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SOCIAL PROBLEMS AND SOCIAL SCIENCE

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I

From even a cursory survey of the growth of social sciences, particularly economics, in the past few decades, three major characteristics can be discerned. First, there is the impressive expansion, diversification and sophistication of these sciences. The rapid increase in the research output, the substantial growth of professional journals and the emergence of funding agencies specifically meant to support social science research have been noticed all over the world, including in our own country. Not surprisingly such rapid expansion has also led to increasing specialisation even within individual disciplines. Following from it there has also been visible improvement in the professional quality of the work being done. Here, perhaps, economics is way ahead of the other social sciences. The general systematisation of its body of knowledge, the logical perfection of the large number of models that have been produced and continue to be churned out, the sharpness and precision of its analytical tools, and the overall elegance and finesse that it has gained have all given it a unique standing among the social sciences and formal recognition as a science. The second aspect that can be noticed is the way in which many mundane problems of society have made inroads into the deliberations of the scholarly community. Here again, at least economists have come to have, during the past two or three decades a recognised and respectable standing as advisers on questions of social policy. Neither has it been a one-way relationship. Involvement with practical problems has led to professional advancement also. It is on record that the development of linear programming as

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an analytical technique owes much to the efforts to coordinate military operations. Such involvement with practical problems has also led to the third characteristic that can be noted of the development of social sciences in the past - a general feeling of inadequacy to comprehend and analyse pressing problems of the day. Leontief expressed it about a decade ago in words which have now become well known. Referring to the growth and popularity of economics as a science on the one hand and an uneasy feeling about its analytical adequacy on the other he asserted before his professional colleagues: "In an almost Pavlovian reflex, whenever a new complaint is raised, President Nixon appoints a commission and the university announces a new course... The trouble is caused, however, not by an inadequate selection of targets, but rather by an inability to hit squarely any one of them. The uneasiness of which I spoke before is caused not by the irrelevance of the practical problems to which present day economists address their efforts, but rather by the palpable inadequacy of the scientific means with which they try to solve them".[1] Joan Robinson is even sharper when she complains of "the evident bankruptcy of economic theory which... has nothing to say on the questions that, to everyone except economists appear to be most in need of an answer".[2] That these are not merely personal expressions of a passing mood can be seen from the many writings of the same genre that have been appearing in recent years.[3]

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1. W.Leontief, "Theoretical Assumptions and Nonobserved Facts", Presidential Address, American Economic Association, December 1970, American Economic Review, Vol.LXI, March, 1971 (Emphasis in the original).

2. Joan Robinson, "The Second Crisis of Economic Theory", American Economic Review, Vol.LXII, May 1972

3. For instance see - E.H. Phelps Brown, "The Underdevelopment of Economics" Economic Journal, Vol.82, March 1972; G.D.N. Worswick, "Is Progress in Economic Science Possible?", Economic Journal, 82 March 1972; E.J. Minhan's The Costs of Economic Growth (Harmondsworth, Penguin Books Ltd., 1967) and Narindar Singh's Economics and the Crisis of Ecology (Delhi, Oxford University Press, 1976) also deal with the inadequacies of economics to deal with pressing economic problems.

The lamentations are also accompanied by diagnoses. Thus according to Leontief the root of the problem is the shaky empirical foundations of the subjects; Joan Robinson puts the blame on the constricting equilibrium analysis; Georgescue-Roegen sees the problem in the economists' tendancy to ignore the physical nature of economic processess; and Myrdal locates the malady in the neglect of the institutional dimension in modern economic theory.

All these are true as far as they go. But they do not seem to go far enough to diagnose the emptiness that accompanies elegance in modern social science theory, particularly economics. The problem is in fact much deeper. One is reminded of Oswald in Ibsen's Ghosts. He sets out to be a creative artist, but soon learns that he has an incurable disease which makes it impossible for him to think and work. And then he discovers that he had had the disease practically from his birth - "the sins of the father" whom he was taught to admire as a virtuous man. The fact is that a great deal of what parades as the science of economics was conceived in sin. The sin has been that of a systematic rejection of the social context and content of the subject to establish its claim as a science 'pure and simple'.

The burden of this paper is that the glaring gap between social problems and the sciences that claim to deal with them can be reduced only by reversing the process that has been going on for over a century and by discovering ways of earthing them in the social reality which they are supposed to represent and analyse. It is, therefore, not enough merely to identify the symptoms of today's maladies or even to discover the nature of the original sin, but to trace the character of the 'development' of the science during the past century or so to be able to ask whether the future course of development can be given a different orientation. A quick survey of the manner in which economics has deviated from its original moorings and purposes even as it metamorphosed itself into a sophisticated system of logical propositions and quantitative procedures is attempted in the next section.

In the early days of the emergence of economics as a special field of enquiry it was concerned with the crucial and real problems of the times viewing them in a wider social context. Adam Smith, for example, produced his analytical system within the context of the social classes of his own country. And all the classical economists were concerned with the laws of economic development, i.e., the processes of production and distribution in the course of which relationship among men are generated through the use of material resources. The emphasis on the societal aspect of economics was so strong that even Walras writing one of the most abstract treatises of his times which he called the Elements of Pure Economics had to concede that it was also The Theory of Social Wealth. In the early days of economics it was generally recognised also that production and distribution were not separate activities, but closely linked up. In the appendix to his A Contribution to the Critique of Political Economy (1859) Marx expressed it thus: "The structure of distribution is entirely determined by the structure of production. Distribution itself is a product of production, not only with regard to the content, for only the results of production can be distributed, but also with regard to the form, since the particular mode of men's participation in production determines the specific form of distribution, the form in which they share in distribution." Adam Smith and Ricardo, we can surmise would have had no hesitation in endorsing formulation. Smith, Ricardo and Marx were also concerned with the unfolding of the economic processes, which necessitated history and historic time being recognised as an inherent part of the analytical procedure.

The first major deviation from the classical position in economics described above came in 1848 when Mill sought to draw a distrinction between production and distribution because, as he claimed, 'the laws and conditions of the production of wealth partake the character of physical truths' while distribution of wealth was 'a matter of human institutions solely', paving the way for the view stubbornly held by many economists even today that what is required in the context of

mass poverty is an increase in production first and then a more equitable sharing of the larger cakes. For the sake of the logical systematisation of the emerging science Mill had - "for convenience of analysis", as it would be described by the latter day "scientists" - distorted one of the basic aspects of economics as a discipline dealing with social issues. Once such procedure was started it would soon be picked up and built upon by others.

The marginalists for instance, in their eagerness to convert economics into a "physico-mathematical science" found it convenient to discard all the historico-relative aspects that Mill recognised as governing distribution and to concentrate on production alone. Jevons, in particular, in his attempt to rebuild economics as "the mechanics of utility and self-interest", found it necessary to have a unifying principle for his system, like gravity in Newtonian physics, and hit upon margianl utility as the central concept of his analytical system with anything else that was not quite compatible with it having to give way for the sake of logical clarity and convenience. The marginalists succeeded in unifying the whole organon of 'pure' economics in the light of a single principle in which Walras, with his simultaneous equation system showed "revolutionary creativeness" according to Schumpeter. But again national purity and logical perfection was achieved by discarding a great deal of societal considerations which, by this time, had come to be dead weights from the point of view of pure theory. Thus to demonstrate the timeless logic of allocation it became necessary to work with given resources. The supply - demand equality of the given resources (or commodity set in more modern formulations) at equilibrium prices led to the concept of general equilibrium and consequently to the assumption that all resources can be treated as fully employed, or as capable of being fully employed if only prices would be permitted to be flexible. Further, it led to the formulation, on the basis of logical derivations that after all, even distribution was but part of the pricing process. As Schumpeter sums up this point: "The requisites or

