# A Theoretical Approach to the Indian Economy

## C.T. Kurien

# A THEORETICAL APPROACH TO THE INDIAN ECONOMY

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Christopher Thomas Kurien
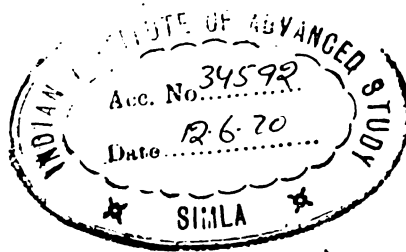
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*To My Parents*
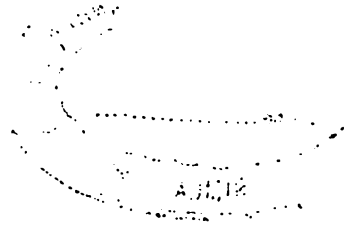
# PREFACE

THESE LECTURES were delivered in February 1966 at the invitation
of the Syndicate of the Madras University as the Father P. Carty
Endowment Lectures and are now published with the permission
of the University. I am grateful to the University, my *alma mater*,
for the honour done to me. The terms of the Endowment specify
that the lectures are to be "on a subject connected with Economics
with particular reference to Indian conditions." The invitation to
deliver the lectures, therefore, gave me an opportunity to attempt
a systematic presentation of some theoretical problems pertaining
to economies like ours which have engaged my attention for nearly
a decade. Though the first two lectures appear here with but a few
minor alterations, the arrangement of the third lecture has under-
gone some change.

Ever since I began a serious study of the problems of under-
developed countries like ours, I have felt the need to clarify the
theoretical issues involved in them. Often this has meant going
against the belief widely held by the profession that under-
development is a very important practical problem that needs to
be solved by national and international effort, but that it presents
no major theoretical problem. In my doctoral dissertation (*Factor
Market Structure and Technological Characteristics of an Under-
developed Country : An Indian Case Study* — Stanford Univer-
sity, 1962) I attempted a theoretical formulation to explain the
continued coexistence of different techniques for the production of
the same commodity — a very familiar phenomenon in economies
like ours, which is usually explained, or explained away, as a
socio-cultural problem. I pursued the theme further in a paper
entitled "Some Problems of Factor Allocations in an Under-
developed Economy" presented at the University Grants Com-
mission's Seminar on The Theory of Economic Development
held at the Annamalai University in October 1964 (published in
K. S. Sonachalam (ed.), *Economic Development : Seminar Papers
and Proceedings*, Annamalai University, 1967). The purpose of
the paper was "to point out the limitations of the received

economic theory of factor allocations in the context of an over-
populated underdeveloped economy such as India". I added, too,
that "the major contention of the paper is that if the basic
propositions discussed in it are valid, there is a sufficiently strong
case for an alternative model, but that much more research of
a theoretical and empirical nature will have to be carried out
before such a model can be attempted." Professor K. J. Arrow
read through that paper, and pointed out one major gap in the
chain of my reasoning. Correspondence with him helped me to
clarify many points and I wish to express my gratitude to him.
I also had the benefit of a conversation with Professor L. Hurwicz.
A Research grant by the University Grants Commission in 1964-65
enabled me to extend and refine the work further. The model
developed in the third lecture is the result of the research done
with the U. G. C.'s help, for which I am grateful.

The approach to theory that I have used in these lectures is
the postulational method. My first introduction to that method
came through a study of T. C. Koopmans' *Three Essays on the
State of Economic Science* (1957). I wish to acknowledge here
the great stimulus that I have received from the reading of the
*Essays;* their impact on my thinking cannot be overestimated.
The importance that Koopmans assigns to the postulational
structure of economic theory and Schumpeter's claim that the
axiomatic method is as old as economics itself prompted me to
go deeper into the method. In this expedition I had the good
fortune to have the guidance of my colleague Professor C. T. K.
Chari. I soon discovered that neither Koopmans, nor T. W.
Hutchison, in his *The Significance and Basic Postulates of
Economic Theory* (1938) had given an adequate exposition of
the method they believed was the basis of economic reasoning.
Hence I felt that it would be a useful thing to have a general
introduction to, and a brief treatment of, the method itself. This
I have attempted in the first lecture. The acquaintance with
the method led me to wonder whether it is the most appropriate
procedure for theory formulation in economics. I raise a question
about this towards the end of the first lecture, but I have
provisionally accepted it as *the* method, and have tried to see how
best it can be used in economics.

Whatever may be the limitations of the postulational method as far as economics is concerned, it has helped me to see, as Koopmans puts it, the need for a "separation between reasoning and recognition of facts, for the better protection of both." In particular, it has enabled me to recognise the need to distinguish between the logical validity and empirical relevance of theory and to feel that much of the poverty of economics results from the inability or the refusal to appreciate this distinction. And it has shown me that received economic theory must be constantly challenged to establish its relevance when confronted by new practical problems, which at first may appear to be little more than commonplace phenomena with no theoretical overtones. Hence I believe that preoccupations with the practical problems of economic underdevelopment can lead to significant advances in our knowledge of economic theory itself.

I have not undertaken any serious studies in methodology since these lectures were first delivered. During the past two years my main concern has been with the Indian economy itself, first in the writing of a popular exposition of the Five Year Plans (*Our Five Year Plans,* Bangalore, 1966) and subsequently in a major piece of research examining the nature of the savings-investment relationship in the economy. Both these have strengthened my conviction that further theoretical reformulations are necessary if we are to understand the nature and working of our economic system. In fact, they have convinced me that such reformulations have to go beyond treating an economy like ours as something "a little different from the competitive system", which is the approach I have taken in these lectures. As may be seen from my more recent work *Indian Economic Crisis—A Diagnostic Study* (Asia Publishing House, 1969), I have now developed an analytical scheme with which to construct and evaluate economic systems, and in terms of which a system representing an economy like ours is a more general case than the more familiar and conventional systems. In a way, I anticipate this conclusion in the third lecture. It appears that the development of theory is rather like watching a child grow. When her new-born son arrives the proud mother is struck by the features he shares with his father ; as

the child grows she begins to notice how different he is from his father although the point of reference is still the father. Soon she comes to accept them as two different individuals, related and yet different, while those who are less familiar with the child may be in the second or even in the first stage. Looking back on these lectures, I feel they belong to the second stage—a necessary link between the first and the third.

The members of my department have been of immense help to me in the development of the ideas contained in these lectures. I have had many discussions, formal and informal, with them and their comments and searching questions have led me to reformulate many ideas and to rewrite many pages. During my stay at Yale University in 1968-69 I had the opportunity to discuss the problem raised in these lectures with Professor T. C. Koopmans and Professor C. E. Lindblom, both of whom read through the entire typescript and made some very valuable comments. I would like to express my deep debt of gratitude to all these friends without, however, implying that they are in any way responsible for what follows.

Finally, I would also like to pay my homage to the man in whose honour these lectures were delivered. I belong to a generation of students who came too late to have direct contact with Father P. Carty. In his days he was a towering figure in the academic world of South India, especially in the field of economics. And he has left the indelible mark of his genius on the thinking of all who are concerned with the economic problems of this country, which he made his second home. He insisted that a thorough grasp of economic theory was essential for an adequate and enlightened discussion of these problems. I hope that these Lectures will be a further step, however faltering it may be, in the direction in which Father Carty taught us to proceed.

*Sept.* 1969                                        C. T. K.

# CONTENTS

# THE NATURE OF ECONOMIC THEORIES

## I

THE GENERAL title of these lectures, "A Theoretical Approach to the Indian Economy," raises a number of pertinent questions : "Why attempt a theoretical approach to the Indian Economy ?" "Does it imply the application of the general principles of economic theory to Indian conditions, or is it an attempt to evolve a theory about the Indian economy ?" "If it is the former what is new about it, and if it is the latter, then can there be, and should there be a theory about each economy ?" These and similar queries can perhaps be reformulated as a more direct question : "What is meant by a 'Theoretical Approach' ?" The first lecture attempts an answer to this question.

Whatever may be implied by 'A Theoretical Approach', the subject matter for treatment is the Indian Economy. I do not propose to raise the question "What is the Indian Economy ?" The question is not being raised in that form because that is what these lectures are attempting to discuss. To begin with, I proceed on the assumption that we know a good deal about the Indian economy—that it is a poor economy; that it is an agricultural economy ; that it is a mixed economy ; that it is a developing economy, etc. One may claim that these are facts about the Indian economy. We know also more detailed and more accurate facts about the Indian economy, its per capita income, its rate of growth, its balance of payments gap, etc. for instance.

These and many more facts that we know and are trying to know constitute one possible approach to the Indian economy. In an empirical science like ours the quest for facts cannot be over-emphasised and all theoretical pursuits must begin from facts and lead to further quest for facts. For, an empirical science, and an empirical science dealing with society cannot

but presuppose the existence of the social world and its realities. It is true also that if we do not know enough about the Indian economy, it is in a large measure because of our woefully inadequate knowledge about the relevant facts.

But an enquiry about facts can be only a beginning. Explanation, understanding and appreciation of facts can never come from facts themselves. The shaking of the head is an observable fact which can be described fairly adequately in terms of the intricate physiological mechanisms involved. And yet it may mean either assent or dissent depending on circumstances and the conventions associated with the fact, which of course do not emanate from the facts themselves. Thus in ordinary life, as well as in scientific discussion, facts form the stuff with which we deal; but facts come to us in bits and pieces, often as pieces from a jig-saw puzzle. To decipher them and to put them together coherently—in fact to 'know' them and to discover their meaning—we need to look elsewhere.

It is not only the partial nature of the facts that we come across and their usual disarrayed and hazy form that make it impossible for us to draw inferences from them. Even if our knowledge of the 'facts' is full we may not know enough about the phenomenon of which the facts are the manifestations. For instance, even if we know in detail the dates on which it rained in Madras City over the past decade and know also the measurement of rain for these days we shall not know why it rains and when to expect rain again. We begin to understand the phenomenon that we call rain only when we learn to relate the drops of water that come from above with changes in atmospheric pressure elsewhere, with the course of the winds and the lay-out of the mountains. Thus to understand a phenomenon we must wade through the undifferentiated flux of facts and impose upon them concepts and structures that help us to classify them, correlate them with other facts, put them in relief against other facts, so that we begin to see the emergence of a coherent pattern. This, indeed, is the role of theory: to inform, to explain, to reveal the general relationships among the seemingly different and distinct clusters of facts. Hence, if we do not yet properly understand the Indian economy,

it may also be because we have not discovered the clue to the jig-saw puzzle that we confront, even though we are very familiar with each bit of the puzzle.

During the past two decades our knowledge about the characteristic features of our economy, or at least economies like ours, has grown enormously. But the growing body of literature on the subject presents a bewildering variety of 'special features'. Is there a clue that will put them all in their proper places ? That, briefly, is the central theme of these lectures.

## II

Granted that in an empirical science like ours the ultimate goal of theory is to enlighten the realities with which the science deals, there is still an open question about procedure. Do we approach the phenomena to be explained with a given body of principles and 'theories', or are there cases where new facts or new situations call for the formulation of new theories ? How universal are the laws of a science like Economics ? These questions, of course, are not new. They have been with us ever since our discipline achieved self-consciousness. But I must single out one area where these questions have again gained some immediacy and the debate has been carried on with unusual vehemence. I refer to the economic problems of "underdeveloped" countries. In this field we have good examples of both the views mentioned above pushed to their polar extremes. I shall not comment on them yet, because in one sense this is the question I am addressing myself to in these lectures. Instead, I shall rephrase the question and ask which of these two views we in this country have traditionally subscribed to. I suggest that we will not find it easy to answer this question. For, our approach to economic theory has been rather different. For us economics has essentially been the study of what "the Masters" had to say. Thus, for a long time our knowledge of economics consisted of acquainting ourselves with the Malthusian theory of population, the Ricardian theory of rent, the Marxian theory of exploitation, the Marshallian

theory of demand, the Keynesian theory of employment . . . . ; the theories themselves, and what some others had said about these theories, the so-called 'criticisms'. We approached economic theory largely as a set of theorems to be mastered, and approached practical economic problems as though they were totally unrelated to economic theory or theories. The best examples of this approach are the many books we have on "Indian Economics" which consist mostly of undigested facts from Government reports and documents, or often from other books on Indian Economics! It is perhaps fortunate that we do not attempt a theoretical discussion of these problems, for at the theoretical level we are hopelessly confused. Whose theories, for instance, will we use to explain wage movements or interest rates in our country!

However successful we may have been in the past in keeping economic theory and economic problems in watertight compartments, it is clear that this state of bliss is not likely to continue for long. Already we have begun to show restlessness with the state of affairs. We have begun to ask what relevance the Malthusian theory of population or the Keynesian theory of employment has to Indian conditions. We have also begun to bother how our chronic unemployment and our slow growth can be explained. Economics as an empirical science makes its most significant strides when economic theory and economic problems confront one another in this dialectical manner and hence this is the most appropriate time to enter upon a theoretical approach to our economic problems.

