why these characteristics (e.g., the same rate of profit in all enterprises) could be expected to hold, and it is possible to argue that such a justificatory inquiry would take one in the direction of equilibrium economics, involving the use of counterfactual considerations. But that was not the subject of Sraffa's investigation.

Sraffa's is an important methodological contribution in trying to understand the relation between prices, costs, production, and distribution. The concept of "determination" used in this approach is a broader one than that of *causal* relations, which would necessitate counterfactual analysis. It can be argued that Sraffa focused attention on the "coherence" of observed reality and the power of that coherence, showing how observing a part of the reality (e.g., quantities of inputs and outputs and the profit rate) can tell us about another part (in this case, the wage rate and all the relative prices). This type of inquiry has been neglected in modern economics (partly because of the dominance of the positivist methodology and the narrow range of scientific interest in contemporary economics) and it can be seen as a return to an important classical interest underlying a part of the motivation for the search for a "theory of value."³

However, it would be, I believe, a mistake to see (as has been sometimes suggested) in Sraffa's analysis a *causal* system rival to the standard neoclassical model of the determination of prices, quantities, and the distribution of incomes. Sraffa was changing the nature of the inquiry—toward an important but neglected theme—rather than providing a different answer to a given question already in vogue in contemporary economics.

TESTING: IMMEDIACY AND ULTIMATE USE

As far as causal theories are concerned, the need to test them with empirical information is fairly universally accepted *in principle* by

economic theorists. However, only a relatively small minority of economic theories on causal relations are, *in fact*, tested. There would seem to be here a remarkably large gap between principle and practice. There is an important problem of methodological strategy involved in this tension.

The belief that is meant to justify the gap is rather rarely articulated. It mainly takes the form of seeing testing as an ultimate, rather than an immediate, step to be undertaken. The complexity of many economic problems requires that conceptual and analytical issues be explored very substantially to understand what types of relationships might be involved. While the analyses *at this stage* are not meant for immediate testing and verification, they can greatly help cognitive assessment of the problems and the relations under examination. A great deal of economic theory involving causal relationships is, in fact, of this kind, and the practical value of insights obtained from analytical reasoning may well be very substantial.

While this may seem fair enough, it may well be asked whether economic theorists do not tend to overindulge in lingering on at this allegedly preliminary and less immediate stage. The analytical structures may end up being, in effect, *substitutes* for, rather than *complementary* to, empirical investigations. There is a narrow passage between the Scylla of over-immediate and breathless search for testing and the Charybdis of self-indulgent pure theory, never coming to terms with empirical examination. The important role of analytical reasoning in predictive investigation has to be recognized, but at the same time the tendency to make the intermediate product in effect also the *final* one deserves hard scrutiny.

EQUILIBRIUM ECONOMICS

One specific field of economic inquiry in which the conflict discussed in the last section comes through very sharply is that of general equilibrium analysis. In terms of a rather simplified structure of causal relationships, competitive general equilibrium models have been well explored (Hicks, 1939; Samuelson, 1947; Arrow, 1951; Debreu, 1959;

McKenzie, 1959; Arrow and Hahn, 1971). It is plausible to argue that the many insights derived from these analyses are helpful even in understanding less simple market structures encountered in the real world, and that these exercises in pure theory ultimately have much practical relevance.

On the other hand, it is hard to be sure that the aspects of the real world on which these theories provide useful illumination are really more important than the respects in which these simple models distort reality. Some economists have forcefully argued that not only is the nature of contemporary capitalism very different from what we see in models of competitive general equilibrium, but also that the most momentous features of modern capitalism are largely lost in these models.⁴

Janoš Kornai has argued persuasively that “equilibrium theory is merely an intellectual experiment” and that “its propositions have not been verified” (Kornai, 1971). That observation is certainly correct. But the hard question to settle is the extent to which the intellectual experiment has or has not been indirectly valuable in the formulation and assessment of empirical propositions about the real world. If general equilibrium theory has been at least in some respects valuable (as, I tend to believe, it has been),⁵ this is not because its results have been anything like verified, nor because the theory is free from serious distortions of reality. The claims lie elsewhere—in the insights that this theory may have provided about how a class of crucial economic interrelations work. That claim is not easy to dismiss, given the way general-equilibrium relations are invoked—often implicitly—even in very practical debates about prices and markets.