factors or agents of production are assigned use values : they acquire their indices of economic significance and hence their exchange values from the same marginal utility principle that provides the economic significance and hence explains the exchange values of consumable goods. But those exchange values or relative prices of the factors constitute the cost of production for the producing firms. This means, on the one hand that the marginal utility principle now covers the cost phenomenon and in consequence also the logic of the allocation of resources (structure of production), hence the 'supply side' of the economic problem so far as all this is determined by economic considerations. And it means, on the other hand, that, in as much as costs to firms are incomes to households, the same marginal principle, with the same proviso, automatically covers the phenomenon of income formation or of 'distribution' which really ceases to be a distinct topic, though it may, of course, still be treated separately for the sake of convenience of exposition."[4] J.B. Clark would later on push this further and claim that since the single theory of marginal productivity was capable of explaining all factor prices, it was a universal and natural law and consequently paying each factor the equivalent of what it 'contributed' to production was a morally valid principle. It is interesting to see how a science that claims to be value neutral and institutionally neutral comes in handy to confer moral sanctity to the institutions of private prosperity, market economy and the socio-economic system of capitalism which is but a short-hand description of such institutions.[5]

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4 J.A. Schumpeter : History of Economic Analysis (New York, Oxford University Press, 1954) p. 913 emphasis in the original.

5. This is not altogether an accident. Meek has pointed out that in developing their theoretical system one of the consideration of the marginalists was to oppose the labour theory of value - "a task which became more and more urgent as Marxist ideas began to grow in popularity". He also says: "The key fact about J.B. Clark's marginal productivity theory of distribution, as his son has recently reminded us, was probably that 'his statements are oriented at Marx, and are best construed as an earnest and not meticulously qualified rebuttal of Marxian exploitation theory'. Ronald Meek, "The Marginal Revolution and its Aftermath" in E.K. Hunt and Jesse G. Schwartz eds., A Critique of Economic Theory (Harmondsworth, Penguin Books Inc., 1972) p.92. The point here is to note that what is frequently claimed to be "pure" theory may not be so "pure" after all!

What is of greater significance is to note how a field enquiry that started out as a study of the social conditions and consequences of the production and distribution of resources over time becomes, through the application of the scientific method of abstraction and systematisation a science concerned with the psychological propensities of isolated individuals, the timeless process of the transformation of one set of goods into another set of goods producing 'utility' (and only utility) in that process, the equating of the 'objective' and 'subjective' rates of transformation and generating a set of indices of economic significance. Is it surprising that a 'science' reduced to (or raised to!) the level of a private affair between me and my goods has become palpably inadequate to deal with the wide range of social problems of the contemporary world?

It must not be considered, however, that that kind of economic theory was trying to glorify the individual or individualism as a philosophy. Far from it. As the theory developed it became clear that the individual, including the consumer who was said to be king, could easily be dispensed with. Pareto had maintained that once the theorist determined the means at the disposal of the individual and obtained a photograph of his tastes, the individual could disappear. And, according to Schumpeter even in the theory of consumers' preference, the consumer is but the clothesline on which the economist hangs his predetermined analysis. Stigler has gone a step further and stated that it does not affect the formal theory of demand in the least whether the individual maximises wealth, religious piety, the annihilation of crooners of his waistline! In other words economics is concerned with maximisation subject to constraints, irrespective of what is being maximised and what the constraints are. Economics has become a highly formalised and 'precise' discipline, although in that process it has had to shed practically all of its social content. As Meek aptly expresses it "modern economists, like modern artists and poets seem all too often to feel quite at home in a world where the form is reality, of which the substantial is only a shadow"[6]

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6 Ronald Meek, loc. cit., p.96

III

It will certainly not be correct to maintain that what has been described above is the only tradition in economics. But no other school of thought in economics shows as sharply as the neo-classical system how a social science can become perfected in its formal structure by deliberately abstracting itself from the social problems from which it arose and which still continue to be the stuff that it is supposed to deal with.[7] In fact, if the neo-classical system were to be treated as merely a logical or mathematical system, it would have been possible to appraise it along those lines. But the claim of neo-classical economics is that its formal structure enables it to deal with problems of production, distribution, trade, growth, and anything else that can be claimed to be an economic problem. It is well known also that the forerunners of the neo-classical tradition like Jevons and Walras and its modern adherents have all been interested in "policy" questions. Hence it is important to see the extent to which a highly abstracted and formalised "science" can address itself to practical problems.

First it must be conceded that in the world as it really exists there are many problems that lend themselves to be dealt with by the neo-classical theory. There are profit maximising producers who confront market situations of similar to what neo-classical theory postulates. Many aspects of the pricing of goods and even of factors of production approximate the pattern that it describes. The partial equilibrium version of it does provide a frame for the analysis of a large class of problems and its general equilibrium version draws attention to a certain kind of interdependence within the economy.

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7 Even while laying the foundations of a formal science of economics, Lionel Robbins in his An eassy on the nature and significance of Economics Science stated that the postulates of economic theory "are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious".

But a logically self-contained axiomatic system also has to insist that only those problems that come within the frame of reference can be treated as its problems. Thus, a theorist may recognise that an individual's consumption patterns are substantially influenced by a variety of factors other than prices, but he would have to class them all as "non-economic" and confine his analysis to those factors that his theory would permit him to accept as "economic". Or, it may be a well known fact that an increase in the price level has been caused by hoarding, but the analyst would feel obliged to explain the phenomenon in terms of the categories that his system provides, possibly the quantity of money and the velocity of its circulation. Hence a theoretical system that is logically self-contained and thus closed has, of necessity, to categorise all phenomena into two - those that belong to its domain and those that do not. Frequently, the excluded category contain sets of items discarded at some stage "for convenience of analysis" and thus completely arbitrary in terms of the real nature of the problem itself. And yet, once they are excluded for logical reasons, they remain for ever excluded from the domain of the system. This is how property and power came to be excluded from the neo-classical system - property, by the decision at some stage that what is important is to see how resources are allocated taking the distribution and ownership of resources as "given", and power, by assuming that it is equally distributed[8] - and they have for ever remained outside the purview of the theory, and thus by definition beyond the scope of "economics". 'A highly formalised and systematised "science" thus may have as its basis a series of illicit abstractions which make it for ever blind to a number of important substantive issues even within the domain of its subject matter.

A related problem is the inability of a closed system to recognise new problems. This is partly because a closed system comes to concentrate on its internal logic and logic, as Georgescue-Roegen points out, helps only present thought already thought out, but cannot

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8 See J.K. Galbraith, "Power and the Useful Economist" American Economic Review, Vol.LXIII, March 1973.

help think out new thoughts.[9] In the case of a social science like economics it is also because of difficulty of distinguishing between the logical constructs of the system on the one hand and their everyday or lexical connotation on the other. There have been several classical examples of this kind of confusion. Thus, in the midst of the most acute and extensive unemployment situation that the Western World was facing in the early thirties distinguished economists were busy demonstrating that "unemployment" was a theoretical impossibility! In our own times well known economists have found it difficult to draw the necessary distinction between the theoretical concept of "scarcity" and the finitude of physical resources in the "limits to growth" discussions and have asserted that economists have always known that resources are scarce and have also known how to deal with such problems![10] We know also that in the early stages of the discussion on the problems of "underdevelopment" most economists believed that there were really no new issues involved in them and that the "theory of growth" with appropriate modifications would be able to deal with them all. Even to-day there are economists for whom the last decades of the twentieth century does not present problems substantially different from what they were at the beginning of the century, if not earlier. Social sciences can thus become blinders distorting and constricting the views about reality instead of illuminating it.