## III

Unfortunately, economists have not given sufficient attention to the question of the formulation of economic theories. Traditionally we have been shy about the problems of methodology and the statement attributed to Schumpeter that "Methodology is the last refuge of the scoundrel" pithily summarises our attitude towards basic issues that lie behind the construction of economic knowledge. In our own country,

particularly, there has hardly been any major exercise of this kind, although we have had some penetrating discussions about the nature of economic science.

Since we are still beginners in this area, it is worth looking around to see what we can learn from other disciplines in this respect. At the outset it is necessary to warn ourselves about the danger of blindly following the procedure adopted in other disciplines, particularly the physical sciences. While we must be ready to accept what we have in common with other sciences we must be constantly on the guard not to neglect the distinctive problems of our own discipline, both in subject matter and in the treatment of the subject matter.

To illustrate the problems of theory formulation I have selected geometry. The choice may appear strange because geometry as a discipline is far removed from economics in subject matter, and geometry is but one of the tools that we use in economic analysis. There are other and more pertinent differences between geometry and economics which I shall touch upon later on. At the moment, therefore, I want to point out certain similarities between the two disciplines which justify my choice of geometry to illustrate the methodological problems we wish to have examined.

Geometry and economics are both what may be called 'native sciences'. I have used this term to refer to the obviousness and common familiarity of the subjects that the two disciplines deal with—angles, lines and cubes in the case of geometry ; goods, production and markets in economics, all of which are so much the stuff of our everyday existence. These are physical realities that we can see and feel and experience, unlike the electrons and atoms of the physical sciences, or the cells and protoplasm of the natural sciences. Secondly, therefore, both geometry and economics have had a kind of natural evolution as sciences dealing with realistic entities. Euclidean geometry, for instance, had its origin and development in the generalisation and systematisation of many empirical discoveries in connection with the measurements of areas and volumes, in surveying and in astronomy. Economics too had its origin in observations about the nature of exchange, the characteristics of production, the observable effects

of competition. From the immediate point of view of theory formulation, these similarities between geometry and economics are extremely relevant, and hence I shall proceed to a more detailed discussion of the construction of theories in geometry.

Geometry, to most of us, consists of a set of theorems and their proofs. But where are these theorems derived from? What are their basic sources? Euclid himself, after whom Euclidean Geometry is named, based his theoretical system on what he and his followers for many centuries considered to be 'self-evident' propositions.[1] Successive generations of scientists and philosophers were impressed by the harmony between the 'theorems' which could be logically derived from these 'axioms' and what could be established by observation and experiment. So closely established were the connections between the 'self-evident' basic propositions and the empirical counterparts of the theorems derived from them that the former were considered to be essential aspects of empirical reality, and then the chain of reasoning was reversed to say that, therefore, the axioms *must* be 'true'. A consequence of such a position was the belief in the adequacy of the conventionally accepted axioms. Euclidean geometry was Geometry and its assumptions and theorems were universally valid. The sum of the angles of a triangle was 180 degrees whether the triangle was drawn in Greece or Europe or in the mysterious Orient!

More than two millenniums passed before geometry experienced some revolutionary changes. In the 19th century it was shown that Euclidean assumptions could be rejected and non-Euclidean geometries could be constructed on the basis of non-Euclidean assumptions.[2] On the basis of some of these assumptions it could be proved that the sum of the angles of a triangle is not 180 degrees! There then is no longer a universally valid Geometry, but only geometries depending on the nature of basic assumptions

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[1] Euclid, the Greek mathematician, wrote his main work, *The Elements*, in which he presented his deductive system of geometry about 325 B.C.

[2] Now-Euclidean systems of geometry were put forward by the Russian mathematician Lobatchevski (1793—1856) and the German mathematician Riemann (1826—1866).

made. Further it was shown that even for Euclidean geometry Euclidean assumptions were neither sufficient, nor even necessary, in the sense that not all conclusions of Euclidean geometry followed from Euclidean assumptions, and that Euclidean theorems could be deduced from different sets of basic assumptions.[3] It then turned out that the basic axioms which were so far considered to be 'self-evident' were but formal statements devoid of empirical content or connotation. Construction of theories had to become formal, and a distinction had to be drawn between two aspects of theory : one dealing with its form and structure ; the other dealing with its content and meaning. What appeared to be a destructive development, the consequences of which took away much of the metaphysical awe about conventionally accepted theories, in fact, ushered in a revolution in the methodology of science with far-reaching implications.[4]

## IV

The distinction between the two aspects of theories mentioned above is crucial to an understanding of the nature of theories and of the principles of theory formulation, and hence must be pursued at the general level leaving geometry aside for the time being. The two conceptually distinct aspects of theory may be called, respectively, the 'Syntactical' and the 'Semantical' aspects.[5]

[3]David Hilbert, the German mathematician whose *Foundations of Geometry* discussed these issues, is said to have inaugurated the modern axiomatic method in geometry. *Foundations* appeared in 1899.

[4]It is well known that Einstein's 'Theory of Relativity' is based on Riemann's elliptical geometry.

[5]I am aware that there is a semantic controversy over the terms 'Syntactical' and 'Semantical'. My use of these terms is based on Rudolph Carnap who in his *Formalization of Logic* speaks of the "two tendencies in modern logic". He says : "The one tendency emphasizes form, the logical structure of sentences and deductions, relations between signs and abstractions from their meaning. The other emphasizes just the factors excluded by the first : meaning, interpretation, relations . . . . Using contemporary terms, we may call them the syntactical and the semantical tendency respectively."—Quoted by Philip Frank, *Philosophy of Science* (Prentice-Hall, 1957) p. 71.

At the syntactical level any theory is an exercise in logic, and consists of the general form "P → T" where the P is a (set of) Postulate(s) and the T a (set of) Theorem(s) with the arrow in the middle stating "if P then T". This formulation shows that at the formal level theories emerge solely from the postulates on which they are based. To understand the nature of theory construction, therefore, we must, in the first place, acquaint ourselves with the characteristics of the postulates.

Postulates are, to use the terminology of formal logic, analytic *a priori* propositions.[6] As analytic propositions they are simply concerned with the meaning of words, and as *a priori* propositions their truth or falsity can be shown by pure reason, prior to observation. Postulates are, therefore, mainly concerned with bringing out the meanings of terms by giving them appropriate definitions. But since no terms can be defined without reference to other terms, initially it is necessary to select some undefined terms called 'primitives'. It is important to note that the 'primitives' are not terms that cannot be defined; the postulates, in fact, define them. But a primitive is defined in terms of other primitives. This aspect is emphasised because the primitives and postulates stand in a different category from what in some conventional theoretical structures are considered to be *basic* principles or propositions or terms, and hence to be accepted as ultimate assertions. On the contrary, the primitives are, strange as it may appear, any terms accepted as primitives. A simple example will help us to grasp these ideas. Let us accept 'A', 'B', 'C' as a set of primitives, and using these primitives let us make the following two 'postulates': "All A are B" and "All B are C". These two postulates now 'define' the primitives 'A', 'B' and 'C'. The 'definitions' may appear simple and empty, and so they are, but they convey the very meaningful idea that we are talking about A, B and C such that A is included in B and B is included in C. What is perhaps more important is that since the primitives are arbitrarily chosen, they can stand for any and all classes that

[6]The discussion here is based much on Arthur Pap's *An Introduction to the Philosophy of Science* (Free Press, 1962).

satisfy the definitions given by the postulates. Thus pairs of statements such as "all Indians are human beings", "All human beings are mortal" and "All donkeys are mammals", "All mammals are animals" are covered by the two postulates given above. From the two postulates we can also draw the conclusion or 'theorem' that "All A are C". The theorem thus drawn, of course, must be certain, and must hold good under all circums-tances. It means that from our examples we can draw the definite conclusions "All Indians are mortal", "All donkeys are animals". The postulates being empty, and being concerned only with form, can establish the truth of an infinity of propositions, all of which conform to the same form.

It must now be clear that it does not matter at all what the primitives 'A', 'B' and 'C' really are. The only thing we can say about them is that they are what they are! It is significant also that the postulates do not define A, B and C in isolation —the postulates define the entire set A, B and C. This is the essence of postulational definition : "A set—more exactly an ordered set—of entities is defined by the logical relations expressed by the postulates, but nothing is thereby said about the specific nature of the entities."[7] In formal geometry, for instance, 'point', 'straight line', 'plane', etc. are primitives in this sense, and can be understood only in terms of the postulates that define them by their relation to one another, whatever preconceived ideas one may have as to what a 'point' or 'straight line' *really* is. An ordered set of classes defined by postulates is called a *model*. And formal or pure theories are propositions derived from a model so constructed.

This approach to theory enables us to examine the nature of theory more adequately. It reveals that at the formal level theories are unambiguously certain, meaning that given the postulates the theory *must* follow. In this sense a theory cannot be revised or improved upon, and it remains the same universally and eternally. In other words, the logical 'proof' of a theory consists in establishing the fact that the propositions derived from the postulates are implied by the postulates themselves.

[7]*Ibid*, p. 47.

At the same time it is equally important to point out that what is universally valid is not what the theory asserts but the fact that the theory can be derived from the given postulates, no matter what may happen in the world of experience.

This intimate relationship between a theory and its postulates also means that in a very pertinent sense logically valid theories are 'tautologies', in so far as they lie latent in the premises from which they are derived. In the syntactical aspect no theory is objectively new when compared with the postulates from which it is derived ;[3] propositions deduced from premises are new when we deduce them for the first time, but immediately it will be obvious that they were there all along, simply waiting to be 'discovered'. Theories are never new in the sense that they are never invented, but only discovered. Progress in the construction of theories largely lies in discovering new derivations from basic postulates and in stating new postulates in terms of the primitives.

To say that all logically valid theories are tautologies in this sense may come as a disappointment to those who usually get excited about the glamour of 'new' theories, and certainly it takes away the source of one standard criticism that one so often comes across that "such and such a theory is a mere tautology", or that "such and such a theory involves circular reasoning because it comes from the 'assumptions' on which it is based." In fact, if a critic of a theory has proved to himself and to his audience that a theory which he examines involves circular reasoning, he is doing a great service to the theory. For, if and only if theories are tautologies can we check on the correctness of reasoning and decide whether supposedly different

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[3]"Logical deduction is a technique of conceptual analysis: it discloses what assertions are concealed in a given set of premises, and it makes us realise to what we committed ourselves in accepting those premises ; but none of the results obtained by the technique ever goes by one iota beyond the information already contained in the initial assumptions." Carl G. Hempel "Geometry and Empirical Science" in *The Structure of Scientific Thought*, ed. Edward H. Maddan (Houghton Miffin Company, 1960) p. 73.

theories may not be really equivalent or where their differences lie.[9]

The logical characteristics of theories spelled out in the preceding paragraphs are derived from the fact that they are only logical theories (and *not* scientific theories) and as such are devoid of all empirical contents. At this level, therefore, theories cannot be subjected to empirical tests, either for verification or refutation, because they are only formal statements, only empty frames.

V

Theories which are but formal statements begin to assume substance when we pass from the syntactical to the semantical aspect. Having built up a formal model or system which simply shows how the primitive terms are connected with one another (this is what we mean by 'defining' the terms) we must now try to see whether the model has any meaning or relevance. This is where the logical statements of the formal model take on the form of scientific theories. We must try to see whether there are objects in the world of experience which have the properties formulated in the postulates : if there are, they will also have the properties formulated by the postulates. Scientific theories are, therefore, derived by a semantical interpretation of a formal system. Generally speaking, the semantical interpretation of a pure theory whose primitives are not assigned any specific meaning consists in giving each primitive a specific meaning or designatum. In so doing we produce a system corresponding to the 'real' world, with the formal theories now taking the form of empirical hypotheses which can be tested against facts for empirical verification.

A scientific theory, therefore, consists of two conceptually distinct aspects—a formal frame which is logical in its structure, and which has no connection to reality, and an empirical content directly related to the realities of the world of experience that the science concerned deals with. Neither a chain of

[9]Cf. Milton Friedman, *Essays in Positive Economics* (The University of Chicago Press, 1953) p. 11.

reasoning by itself nor a collection of facts by itsetlf—however accurate and thorough these may be in their own spheres—constitutes a science or scientific analysis. Any scientific analysis which deserves that name must be at once logically sound and empirically relevant.

Having thus established the essential nature of scientific theories we may examine some of their characteristics also. Once a syntactical structure is given a semantical interpretation, the verifiability of the system with reference to empirical evidence assumes primary importance. While the accuracy of the syntactical structure is a precondition of a scientific theory, it does not ensure the theory's empirical acceptability. The only assertion that we can make about a scientific theory, therefore, is that if all the postulates of a given formal system are empirically true, then all the theorems derived from it also must be empirically true. Scientific theories are thus of the general form : "Since P, therefore T", which presupposes that P is accepted to be true or valid in its empirical interpretation. Scientific theories in this sense are always provisional in character —to be accepted "until further notice" as a writer puts it, whereas a logical theorem, once proved, is established once and for all.