Underlying this problem is the issue of the usefulness of the idea of “general equilibrium,” and more generally of the concept of “equilibrium” itself as a basic building block in economic analysis. In standard “general equilibrium theory,” the assumption of equilibrium has several different requirements. They include each agent being in a maximized position given the constraints faced by the agent and there being no excess demand in the market (and if there is excess supply of

some commodity, its price being zero) (Koopmans, 1957; Debreu, 1959; Arrow and Hahn, 1971).

The concept of equilibrium has an important reductionist role, and it makes it possible to deal with complexities of human behavior and of interdependences in markets in a relatively simple way. There cannot be any doubt at all that the assumption of equilibrium has contributed both tractability and elegance to a good deal of economic reasoning. What is less clear is whether these achievements have been purchased at a high price of remoteness from descriptive reality or predictive efficiency.

One type of criticism of “equilibrium economics” concerns the use of equilibrium-based reasoning. Sometimes this type of economics is invoked in a starkly simple form, drawing instant political conclusions—for example, about the efficiency of the market mechanism. This does little justice to general equilibrium theory as such, which distinguishes between and provides partly disparate answers to different questions regarding equilibrium, in particular (1) existence, (2) uniqueness, (3) stability, and (4) efficiency.

General equilibrium theory explicitly acknowledges the possibility of the *nonexistence* of a general equilibrium even when all markets are competitive. Indeed, a good deal of the theory has been concerned with identifying conditions that would ensure an equilibrium. Even when such an equilibrium exists, it may be *nonunique*. It is possible also for an equilibrium to exist, and even be unique, but nevertheless be *unstable*, so that departures from it may not be corrected, and it could not be assumed that irrespective of initial conditions the economy will move to such an equilibrium. Also, an equilibrium could exist (and also be unique and stable) but be economically *inefficient* in the sense of not achieving Pareto optimality.

In popular use of general equilibrium theory, many of these different concepts are confounded, so that the mere presence of a competitive process is sometimes taken implicitly to entail the existence, uniqueness, stability, and efficiency of a general equilibrium. Sometimes policy arguments are based on these implicit assumptions.

One has only to look at the simple pronouncements in the early years of Mrs. Thatcher's government in Britain to see this process at work. Like Monsieur Jourdain, who was unaware of the fact that he "spoke prose," Mrs. Thatcher may not have known that she "talked general equilibrium," but her economic analysis could have actually profited from some real general equilibrium theory and from seeing the contingent nature of market-based economic efficiency. If there are problems in these applications, the blame for this cannot be put on general equilibrium theory as such, since that theory explicitly recognizes the substantive nature of each of these questions and investigates the conditions under which satisfactory answers to the respective questions do or do not emerge.

Another important issue concerns the use of the idea of equilibrium itself. The reductionist role of this idea and its contribution to tractability are highly prized in economic theory, but the point is often made that it is an artificial construct. There is some truth in this claim, but it is also important to recognize that the idea of equilibrium is widely used—usually in an implicit way—in our common practical reasoning. If, for example, we arrive at a railway station and find that of the two ticket counters one has a long queue in front of it and the other has only one person standing alone, we do not typically tend to assume that people are just being silly in sticking to the first counter and that it would make sense immediately to run to the second. The presumption that we might entertain could be that the people standing in the queue know something that we do not yet know—for example, the nature of the transaction that is being made at the uncrowded window, involving perhaps a time-consuming ticketing or a slow-moving payment. That commonsense argument is, of course, based essentially on the idea of an equilibrium—in this case of ticket buyers choosing between windows. In one way or another the notion of equilibria is very widely used in informal as well as formal reasoning involving human behavior and interaction.

In fact, the implicit use of such an assumption can be seen also in some of the literature critical of standard neoclassical theory. For exam-

ple, in Sraffa's analysis of the "production of commodities by means of commodities" it is assumed that the profit rate is the same in all enterprises and the wage rates do not vary from one enterprise to another (1960). While no reason is given as to why this assumption is made (and for Sraffa's own exercise it is not really necessary, as we discussed, to go into that causal connection), a defense of such an assumption would tend to require some kind of an equilibrium argument. It would be very hard to reject the concept of equilibrium altogether and still be able to deal with complexities of social and economic problems.