A logically closed system which in its quest to arrive at formal precision has discarded vital substantive aspects also becomes, not unexpectedly, a close ally of the status quo. There is first the decision that what is more important is to spellout the static nature of the interconnections between the parts of the system than to understand its metamorphosis over time. The mechanistic analogue

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9. N. Georgescu-Roegen, Analytical Economics: Issues and Problems (Cambridge, Harvard University Press, 1967) p.14.

10. See R.M.Solow, "Is the End of the World at hand?" in Andrew Weintraub et al (eds.) The Economic Growth Controversy (London, Macmillan, 1974).

used for the building up of the theory then makes it unnecessary to take time seriously at all which makes it even difficult to recognise that the whole analysis cannot but land support to the status quo. "Mechanics knows only locomotion", says Georgescu-Roegen, "and locomotion is both reversible and qualityless. The same drawback was built into modern economics by its founders... And these architects succeeded so well with their grand plan that the conception of the economic process as a mechanical analogue has ever since dominated economic thought completely. In this representation the economic process neither induces any qualitative change not is affected by the qualitative change of the environment into which it is anchored. It is an isolated, self-contained and a historical process."[11] Koopmans has also referred to the scientific conservatism of economics which "shields received economic theory with an appearance of invulnerability"[12] which must also be related to the basic assumption that economics as a science deals with timeless, universal "realities".

The pathological inability to perceive the inevitability of change also leads neo-classical economics to pretend that as a science it can be and should be value-neutral.[13] If the possibility of change is recognised, it becomes necessary to consider how to respond to it, and also how to influence it. Under such circumstances value-neutrality is not a tenable position. The paradox of the neo-classical position is that in a world of rapid changes it must defend the status quo by not recognising change, and yet profess to be value-neutral as well.

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11. N. Georgescu-Roegen, The Entropy Law and the Economic Process (Cambridge, Harvard University Press, 1971) p.2.

12. T.C. Koopmans, Three Essays on the State of Economic Science (New York, Mc Gran-Hill, 1957) p.142. But what we have here is not only "scientific conservatism". The statement that Pareto optimality is reached if it is not possible to make any one better off without making someone else worse off in terms of his own preferences gives every single individual who is contended with things as they are the veto power against any attempt to change the status quo.

13. Narindar Singh refers to this as economic theory's "canonization of oughtlessness". op.cit. p.80

IV

What emerges from the discussion of the neo-classical science of economics and its relationship to the social reality it is purported to deal with is that "the more intricate a modern science becomes and the better it understands itself methodologically, the more resolutely it will turn its back on the ontological problems of its own sphere of influence and eliminate them from the realm where it has achieved some insight" as Lukacs stated more than half a century ago. He had gone on to say: "The more highly developed it becomes and the more scientific, the more it will become a formally closed system of partial laws. It will then find that the world lying beyond its confines, and in particular the material base which it is its task to understand, its own concrete underlying reality lies, methodologically and in principle, beyond its grasp."[14]

In understanding the relationship between social problem and social sciences it raises a number of crucial issues. It has been debated, for instance, whether a subject like economics can become a theoretical science at all. The positivist position on this question, as is well known, is that while the physical and the social sciences differ in their subject matter, in terms of method, there is a universal scientific method and there is no reason why the social sciences cannot appropriate it.[15] The opposite view that social sciences cannot become theoretical sciences has also been put forward with convincing arguments to support it.[16] The debate has not been conclusive; in fact it cannot be because concepts such as "science" and "theory" are not used unambiguously in the discussion.

Another position, more as a protest against the problems created by a priori "theoretical" investigations, has been to suggest that social problems can be examined and understood solely in terms of factual evidence. But such anihilistic attitude is not rewarding in the

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15. See Ernest Nagel, "Problems of Concept and Theory Formation in the Social Sciences" in Maurice Natanson ed., Philosophy of the Social Sciences: A Reader (New York, Random House, 1963).

16. N. Georgescu-Roegen, Analytical Economics: Issues and Problems (as cited in 9 above) Introduction.

14. George Lukacs History and Class Concciousness (London, Marlin Press, 1968) p.14 (Emphasis in the original)

long run because in an evolutionary as against a mechanistic context gathering of facts also would require some theoretical frame, however implicit it may be.

The point is that no purposive enquiry of social problems can be conducted, and no adequate understanding about them can be arrived at without some "method", some "theory", some "science", however these terms are defined. Hence what is important is to specify, as clearly as possible, what procedures of enquiry are possible and necessary to make sense of social problems.

There are two distinct procedures that require careful examination. The first is the postulational or axiomatico-deductive method. If we are to take Schumpeter's word for it this method is as old as economics itself. Discussing the characteristics of the "classical system" he says: "Their achievements therefore were analytical and it is this which is usually meant by the most unfortunate terms 'deductive' abstract, 'aprioristic'. Their chief aim was to order intellectually and to clarify the day to day happenings in the economy in order to arrive at an axiomatic understanding of its basic factors".[17] But it is not quite correct to say that an attempt to "order intellectually" the unordered facts of a science makes its axiomatic. The axiomatic or postulational method consists of dividing theories into two distinctive parts, the first "syntactical", subject only to the laws of logic and the second "semantical" consisting of the empirical content which calls for interpretation. The procedure is to set up a purely formal syntactical system of logical relationships first in terms of postulates which simply express logical relationships among primitives of the kind "If P, then T", and then to give the system a semantic transformation through finding appropriate empirical counterparts to the primitives. In this strict sense it is neo-classical

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17. J.A. Schumpeter, Economic Doctrine and Method (London, Allen and Unwin, 1954). p.90.

theory that has been stated in postulational terms in recent years.[18] The substantive problems of neo-classical economics have already been noted, and so only a few brief comments about the postulational method as a method in the social sciences will suffice here.

First, although at the conceptual level one can speak about converting a syntactical system into a semantical one , the procedures for achieving such a transformation are not at all clearly laid down. Part of the problem is that the transformation is not a logical step and hence cannot have a priori rules. Consequently, it has not been easy to decide whether the primitives and postulates of a semantical version have to be "realistic" or not.[19] In other words, while the postulational method can spell out the logical procedures required for the formal aspects of a theory, it cannot say anything about its content. Hence theories derived via the postulational procedure cannot be claimed to be realistically valid even when they are formally sound. Surely, this is the basis of a great deal of empty formalism.

Secondly, and following from the first, it is difficult to decide how much of the formal properties of the syntactical version can be claimed for the semantical part also. One of the commonest confusions arising from this is in relation to the "universality" that is claimed for theories. In the syntactical sense the "theoretical" propositions deduced from the postulates are "universal". But that is only logical universality in the sense that given the premises the conclusions necessarily follow. That by itself does not imply any kind of empirical universality of the semantical version, and yet frequently such claims are put forward.

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18. See particularly T.C. Koopmans, op.cit and the literature referred to in it. Also C.T. Kurien, A Theoretical Approach to the Indian Economy (Bombay, Asia Publishing House, 1970). In passing it may be mentioned that in this work I had attempted a postulational procedure to depict the Indian Economy. For a very different procedure to understand the Indian Economy see my more recent work, C.T.KURIEN, Poverty, Planning and Social Transformation (Delhi, Allied Publishers, 1978).

19. The "Friedman Controvery" relating to this issue is well known. See Milton Friedman, Essays in Positive Economics (New edition Phoenix Books, University of Chicago Press 1966) and also discussions on "Problems of Methodology" in American Economic Review, Papers and Proceedings, May 1963.