This is because empirical propositions—and scientific theories are empirical propositions—are conceivably falsifiable whereas formal propositions of logic are not. Thus the generality and certainty that formal propositions possess are achieved only because they completely lack empirical or realistic contents. Einstein expressed it thus : "As far as the laws of mathematics refer to reality, they are not certain ; and as far as they are certain they do not refer to reality."[10]

But the insistence that scientific theories must be about reality must not be interpreted to mean that unlike formal theories they must be 'realistic'. There are many—particularly in disciplines like ours—who stubbornly insist that theories must be 'realistic', although it is hard to understand what they mean by the term. If the demand for 'realism' is pressed to its

[10]Quoted by Hempel, *loc. cit.*

fullest extent a 'theory' will have to be as particular as the event or phenomenon it purports to explain, in which case it ceases to be a theory, and turns out to be a description. We have seen that a theory must concern itself with general relationships, with a class of phenomena. A theory which attempts to be 'realistic' will convert itself into a catalogue of facts, which, we have already seen, cannot be considered to be a scientific theory.[11]

We can—and it has been shown that we must—legitimately insist that a scientific theory must correspond to the reality of the field of operation of the science. But the correlation between the theory and the reality can be neither simple nor direct.[12] A theory is more like a map of reality with its own systematic devices of projection systems and conventional indications. A terrain obviously will not have longitudes and latitudes marked on it, but we understand it a good deal better with a map of it which shows its latitudes and longitudes. Or, a building is made of bricks and cement and timber, but it will be difficult to build it without having a blue-print which consists of only lines and angles.

We must go a step further and say that the empirical testing of a theory—to see whether it corresponds to reality—can also be done only indirectly. The empirical validity of a theory cannot be established by demonstrating that its basic postulates are realistic.[13] Very often it is simply because the basic postulates, even with their semantical interpretations, do not

[11]"A theory is a statement of general relationships : a theory of unique events is a contradiction in terms, and a theory of local events is simply uninteresting from the scientific viewpoint." George Stigler, "Influence of Events and Policies on Economic Theory", *The American Economic Review*, May, 1960. pp. 36—45.

[12]Cf. William Sacksteder, "Theories and Usage", *The Journal of Philosophy*, June, 1962, pp. 309—19. See also an interesting exposition by Harold Peterson, "The Wizard who Oversimplified : A Fable", *The Quarterly Journal of Economics*, May, 1965, pp. 209—11.

[13]This does not mean, however, that the realism of the basic postulates is immaterial as far as the validity of the theory is concerned. This question is discussed in the next lecture. See the Second Lecture, Section III and footnote 20.

lend themselves to easy empirical verification. Hence we must, first of all, deduce from the theorems consequences and predictions which are amenable to empirical verification. It is in the nature of a fruitful scientific theory that it yields such 'predictions', and this is where a scientific theory differs from the commonsense use of the term 'theory', which often means little more than 'opinion'. A theory which cannot 'predict' is not a scientific theory.[14]

However, a scientific theory is never 'proved' by showing that empirical evidence supports its predictions. If a test bears out the predictions of the theory, we can at best say that it constitutes confirming evidence, but not conclusive proof for the theory. The reason for this is two-fold. In the first place, we cannot be quite sure that we have checked the predictions against all conceivable forms of empirical evidence that are pertinent to the 'prediction'. Further tests or new facts may contradict the predictions. Progress in science has often come about when newly discovered facts have contradicted propositions and theorems conventionally considered to be well established with reference to known facts. Secondly, and more pertinently, theories cannot be 'proved' by testing them against facts because observed facts are necessarily finite in number, possible hypotheses or theories infinite. If there is one theory that is consistent with the available evidence, there can be many more that are.[15] This problem of multiplicity of potential theories is an inevitable concomitant of the general postulational method. One of the burdens imposed upon a science by this method, therefore, is to make it necessary to have rules by which choice is made from among competing theories and theoretical systems.

[14]The term 'prediction' is used here in its scientific sense, and does not necessarily imply that theories must be able to 'predict' future events in the ordinary sense of the term.

[15]For instance, it is well known that Duesenberry's Relative Income Hypothesis and Friedman's Permanent Income Hypothesis—rival theories of the Consumption Function—were both verified using the same data. See Milton Friedman, *A Theory of the Consumption Function* (Princeton University Press, 1957).

Finally, as a characteristic of scientific theories it may be mentioned that they seldom, if ever, proceed in the order that we have described here—from the construction of their formal structure to the filling up of their empirical content. "Sciences never begin with problems which come logically first", says Bertrand Russel, "—if they did they would probably make slow progress—but they start at a commonsense level and have to build upwards their structure of laws and relations, and downwards their foundations".[16] In science as in pedagogy the dictum "from the concrete to the abstract" seems to be the most appropriate *modus operandi*. For, after all, however elegant and necessary logic may be to science, the main concern of science is with reality. To quote Einstein again, "Pure logical thinking can give us no knowledge whatsoever about the world of experience : all knowledge of reality begins with experience and terminates in it."[17]

## VI

From the long expedition to the world of sciences in general, we must now get back home to economics. What bearing do these general methodological discussions have on economics ? A detailed answer to this question must wait till the next lecture. In anticipation of that discussion, however, we may make a quick historical survey of the evolution of our science. The appropriateness of my choice of geometry to illustrate the general methodological problem of scientific theories will become obvious when we see the close resemblance between geometry and economics in their evolutionary process which I have already referred to. I think that in terms of the natural evolution of the disciplines, in the close connection between the empirical realities in their respective spheres on the one hand and

[16]*Introduction to Mathematical Philosophy,* p. 2. Also, "It is a matter of fact that the advance in science has consisted to a large extent in the replacement of the commonsense world by a world of abstract symbols", Philipp Frank, *op. cit.,* p. 45.

[17]In his "Method of Science" as reproduced in Edward H. Maddan, *op. cit.*

systematical logical thinking on the other, the discovery of which led the early votaries to believe in the generality and universal validity of their disciplines, Euclidean geometry and Competitive economics have much in common. Just as until the nineteenth century Euclidean geometry was the Geometry, Competitive economics was (and for many still is) *the* Economics. It is this general theme that I wish to discuss in what follows.

During the past two decades many formulations have been put forward demonstrating that the perfectly competitive system is a theoretical model in the sense in which theoretical models have been described earlier in this lecture. That aspect of perfect competition will be examined in some detail in the next lecture. For the present I shall take one of the most commonly accepted conclusions and one of the most widely held assumptions of the competitive economy and show how they were considered to be part of the natural order in the pre-scientific approach to economics.[18]

As a theoretical model perfect competition is noted, above all, for the 'optimal allocation of resources' that it achieves, and one of the crucial assumptions of the model is that the participants in the system are 'maximisers'. In the pre-scientific discussions of the competitive economy these two corresponded to the belief that unhindered competition brings about the best utilisation of the resources of the economy and that to achieve this end producers must be maximising their profits. Both these, in turn, were thought to be derivable from the proposition that human beings are essentially self-interested. The validity of this proposition, of course, was considered to be self-evident. In the *Wealth of Nations*, therefore, Adam Smith made a scientific analysis of division of labour, and then 'proved' that it can never pay an individual (or family) to produce for himself (or itself) what he (it) can buy cheaply from someone else.[19] And

[18]For a detailed exposition of the influence of the concept of Natural Order and Natural Law in the evolution of economics see J. A. Schumpeter, *Economic Doctrine and Method*, (Allen and Unwin, 1954) Ch. I.

[19]*Wealth of Nations*, Book I.

then he argued, or simply asserted that therefore free trade must be good for an economy as a whole, for "what is prudent in the conduct of every private family can scarcely be folly in that of a great Kingdom".[20] British economists were later to argue, often implicitly, sometimes quite explicitly too, that "what is prudence in the conduct of the United Kingdom can scarce be folly in that of the world at large" !

The history of the growth of the principles of laissez-faire and free trade first as economic doctrines and then as political dogmas is too well known to require elaborate survey. Suffice it to say that 'free enterprise' was soon accepted as a scientifically demonstrable, and hence irrefutable principle of policy for all times and all situations.

After all, this profound principle of policy was ultimately based on the quite obvious, and from a commonsense point of view quite legitimate notion that those who go into business are there to make profits. As simple as all that ! Not that this simple principle of profit-motive could not be undergirded by erudite philosophical considerations. The point was that commonsense, business practice, logic, science, ethics,—all seemed to support the basic principles and conclusions of economic theory. Nothing could be more universal and more irrevocable than these propositions of economic theory.[21]

But the growth of economics as an academic discipline gradually took away this metaphysical awe. Defects were pointed out and protests were raised about the principle of laissez-faire. The demonstration of the conditional validity of laissez-faire went almost hand in hand with the growth of economics

[20]*Ibid*, Book IV, Ch. 2.

[21]"It seemed a miraculous fact that the profit motive, operating through the price mechanism could—indeed must—lead to the best of all possible worlds. Here was a perfect harmony of self-interest and social morality. That the economic system, which had been designed by no man, should exhibit a kind of perfection was surely evidence of the benevolence of nature, or of divine purpose. Thus *laissezfaire* could become a creed, and intervention immoral". I.M.D. Little, *A Critique of Welfare Economics* (Oxford University Press, Second Edition, 1956) pp. 258—59. Little then goes on to point out the fallacy of this argument.

as a disciplined science. It became imperative to sort out facts and fancy, empirical evidence and logical arguments. The scientific process in economics was already on the move.

Late in the nineteenth century when Walras was attempting the first formal presentation of the competitive model he could say sarcastically : "Economists have been less concerned with establishing the proofs of their arguments in favour of laisser-faire, laisser-passer than they have been with using them as weapons against the socialists, new and old, who for their part are equally negligent in establishing proofs for their arguments in favour of State intervention".[22] Here was somebody insisting that the 'obvious' conclusions of economic theory, which had stood the test of time had to be 'proved' ! No wonder contemporary economists ridiculed him. And yet, Walras, who put forward a rigorous and systematic exposition of the principle of Competition must be considered to be one of the founders of the *science* of economics, whoever else may be considered as the founder of political economy.

## VII

In conclusion I am eager to point out that the analogy I have drawn between geometry and economics should not be stretched too far. It will be misleading to suggest that alternative systems in economics should be (or even can be) produced as non-Euclidean geometries were devised. Alternative geometrical systems can be produced by logical exercises. While logical systems have their role in economics also, alternative economic systems cannot be mere products of logical exercises : they must emerge primarily with respect to some aspect of economic reality. In other words, in economics we can only speak about the logic of alternative systems, and not about alternative logical systems.

This, in fact, raises the more serious question whether the postulational method with its emphasis on logical systems is the most appropriate method for economics. We do not know. And we cannot know till we try. It is with this belief that I turn to an examination of the applicability of the method to economics.

[22]Leon Walras, *Elements of Pure Economics,* (Translated by William Jaffe : Allen and Unwin, 1954) Lesson 22.

# ECONOMIC THEORIES AND ECONOMIC SYSTEMS

## I

We must now address ourselves to the question whether the axiomatic method or the postulational approach to theory described in the previous lecture is applicable to Economics. To do so it is necessary to distinguish between two major stages of theory formulation : devising a logical system relevant to the discussion of the problem on hand and the testing of the derivations or 'predictions' of the system against empirical evidence. Both these are equally important stages in any scientific theory, but it is important to recognise that they deal with conceptually distinct aspects of theory formulation. Lack of clarity on this basic issue often results in many methodological muddles. For instance a perennial theme in methodological discussions in subjects like economics is that of the problem of measurability. It is argued—although one does not see why there has to be so much of argument about it—that economic variables are not as precisely measurable as the variables in the physical sciences. And then, inadvertently perhaps, science gets identified with measurement, and precise measurement at that, leading to the familiar lament (and the disillusionment that goes with it) that after all the social sciences cannot be sciences "in the strict sense of the term 'science'."

I recognise the importance of measurement and the special problems it poses to the testing of hypotheses which constitutes an important element in the formulation of theories. But in these series of lectures I am only concerned with the earlier stage of theory formulation, viz., the devising of a logical system. Hence the only question I wish to raise is "can we show that the theories of economics depend on, are derived from, the basic postulates from which they are built up ?" This

is a very significant, and if one may say so, a very revolutionary question.