The methodological difficulties of equilibrium economics do not lie in the idea itself. They lie in the rather narrow and simple way the idea is often interpreted and the overextensive use that is frequently made of this one concept. Here again there is scope for considerable disputation based on a discriminating analysis of the uses and abuses of the idea of equilibrium in economics.

VALUE JUDGMENTS AND WELFARE ECONOMICS

Under the influence of positivist philosophy, the need to keep value judgments out of economics was fairly widely accepted in the writings that followed Lionel Robbins's powerful critique (Robbins, 1935). Many battles have been fought since then in the field of economic methodology on the subject of value judgments and their roles.

The decision to keep economics "value-free" would, of course, militate against the subject of welfare economics as such. Indeed, welfare economics was in a state of very considerable decline for a long time after Robbins's attack, and the revival of this subject has been relatively slow. However, insofar as welfare economics (and, more generally, the evaluative concerns within the field of economics) are accepted as legitimate, staying clear of value judgments could scarcely be accepted as good practice. If evaluations have to be made, norms and values have to be used, and there is really no escape from this necessity. On the whole, this is rather an old and tired issue, and it cannot be said that this is an area of burning urgency among the methodological problems to be addressed today.

However, there can be fruitful arguments on the need for more explicit and well-defined presentation of value judgments used in welfare economics exercises. This has some implications for constructing the axiomatic foundations of welfare economics. A number of powerful results have been derived in analytical welfare economics by making extensive use of such explicit conditions.

By far the most impressive of these results is, of course, Arrow's "impossibility theorem" (Arrow, [1951] 1963). This theorem shows the impossibility of combining together a number of plausible-looking value judgments in deriving social judgments, or social choice, on the basis of individual preferences. One of the lessons that has emerged from the large literature that has developed in social choice theory following Arrow's work is the possible opaqueness of apparently transparent value judgments. Many innocuous-looking value judgments have surprising implications, especially when they are combined together. In the case of Arrow's "impossibility theorem," a set of four mild-looking conditions for social choice invoked by Arrow generates an "impossibility" in the sense that they cannot all be fulfilled together (Arrow, [1951] 1963). The informational bases of various value judgments do call for close scrutiny (Sen, 1970; d'Aspremont and Gevers, 1977; Hammond, 1982; Pattanaik and Salles, 1983; Suzumura, 1983), but the requirement of "explicitness" of value judgments is not altogether a simple and unambiguous one. Value judgments stated in formal and exact terms can, nevertheless, be extremely opaque. There are important methodological issues involved in the presentation of value judgments in welfare economics.

VALUES, DESCRIPTION, AND PREDICTION

While the systematic role of value judgments in the *evaluative* exercise of welfare economics is easy to accept, it is possible to have more of an argument on the relation between values, on the one hand, and the exercises of *descriptive* and *predictive* economics, on the other. The two types of issues are taken up in turn.

The role that value judgments might play in the *descriptive* exercises involved in such activities as measuring poverty or inequality

or unemployment has been a matter of some disputation. It has been argued, with much plausibility, that in choosing between different descriptions, we must be guided by what is regarded as valuable, and as such the apparently descriptive exercises would tend to have various *implicit* value elements (Myrdal, 1953, 1958; Streeton, 1958; Blaug, 1980; Dyke, 1981; Dasgupta, 1985; Heilbroner, 1988).

Sometimes these implicit elements have been made quite explicit, and a very important and influential example of this can be found in the class of economic measurements based on explicit formulation of valuation functions—for example, in Atkinson’s ethical measure of income inequality (Atkinson, 1970). In this approach the extent of inequality is measured by the loss of social welfare—in terms of equivalent income—resulting from the inequality in question.

The need for selection between different descriptions cannot, in fact, be disputed, and the case for doing the selecting on the basis of appropriate values would be hard to deny.⁶ What is less clear is whether the discrimination must be done specifically in terms of value judgments concerning such matters as *social welfare* (related to some underlying notion of a “social welfare function”). It can be argued that our interest in seeking the description may well arise also from motivations *other* than the pursuit of social welfare. The evaluative exercise in choosing between different descriptions is, thus, not necessarily a matter of “social welfare functions” as such (i.e., what is good for the society), but of better fulfilling the diverse interests that are served by such descriptions or measures (i.e., what is good for the inquiry, given the objects of the exercise). To check what serves the goals of the inquiry best will of course involve value judgments regarding the nature and uses of the inquiry. For example, in examining the role of economic inequality in fostering serious crime, the relative seriousness of different types of crimes would have to be judged along with judging the different ways of describing economic inequality, but these value judgments need not be just about the size of “social welfare” as such. There is a complex issue here, and there is obviously scope for considerable argument on the precise role of values in the assessment of alternative descriptions.