Thirdly, and from the point of view of the relationship between social problems and social sciences perhaps the most important, an underlying assumption of the postulational method is that reality is the empirical counterpart of a basically mental operation, that knowledge is essentially praxeological as Ludwig Von Mises claimed. It can be seen that very deep and fundamental issues are involved here.

In this sense Max Weber's "ideal-type" approach is closely related to the postulational method. Max Weber's procedure is to set up "ideal types" to have a standard by which to guage the real world. The real is to be understood in terms of its deviation from the ideal. Says Max Weber: "The kind of ideal typical model of social action which is constructed, for example, for the purpose of economic theory is therefore 'unrealistic' in so far as it normally asks how men would act if they were being ideally rational in pursuit of purely economic goals."[20] The perfectly competitive model with its highly unrealistic assumptions is, thus, claimed to be an ideal type - not to be seen anywhere at all in the world - to see to what extent any actual situation deviates from the ideal, theoretical norm. What is implied here is that reality must be perceived as a deviation from some mental construct: primacy, once again, is to the mental construct.

Whatever may be intensions of the postulational and ideal-type approaches, they both can, and often do, turn out to be distortions of reality. They both arise from the powers of the mind to produce versions of reality through the process of abstraction. Abstraction is a necessary aspect of any science. But it must be recognised that there is both a legitimate and illegitimate procedure of abstraction.

The most important aspect in understanding social problems is to discard all false abstractions that distort them, and to go to their essence through a legitimate procedure of abstraction. Is there

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20. W.G. Runciman, ed., Max Weber-Selections in Translation (Cambridge, Cambridge University Press, 1978) p.24.

such a method available to the Social sciences?

Before answering that question it is important to establish why abstraction is necessary in the process of understanding. One of the clearest formulations of this is in Mao-Tse-Tung's well known piece "On Practice". After unambiguously stating that "the truth of any knowledge or theory is determined not by subjective feelings, but by objective results in social practice" Mao goes on to describe the process of the development of knowledge. He draws a distinction between the perceptual and the conceptual stage of cognition. The perceptual stage captures the external relations of things in terms of their visible manifestations. From several perceptual impressions (in the process of practice) a leap is made into the conceptual stage. "Concepts are no longer the phenomena, the separate aspects of external relations of things; they grasp the essence, the totality and the internal relations of things.' It is from this stage that inferences are drawn and judgements are made. Abstraction is necessary to move from the perceptual to the conceptual level, and is thus an important aspect of the efforts to understand concrete social problems.

The difference between this - dialectical method and the postulational method described above is, therefore, not in terms of abstraction per se which is involved in both of them. And yet the second method is distinctly different from the other.[21] First it is glued to the real world as it is, and to concrete problems as they are. The attempt is not to produce an ideal or ideational representation of reality through a priori logical speculation and to go about searching for its empirical counterpart or to see how far reality is removed from it. The attention is always on the real and the concrete;

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21. Among the many writings on the dialectical method I have found the following two particularly helpful. George Lukacs, op.cit and Louis Althusser, For Marx (New York, Vintage Books, 1970). It is not certain whether there is, or can be, any definitive account of the dialectical method. By far the best application of the method is Marx's The Eighteenth Brumaire of Louis Bonaparte.

abstraction is only an aid to understanding. The flight to the realm of logic is only to get a better vision of the real world to return to it to continue the action.[22] Secondly, the dialectical method does not provide any logical procedures to see whether the abstraction is legitimate or not. The validation of theory comes only through practice. "Practice", in this context must not be misconstrued to mean some a priori determined line of action. Mao gave it a very broad span. "Man's social practice is not confined to activity in production", Mao said, but takes many other forms - class struggle, political life, scientific and artistic pursuits; in short, as a social being, man participates in all spheres of practical life of society. Thus man, in varying degrees, comes to know the different relations between man and man, not only through his material life but also through his political and cultural life (both of which are intimately bound up with material life)." What the dialectical method insists upon is, therefore, that the divorce between life and learning is not tenable and that scholarly activities have no validity except in the context of the full and varied dimensions of social life. Any social science that explicitly or even implicitly negates or ignores the vital link between life and learning, between society and scholarship can no longer claim to be a social science, whatever else it may be'.

In passing, it must be pointed out that by merely invoking dialectices, a method does not become dialectical. For although the postulational method cannot be metamorphosed into the dialectical method, what is claimed to be the dialectical method can easily degenerate into a postulational procedure with firm a priori decisions not only about the methods of enquiry, but also about the nature of the problem. The emphasis on history in the dialectical method, for instance, frequently results in producing "as if" history.[23] And in our own context today, it is not

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22. "Abstraction is the most valuable ladder of any science.... However, the task of science is not to climb up the easiest ladder and remain there for ever distilling and redistilling the same pure stuff." N.Georgescu-Roegen, Analytical Economics p.104.

23. C.T. Kurian, "Abstract Generalisations" Economic and Political Weekly, Vol.XII, No.10, March 5, 1977.

difficult to see that many who swear by the dialectical method are busy trying to fit production relations where neo-classical economists were once known to fit production functions.

V

We may now turn to a more detailed examination of the relationship between social problems and social sciences. It can now be stated unambiguously and unapologetically that the subject matter of social sciences is - must be - actual social issues, however mundane and"non-theoretical" they may appear to be. The tendency noticed among social sciences, particularly economics, to parade as "pure and universal sciences" (whatever that expression may mean) is to go contrary to their essential and inevitable social and historical conditioning. The main responsibility of the social sciences is not to discover "laws" or "law like statements" about their own created universe of discourse. Rather, social sciences are, and can only be applied sciences "closely related to judgements and assessments of actual systems and policies".[24]

The statement that the social sciences must accept social problems as their areas of concern should not be given too abstract an interpretation. It would mean, for instance, that what are commonly described as "current problems" would constitute a large share of the problems that social scientists take up for research and study in depth. Dobb explains why this is so. "Current problems are something created as much by thought-inspired human action upon an existent situation as by the given objective (but changing) situation itself; and in this sense can be said to represent continually, in varying degree, a contradiction between the two. Problems arising in this way then form the starting-point of new thinking, the formation of new concepts and of

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24. Maurice Dobb, Theories of Value and Distribution since Adam Smith - Ideology and Economic Theory (Cambridge University Press, 1973) p.16.

new theories".[25] And Myrdal has observed: "Rarely, if ever, has the development of economics by its own force blazed the way to new perspectives. The cue to the continual reorientation of our work has normally come from the sphere of politics."[26]

If this is correct, it has three implications. The first is that the polarisation that is usually sought to be established between applied or "problems oriented" research on the one hand and "basic" or "fundamental" research on the other is not valid in the social sciences. The responsibility of the social scientists is to go to the basics or fundamentals of the concrete problems that they take up for study so that these become adequately illumined and properly understood. Fundamental research, therefore, must aim to supply an additional dimension or a deeper perspective to practical social problems, and the role of the social scientist qua scientist arises from the fact that among the many (including himself) for whom the problem is a real one, he alone has the training, the insight, to supply that additional dimension or deeper perspective. Secondly, if social sciences deal with live (and for that reason possibly controversial) current problems which are of concern and consequence to non-specialists, the scientists have a responsibility to communicate their insights in a language that the non-specialists can understand. This will not be easy because when the scientists move from the perceptual to the conceptual stage of understanding through the process of abstraction they are, in a very real sense, setting up a new universe of discourse: they are indeed, "taking off" into new heights. But the take off is not meant to escape into a neater world of logic, but to return to the crude, but real world with new perspectives. However, "re-entry" is even more difficult to achieve than take off itself, partly because the scientist's mind has a craving for the neat regularities of the world of