A further excursion to the history of our discipline becomes necessary here. If we are to take Schumpeter's word for it the axiomatic method in economics 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. For this purpose they stressed those elements which seemed important to them, tried to imagine how things would turn out, if no other factors operated and subjected these elements to a few and simple basic assumptions which experience had suggested to them."[1] Certainly this can be taken to be a rudimentary version of the scientific method described in the previous lecture. Schumpeter also adds in a foot-note that the classical economists thought of this procedure not as a special 'method', but as the only possible approach to the treatment of economic problems.[2] We also have Keynes's testimony that "Ricardo offers the supreme intellectual achievement, unthinkable by weaker spirits, of adopting a hypothetical world remote from experience as though it were the world of experience and then living in it consistently."[3]

In fact, we can go even further, Schumpeter again, in a different context says :[4] "To Senior belongs the signal honour of having been the first to make the attempt to state, consciously and explicitly, the postulates that are necessary and sufficient in order to build up—it is misleading to say 'deduce'—that little analytic apparatus commonly known as economic theory,

[1] J. A. Schumpeter, *Economic Doctrine and Method*, p. 90.
[2] *Ibid*, pp. 90 and 91.
[3] J. M. Keynes, *General Theory of Employment, Interest and Money* (Harcourt, Brace and Co., 1936) p. 192.
[4] J. A. Schumpeter, *History of Economic Analysis* (Oxford University Press, 1954) p. 575. The following discussion of Senior's basic postulates is also taken from the same book, pp. 576—86.

or to put it differently, to provide for it an axiomatic basis."
Senior had four basic postulates.

1. "That every man desires to obtain additional Wealth
with as little sacrifice as possible."

2. "That the Population of the World, or in other words,
the number of persons inhabiting it, is only limited by moral
or physical evil, or by the fear of a deficiency of those
articles of Wealth which the habit of the individuals of each
of its inhabitants lead them to require."

3. "That the Powers of Labour, and of the other instru-
ments that produce Wealth, may be indefinitely increased by
using their Products as the means for further Production."

4. "That agricultural skill remaining the same, additional
Labour employed on the land within a given district produces
in general a less proportionate return, or in other words, that
though, with every increase of the labour bestowed, the
aggregate return is increased, the increase of the return is
not in proportion to the increase of the labour."

If economics has such a long history of the 'postulational
method' how is it that Koopmans writing in 1957 can say that
the practical consequences of the postulational structure of
economic theory have not yet been generally realised or
accepted ?[5]

The answer to this puzzle, it seems to me, is two-fold. There
is, in the first place, a formal difficulty. The 'axioms' or
'postulates' of classical writers are not postulates in the sense
in which we defined them in the first lecture as propositions
that define the 'primitives'. In other words, the postulates of
classical economics are not logical constructs, but rather as
Senior himself claimed "a matter of consciousness" (the first
postulate) and "matters of observation" (the other three). The
classical attempts, therefore, was to find out those statements of
empirical generality from which the theoretical organon of

[5]T. C. Koopmans, *Three Essays on the State of Economic Science*
(McGraw-Hill, 1957) p. 132 foot-note 2.
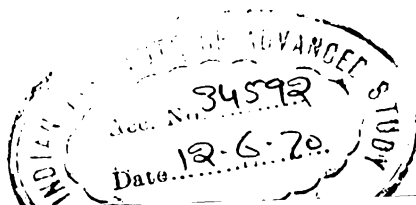
economics was to be shown to arise. There is, therefore, the confusion of logic and facts, of analysis and empirical evidence, of commonsense breaking in even while the logical system is just being set up. I shall return to this problem in Section III.

The second difficulty is closely related to the first. Since the early writers did not distinguish between logical analysis and empirial evidence, the tendency has grown subsequently to confer upon their theories the claim of 'universality', whatever may have been their own intentions in this regard. The term 'universality' is used here, it will be noticed, in its every day meaning, that is, observable or applicable in all places and at all times. Such a forced 'universality' of economic theory has done much mischief in the history of economic analysis, and has also contributed in a large measure to the slow development of economic theories. This point requires further elaboration.

If we put forward the claim that received economic theory depends on, and only on universally valid premises, then any protest against *received* economic theory becomes a protest against *economic theory* itself. It is then no wonder that for a long time protests against received economic theory came from the Historical School and the Institutionalists, the 'rebels' who felt it necessary to cut off the arm of theory to strengthen the arm of empirical evidence. It is understandable too that almost till recently we could only speak of two kinds of economists—those who were adherents of received economic theory, i.e., the theory of the competitive system, and considered it to be universal, and those who considered that economic laws were but relative propositions—relative, that is, historically, culturally or anthropologically.

The shadow fight between 'universality' and 'historico-relativism' in economic theory is clearly seen in Lionel Robbins's *An Essay on the Nature and Significance of Economic Science*, especially the two chapters dealing with "The Nature of Economic Generalisations" and "Economic Generalisations and Reality" (Chapters IV and V). In the latter Chapter Robbins, discussing the prognostic value of theory is ready to admit that "It is only what follows from these given assumptions that has

the character of inevitability".[6] And "If certain conditions are present, then, in the absence of new complications, certain consequences are inevitable."[7] In this chapter, however, Robbins does not bother to discuss whether economic laws are 'universal' or 'relative'. That discussion has already been done in the preceding chapter where Robbins mentions that "The propositions of economic theory, like all scientific theories, are obviously deductions from a series of postulates."[8] The postulates, however, mentioned earlier in the chapter, are the theory of value, that is, "that different things that the individual wants to do have a different importance to him, and can be arranged, therefore, in a certain order," and the law of Diminishing Returns, that is, "that different factors of production are imperfect substitutes for one another."[9] About these postulates Robbins claims : "These postulates are all assumptions involving in some way simple and indispensable facts of experience.... These are not postulates the existence of whose counterpart in reality admits of extensive disputes once their nature is fully realised. We do not need controlled experiments to establish their validity : they are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious. Indeed, the danger is that they may be thought to be so obvious that nothing significant can be derived from their further examination. Yet, in fact, it is on postulates of this sort that the complicated theorems of advanced analysis ultimately depend. And it is from the existence of the conditions they assume that the general applicability of the broader propositions of economic science is derived."[10]

Robbins, later on, speaks about the need for "a great multitude of subsidiary postulates regarding the condition of markets, the number of parties to the exchange, the state of law, the minimum sensible of buyers and sellers, and so on and

[6]Lionel Robbins. *An Essay on the Nature and Significance of Economic Science*, (Macmillan. Second Edition, 1940) p. 127.
    [7]*Ibid*, p. 127.
    [8]*Ibid*, p. 78.
    [9]*Ibid*, p. 75.
    [10]*Ibid*, pp. 78—79.

so forth.[11] The technique then is to assert that the main postulates are obvious and general, and that hence economic theory in general *must* be universal, and that it is "too extensive a pre-occupation with the subsidiary assumptions, which can lend any countenance to the view that the laws of Economics are limited to certain conditions of time and space, that they are purely historical in character, and so on."[12]

Here is honest confusion about the nature of theory : that because its main postulates are universal, its main conclusions also must be universal, but since the subsidiary postulates may be limited, some minor aspects of the theory also may have only limited validity. This kind of confusion, so common in economics even today, arises from thinking of theory (laws) without relation to its total frame of reference—its universe, or system or model. In the previous lecture it was made clear that theories are propositions derived from a model, and that a model is an ordered set of classes defined by the postulates. It is in this 'universe' and only in this universe, set up by the postulates, that theory, even in its purest state, has un-conditional generality. Universality of scientific theories, including economic theories, therefore, does not mean "As it was in the beginning, is now and ever shall be, world without end." It only means what was mentioned in the first lecture, that given the postulates, the theories must necessarily follow. In other words, scientific theories are 'axiomatico-universal'. And since they are 'universal' only within the universe of discourse defined by the axioms it is equally right to say that they are 'axiomatico-relative'. In fact, the expressions, 'axiomatico-universal' and 'axiomatico-relative' denote the same fundamental characteristic of theory, that it is derived from, and only from its postulates.

It follows then that any scientific theory presupposes a system, and that, therefore, the postulates of a theory, or more accurately, of a system of which the theory is an integral part, must be adequately and unambiguously laid out. We must insist that the

[11]*Ibid*, p. 79.
[12]*Ibid*, p. 81.

postulates must be consistent, independent and complete. And since the system is the whole structure of postulates and theories, there can be no distinction between main and subsidiary postulates in the sense in which Robbins uses these terms.

<div align="center">II</div>

Fundamental theoretical research in economics during the past two decades has been concerned with setting up a System of Perfect Competition, to redeem it from its vulgar universality and to demonstrate adequately its scientific universality. A glance through the literature will show that it was no easy task.[13] In particular, the scientific universality of perfect competition was not particularly obvious. Far from it !

I have neither the time, nor I must frankly admit, the necessary expertise, even to review the literature. Instead, I shall briefly comment on some of the salient features of Perfect Competition as a theoretical system, mainly to clear up some very common misconceptions that one generally comes across in connection with perfect competition.

There is, first of all, the belief in the 'automatic' working of the system, with special reference to its powers of the optimal allocation of resources, whose vulgar counterpart, the faith in the necessity to leave the market to decide economic issues according to the general laws of economics I have already referred to, and which is an intellectual, if not political creed for many economists even today. When it was held that general welfare was best promoted by unhindered competition, it was thought that the Invisible Hand was responsible for it. But the theoretical model of perfect competition is set up to bring about the optimal allocation of resources in such a way that nobody could be made better off, through a different combination of factors, through the transformation of factors into products or through the substitution of products themselves, without making somebody else worse off. The model is set up to achieve this

[13]For a detailed list of such studies see Koopmans, *Op. cit.*, particularly references in the foot-notes on p. 39 and p. 42.

objective in the sense that the conditions laid down are all such that they support the state of affairs referred to as 'equilibrium'. To say then that the perfectly competitive model 'automatically' leads to the optimal allocations of resources is just as valid and just as meaningful as saying that a carefully constructed watch is an instrument of precision in the sense that its minute hand completes a full circle in exactly 60 minutes—no less, and no more. Another instance of the working of the Invisible Hand—obviously !

Secondly, popular notions about competition have led to the belief that rationality on the part of the producer implies the *maximisation* of profits. It should not be difficult to prove that the producer under perfect competition has little choice but to maximise profits.[14] It is also easy to show that when the restrictive conditions of perfect competition do not hold, rationality may be compatible with many other behaviour patterns on the part of the producer, be it the preference for the 'quiet life' or the maximisation of sales.[15] Further, even if we wish to restrict ourselves to the question of profit, it can be shown that the more general behavioural assumption is that producers act in the way they *expect* will maximise their net returns, and the profit maximisation hypothesis of perfect competition is only the limit case of perfect expectations.[16] In

[14]Since under perfect competition the system ensures that a producer is left with nothing but normal profits, (i.e., average revenue = average cost) a producer who is not *maximising* profits will be making losses.

[15]"The best of all monopoly profits is a quiet life" says J. R. Hicks in his "Annual Survey of Economic Theory: The Theory of Monopoly" Reproduced in American Economic Association, *Readings in Price Theory*. See also W. J. Baumol, "On the Theory of Oligopoly" *Economica*, August, 1958, where a model is built on the assumption of sales maximisation subject to a profit constraint. In general, "Profit maximisation does imply rationality, of course; but rationality is consistent with maximisation of other things as well as profits"—A. G. Papandreou, "Some Basic Problems in the Theory of the Firm" in American Economics Association, *A Survey of Contemporary Economics*, Vol. II.

[16]For a lucid discussion of this point see T. W. Hutchison. *The Significance and Basic Postulates of Economic Theory* (Macmillan, 1938) Ch. IV.

general too, the perfectly competitive model is seen to be one of perfect expectations with no role for risk and uncertainty.

Thirdly, the formal presentations of the competitive system bring out the very restrictive assumptions about technology on which the optimal allocation of resources through decentralised decision makers can take place. The crucial role of technology in the formulation of economic theories has always been recognised as seen from the basic postulates formulated by Senior and Robbins. But recent formulations have shown that it is not merely the Law of Diminishing Returns (more accurately the Law of Diminishing Marginal Productivity or the Law of Variable Proportions) that is required for the working of the competitive system, but the more general condition of convexity of the production surface. This condition imposes a severe limitation on the vulgar universality of the competitive economy.[17]

Finally, the competitive model is extremely limited in the sense that it can only deal with cases where the endowment and the full utilisation of resources are taken for granted. This is another major limitation.

To say all this is not to belittle the importance of the competitive model or to undermine our confidence in the usefulness of economic theorising in general. The competitive system as rigorously formulated by Walras, Arrow and Debreu, Koopmans and others is a beautiful system, comparable in economics to Euclidean geometry in its sphere. Einstein said that "The man who was not enthralled by Euclid's geometry was not born to be a scientific theorist." Neither is a man who is not enchanted by the Walrasian system born to be a scientific economist.

However, to define is always to limit, and the precise statement of a theoretical system will reveal at once its generality and its limitation—that is, its scientific universality. The search must therefore, begin to see if alternative theoretical systems are possible.

[17]This point is discussed at great length in Koopmans, *op. cit.*, especially pp. 35—37. For a more elementary discussion see F. M. Bator, "The Simple Analytics of Welfare Maximisation", *The American Economic Review*. March, 1957 pp. 22—59.