As far as prediction is concerned, the role of value judgments is typically rather limited. Indeed, it is precisely in this context that the implicit use of value judgments in compromising the objectivity of tests of predictive hypotheses has been seen as being especially objectionable. This is, I believe, a largely plausible position to take, and I do not propose to challenge it here.

However, the importance of values in *motivating* predictive inquiries of different kinds has also to be acknowledged. Researching is an action, and like any other action, there is ultimately a question of its justification. The questions “How should I act? What should I do?” are critically evaluative ones in the choice of research actions, in the same way they are in other types of choices.

Choosing to investigate one type of predictions rather than another cannot, in this sense, be divorced from the values of the investigator. But this does not, of course, in any way deny the need for objectivity in the predictive accounts themselves. Even when the motivational point is granted and research actions are seen as acts of volition that call for justification, the importance of objective assessment of the predictive hypotheses, by using appropriate tests and verification procedures, need not be—for that reason—compromised.

It is in these more complex issues involving the use of value judgments (1) in the choice of descriptions and (2) in the choice of problems to be selected for study and reporting that the methodology of investigation can encounter interesting and challenging questions at the present time. In contrast, the old issues of keeping economics “value free” and dumping welfare economics in favor of “no-nonsense” positivism are hardly worth investigating, given the peculiar naiveté of those positions.

FORMALISM AND MATHEMATICS

The formalization of economics has received a good deal of critical attention recently. Some have questioned the cognitive limitation that is imposed by a formal language, undermining “other cognitive potentials and strengths” (Woo, 1986). It is certainly arguable that the language of standard mathematical expression is often not rich enough

to capture many subtleties that call for informal understanding (the formal expressions of which, if possible, would be extraordinarily unhelpful, long, and complex). On this line of critique, the failing of overformalized economics is one of lack of “balance.” This does not, of course, demand that formal reasoning be abandoned in economics, but only that it be used with greater discrimination.

Certainly, the belief that mathematical expressions must be more precise in capturing what we wish to say can be badly mistaken, since subtleties may be eschewed in choosing particular formal statements. Indeed, the precise expression of a complicated truth cannot possibly take a simple form, however precise-looking the simple formula might be. A formal expression can be extremely precise without being at all a *precise representation* of the underlying concept to be captured. In fact, if that underlying concept is ambiguous, then the demands of precise representation call for *capturing* that ambiguity rather than replacing it by some different idea—precise in form but imprecise in representing what is to be represented. It is in this context that such mathematical structures as partial orderings, fuzzy sets, etc., have much to offer.⁷

Another issue concerns the “excluding” role that mathematical economics allegedly plays in making it hard for many economists—without the needed mathematical training—to follow what is happening in that select territory. Some have even seen in the emergence of mathematical economics the construction of a “symbolic language of priesthood,” excluding the nonselect from claims to understanding the “real” intricacies of economics. There may be some points of substance in this diagnosis of the allegedly “excluding” uses of mathematics in economics, but the dichotomy between mathematical and nonmathematical economics is rather less sharp now than used to be the case even some decades ago. One reason for this, undoubtedly, is that most economists these days, even those who are not particularly involved in mathematical reasoning, do have some acquaintance with the kinds of mathematics standardly used in economics (e.g., basic calculus, elementary linear algebra), and the sense of distance is much less now than it used to be. Furthermore, the aura of glory that was associated

once with being “rigorous,” “exact,” and “modern”—available only to the chosen mathematical few—has rather dimmed in recent years. The fact that remarkable exactness of operations can be combined with extremely crude conceptualization of the nature of economic problems has become rather more transparent than might have been the case earlier. The division has certainly become less spectacular, and shrill presentations of the positions—“for” or “against”—have become, happily, less frequent.