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25. Ibid, p.17

26. Gunnar Myrdal, Asian Drama (New York, Pantheon Publishers, 1968) Vol.I p.9,

words, and partly because the world of words is highly susceptible to the dangerous virus of reification. It is generally recognised that scientists must communicate with fellow scientists to be stimulated and challenged, but in the social sciences where the object of study are real to non-specialists also, the scientists must be challenged by them as well.[27] This is part of the process of validation of scientific endeavours in and through "practice". It must be admitted that social scientists, particularly those who claim to be committed to the dialectical method, appear to be unwilling to be challenged by the non-specialists. Social scientists seem to be more eager to mystify those around them by reciting mantras (the mantras may be bits and pieces of sacred texts from the past or mathematical equations from the latest professional journals - the distinction between the two is immaterial from this point of view) than to enter into serious dialogues with them. In this attempt to preserve the status of the trade, they also shut themselves from the breath of fresh air that can come only from those for whom encounter with social reality is a matter of first hand experience. Thirdly, where social scientists are concerned with live problems they cannot afford to retreat into the sanctuaries of their own disciplines. The procedure of viewing social problems in terms of disciplinary specialisations is at times a matter of convenience, but if it is recognised that social reality cannot be partitioned on the basis of subject specialisations, it will become obligatory to examine how useful it really can be. In fact, the boundaries between disciplines are, to some extent, an indication of the alienation between reality and the mental reconstructions of it. Marshall had warned about the ease with which edifices of pure crystal can be constructed by imagination when we shut our eyes to realities. The way to overcome the problem is not to throw them all together into "multidisciplinary" or "interdisciplinary" approaches to problems, but for each discipline to discover how far removed it is from the reality it claims to represent.

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27. This would appear to be necessary in science in general. It was the physicist Erwin Schrodinger who said: "Never lose sight of the role your particular subject has within the great performance of the tragi-comedy of human life; keep in touch with life... and keep life in touch with you. If you cannot - in the long run - tell everyone what you have been doing, your doing has been worthless." Science and Humanism - Physics in our Times (Cambridge, Cambridge University Press, 1951) pp.8-9.

The question of how to study concrete problems must also be taken up for examination. A specific problem that becomes the subject matter of an enquiry is not an isolated phenomenon. It is an artificially dismembered aspect of a much wider and more complex social reality. The ability to scoop it out for purposes of observation and analysis is an achievement of the mind. But unless it is constantly viewed against the background of the totality of social reality it will become an isolated, self-contained entity. Thus, for the study of any specific problem relating to part of social reality it is important to have a frame of reference encompassing the whole of that totality and its characteristics.

But that totality itself is not an eternally permanent one. Rather, it is a constantly changing totality which cannot be isolated in time without doing violence to it. Hence specific problems must also be seen in their proper historical context. To "study" a specific problem, then, is to locate it in a total frame temporally and "spatially" larger than itself. Only than can we come to know it.

It is important to note also that the total frame referred to here is not a material structure with an immutably programmed pattern of evolution. It is essentially a social frame where human beings are constantly relating themselves to material things and other human beings. A main characteristic of this interaction is that it is directed towards influencing the social process themselves, but the ability to influence them is hardly ever evenly distributed. Thus, the social frame is always subjected to conflicting and contradictory pressures of various kinds and intensities. These pressures influence the social processes and also constitute, as Dobb puts it, "a refracting medium" affecting the perceptive and resulting vision of the particular problem or situation being studied. Dobb continues: "Social or economic action, at least, can only be conceived with some subject, whether institution, person, social group, class or organisation, in mind; and for problems to have an operational interpretation, it would seem that they must have some implied reference of this kind. This inherited framework

within which (or in reaction against which) real problems are formulated, and against the background of which, if not literally in terms of it, theoretical debate occurs, necessarily includes presumptions and general statements... These, forming a 'conceptual web' (as it has been called) or set of conceptual categories or 'boxes' in terms of which our thinking operates, are crucial both to the way in which problems are framed and to the methods and instruments devised for yielding answers to them".[28]

Concrete problems are always set in such a social milieu of inherited ideas and concepts, of existing structures of conflicting views as to what needs to be done and can be done, of urges to action and diverse efforts to influence the course of action. It is this highly complex welter that the social scientists invariably confront however limited they may think topic of their enquiry is.[29] Even their effort to understand and interpret a concrete problem, therefore, is a kind of intervention in the ongoing social process. If they realise this they will shed all pretensions of neutrality, clarify their own positions and approach their task with conviction and humility.

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28. Dobb, op.cit., pp.17-18.

29. Engels put it thus: "...History proceeds in such a way that the final result always arises from conflicts between many individual wills, and everyone of them is in turn made into what it is by a host of particular conditions of life. Thus there are innumerable intersecting forces, an infinite series of parallelograms of forces which give rise to one resultant - the historical event." - Letter to Joseph Bloch - Reproduced in Marx and Engels, On Religion (Moscow, Progress Publishers, 1957) p.24.

(This paper is based on a note that I prepared in 1978 to outline the research perspectives of the Institute when I became its Director. Since then we have had several "House Discussions" when my colleagues and I have tried to understand the academic and social responsibilities of researchers in social sciences. My colleagues have contributed a great deal in sharpening my understanding of the issues involved. I am most grateful to them, but I am solely responsible for the views expressed in this paper and the position I have taken).

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Comments on Prof. C.T. Kurien's paper "Social Problems and Social Sciences"

1. Professor Kurien, in his provocative and stimulating note has raised a number of important questions concerning developments in economic theory and offered a methodological as well as philosophical critique. I share with him his view that economic theorising must address itself to its eventual, if not immediate, applicability to social problems. I also share with him the general feeling that economic theory - the established and dominant theory - has revealed inadequacies at various levels. Here again we are in high and strong company as this inadequacy has bothered many a scholar. Having said this, I part company from Prof.Kurien on a number of points in his critique - some substantive and some referring to interpretations of the theory and to the criticisms levelled against it. In order to pinpoint these, I shall proceed along Prof. Kurien's line of reasoning.

2. Professor Kurien begins with a reference to three significant developments; first, the achievement of economic theory in terms of the general systematisation of its body of knowledge, the logical perfection of its models, the elegance and the finesse acquired; secondly, the involvement of scholars in policy problems; thirdly, the inadequacy of theory to comprehend and pressing problems of the day. Prof. Kurien refers to the criticism offered by various scholars and their attribution of the theoretical inadequacies (the aridness of theory) to a number of reasons. Prof. Leontief stresses the weak empirical foundations of theory, Prof. Joan Robinson attacks particularly the 'equilibrium' method; Georgescu-Roegen emphasizes the missperification(?) ofthe economic process; Myrdal highlights the neglect of institutional dimensions. Kurien, while accepting all these, wishes to proceed to the root of the malady - 'the original sin' which he identifies

as follows: "the sin has been that of a systematic rejection of the social context and content of the subject to establish its claim as a science pure and simple". While not denying that the theory as it has grown has shifted attention away from certain kinds of problems, restricted the domain of our understanding of the functioning of economies, I think Kurien's assessment misconstrues both the form and the source of these difficulties. On the one hand, he appears to accept the logical neatness and soundness of the neoclassical system without questioning; As is known, recent developments have proceeded to question this position, especially with regard to the theories of distribution. On the other hand, he implicitly tends to argue as if logical precision and rigour would invariably lead to aridity of theory. I shall try to argue that the main burden of the criticism of the prevailing theory qua theory must be placed on the structure of that theory as it has developed and the particulare framework it has adopted. What is required is an alternative theoretical structure and framework which also needs to be logically cogent and rigorous. I am afraid that the way Professor Kurien analyses the situation, we would be led into arguing - in my view quite erroneously - that (a) the dominant economic theory does not concern itself with social problems (which is different from saying that there are important social problems which are only inadequately analysed by the theory) and (b) that the fault lies in insisting on formal accuracy or logical vigour. I find that such criticisms are not only misplaced but may lead to other dangers. These dangers are not quite fanciful. There has been a temptation to indulge in an uncritical, irresponsible refutations of theory while substituting attractive cliches and jargon in place of analytical rigour. Let me hurry to clarify that I do not imply that Professor Kurien would disagree with me on the need to avoid such basically escapist inferences. I am

sure that he, as a reputed and excellent teacher, is aware of and forewarned of this danger. My second point of departure from Kurien is substantive and this concerns the critique of neoclassical theory which Prof. Kurien develops. I have elsewhere discussed a methodological point (cf my article on Dobb) that directing criticism against isolated elements or concepts in a theory without looking at their analytical placement in the structure of the theory, may lead to a weaker and sometimes misconstrued critique of the theory. I think, Prof. Kurien's critique may have suffered from the same. I shall illustrate this with reference to Prof. Kurien's discussions in Section II.