### III

The search for alternative theoretical systems has a major inherent difficulty. So far we do not have an accepted set of 'primitives' to form the basis of theoretical systems. This difficulty has already been referred to. Even Koopmans' formal presentations of the competitive system in terms of postulates do not present an inventory of 'primitives'. Rather he defines postulates as follows : "We shall use the term postulates for any premises used in any piece of economic analysis, which are not themselves conclusions from earlier parts of the reasoning in the same piece of analysis."[18]   From what has been said about the nature of postulates in the first lecture, it must be obvious that this definition, at best touches only one aspect—an important aspect indeed—of postulates.   However, since the postulates are not definitions of an accepted set of primitives the syntactical and semantical aspects of theory formulation tend to merge in the construction of economic theories.   This problem is not quite unique for economics, and exists to a certain extent even in geometry.[19]   But in economics it implies the almost inevitable mixing up of logic and empirical entities in any theoretical system.   Similarly, in the absence of an accepted set of 'primitives' it is difficult to ensure that the 'postulates' of one system will have any relation to the postulates of an alternative system.   On both these counts it is necessary to insist that economic analysis is still only quasi-postulational calling for some methodological safeguards in theory formulation.

In the first place, since even our basic postulates tend to be empirically oriented propositions, it is necessary to subject the postulates themselves to thorough scrutiny from time to time to sort out the logical content and the empirical elements in any system of economic theory.   This is particularly true about those systems that have already found wide acceptance where the confusion between logic and facts is likely to be the

[18]Koopmans, *op. cit.,* p. 132.

[19]This, it seems to me, is the essence of Henri Poincare's discussion of conventionalism in Geometry.

greatest.[20]    Secondly, when there are alternative systems built from postulates which have little in common in appearance, a deliberate attempt has to be made to ensure that 'axiomatico-relative' systems do not become relative in an absolute sense. Hence to understand the essential similarities and differences between alternative systems, constant evaluations and comparisons of the basic postulates are called for. In the final analysis, only an agreement on 'primitives' can pave the way for effective comparative studies of alternative systems. But since we do not, or do not yet have 'primitives' we must at least search for what may be discerned as the common sources of different economic systems. This is rather new territory for us and hence what I say here must be taken as tentative and preliminary formulations, undertaken with special reference to the problem that I wish to deal with.

[20]This seems to be the most appropriate place to comment on whether the assumptions or basic postulates of economic theory must be 'realistic'. One extreme position here is Milton Friedman's view that "the relevant question to ask about 'assumptions' of a theory is not whether they are descriptively 'realistic', for they never are, but whether they are sufficiently good approximations for the purpose in hand; and this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions". (*Essays in Positive Economics*) p. 15. Subsequent, discussions of this issue are seen in Koopmans, *op. cit.*, pp. 132-42 and in the papers and discussions on "Problems of Methodology" in *The American Economic Review*, Papers and Proceedings, May, 1963, especially the discussion by Paul Samuelson. One of the papers there is by Ernest Nagel ("Assumptions in Economic Theory") who points out the need to clarify the concepts 'realistic' and 'unrealistic' in this context.

I shall not go into the details of these discussions, but simply point out that at the syntactical level postulates are analytic *a priori* propositions and hence can be neither false nor true. But when postulates are given their semantic interpretations also, they take on an empirical content and hence their 'realism' is a matter of concern for the theory, simply because such postulates already define the class of phenomena for which the theory concerned will be relevant. In economic theories, therefore, at least in their present state, we cannot entirely neglect questions about the relevance of the 'basic postulates', because they are not basic enough.

It seems to me that what may be called the Rudiments of economic reality and hence of economic analysis are Motivations, Technology and Resources. These trio are, of course, not to be thought of as 'primitives' in the formal sense. They are rather the basic ingredients of economic reality and hence can form the basis for a formal and more detailed cataloguing of 'primitives' from which to build theoretical systems in economics. However, we may say that any theoretical system in economics will be an unfolding of the relationships among these trio.

In conventional economic theory the first two are held to be 'universal' (in the vulgar sense of the term). The third will become the source for the 'subsidiary postulates' in Robbins's terminology. But we have seen that it is neither possible nor even necessary to claim such universality to the basic premises of a theoretical system. Whatever may have been the claims of universality of the motivational assumptions in classical economic theory, in recent times it has been demonstrated that quite consistent and rigorous economic systems can be constructed by making motivational assumptions different from, and almost the opposite of, what was considered to be universal by the classical writers.[21] But as long as postulates regarding Motivations and Technology were considered to be universal, and postulates regarding Resources were considered to be peripheral, any serious challenge to economic theory had to be, or had to be interpreted as, challenges to the universal premises on which economic theory was said to be based. This theme has been dealt with earlier.

I have also shown that the system of perfect competition which deals with the optimal allocation of resources is silent about some important questions regarding the third Rudiment, viz., Resources. In particular, the system has nothing to say about the ownership of resources and the extent of the utilisation of resources. Since in the competitive system the ownership of resources does not affect the allocation of resources, competitive theory considers the question of ownership to be an

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[21]A good example of such a model is Henry Smith's "The Minimal Economy", *The Economic Journal*, March, 1965, pp. 31-43.

'institutional factor' and hence not falling within the professional discourse of economics. I shall show in the next lecture that here, as in many other matters, the competitive model is but a limit case, and that in a model not far removed from it the ownership of resources is a very pertinent factor in the determination of the allocation of resources. As for the extent of the utilisation of resources it is well known that competitive theory assumed away this problem insisting that quite automatically resources would be fully utilised. Keynes was to show that here again the competitive theory was dealing with a special case and that a more general theory of employment was called for. To this we must turn now.

## IV

For thirty years since the publication of the *General Theory* in 1936 the economic profession has been trying to evaluate the significance of Keynes's contribution and to assess in what sense it constitutes a 'revolution' in economics. I hope that one more contribution here will not be superfluous. Keynes, like many others before him, began his great work with a sense of restlessness about the sterile nature of received economic doctrines and their utter irrelevance to the economic realities that he saw around him. Confronted with the major economic calamity of mass unemployment the only explanation that classical economic theory could offer was to say that the unemployment must be the result of the unwillingness of workers to work at low wages, and hence it was not a theoretical problem at all, but mere perverse behaviour! The great advantage of accepting an untestable hypothesis about human motivations as the universally valid basic premise on which the entire economic theory rests is that all 'theoretically' unexplainable phenomena can be explained away' as the result of the human beings not playing the game according to its rules. Economists have used this weapon of professional selfdefence every now and then, and are still continuing to do it today as I shall demonstrate in the next lecture. Economists who

specialise in this game present their opponents with two clear-cut alternatives : Either you accept the universally valid motivational and technological assumptions on which economic theory rests, and hence accept economic theory also as universally valid, or you reject those universal assumptions and you not only destroy the entire fabric of economic theory, but you show yourself to be a nihilist as well.

Keynes took the bull by the horns. It seems to me that the essence of the Keynesian revolution lies in his demonstration that an alternative theoretical system could well be constructed without disagreeing with the motivational or technological assumptions of conventional economic theory. Or, Keynes became the founder of the first 'respectable' alternative to the theoretical system of perfect competition. A new era in economic analysis was beginning as significant as the beginning of non-Euclidean geometry.

We must now examine Keynesian Economics as a theoretical system. In the first place, watch how the new system emerges. Keynes recognises at once the logical consistency of the 'classical' theory of economics on which he was brought up, "and which dominates the economic thought, both practical and theoretical, of the governing and academic classes of this generation" but also that it will be "misleading and disastrous if we attempt to apply it to the facts of experience".[22] If, therefore, "orthodox economics is at fault, the error is to be found not in the superstructure, which has been erected with great care for logical consistency, but in a lack of clearness and of generality in the premisses."[23] And so the General Theory opens with a discussion about the two 'fundamental postulates' of classical theory. In a straight forward manner and simple style which are so

[22]The General Theory, p. 3.

[23]Ibid, Preface, P. v. Keynes also said: "The classical theorists resemble Euclidean geometers in a non-Euclidean world who, discovering that in experience straight lines apparently parallel often meet, rebuke the lines for not keeping straight—as the only remedy for the unfortunate collisions which are occurring. Yet, in truth, there is no remedy except to throw over the axiom of parallels and to work out a non-Euclidean geometry. Something similar is required today in economics." p. 16.

uncharacteristic of the rest of the book, the postulates are given as follows :[24]

1. The wage is equal to the marginal product of labour.
2. The utility of the wage when a given volume of labour is employed is equal to the marginal disutility of that amount of employment.

My contention that Keynes accepted the motivational and technological assumptions of classical economic theory but built his system on a different postulate about 'Resources' can be demonstrated in terms of his attitude towards these two postulates. Keynes accepts the first postulate although he does not bother to show how it is relevant to his system. Acceptance of the first postulate, it can be easily shown, amounts to the acceptance of the maximisation principle and the 'Law of Diminishing Returns.' The second postulate he rejects because he claims that classical theory did not have enough 'data' to say what decides the 'supply' of (in fact, utilisation of) labour. The General Theory, of course, is an exposition of this contention.

With liberal interpretation, therefore, it can be argued that Keynes was following the axiomatic method to build an alternative theoretical system which he expected to be compatible with the facts of experience. But I must insist that this is an ex-post interpretation, for Keynes himself did not realise the implications of this methodological device. Keynes, who set about to work out 'The General Theory' is from time to time seen to be either unable or unwilling to recognise the axiomatico-universality of the classical system and the axiomatico-relativity of his own system. In the heat of the argument he has described the whole or part of the classical theory as 'nonsense theory', and has also insisted that the so called 'free-enterprise system', with its flexibility which according to him the classical economists espoused, could function only in highly

[24]*Ibid*, p. 5. Notice also the nature of these 'postulates'. Neither in content nor in terminology are they analytic *a priori* statements, but are empirical statements that are verifiable.

authoritarian societies like Italy, Germany or Russia (of those days) but not in France, the United States or Great Britain ! Keynes could not help pretending that those who preceded him were all confused and wrong !

Not only Keynes, but even some of his most logical critics failed to see the essential nature of his system. Patinkin, for instance, is eager to demonstrate that the classical system must be analysed in terms of its own logical sequence, but is unwilling to apply the same principle to the Keynesian system as revealed by his attitude to the role of expectations in the two systems, the fallacy which Hicks has so clearly brought out in his review of Patinkin's *Magnum opus*.[25]

What this demonstrates is that once a system is set up, once its universe of discourse is defined, then we must travel through it in terms of its own logical sequences, however strange and paradoxical they may appear in terms of the logical sequence of another system. Apparently paradoxical statements such as "There is unemployment because there are not enough people to be employed" are meaningful within the Keynesian system and may not be meaningful in anothter 'context'. The 'context', in this case is a system.

The success that the Keynesian system has had in challenging the conclusions of classical theory in terms of their applications to the facts of experience proves also that a theoretical system can be successfully challenged in its entirety or even in its parts only by another system. Factual evidences to the contrary, and arguments, however convincing, are unlikely to destroy faith in the 'vailidity' of a theory widely accepted for a long time. It is a temptation to dogmatic despots as well as to 'open-minded' theorists to say "If facts do not agree with theory, change the facts."[26] The theorist, of course, is more subtle than the despot, for he does not wish to *change* the facts, but only to *ignore* them for 'convenience of

[25]D. Patinkin, *Money, Interest and Prices*, (Row, Peterson and Co., 1956) *passim* and J. R. Hicks, "A Rehabilitation of 'Classical' Economics ?", *The Economic Journal*, June, 1957, pp. 278—89.

[26]This statement is attributed to Stalin.

analysis' protesting that such procedure "does not affect the substance of the argument".[27] As for theoretical arguments, Pigou in his *Economics of Welfare* perhaps posed a more serious challenge in terms of external economies, than Keynes in terms of effective demand, to the classical theory of resource allocation. And yet Pigou is considered to be one of the arch-defenders of classical theory and Keynes its most serious challenger! The clue to this peculiar phenomenon seems to be that theoretical analysis, like nature, abhors a vacuum, and insists on having an alternative system before an existing one is given up. What is often referred to as a 'theoretical vacuum' is only the stage when one system gives place to another.

## V

Two questions arise from these discussions about the nature of theoretical systems, both of which are related to the possibility of the existence of alternative systems. The first pertains to the relationship between theoretical *analysis* and theoretical systems. The second is concerned with the choice from among alternative systems.

As an introduction to the answer to both these questions I wish to recall two propositions that I have already discussed. The first is that theories are derived from theoretical systems, and the second is that it is possible to have alternative theoretical systems even to deal with the same empirical situation. Keeping these two in mind we may turn to the first question posed above, about the relationship between theoretical analysis and theoretical systems.