There do remain, however, some substantial issues to be discussed on the use of formalism and mathematics in economics. Michio Morishima has discussed the distinction between what he calls “the good and bad uses of mathematics” (Morishima, 1984), and in the debate involving Morishima and Gorman the advantages as well as the difficulties of the use of mathematical methods in economics have been illuminatingly brought out (Gorman, 1984; Morishima, 1984). It is, in fact, easy to sympathize both with Morishima’s criticism of the way mathematics has often been used, even by the finest economists, in constructing economic theory, and at the same time accept Gorman’s conclusion that in some other ways we do need *more* mathematics, not less, to sort out the real economic problems.

I believe Morishima brings out the central issue clearly when he states: “I would not care in the least if this Centre [at LSE where Morishima works] were 100 percent occupied by mathematicians, as long as these mathematicians were capable of furthering the study of economics by using the achievements of related disciplines, and as long as this is what they actually did” (Morishima, 1984: 76-77). The issue, in this view, is not the use of mathematics as such but rather the need to include influences that standard mathematical models often tend to leave out.

In fact, Morishima notes that “the development of economics has owed much to the Econometric Society’s idea that there should be a trinity consisting of mathematics, statistics and economic theory,” but argues for its replacement by a new trinity of “economic theory, statistics and related disciplines” (Morishima, 1984: 77). The real issue,

therefore, does not concern doubts about the *positive* contributions that mathematics can make to economic reasoning of both analytical as well as empirical kinds, but relates to worries about the *negative* effects—in the form of exclusions—that overconcentration on standard mathematical methods often tends to produce. There is scope for methodological arguments on the importance that mathematical formalizations should play in economics, but the argument cannot fruitfully take the form of being “for” or “against” mathematics in economics.

RATIONALITY AND SELF-INTEREST

Traditional economic theory incorporates considerable use of certain “reductionist” assumptions which make a complex problem tractable by reducing the dimensions that have to be considered in economic interrelation. The assumption of “rationality” is one such postulate, which is used—explicitly or by implication—pervasively. The concept of rationality as used in economic theory tends to have two different interpretations, viz., (1) internal consistency of choice and (2) maximization of self-interest. They are related to each other in the sense that maximization of self-interest, when self-interest itself takes a rather orderly form, may induce consistent choices in terms of standard criteria of consistency. On the other hand, consistent behavior need not invariably involve the pursuit of one’s self-interest, since other types of goals and objectives may also induce similar consistency. Also, it is by no means clear that in situations of game-theoretic interdependence, “enlightened self-interest” may not permit behavior patterns that might apparently look “inconsistent,” in the sense of violating certain mechanical criteria of consistency that are frequently used (Sen, 1987). Motivationally, the demand of consistency and that of self-interest are rather distinct from each other, since the former is a procedural demand without any substantive content as to what is being pursued, whereas the latter is a particularly substantive command as to what is being pursued without necessarily inducing a rigid procedural structure.

The assumption of self-interest maximization has played a central part in economic model-building of both formal and informal

kinds. That assumption is often traced to Adam Smith and the conception of the “economic man,” and there is certainly some evidence that Adam Smith entertained the possibility that individuals in the context of exchange relations in the market pursue their self-interest and in the process help each other to satisfy their respective interests. But it is also true that Smith extensively discussed the importance of social interdependence and the communal advantages of following “rules of conduct,” even when they go against dictates of what he called “self-love” (Smith, [1776] 1975, [1790] 1975; Sen, 1987: 22-28, 87-88).

Be that as it may, self-interest maximization has become the standard—almost universal—assumption used in modern economics. This uniformity is rather remarkable given the fact that many other types of objectives and values clearly do have a role in human thinking and action. The issue is not so much that individual behavior may *sometimes* go against self-interest maximization. That claim would be rarely resisted. The real question concerns the importance of these departures—in particular whether there are certain systematic patterns in it which may help one to understand and interpret economic reality better, and which may be fruitfully used in making predictions or formulating public policies. There is certainly much scope for argument here.

The assumption of rational behavior is used in economics partly because of its interest on its own, but also because it is seen as a way of coming to grips with *actual* behavior. The latter exercise gives rationality an intermediating role in economic analysis. The simultaneous use of the assumptions (1) that people act rationally *and* (2) that rationality requires maximization of self-interest makes actual behavior, in this formulation, take a rather straightforward form.