3. Investigating the metamorphosis of theory Prof. Kurien depicts a transformation of theory from its original social moorings (in classical theory) to 'a sophisticated system of logical propositions and quantitative procedures'. I would object to such a characterisation of change - a constrast posed between socially relevant problems of the former and logical system of the latter. True, the classical theory was firmly rooted in the analysis of crucial and real problems of the day. However it has its own logical structure and quantitative connotations. On the other hand, the neoclassical system has also been shaped by concern for social problems. Economists like Walras, Wicksell, Fisher, Marshall, Jevons and in the modern time Friedman, Solow, Samuelson cannot be charged with non-concern for social problems. Many have worked directly on policy problems, worked on public committees. The real contrast, to my mind, between classical political economy (or Marxian) and neoclassical theory is to be drawn as between alternative theoretical systems.

4. Not focussing on these structural characteristics, Kurien's critique loses its sharpness and sometimes also accuracy. For example, his position regarding the contrasting treatment of the relation between production and

distribution does not come out clearly. He refers approvingly to Marx's statement that the structure of distribution is entirely determined by the structure of production and considers Mill's separation of the two (distribution determined by human institutions and production by technical factors) as a deviation on which the later neoclassicals built upon. Firstly, Marx's statement has to be viewed in the light of his thesis concerning primary of production even within the interconnectedness of production, exchange and distribution and that this was compatible with his position that wages at any time, may be conceived of as historically determined from outside the value-determination scheme. J.S. Mill was to put this assumption into isolated focus and lose sight of the complex interdependence between production and distribution. The neoclassical theory conceives of this interdependence in a very different theoretical structural context, distribution as being factor price-determination which, in turn, is explained within the same general process of price formation. Again Professor Kurien's statement that the marginalists discarding the historico-relative aspects, concentrated on production alone is somewhat surprising. If at all the early marginalists, like Jevons, Wicksteed tried to shift the entire burden of explanation on utility and demand.* It was in the works of Marshall, Walras, Wicksell that a systematic attempt was made to place consumption and production on symmetrical basis and to subsume the problem of distribution within exchange and commodity-pricing. It is true that the particular framework of equilibrium theories, casting the problems of production and distribution in a specific framework, using different

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* There is some typographical error here which I cannot quite figure out. I am therefore leaving the sentence as I found it in the original type script - Kurien.

units of analysis, led to a different view as to how the economy operates. It also directed attention to certain types of problems, eliminating others. This, Kurien recognizes very rightly. But Kurien's attributing the changes in theory to a mere search for formalism and rigour may prompt an anti-theory bias, although it may not be his intention. His attack on neoclassical theory for abstracting from social problems may also not be quite correct - for, as already mentioned, many of the neoclassicals have also been passionate policy-advocates and some, like Wicksell, paid heavily for their theoretical convictions. This does not, however, prevent us from pointing out how the particular theoretical framework might suffer from in-built weaknesses - both in confining the nature of the problems it can tackle as well as limiting the feasible solutions.

5. Similarly some of the specific criticisms which Kurien makes against economic theory seem somewhat misdirected. For example, that 'preferences' are determined by more general considerations outside the price determination, by itself, is not a very serious ground for attack. In the classical - or even in Marxian framework (vide his departmental schemes of reproduction) social output and demand are taken as given by a number of forces outside the value-frame. It is the fact that the neoclassical demand theory would then insist on specific kinds of quantity-price relations that is the source of logical difficulties. Similarly, Kurien's contention that property and power come to be excluded from the neoclassical system because of the 'given' distribution of resources is not quite accurate. The neoclassicists could argue that the class divisions with capitalist producers owning the means of production and labour without them can be captured in terms of the initial resource-distribution. Moreover 'equivalence in exchange' is a common feature of competitive capitalism whether within the neoclassical or Marx's framework.

Again, one would have to look at the structure of theory and the characterisation of causal relations to discover in what sense property and power plays an additionally significant role in classical theory not accounted for in the neoclassical theory.

6. To continue with Kurien's critique: his view that the neoclassical economics suffers from the pathological inability to perceive the inevitability of change appears to be undeserved indictment. One has only to look through Marshall to appreciate the significance he attaches to 'change' and after all the marginal concept itself is 'essentially' based upon change. What may be legitimately criticized is their notion of change and its particular conception.

7. All this is not to be interpreted as purely in defence of neoclassical theory but as an argument for critically examining it with a view to constructing an alternative theory. Also, when we argue for bringing into analysis 'real changes', for bringing in historical dimension etc. we ought to be able to give a rigorous theoretical content to these ideas. It is not merely a matter of adding 't' as an additional variable.

8. In section IV Kurien turns to certain problems of theorizing and of scientific method. I certainly agree with him that theorising is inescapable in any analysis of social problems. I agree also that all theory involves abstraction and the basis of abstraction is important. Kurien draws a distinction between the dialectical and the postulational method. He appears to do so on two grounds: that the dialectical method, unlike the postulational, bases its abstraction on the 'real' and not 'a priori' logical speculation. (Possibly Kurien here has in mind the contrast with the contention of Friedman that the assumptions used in the postulational approach

need not be separately verified, if their deduced inferences are tested). Secondly Kurien interprets that the dialectical method does not provide any logical procedures to see whether the abstraction is legitimate or not. I am not very clear what this implies. He adds that the verification must come from practice, where practice itself is interpreted very broadly. While recognizing Kurien's attack on the positivist postulational approach of the Friedman variety, I am not very clear about his explication of the dialectical method. (esp.p.27). To my mind, the criteria for adopting a theory in social science are both its internal logical consistancy as well as its ability to interpret the more prominent elements of social experience with a view to highlighting the underlying tendencies at work. The theory must decipher the more prominent and persistent forces at work. It is in this sense that the term 'laws' was used in the classical theories. Kurien however denies emphatically the responsibility of social sciences to discover 'laws'. Possibly he attaches a different significance to the term from that originally conceived of.

9. Despite differences of particular points, I am basically in sympathy with Kurien's passionate plea that fundamental research in social sciences must ultimately justify itself on the basis of applicability to concrete issues. His last section which is put in rather abstract and general terms would possibly be appreciated better with reference to concrete instances of applied work.

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C.T.Kurien

April 18, 1980.

Prof.Krishna Bharadwaj,
Jawaharlal Nehru University,
Delhi.