It is often asserted that economics, or economic theory consists essentially of a set of tools of analysis—that it is a box of tools, or a filing cabinet. I personally prefer the second metaphor. If I may build on it, economic analysis or economic

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[27]Keynes has pointed out, for instance, that Pigou did not find it difficult to *ignore* the existence of involuntary unemployment, after recognising it to be an observable fact. See foot-note on p. 5of the *General Theory*.

theory with a capital 'T' is a filing cabinet, of which we have succeeded in filling but a few chambers. The present poverty of economic theory is mainly because there are many more empty chambers to be filled. In fact we do not even know the limits of the filing cabinet of Economic Theory. My preference for the metaphor of the filing cabinet can now be explained by saying that if economic theory is but a box of tools we throw our tools in indiscriminately, and what is even more dangerous, pull them out quite indiscriminately too—a practice so common in our country![25]   Orderly progress in Economic Theory will be possible only "when we arrange and record our logical deductions in such a manner that any particular conclusion or observationally refutable implication can be traced to postulates on which it rests"—in Koopmans' words, which I may borrow again to say : "considerations of this order suggest that we look upon economic theory as a sequence of conceptual models that seek to express in simplified form different aspects of an always more complicated reality".[29]   Training in economics must consist of introducing students to the filing system and insisting that they master the principles of references and cross-references, and be familiar with one or two most pertinent chambers.[30]

Finally there is the most crucial question of deciding which model is the most applicable under any given circumstance.

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[25]The most familiar examples in this respect were the attempts to apply 'Keynesian Economics' to underdeveloped economies—a very fashionable past-time for Indian economists during the early fifties. For some important references see a recent paper by Ashok Mathur, "On Throwing the Baby away with the Bath-Water", *The Indian Economic Journal*, April-June, 1965, pp. 397—416.

[29]Koopmans. *op. cit.*, p. 142. He goes on to say "The card file of successfully completed pieces of reasoning represented by these models can then be looked upon as the logical core of economics, as the depository of available economic theory". p. 143.

[30]This underlies, I imagine, the insistence. by Joan Robinson, for instance, ("Teaching of Economics", *Economic Weekly*, Annual, 1960) that the teaching of economics must begin with a study of economic systems. "Systems" here should not be immediately and necessarily identified with stereotypes such as 'Capitalism', 'Socialism' etc. The training must be to perceive, appreciate and construct theoretical models.

This is one of the most debated questions even in the so-called 'pure sciences'.[31] And since the question has bearing on the content and empirical verifiability of models it raises all the manifold problems associated with measurability and hypothesis testing. These are very pertinent problems indeed which present special difficulties to a social science like economics. But as I mentioned at the beginning of this lecture, I am not directly concerned with these issues in the present series of lectures.

In terms of the theoretical, as against the empirical evaluation of economic models and systems, I think a system which explains economic problems largely in terms of the third Rudiment, Resources must be preferred to alternative systems which may explain the same problems in terms of the other two Rudiments. This is not because Motivations and Technology are less important in economic analysis, but because as economists, we can deal with Resources with greater competence.

[31]See Philipp Frank, *Philosophy of Science* ch. 15.

# TOWARDS A THEORETICAL SYSTEM OF THE INDIAN ECONOMY

## I

HAVING EXAMINED at some length the nature of scientific theories and the problems of theory formulation in the first lecture, and their bearing on economic theories and economic systems in the second, we are ready now to examine the possibilities of setting up a theoretical system to represent the Indian economy. I wish to emphasise that what follows is still in the exploratory stage and that I am feeling my way through.

Like any other economy, the Indian economy is a complex entity and we must approach it from many angles if we are to become acquainted with it. Consequently each approach will be a partial view which must not claim to be either a full picture or the only possible picture. My primary concern is with the allocation of resources, and hence what I wish to examine is that aspect of the Indian economy which brings it under the generic title 'an over-populated underdeveloped economy.'

There is now a vast body of literature dealing with the special characteristics of such economies. Since these discussions are quite familiar, it is not necessary to review them all here. However, there are three main lines of thought whose methodological inadequacy I must point out.

One of the earliest reactions to the problems of underdeveloped economies was to suggest that the peculiarities that were observed in them were due to the fact that the participants in them are not *homo oeconomicus*—that the producers in these economies do not act rationally, that resource owners do not respond adequately to economic stimuli and that at all levels one comes across inertia, immobility, ignorance and irrationality, all of

which hinder the working of the economic system.[1] A detailed discussion about the validity of these allegations is not necessary. I have already pointed out the methodological inadequacy of this approach, that to explain complicated economic phenomena solely in terms of attributed motivational differences is nothing but a defence mechanism, and a very poor one at that. I shall not minimise the importance of motivations in economics. But if they are to be the main explanations for the special features of an underdeveloped economy, a *consistent model* based on a different motivational assumption must be worked out. Such a task has hardly been attempted.

The second major line of approach to underdeveloped economies has come from those who believe in the catholicity of economic analysis, but who see economic analysis only in terms of received economic theory. They hold that received economic theory has enough gamuts to deal with the problems of *any* economy. These economists also recognise that the economic problems of an underdeveloped economy are somewhat different from those economies with which economic theory usually deals and hence call for necessary adjustments in received economic theory to deal with underdeveloped economies. I shall, therefore, refer to this approach as the *mutatis mutandis* method.[2] For example, if in an advanced economy with scarcity of labour the attempt is to maximise output per unit of labour, in an underdeveloped economy with scarcity of capital, the attempt must be to maximise output per unit of capital. This approach again is not adequate because it can recognise as theoretical problems only those which received economic theory

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[1] For a forceful presentation of this view see J. H. Boeke, *Economics and Economic Policy of Dual Societies : As Exemplified by Indonesia* (Institute of Pacific Relations, 1953).

[2] It may not be inaccurate to say that most Western Economists in the field of economic development subscribe to this approach. As an example see W. B. Reddaway, "The Economics of Underdeveloped Countries", *The Economic Journal* March, 1963 pp. 1—12.

can handle, and hence is often forced to rely upon motivational differences to supplement the 'purely theoretical' arguments.[3]

The third approach is to say that if neo-classical economic theory, for instance, cannot adequately deal with the problems of underdeveloped economies, some other existing economic system may be tried. On this advice both the Keynesian system and more widely the classical system have been forced on underdeveloped economies.[4] This procedure is often an optical illusion as revealed by the disillusionment with the Keynesian system in the explanation of 'under-employment'. The classical system has had a greater measure of success in this sphere, but I shall soon point out some major theoretical problems presented by the 'classical' treatment of underdeveloped economies.

## II

Since the problem I wish to deal with is concerned with the allocation of resources, I shall once again go back to the model of perfect competition.

In the examinations of the mathematical formulations of the model of perfect competition, beginning with Walras's presentation of the General Equilibrium System, economists have been concerned with the need to ensure that any theoretical system envisaged was not only mathematically rigorous, but also economically relevant. One of the earliest preoccupations in this regard was to ensure that equilibrium quantities and prices established by a system were non-negative, as negative quantities

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[3] For instance, the *mutatis mutandis* method can, and does advocate 'labour-intensive' techniques for labour-abundant underdeveloped economies, but has no explanation whatever, except in terms of cultural and institutional aspects, as to why techniques of differing factor proportions continue to coexist in such economies.

[4] Reference has already been made to 'Keynesian' studies in the Second lecture (foot-note 28). The best example of the application of 'classical economics' to overpopulated underdeveloped economies is W. A. Lewis's celebrated paper, "Economic Development with Unlimited Supplies of Labour", *Manchester School of Economic and Social Studies*, May, 1954, pp. 139—91.

and prices are meaningless in economics, however useful they may be in arriving at a mathematical solution of a problem.[5]

A similar problem of theoretical specification is to ensure that each participant in the system has sufficient command over resources to enable him to survive and to participate in the activities ascribed to him, as consumer, producer etc. From a commonsense point of view the need for this 'condition' is obvious—so obvious, indeed, that it may appear to be trivial. And yet to those who are concerned with the problem at the theoretical level it is one of the hardest—referred to in technical discussion as the 'survival problem.'[6]

It should not be difficult to see that the 'survival problem' can be and often must be dealt with at two levels : at the level of the economy as a whole, and at the level of the participants themselves. At the aggregate level it is necessary to establish that the economy as a whole has enough resources to produce a total output sufficient to ensure the survival of all the participants. It is further necessary to show that each participant has enough command over resources to ensure his survival. If the second condition is satisfied, the first will be satisfied along with it, but the converse is not true. But if the first is satisfied, the second can be satisfied also, provided that appropriate assumptions are made about the ownership of resources, or a mechanism for the necessary transfer of output is envisaged.

One of the most detailed examinations of these problems is undertaken by Arrow and Debreu.[7] To deal with the survival problem they make two assumptions. They assume, first of all, that the aggregate supply set contains a point which supplies just a little more of every commodity than is necessary, as indicated by some point in the aggregate consumption set for all consumers

[5] For a brief discussion of these issues see section entitled Historical Note in K. J. Arrow and G. Debreu, "Existence of an Equilibrium for a Competitive Economy" *Econometrica*, July, 1954.

[6] For a discussion of the survival problem see Koopmans, *Three Essays* (as cited in the Second Lecture) pp. 59 & 63.

[7] Arrow and Debreu, *loc. cit.* I have followed here the paraphrasing of Arrow and Debreu by Koopmans, *op. cit.* p. 59.

to survive. Secondly, they assume that each consumer can, if necessary, survive on the basis of the resources he holds and the direct use of his own labour, without engaging in exchange, and still have something to spare of some type of labour which is sure to meet with a positive price in equilibrium.

We must discuss these two assumptions carefully as they serve as the point of departure for the Model developed in the subsequent sections. The first assumption, if we rephrase it, maintains that the total output of the economy must not be less than total consumption at survival level. If this condition is not satisfied the economy concerned *cannot* have a realistic counterpart. Hence we shall not go into further discussion on this point, for what we want to examine is a theoretical system that can have an empirical counterpart. The second assumption, however, is not necessary for all systems, although it is necessary for a competitive system to attain equilibrium. *The Model developed in this lecture is of an economic system where the first of the Arrow-Debreu assumptions holds and the second does not.* The system that we envisage, therefore, cannot be a competitive system even if all the other postulates of a competitive system are accepted in its formulation.

### III

At this stage it is not possible to work out a completely new system because we do not know what postulates are necessary and sufficient for that purpose. This is not surprising. After all, even the postulates required for the competitive system are yet to be conclusively established.

Hence what follows is more in the nature of an experiment designed to show the special features of resource allocation when the second Arrow-Debreu condition is absent. The procedure will be to describe in the first instance a situation where a competitive solution is possible, and then to remove the second Arrow-Debreu condition to observe the differences that will be indicated.

Consider an economy which produces one output Y (which is a consumption good) using two resources, L (for Labour)

and P (for Property representing all non-labour resources). The production function

$$Y = f \ (L, P) \tag{3. 1}$$

is assumed to be linear and homogeneous. It is assumed also that unifactor production is impossible, i.e., Y cannot be produced with either L or P alone.

L is measured in terms of 'man days'. A 'man day' is 'usually' 8 hours, but it can be more. Hence 'usually' the daily wage rate (w) is equal to the daily earnings (W). The problem of the measurement of P is ignored as it is of no special significance to our analysis.

Now define W* as the survival income per 'man' per 'day'. If this income is to be earned by wages alone, the (daily) wage *rate* must have a minimum. Let this minimum wage rate be denoted by w*. With the assumptions already made (regarding 'man days') it follows that

$$w* \equiv W*$$

Consider the endowment of L and P in the economy to be such that

$$\frac{Y}{L} > \frac{\partial Y}{\partial L} > W* \tag{3. 2}$$

where $\frac{\partial Y}{\partial L}$ is the partial derivative of (3.1.)

Since $\frac{Y}{L}$ is per capita output, (3.2) satisfies the first of the Arrow-Debreu survival conditions.

Let us now move to an examination of the second Arrow-Debreu condition. Whether the second condition is satisfied or not depends on the *distribution* of the endowment of resources. We shall assume that everybody has L, but that P is randomly distributed. We shall assume also that all owners of resources want to maximise their earnings.

The assumptions made so far, about the production function and about motivations will be retained throughout our analysis.

These assumptions, it will be noticed, are the conventional assumptions of received economic theory regarding Motivations and Technology.

To see how production will be organised and how the output will be distributed among the owners of the resources it is necessary to examine further the ownership pattern of the resources. For this purpose let the owners of resources be identified as 'households'. With the assumptions we have made about the distribution of resources in the economy, *three* types of ownership patterns are possible.

*a*) All households have L and P.

*b*) Some households do not have P.

*c*) Only one household has P.

If all households have L and P, *production* in the economy may be organised either by each household becoming a production unit, or by some households renting out their resources to other households and making an earning on the basis of the rental values of L, P or both. Let us denote these two possibilities as *a(i)* and *a(ii)*.

In cases *a(ii)*, (*b*) and (*c*) there will have to be markets for the hiring of resources, while such markets are not necessary in case *a(i)*. On the basis of assumptions so far made, cases *a(ii)*, (*b*) and (*c*) can all be organised on the principle of a competitive economy. The assumption of a linear homogeneous production function ensures that it is immaterial whether production takes place in a giant firm (or farm) as it may happen in case (*c*)[8] or in many small units as may happen in cases *a(ii)* and (*b*). If the economy is organised according to the competitive principle, for each production unit and for the economy as a whole

$$w = \frac{\partial Y}{\partial L} \quad \text{and} \quad r = \frac{\partial Y}{\partial P}$$

and   w.L + r.P = Y.

where w is the wage rate and r is the rental on property.