There are, in fact, three distinct questions here. The first concerns the presumption that people *will* behave “rationally,” and that departures from rational behavior will not be very frequent. The second question concerns the *characterization* of “rationality” in the form of self-interest maximization. The third issue concerns the claim, which can be derived from the first two presumptions but could also be asserted inde-

pendently of them, that *actual* behavior will take the substantive form of self-interest maximization. These questions can be discussed on their own, and each has received some attention in the existing literature.⁸

As far as *actual* behavior is concerned, it is also possible to base the alleged prevalence of the maximization of profits and of self-interest on “natural selection” and survival rather than on the idea of *reflective* rationality. The profit-maximizers do better, are more prosperous, and survive more easily, even when there are no definitive *internal* reasons for accepting the superiority of pursuing self-interest rather than other goals (Friedman, 1953).

The importance of evolutionary arguments can hardly be denied in economics, since selection and evolution must have relevance to what survives and flourishes (Hicks, 1969; Hirschleifer, 1977, 1984; Guha, 1981; Nelson and Winter, 1982). On the other hand, it is far from clear that natural selection must necessarily take the particular form of consolidating self-interest maximization in general and profit maximization in particular, and contrary arguments have been forcefully presented (Schotter, 1981; Nelson and Winter 1982; Helm, 1984; Matthews, 1984). Indeed, evolutionary processes can be very complex, and the invoking of natural selection must also come to grips with the complexities that have emerged in the biological literature itself (Dawkins, 1976, 1982; Smith, 1982).

It is also important to distinguish between (1) the problem of natural selection of motivations among *different individuals* in a given society and (2) that of natural selection among different general motivations in *different societies*. Even if it were the case that a relentlessly profit-maximizing individual would do better in a society where others in that society were inclined in other directions (Friedman, 1953), it does not follow at all that a society in which everyone is self-interest maximizing would do better than another in which self-interested behavior is standardly constrained by systematic adherence to “rules of conduct” (Smith, [1790] 1975).

Thus, the pervasive use of self-interested behavior in modern economics cannot be adequately defended by natural-selection argu-

ments. The alternative justification through (1) the assumption of rational behavior and (2) characterization of rationality in terms of intelligent pursuit of self-interest has remained relevant as a defense of this standard assumption. It is not, however, a very strong defense either, since it is not at all clear why rationality should not involve the intelligent pursuit of *all* one's goals and values, properly weighted, rather than sticking only to a particular class of goals, viz., self-interested ones. These questions have been raised forcefully and discussed fairly extensively in recent debates (Hirschman, 1970, 1977; 1982, 1985; Sen, 1973, 1982, 1987; Hollis and Nell, 1975; Leibenstein, 1976; Scitovsky, 1976, 1986; Schelling, 1978, 1984, 1986; Collard, 1978; Elster, 1979, 1983, 1986; Hahn and Holli, 1979; Lutz and Lux, 1979; Solow, 1980; McPherson, 1982, 1984; Margolis, 1982; Akerlof, 1984; Parfit, 1984; Frank, 1985, 1988; Pagano, 1985; Putterman, 1986; Schwartz, 1986; Steedman and Krause, 1986; Dasgupta, 1987; Walsh, 1987a, 1987b, forthcoming; Etzioni, 1988; Slote, 1989).

The role of self-interest in human behavior includes different components involving (1) individual welfare, (2) individual goals, and (3) individual choice. Individual welfare can be "self-centered" (with no "externalities," such as sympathy); individual goals can be confined to maximizing "self-welfare" (with no concern for the well-being of others); individual choices can be geared entirely to "self-goal" (with no note being taken of the goals of others). But a person can have good reasons for violating each of these—respectively demanding—assumptions.⁹ The departures from self-interested behavior can take different forms depending on which of the three elements (or which combination of them) get violated. The descriptive, predictive, and normative implications of the different violations can be quite dissimilar.

Also, it would be a mistake to assume that the real issue concerns only the conflict between self-seeking and some kind of a *general* altruism. Group interrelations can be of many different types, and the identity of the person with groups to which he or she belongs can take many different forms. Further, perceptions of "interests" and of "goals" can be influenced by identity with quite *different* groups such

as families, communities, occupation groups, classes, gender categories, and others, and the influence of the different identities can take quite dissimilar forms.¹⁰ This is a rich field for investigation, which has been fairly extensively neglected in modern economics, thanks to the restrictive discipline imposed by the wide—almost universal—use of the assumption of self-interested behavior.