Dear Prof.Bharadwaj,

Thank you for passing on to me the typescript of your comments on my paper on "Social Problems and Social Sciences" which you read out during the IEG Seminar on Relevance of Social Science Research earlier this month. You have raised a number of important issues. But because I did not have a copy of your comments and because I had only 10 minutes to respond to all the comments made during the discussion I could not do justice to your critique of my paper and my position. But it is very seldom that we get an opportunity for serious methodological discussion and hence I do not want to let this opportunity to go by. I am, therefore, responding to you in the form of an open letter which I am sharing with some of the other participants of the Seminar who also expressed keen interest in the general issues. I hope you too will welcome the opportunity for a continuing dialogue. For ready reference I have culled out from your note the sections dealing with my paper leaving out your comments on Prof.Joshi's paper (i.e. I have left out para 1 and the last two paras of your note). For purposes of identification I have also numbered the paragraphs of your comments, a copy of which also I am circulating along with this letter.

1. I would like to begin with a reference to the differences in our perspectives that I mentioned in my response in Delhi. You are a scholar in the history of economic thought and are primarily concerned with the formulation of theory. I cannot claim to have any scholarship worth the name, and although I was a teacher of economics for many years, my primary concern has always been with practical economic problems like poverty, the development of the Tamil Nadu economy etc. I gave up

teaching two years ago because the theory I was teaching (or possibly the way I was teaching theory) could not comprehend and make sense of the problems that I felt should be understood and interpreted. In a note at the end of my paper I indicated its origin - it was written to answer the question "How to deal with current and live economic problems". It seems to me that you missed this crucial point and viewed my paper as a treatise in the development of economic theory or doctrine which it is not. I shall develop this point subsequently.

2. But let me turn immediately to two major points that we agree upon. I shall quote your own words:"I share with him his view that economic theorising must address itself to its eventual, if not immediate, applicability to social problems." (para 1) And "I certainly agree with him that theorising is inescapable in any analysis of social problems" (para 8). Since we agree on these two aspects there is no basic difference of opinion between us. I would insist that these two sentences must be viewed in the order given above, but perhaps you would want to reverse the order!

3. However, the fact that you have viewed my paper from the perspective of a theorist had led you to some misinterpretations of my position. I know you well enough to say that you would not deliberately misrepresent me (or anyone else for that matter!), but unfortunately it has happened. Let me give specific examples. If you concede that I hold that theorising is inescapable in any analysis of social problems, there is then no need for that long section in para 2 where you warn against the "temptation to indulge in an uncritical, irresponsible refutations of theory while substituting attractive cliches and jargons in place of analytical rigour." Of course you immediately exonerate me of these heinous sins by saying: "Let me hurry to clarify that I do not imply that Professor Kurien would disagree with me on the need to avoid such basically escapist inferences." But the very construction of the para shows that you <u>suspect</u> that I am either a nihilist or a theoretical anarchist although for want of adequate evidence you find that you cannot quite <u>accuse</u>

me of such tendencies. This suspicion of yours finds expression again, and more visibly, when you say in para 8:"Kurien however denies emphatically the responsibility of social sciences to discover laws'". I submit that your statement arises from a misreading and/ or misinterpretation of what I stated. Please look at my paper again to see what I said(p.29 top) "The main responsibility of the social sciences is not to discover "laws" or "law like statements" about their own created universe of discourse". I don't say that social sciences should not discover 'laws', but I do state, and quite emphatically too, that the main responsibility of social sciences is not to discover "laws" or "law like statements" about their own created universe of discourse. You overlooked the two phrases I have now underlined and what a big difference in interpretation comes when that happens!

4. The sentence quoted above is crucial to the argument of my paper. As I find it, a great deal of what parades as economic theory is the setting up of a logical universe of discourse which is, by construction, at variance with the social universe it is supposed to understand and interpret. I think theorists like Koopmans argue that theory in fact anything that claims to be theory, is a postulational construct of this kind. I would like to know whether you will agree with my interpretation of Koopmans, Hutchinson, Lionel Robbins, Schumpeter and many others who maintain (i) that theory, any theory, is a postulational construct and (ii) economic theory, especially the kind of economic theory that Arrow, Debreu, Koopmans et al have axiomatised is a postulational construct. In fact in my A Theoretical Approach to the Indian Economy I followed Arrow, Debreu and Koopmans rather closely.

5. You are not right in saying that I "implicitly tend to argue as if logical precision and rigour would invariably lead to aridity of theory" (para 2). I would, however, explicitly argue that it is in the very nature of the postulational method that it can, and frequently does, get away from substantive issues in its search

for logical consistency and completeness. The problem arises because there are not (and cannot be) any logical rules about what the primitives and axioms of a universe of discourse must be, except that what is decided upon is required for the internal logic of the system. But such insistence does not guarantee that the abstraction thus being made is legitimate with reference to the social universe the theory is supposed to represent and analyse. Let me illustrate. In Koopmans' (as also Arrow - Debreu's) axiomatisation of the "competitive economy" the primitives are a set of commodities and decision makers. The decision makers are then partitioned into "consumers", "producers" and "resource owners". This partitioning and the assumption that each of these decision-makers make their decisions independently of the decisions of the others are necessary for the logical structure of the "theory". But what is its bearing on the social reality in the USA, or UK, USSR or India? I submit that logic or "theory" as much cannot answer this question.

6. A second example is the exchange between Kaldor and James Tobin in the Review of Economic Studies (Vol.27, 1959-60) about Kaldor's "Alternative Theories of Distribution". Tobin, the true neo-classical economist that he is (am I right on this?)asks why Kaldor is only concerned with two "factors", capital and labour, using profit and wages as factor returns and not "generalise" it to N factors and N shares as the neo-classical theory claims to do. And to Kaldor's contention that division of output into two "goods" - Investment and Consumption - is independent of Profits and Wages Tobin asks whether it is the decision to produce Goose Livers on a certain scale that determines the income of Gourmets. To this Kaldor's reply was the following: "Macro-economic models, for all their shortcomings, attempt to do something rather more subtle than just aggregating production into two arbitrary categories A and B, and incomes into two equally arbitrary categories X and Y - the division in each case is justified by fundamental behavioural properties of the capitalist system. Investment is picked out, because expenditures debited to a capital account are independent of the current income flow in a way in which expenditures on income account are not; Profits are picked out, because any change in aggregate demand relatively to aggregate supply affects residual incomes in a contrary direction to contractual incomes

and also because in any reasonably stable system the proportion of profit saved is very much greater than savings out of contractual incomes of all kinds. Of course, it would be nonsense to suggest that it is the decision to produce Goose Livers on a certain scale which determines the incomes of Gourmets instead of the other way round. But it is just as nonsensical to imply that the assumption that the division of output between Investment and Consumption is independent of Profits and Wages is equally far-fetched."

7. The point I wish to note here is not the distinction between "neo-classical" general equilibrium models and "classical" macro models, but the phrase I have underlined in Kaldor's words i.e., logic alone cannot distinguish between factors called A and B and incomes designated X and Y, but if one is dealing with a capitalist system (only if one is dealing with a capitalist system?) the categories Investment and Consumption, Profit and Wages make sense.

8. The major methodological question I tried to raise in my paper (either it has not come out sharply, or in your decision to concentrate on Part II of my paper which, incidentally, I do not believe to be absolutely essential to make the point I am making you tended to overlook) is the following: Granted that (as you have yourself stated) theorising is absolutely inescapable in any analysis of social problems, what kind of theorising and how? I submit that your statement: "The criteria for adopting a theory in social science are both its internal logical consistency as well as its ability to interpret the more prominent elements of social experience with a view to highlighting the underlying tendencies at work" (para 8) is just as incomplete as my plea for the dialectical method.