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[8] It will be noticed that case (c) is the Lange-Lerner type of 'socialist' economy with a simulated competitive solution.

These are, in a sense, nothing but the properties of a linear homogeneous function. What competition ensures is that the wage rate (w) and the rental on Property (r) are uniform throughout the economy. Competition further ensures that there are no earnings in the system except as *rentals* of resources. In particular, there will be no 'entrepreneurial income' in the economy. All these features are known at least since Walras rigorously established them in the *Elements.*[9] Since the equilibrium wage rate established is greater than w* the second Arrow-Debreu condition also is satisfied. Our main interest in all this is to point out that *provided all our assumptions are given*, the allocation of resources does not depend on the pattern of the ownership of resources. This has been one of the most widely used inferences from the theory of perfect competition. The perfectly competitive system is concerned with the allocation of resources, but the finding that *ownership* of resources does not influence *allocation* of resources in the *competitive system* has often been generalised to imply that the allocation of resources does not ever depend on the ownership of resources. From what was said about the nature of theoretical inferences at the end of the second lecture, the fallacy of such a generalisation must be obvious. And yet such careless and fallacious generalisations have played havoc in economics. On the strength of such generalisations, for instance, it is often asserted that while matters relating to the *allocation* of resources are theoretical problems, those dealing with the *ownership* of resources are non-theoretical, even non-economic !

## IV

In the discussion so far, the Arrow-Debreu conditions are satisfied by making appropriate assumptions about the endowment. of resources. We must now examine the consequences of removing the Arrow-Debreu conditions.

9 "In a state of equilibrium in production, entrepreneurs make neither profit nor loss. They make their living not as entrepreneurs, but as land-owners, labourers or capitalists in their own or other businesses". Lesson 18.

This is best achieved by imagining a sudden disappearance of a large part of the endowment of Property in the economy, with everything else remaining as before. It may lead to two possible situations. The first possibility is that with the new endowment of Property, $\frac{Y}{L} < W^*$ implying that the first of the Arrow-Debreu conditions is not satisfied. Such an economy is vacuous and hence we may safely leave it aside. The second possibility, on the other hand, is very realistic. The disappearance of part of the endowment of property may result in a situation where

$$- \quad \frac{Y}{L} > W^* > \frac{\partial Y}{\partial L} \qquad (3.3)$$

In this case the functioning of the economy and the organisation of production will depend on the pattern of the ownership of P. We may examine the different possibilities as was done in the case of (3.2).

1. One of the simplest possibilities is case $a(i)$ described earlier, which assumes that the depleted endowment of Property is so distributed that each household has some Property, just enough to make $\frac{Y}{L} > W^* > \frac{\partial Y}{\partial L}$ in the case of each household.

The economy will then consist of a number of producer-households, each producing its own Y, so to say, a little more than what is required for the survival of its members. It is not meaningful to say much about the allocation of resources in such an 'economy' (consisting, in fact, of a large number of miniature economies). But we may note one thing about such an 'economy'—each one of its constituents, and consequently the 'economy' as a whole is capable of growing over time because the Y it produces at any one time is more than sufficient for the survival of its members. The 'economy' *can* save and invest.

2. A second possibility is case (c) which suggests that a simulated competitive solution of the Lange-Lerner variety is possible in this situation. If all Property is owned by one

'household', it can hire the Labour from *all* the other house-holds, thus converting the economy into a giant firm (or farm) whereby an optimal allocation of the resources can be achieved. But it can be achieved *only if* the allocation and distribution decisions can be separated, so that even where $\dfrac{\partial Y}{\partial L} < w*$, the wage rate can be equal to or greater than w*. In this case also it will be noticed that the economy can grow.

3. The distribution of the depleted P may also be as in case (b) where some households do not have P. If this is the case, the households in the economy may be divided into three types.

A — type, which does not have any P

B—type, which has some P so that

$$\frac{y}{1} > W* > \frac{\partial y}{\partial l} \qquad \text{as in (3.3) ; and}$$

C — type, which has enough P so that

$$\frac{y}{1} > \frac{\partial y}{\partial l} > W* \quad \text{as in (3.2)}^{10}$$

What about the organisation of production and the structure of the economy in this case? We can make the following observations :

(*i*) There will be a market for Labour in the economy as the A-type households can earn a living only by working for others, and at least the C-type households will be eager to hire Labour.[11]

(*ii*) We can also say something about the level of wage rate that will get established. If the wage rate turns out to be below w*, A—type households will have to *increase* the supply of Labour (More than 8 hours of Labour will be required to earn the minimum daily income of W* when w $<$ w*). In

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[10] Lower cases are used when the production function refers to households, and subscripts 'a', 'b' and 'c' refer to the type of household.

[11] It may be helpful to recall that we are retaining the major assumptions made in Section III, viz, that unifactor production is impossible and that all households attempt to maximise earnings.

fact the lower the wage rate, the larger will be (and will have to be) the supply of Labour.  There is thus a peculiar situation. At any positive wage rate below w* there will be excess supply of labour reducing the wage rate further, and pushing it down to zero.  At zero wage rate, of course, there will be no supply of labour, but there will be positive demand for labour, thus resulting in excess demand for labour.  So we are led to conclude that there can be no competitive equilibrium wage rate below w* under these circumstances.[12]

(iii) Consider the situation where the wage rate is maintained

at w* by some (institutional) method.  Since $\dfrac{\partial yb}{\partial lb} < w^*$

Labour from the B-type households also would wish to be hired. But at w* wage rate C-type households will be the only hiring units, and they cannot hire all the Labour in the economy at that wage rate.  The C-type households will hire only till

$\dfrac{\partial yc}{\partial lc} = w^*$ But it means that $\dfrac{\partial yc}{\partial lc} \neq \dfrac{\partial lb}{\partial yb}$ or that the optimal

allocation of resources is not achieved.

The experiment that we have conducted, therefore, leads to some very significant conclusions.  It shows that starting out from a situation where resources were optimally allocated by the forces of competition, a change in the resource endowments throws serious doubts about the possibility of achieving optimal allocation.  If the level of resources is such that the first Arrow-Debreu condition is satisfied, optimal allocation can be achieved

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[12] Arrow and Debreu, loc. cit., (p. 281) deal with an economic system with fixed technical coefficients where production requiring a given type of labour also requires, directly or indirectly, some complementary factors, and show that in this case it may be impossible to reach equilibrium.  The discussion makes it clear that the impossibility of achieving equilibrium does not depend on fixed technical co-efficients, but on the peculiarities of the supply function of labour because workers must have a minimum income for survival.  Fixed technical coefficients are brought in only to show that with such technical conditions labour may easily become redundant.

along the Lange-Lerner lines *provided that Property is collectively owned.* If, on the other hand Property is *not* collectively owned, and *if some households do not have Property* and uni-factor production is not possible (i.e., if the second Arrow-Debreu condition is not satisfied) optimal allocation of resources *cannot* be achieved, even if all the other conditions of a per-fectly competitive system, including the presence of 'free' factor markets are present. The *possibility* of the optimal allocation of resources when resources are collectively owned, and the *impossibility* of optimal allocation where some households do not have some of the resources also indicate that in many economically relevant situations the allocation of resources depends on the *level and ownership pattern* of the resources. In fact the perfectly competitive case, where as shown above, the allocation of resources *does not* depend on the ownership pattern of resources must be considered to be a special case indeed. In the light of this finding it is impossible to defend the claim of the competitive system to be 'universally' applicable ! Needless to say that this finding does not challenge the univer-sality of the *logic* of the competitive system.[13]

## V

All this appears to be far removed from our main problem, the structure of an 'over-populated underdeveloped economy'. To move towards a direct discussion of such an economy, let us once again turn to an examination of the ownership pattern of resources in general. On this basis we have already classified the households into three types. The classification must now be fully related to W*. Retaining our assumption that *all* households have Labour and that Property is randomly distributed, and ruling out unifactor production again, we now have

[13] See the discussion in the First Lecture, Section IV.

F—4

A — Households where $\quad \dfrac{ya}{la} < W*$

B — Households where $\quad \dfrac{yb}{lb} > W* > \dfrac{\partial yb}{\partial lb}$

C — Households where $\quad \dfrac{yc}{lc} > \dfrac{\partial yc}{\partial lc} > W*$

This classification provides us an analytical frame to evaluate different theoretical systems. The competitive system, for instance, can exist when all households are of the C-type. There is no implication here that *each* household must become a production unit; since there are no entrepreneurial earnings in competitive equilibrium, it is a matter of indifference to the households (in terms of earnings) whether they do or do not become production units. The knowledge of the household's preference between the status of an entrepreneur, and that of a rentier-worker is needed to say whether it does or not become a producer-household. This aspect is usually overlooked in the theory of perfect competition because in most instances the analysis begins with an *a priori* division of households into worker-consumer households and entrepreneur-producer households.

In terms of our classificatory scheme, an economy which consists of C-type households and A-type households is the classical dual economy of 'capitalists' and 'workers' or 'proletariat'. We are not directly concerned with the classical economy, but some comments about it are warranted mainly because of the widely prevalent belief that the present-day underdeveloped economies are more akin to the classical economic system than to the neo-classical competitive system. The classical economic system is mainly concerned with accumulation, but our main interest now is allocation.[14] There are certain allocation aspects implicit in the classical system which our analysis helps to bring out. We shall concentrate on one of them, the question of subsistence wages. The need for a subsistence wage rate is now

[14] Some problems relating to accumulation are discussed in Section VI below.

considered to be an 'institutional assumption', reluctantly brought into economic analysis, mainly to show that economists are aware of the workers' human rights! But in the classical system it is a little incongruous to believe (to say the least) that die-hard capitalists decided upon or agreed to the level of wage rate out of social concern! Whatever may be the rationale of the subsistence wage rate assumption in economic analysis to-day, in the classical system its only rationale can be that it is an *economic* necessity. Our analysis in section IV has proved this already, that where there are households without Property, and where households with Property desire to employ workers in order to maximise their *own* earnings, there can be no equilibrium wage rate below w*.

A close examination of the classical system shows that the subsistence wage rate is not an assumption, but an equilibrium condition. But the equilibrium is attained and maintained by assumptions which may appear rather unusual today. The assumptions pertain to the mechanism for the elimination of excess demand (positive or negative) in the Labour-market. If the C-Households have enough Property to hire all Labour from the A-Households (i.e., the endowment of Property in the C-Households and the endowment of Labour in the A-Households are such that when the C-Households employ all A-Households

holds $\dfrac{\partial yc}{\partial lc} = $ w*) there is then an 'equilibrium solution' and an

'optimal allocation' of resources. This equilibrium, however, is not a matter of chance. The workers themselves and nature see to it that the equilibrium solution is maintained. If there is positive excess demand in the Labour-market the workers (A-Households) eliminate it by producing and supplying more of it, and if there is negative excess demand nature eliminates it! The logic is impeccable, although the relevance and the workability of some of the assumptions may be questioned. On this score, however, neo-classical economics cannot find fault with classical economics without pangs of conscience!

It can now be shown that the application of the classical model to underdeveloped economies raises some serious analytical problems. The preference for the classical model arises because of its ability to deal with growth. But the way in which Lewis and other modern classical writers divide the economy into two raises many issues. For example, how does the 'subsistence sector' manage to have a subsistence income ? Is there any factor in this sector which cooperates with labour in the production of that income ; what is the principle of factor allocation in that sector, and in the economy as a whole ? If the combined earning of that factor and of labour is just enough to provide a subsistence income for labour, what is the remuneration for that factor ? If the income produced is more than subsistence level, can the 'subsistence sector' also save ? (however contradictory the question may appear to be). And if the subsistence income is produced by labour alone in the subsistence sector, does that not in itself constitute a solution for the practical problem that all the 'overpopulated underdeveloped economies' face—that of providing a bare minimum level of living for their people ?

Modern writers who exhort us to return to the classical world to understand the problems of underdeveloped economies do not raise any of these questions. But these queries show the inadequacy of the classical model to analyse the problems of underdevelopment.

If both the classical and neo-classical systems are inappropriate to deal with 'over-populated underdeveloped economies' we must search for a new system that can. From our classificatory scheme we can produce one more system, viz., one which consists of all three types of Households, A, B and C. Obviously it is more general than the neo-classical system (consisting of C-type Households only) and the classical system (consisting of C-type Households and A-type Households). *Or, the neo-classical competitive system and the classical system are special cases of the new system that we have arrived at.* Whether there is an empirical situation corresponding to it or not, the new system is, therefore, a significant conceptual entity.

Reviewing the manner in which we have set it up, it is clear that the essential feature of the system arises from the level and distribution of resources in the economy, particularly the non-labour resource, Property. Hence the chief characteristics of the system must also be developed in terms of these two.