ECONOMICS AND NONECONOMIC VARIABLES

The emergence of economics as a separate discipline is not new. It is arguable that modern economics has had two distinct origins.¹¹ One of these origins relates to philosophy, and in particular to ethics, and there is a long tradition whereby economic contributions have come from people deeply concerned with moral philosophy. The line from Aristotle to Adam Smith, John Stuart Mill, Henry Sidgwick, Francis Edgeworth, Knut Wicksell, Alfred Marshall, and A. C. Pigou can be easily traced. The other origin relates more to natural science and engineering, and here one thinks of the line of tradition running through Petty, Quesnay, Lavoisier, Ricardo, Cournot, Walras, and others.¹² While the former origin makes it easy to link economics with complexities of human behavior and reflection, the latter has tended to emphasize the problems of technology, tastes, input-output relations, and other such interdependences. Neither tradition can encourage economics to be a discipline unrelated to others.

However, the point has been often made—with some justice—that the nature of economic discipline has become unduly narrow in recent years, and even if technological interdependences have not been ignored, the social influences on economic variables have been frequently neglected. Human behavior is influenced by a variety of considerations that go well beyond the narrowly economic. Political, sociological, anthropological, and other influences on economic variables are often extremely important. That these influences are often neglected is hard to deny. Of course, it is true that deliberately limiting the scope of one's exercise may make it more tractable, and from this point of view, neglecting some influences may not, in itself, be a

wrong thing to do. It is, however, arguable that the neglect has been much more severe than that, and it has played a counterproductive role, making the assumed economic behavior thoroughly implausible and, in fact, also unhelpful for causal and predictive analyses.

Whatever the reason for the neglect of related disciplines in economic arguments, the case for having closer relations is fairly widely accepted. But an interesting question arises regarding the form that such a relation should take. Sometimes the proposed relation has been given a rather “imperialist” form, with economic theorists adhering strictly to their astonishingly narrow methodology and then applying, with remarkable confidence, that slim methodology to other disciplines as well. Interesting and innovative examples of such extremist consistency can be found in the writings of Becker (1976; 1981), among others.

The motivation underlying the proposed integration has much to commend. It is indeed arguable that if economists really do believe that these narrow technologies work so well in economics, then it is natural to see whether they could not be fruitfully used in related disciplines as well. But the rub lies precisely in the antecedent assumption, since it is far from clear, to put it mildly, that these narrow technologies work at all well in analyzing economic problems themselves.

The variety of social, political, and ethical influences that “the economic approach to human behavior,” as it is rather misleadingly called, leaves out makes that approach inappropriate for use not only in the related disciplines but also in economics itself. What is problematic in this attempt at extending the so-called economic approach to other disciplines is not so much the differences between the nature of economics and these other disciplines, but the limitations of the “economic approach” applied to *economics itself*. The theoretical models of the economic approach prove to be inadequate not only in handling social problems, but also in providing a reliable guide to human behavior in economic decisions—for example, in the determination of work efforts, factory discipline, and economic productivity. The narrow structure of human motivation assumed in the “economic approach” offers very little explanation of some central economic phenomena, such as

differences in the experience of productivity growth in different economies—for example, in Japan with its enormously powerful social motivation vis-à-vis, say, the United Kingdom (Morishima, 1982; Dore, 1987). Especially in dealing with production efficiency involving individual efforts and traditions of cooperation and discipline, the need to take a broader view of the economic problem itself is hard to deny.

It is, in fact, plausible to argue that while the gap between economics and related disciplines should clearly be reduced and while the program of “extending the domain” of economics is not in itself objectionable, the expansion has to take a broader view of the respective disciplines themselves (Sen, 1973, 1987; Hirschleifer, 1984). Many *economic* variables are also social, political, or anthropological. The different disciplines do not provide a “partition” of areas of concern, with each phenomenon being *either* economic *or* social but never both. The issue, therefore, is not so much that of pursuing interdisciplinary works, and even less of just expanding the domain of economics. It is primarily one of recognizing the inescapable intersections between the different disciplines that make economic phenomena frequently inseparable from those that are traditionally studied in the related disciplines.

A CONCLUDING REMARK

In this essay on the methodology of economics I have been both defensive and critical. The starting point of the paper was the recognition of the heterogeneity of economics as a subject and the diversity of our concerns, motivations, and objectives. I have argued against those criticisms of particular traditions or particular approaches that seem to take inadequate note of the pluralities involved.