9. I cannot agree with you that "the real contrast, to my mind, between classical political economy (or Marxian) and neo-classical theory is to be drawn as between alternative theoretical system." (para3). I shall go into the reason for my disagreement shortly, but let me make it very clear that although in my paper I attempted a critique of the neo-classical system, I did not plead for any alternate theoretical system whether the "classical system" or the "Marxian system". This has been quite deliberate. For, I hold the view that the theoretical procedure in the social sciences is not to choose between alternative theoretical system, but between alternative approaches to theorising. (Since you hold

the view that the choice is between theoretical systems, you have the responsibility to indicate the criteria and procedures for such choice, because obviously there is only one social universe and there can be an infinite number of logically rigorous theoretical universes, even quite "relevant" ones.) And I believe the choice, as I indicated in my paper is between the postulational method and the dialectical method. I have indicated also that even what is sometimes claimed to be a dialectical approach to the study of social problems degenerates into a postulational, logical method. In fact, there are many studies on social problems which claim to be "Marxist", but which I would consider to be just as postulational as good neo-classical pieces. Hence for me the choice is not between paradigms, but between methods.

10. From this point of view any analysis that makes an "as if" approach to social problems is postulational. Consequently it is not only "theory" that becomes postulational. It has been one of my perennial complaints that "planning" in our country - by this I mean the professional work that the Planning Commission carries out - is extremely postulational. All planning procedures which simply indicate that "if this can be done, then that can be achieved" ("If a growth rate of 12 per cent can be achieved poverty can be eliminated", "If the rural poor can be organised, they will be able to raise their levels of living" etc.) are nothing other than postulational exercises. Of course, it gives the academics within the Planning Commission the satisfaction that they have done their share of the work. But where does it take us?

11. My interest in the neo-classical system in the paper is (as I have indicated on p.21) because among the theoretical systems in economics it is the one that has been most completely axiomatised as an exercise in postulational method. I am aware of the fact that its axiomatisation has revealed both its formal strength and weakness. Hence you are not right in saying that I appear "to accept the logical neatness and soundness of the neo-classical system without questioning" (para 2). On the contrary I state on p.12 that "if the neo-classical system were to be treated as merely a logical or mathematical system, it would have been possible to appraise it along those lines". I did not

enter into the logical soundness of the neo-classical system because that was not my main interest. I would say that even if the neo-classical system were logically sound (I happen to hold the view that it is logically neat) my critique of it in terms of the relationship between its logical perfection and the social reality it claims to represent would hold.

12. That leads me to an issue that you make much of - that neo-classical economics also addresses itself to social problems and that many neo-classical economists have worked and continue to work on policy problems and public committees. (paras 3 and 4). I have stated quite explicitly that "It is well known also that the forerunners of the neo-classical tradition like Jevons and Walras and its modern adherents have all been interested in policy questions" (p.12). And surely I know that at least Milton Friedman is just as busy with practical problems as with theoretical issues! And on p.13 I concede that there are many situations in the real world where neo-classical principles and tools of analysis can be put to use. Hence your comment could not have been addressed to me.

13. But there are two issues worth noting. I shall illustrate the first in terms of an anecdote. I was in Yale in 1968-69 and wrote a series of irritated notes on "The Economics of Poverty and the Poverty of Economics". James Tobin was at that time an ardent advocate of the America poor and so we used to get together at times. Tobin was somewhat amused that I should be so much concerned about the economics of poverty while he himself was concerned so much with policies to eradicate poverty. I told him that if the economics that I was professing did not have anything to say about the most concrete economic problem of my country there was something wrong with that economics. I then asked him if there was any link between his professional economics and his concern for the poor. This was something of an unexpected question for him, I thought. Because he looked at me for a while and simply stated: "I really don't know; but I guess not". I must say also that the people at the Growth Centre at Yale were not concerned with poverty, in theory or practice, in America or India. The point is

simply this. The fact that many neo-classical economists work directly on policy problems and on public committees does not necessarily imply anything about the policy orientation of neo-classical economics. Let me concede also that precisely because neo-classical economics does not have any in-depth diagnosis of concrete problems, it is one of the most "policy oriented" theoretical systems. For instance, practically under all circumstances it can come to the "policy" recommendation that an increase in production is necessary (look at all our neo-classical studies of poverty) or that free trade, or at least freer trade is beneficial in most instances.

14. The second issue is what I referred to on p.17 and in footnote 12 that "scientific conservatism" is built into neo-classical economics, and that the logic of neo-classical economics is to protect the status quo. I would like to know how you react to this position. I shall readily grant that even neo-classical economists at times ask for and work for radical social transformation. We have to be grateful that commonsense sometimes breaks into our mighty logical systems!

15. I don't know whether I should respond to some of your other comments about my critique of neo-classical economics. For instance in para 6 you say that my view that "the neo-classical economics suffers from the pathological inability to perceive the inevitability of change appears to be undeserved indictment" adding that "after all the marginal concept itself is 'essentially' based upon change". Surely the change that I was referring to was not the first derivative of a continuous function! Again, in para 4 you say that it is surprising to see my statement that the neo-classical economists came to concentrate on production alone, while it is a well known fact that they were the ones who shifted the emphasis on to utility and demand. I suppose you are referring to my observation on pp.6 & 7 where I say that as against the classical emphasis on distribution the neo-classical economists came to concentrate on production. And on p.10 you will see that I know that neo-classical economists were also concerned with utility. As for using 't' to represent time and the historical dimension (your para 7) you know, of course, that it is part of the great neo-classical tradition! But let me

state again that since I cannot claim to have any special competence in the history of economic thought I am willing to concede the possibility that there are errors in my arguments. But you will have to point them out more convincingly than you have done. In closing this section I would simply ask whether I am right in saying that neo-classical economics has reduced economics to a private affair between me and my goods and consequently is incapable of addressing itself seriously to a large class of pressing social problems. If I am right on this, you must forgive me all my minor errors.

16. Now we can turn to dialectics. You are right in pointing out in para 8 that my explication of the dialectical method is not very clear. But then I was not going to use the paper to make an exposition of the dialectical method (or the postulational method for that matter). I find too that it is not very easy to produce an elementary version of the dialectical method. Here, there is a big difference between the postulational and dialectical methods. Since the former deals with logic, a logical exposition of it is relatively easy. But since dialectics is related to life it cannot be plucked out for logical treatment. This is not because I am biased against logic. It is because life is much vaster and more complicated than logic. Consequently the dialectical method is more easily practised than written about. In footnote 21 I have given a couple of references to writings on the dialectical method. One could add a few more. But if you will permit me an act of immodesty, I would say that I practised the dialectical method (somewhat imperfectly) when I wrote my Poverty, Planning and Social Transformation. If you will be good enough to go through it you may want to ask yourself whether my treatment of poverty is qualitatively different from the many quantitative research pieces on poverty that we have.

17. Lastly, I would like to say that there is no mystique in my statement on pp 26-27 that the validation of theory comes only through practice. I am not referring to the logical validity of a theoretical system (its internal consistency, for instance). What I am stating is what I have already mentioned at the end of para 5 above that there is no logical way of establishing the link between theory as a logical system and the life problems it claims to deal with. In other

words, there is no logical way of ascertaining whether a certain abstraction is relevant or not, permissible or not. Such links between theory and life can be established only in and through life. I admit that this presents a special problem to social scientists who not only want to display their academic skills by producing an internally consistent model, but have to ask at each stage in their work whether what they are doing corresponds to an objective reality which, with the best of intentions and efforts can be seen only as reflected in a none-too perfect mirror. This is precisely the reason why social scientists must show a willingness to learn directly from life, from the lips of the savants and the servants. It is the reason why also the cardinal virtue of a social scientist has to be humility.

I would appreciate your response to this rather lengthy reaction of mine.

With warm personal regards,

Yours sincerely,

(C.T.Kurien)

Copy : To members of the IEG Seminar
Comments will be welcome.