I have tried to show that a number of disputes that have engaged a great deal of attention are, in fact, based on misunderstanding the diversities of the exercises involved. For example, Friedman’s insistence on the importance of predictive efficiency can be easily accepted on its own, *without* that acceptance having the effect, claimed by Friedman, of undermining the concept of descriptive reality and of endorsing the

positivist methodology in general. Similarly, Sraffa's attempt at analyzing economic reality without counterfactual concepts turns out to be of much interest of its own, without doing much to undermine the prevalent traditions of predictive economics. Similarly, while testing is of crucial relevance to certain types of economic propositions, its usability in many other types of descriptive, evaluative, and even predictive economics has to be seen as severely constrained.

However, even after taking note of the heterogeneity of the subject and the diversity of our pursuits, there remain pertinent criticisms of the dominant traditions of modern economics, and several of them turn out to be significant and important. I have tried to discuss what is involved in some of these challenges, concerning the nature of economic rationality, uses and abuses of the idea of equilibrium, the relation between economics and related disciplines, the use and legitimacy of values in economic analysis, and so on. Frequently, the answers that have emerged have also pointed to the need to extend and reformulate the critical questions. Given the nature and scope of the attempted scrutiny, that fact, at any rate, need not come as a surprise.

NOTES

This paper is written in honor of one of the great teachers of economics, Professor Bhabatosh Datta of Presidency College, Calcutta, with warm memories of some of the finest lectures I have ever heard. For helpful discussions and comments, I am most grateful to Sukhamoy Chakravarty, Terence Gorman, Dieter Helm, John Hicks, Arien Mack, Henry Peskin, and two anonymous referees. The paper draws on an earlier essay written for *Resources for the Future*.

1. On different types of uses of counterfactuals in the social sciences, see Elster (1978).
2. For a normative defense of such a framework, see Bauer (1981). On related issues, see also Buchanan and Tullock (1962).
3. On that motivational question, see Dobb (1937). I have discussed some aspects of this question in Sen (1978).

4. One early and consistent critic has been John Kenneth Galbraith, and the particular roles of modern businesses and professions on which he has focused are certainly hard to capture within the restricted structure of general equilibrium theory. See particularly Galbraith (1952, 1958, 1967).
5. I do not think Kornai will deny this either. Indeed, in his own pioneering contributions to the “economics of shortage” (1980), he does make extensive use of general equilibrium reasoning.
6. I have discussed the nature of “description as choice” in Sen (1982). The issue relates closely to the general methodological question concerning the so-called science-ethics distinction, which has often been taken to be a much firmer dichotomy than it can really be, given the nature of scientific knowledge itself. On this, see Putnam (1988). On related matters concerning economic methodology, see Walsh (1987, forthcoming).
7. On the need for using less restrictive structures in choice theory in general and in social choice in particular, see Sen (1982, 1986).
8. Even when the objectives are fixed, there remain important questions as to what would be, in fact, demanded by the intelligent pursuit of those objectives. This becomes a particularly difficult question in the presence of uncertainty, and important analytical as well as empirical issues have been investigated in this context. See, among others, Allais and Hagen (1979); Machina (1981); Kahneman, Slovik, and Tversky (1982); Levi (1986); Dasgupta (1987).
9. I have discussed the relevance of these distinctions and the relations between them in Sen (1987). On related matters, see also Parfit (1984), Dasgupta (1987), and Walsh (forthcoming).
10. For example, individual identity with the *family* interests can serve important cooperative functions within that institution and also be the means of systematic inequalities between men and women in the division of joint benefits, going against *gender* identities. I have discussed the importance of the coexistence of these issues in Sen (1985, 1989).
11. For a more extensive discussion of this claim, see Sen (1987). For

an analysis of a different but related principle of classification of economic traditions, see Baeck (1988).

12. To some extent each economist in the two lists had diverse interests and motivations, and the classification is an attempt to identify the *main thrust* of the contributions of each. Among the great nineteenth-century economists, Karl Marx is perhaps the hardest to classify, since he had remarkably strong links with both traditions. The Marx of *The Economic and Philosophic Manuscript* and *The German Ideology* was more clearly in line with the former legacy than the Marx of *Capital* and *Critique of the Gotha Program*, though even these later documents have strong elements of social philosophy and psychology.

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