It is a Labour-abundant, and Property-scarce economy, with the scarce resource widely, but unequally distributed. According to 3.1 (the production function) $\frac{Y}{L}$ is a positive and increasing function of $\frac{P}{L}$ and since $\frac{P}{L}$ in this economy is rather low, $\frac{Y}{L}$ in the economy must also be low. There are two distinct types of production units in the economy, also arising from the level and distribution of resources in the economy. There are production units (the C-Households) which organise production on the basis of hired resources (particularly Labour) and other units which engage in production on the basis of owned-resources (the B-Households). It may be noted too that the existence of the two types of production units does not arise from any kind of motivational difference on the part of the resource owners : it is not as though some resource owners *prefer* to become producers and others *prefer* to become workers. The level and distribution of resources is such that some have to become producers (the B-Households) and others *have* to become workers (the A-Households). Therefore although there is some mobility of resources in the economy, there is nothing like free mobility which the competitive system usually assumes. The lack of mobility in the new system, however is *not* an assumption indicating the presence of non-economic impediments, but a conclusion derived from the basic assumptions, which, save for the level and distribution of resources are the same as in the competitive system.

Thus we get the picture of a labour-abundant low-income economy one of whose main characteristics is the presence of a large number of owner-producers (self-employed). We are, therefore justified in identifying the new system as an 'over-

populated underdeveloped economy'. Just as $3.2 \left( \dfrac{Y}{L} > \dfrac{\partial Y}{\partial L} > W^* \right)$ served as a representation of the competitive system, 3.3.

$\left( \dfrac{Y}{L} > W^* > \dfrac{\partial Y}{\partial L} \right)$ may be seen to be a representation of the 'overpopulated underdeveloped economy'.

The characteristics of a system of this kind will depend on the relative size of the three 'sectors' of the economy, the A, B and C—type households. If the A—Sector is so large that the C—Sector cannot fully absorb it, the system will be close to the classical system, and the adjustment mechanism also will have to be classical, with the Labour endowment getting reduced by the intervention of nature. If the C-Sector manages to absorb all the Labour from the A-Sector (no more and no less) the system turns out to be a two-sector economy, consisting of the B—and C—Sectors only. This is what is often referred to as the 'dual economy', but the earlier discussion will show that it is *not* what may be called ' classical dualism, where the two sectors are the A-Sector and the C-sector.

A more interesting case is where the C-sector has absorbed all Labour from the A-sector, and still needs Labour which can come only from the B-Sector. Since $\dfrac{\partial yb}{\partial lb} <$ w* and since the wage rate that the C-Sector offers is equal to w* there will be a tendency for Labour to move from the B-Sector to the C-Sector. But if on the whole the situation is known to be one where employment opportunities are scarce, workers from the B-Sector may not readily move into the C-Sector. Their movement will depend on each B-Household's expectation of finding employment in the C-Sector. Let $\propto$ denote this expectation coefficient. Then $1 \geqslant \propto \geqslant 0$, with each B-sector household having *its own* value of $\propto$ within this range. Workers from the B-Sector will move into the C-Sector only if

$\frac{\partial yb}{\partial lb} \cdot \frac{1}{\propto} < w^{*[15]}$. We may consider $\frac{\partial yb}{\partial lb} \cdot \frac{1}{\propto}$ to be the B-Sector household's 'reserve price' of Labour. Even if it is assumed that all B-Households have the same $\frac{p}{1}$ each Household is likely to have a different 'reserve price' as it is impossible to assume that every Household will give the same value to $\propto$. If we accept that $\frac{p}{1}$ is not uniform in all B-Households, then there will be a much wider range for the 'reserve price'.

The concept of the 'reserve price' can be strengthened further if it includes also the resource owners' preference between 'self-employment' and 'other-employment'. Such a preference function is not only compatible with the competitive system (to ensure that in the analysis of the new sustem we are not bringing in new assumptions through the back door) but is necessary also. This is because (as has been established in Section III) there is no entrepreneurial income when the competitive system is in equilibrium, and hence entrepreneurs (producers) are entrepreneurs only because they prefer to be entrepreneurs rather than workers even when the earnings are the same in both cases. Hence there is nothing unreasonable if we argue that many B-Households may have high preference to be self-employing producers.[16] But, of course, such an assumption is not necessary to establish the possibility of a wide range of 'reserve price'.

The significance of the 'reserve price' is to show that the C-Sector may have to offer a wage rate higher than w* to get its requirement of Labour even while

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[15] A. K. Sen makes use of this device in his "Size of Holdings and Productivity" in *Economic Weekly*, Annual Number, February, 1964.

[16] The need to introduce a preference function between the producers' income and non-income satisfactions has been brought out by T. Scitovsky. See his "Note on Profit Maximization and its Implications", American Economic Association, *Readings in Price Theory* (Richard D. Irwin, 1952).

$$\frac{\partial yc}{\partial lc} > w^* > \frac{\partial yb}{\partial lb}$$

In effect this means that the C-Sector may face an *upward sloping* supply curve of Labour, which is contrary to the usual assumption that in economies of this kind the supply curve of Labour is infinitely elastic at the subsistence wage rate.[17]

Some more features of the system may be drawn out from the structure we already have. For instance, it may be noted that there is no overt unemployment in the system. The B-Sector is a big labour-absorbing sector, and it achieves the labour absorption by substituting Labour for Property as far as it is technologically possible. A consequence of this process is to bring down $\frac{yb}{lb}$ and $\frac{\partial yb}{\partial lb}$, but y itself must be increasing as long as $\frac{\partial yb}{\partial lb} > 0$. Whether $\frac{\partial yb}{\partial lb}$ will become zero, and be allowed to become negative is a debatable point. The debate is the familiar 'surplus labour' controversy. We are not interested in the details of the debate,[18] but it may be pointed out that in our analysis it is irrelevant whether $\frac{\partial yb}{\partial lb}$ does or not become zero. Our analysis will hold good as long as $\frac{\partial yb}{\partial lb} < w^*$. Whether $\frac{\partial yb}{\partial lb}$ becomes zero or not depends partly on technological conditions, but partly also on the marginal disutility function of the B-Households, because their out-put decision depends not on equating $\frac{\partial yb}{\partial lb}$ and the wage rate, but $\frac{\partial yb}{\partial lb}$ and the marginal disutility of work.

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[17] The perfectly elastic supply curve of labour plays a crucial role in the Lewis Model, and in G. Ranis and J.C.H. Fei, "A Theory of Economic Development", *The American Economic Review*, September 1961, pp. 533—65.

[18] For a survey of the literature on this subject see Morton Paglin, "Surplus Agricultural Labour and Development", *The American Economic Review*, September 1965 pp. 815—34. Paglin shows that the marginal productivity of labour in India is not, or cannot be zero.

This leads us to a further feature of the system. Granted that the B-Households do not confront factor markets (or at best confront only limited factor market opportunities) the allocation of resources *within* each B-Household can be appreciated only in terms of each household's utility function. For instance, each household will have its own estimate of the 'real cost' of its Labour, even when it may appear that the 'opportunity cost' of its Labour may be close to zero. In fact this is implied in the $\propto$ coefficient. The discrepancy between 'real cost' and 'opportunity cost' is a feature of our system arising from limited market opportunities, and allocation of resources in the B-Sector will be governed by the former. It means that attempts to 'impute' resource prices in the B-Sector on the basis of equilibrium (shadow) prices of resources in the economy as a whole are little more than irrelevant exercises in logic and algebra.

The subjective nature of the 'real costs' in the B-Sector also implies that each B-Household will have its own 'technique' of producing y. The choice of technique most appropriate in terms of its own factor endowments is certainly rational procedure on the part of each household, however 'irrational' the coexistence of a spectrum of techniques for the production of the same commodity may appear from the point of view of the economy as a whole. This is just one case where what is 'rational' in terms of one system may appear to be 'irrational' in terms of another.

Further, the fact that the allocation of resources in the B-Sector is based on subjective real cost calculations also indicates that output in the B-Sector can vary significantly even where the technology and the endowment of resources are 'given'. The increase in numbers in a B-Household, for example may reduce its estimation of the 'real cost' of labour. Consequently it may adopt a more labour-intensive technique, and thus increase output.[19]

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[19] The argument here is analogous to saying that if the wage rate goes down, a more labour-intensive technique will be adopted.

The picture that emerges is very different from the theoretically familiar competitive system. Most of the characteristics of the new system discussed above are empirically familiar problems, well known, and much discussed in studies on 'overpopulated underdeveloped economies'. But the general tendency has been to dismiss them as non-theoretical problems which call for remedial practical solutions, but which necessitate hardly any theoretical analysis. What we have attempted is to set up a system, from which these characteristics are derived by *a priori* reasoning. And it is worth repeating that in the construction of the system we have not resorted to any 'non-economic' variable, or any unusual assumptions, and that in fact except with reference to the level and distribution of resources in the economy *all* our assumptions are the same as in the competitive system used in Section III.

In the analysis, so far, we have not said anything about the market for Property. If the C-Sector is eager to hire Labour, the B and A-Sectors may wish to hire Property. The analysis can be modified to take this possibility into account in which case some of the details of the system described will undergo change. However, the system we have set up *cannot* be converted into a competitive system by introducing a market for Property, because the new system has been generated by removing one of the major postulates of the competitive system. It is readily granted also that many features of the new system remain unexplored. And yet we have established that the allocation problem in an 'overpopulated underdeveloped economy' is significantly different from that in a competitive economy, and that the mere adherence to the rules of the game of the latter will not suffice to achieve an 'optimal allocation of resources' in the former.

# VI

It is legitimate to ask whether a static study of the problems of resource allocation is the most fruitful approach to an understanding of the problems of underdeveloped economies. A

theme that frequently appears in the study of economic development is that the static analysis of allocation must be rejected in favour of the dynamics of growth. But I have concentrated on the static problem on the belief that unless we understand the state of underdevelopment as such, we are not likely to appreciate the problems of the growth of an underdeveloped economy. The study of the economic development of underdeveloped countries has suffered greatly by too hasty generalisations and by too uncritical acceptance of 'growth theories'. The only effective way to protest against such facile approaches is to show, as rigorously as one can, that underdeveloped economies are significantly different from developed economies in their very structure, and not merely in the level of their *per capita* income.

The model that has been set up, therefore, has little to say directly about the possibilities of growth. But it has certain implications regarding growth.

The model is optimistic about growth in so far as it can show that the economy described *can* grow. Since the model makes no extraordinary Motivational and Technological assumptions, the economy can grow without any change in human motivations or any radical technological innovations. Even in a limited sense the economy has a growth potential since $\frac{Y}{L} > W^*$, meaning that the output of the economy is greater than what is absolutely necessary for subsistence.

On the other hand, the optimism of the model is a guarded optimism, because it provides for no automatic mechanism of growth. In this respect it stands in sharp contrast to the usual growth-oriented models of underdeveloped economies, Arthur Lewis's celebrated 'classical' model, for instance.[20] In the Lewis dual economy model growth is 'automatically' achieved by identifying one sector of the economy (the subsistence sector) with consumption, and the other sector (the capitalist sector)

[20] As cited in foot note 4.

with savings and investment.[21] The assumption of the model, therefore, is that one sector *cannot* save, and the other sector *cannot but* save, and on this assumption, the growth of the capitalist sector *is* the growth of the economy. Hence once the distribution of income between the two sectors is determined (and it is determined by appropriate assumptions) the growth rate of the economy also is determined.

In the model that I have discussed, on the contrary, there is neither automatic growth, nor is the growth rate determinate.[22] In the model the *necessary* condition for growth is satisfied, but not the *sufficient* condition. The necessary condition, of course, is that per capita *income* is higher than subsistence level ; the sufficient condition is that per capita *consumption* is less than

per capita income. If actual per capita income is $\dfrac{Y}{L}$, and actual

per capita consumption is $\dfrac{C}{L}$, the necessary condition for growth

is

$$\frac{Y}{L} > W^*$$

and the sufficient condition is 

$$\frac{Y}{L} > \frac{C}{L}$$

Combining the two, we may say that an economy will grow only if 

$$\frac{Y}{L} > \frac{C}{L} > W^*$$

In the Lewis model and similar 'classical' models

$$\frac{C}{L} \equiv W^* \qquad \text{(by construction)}$$

and hence if $\dfrac{Y}{L} > W^*$, then $\dfrac{Y}{L} > \dfrac{C}{L}$ and so both conditions are simultaneously satisfied by construction.

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[21] The Lewis model 'automatically' ensures growth as the perfectly competitive model 'automatically' ensures the optimal allocation of resources. See the discussion in the Second Lecture, Section II.

[22] This is not surprising because the model is a static allocation model.

In the present model, on the other hand, there is no discussion about consumption behaviour. And there is nothing to suggest that the consumption behaviour in the kind of economy we have dealt with will be as in the classical system or in the Keynesian system. A relevant consumption function can be formulated only after detailed empirical studies.

But there are two inferences about savings that can be drawn from the model. In the first place, the larger the A-Sector, the more difficult will it be for the economy to grow as it is a sector that cannot save at all. In fact a very large A-Sector may be an open invitation to Malthusian miseries as has already been indicated.

Secondly, in the model both the B-Sector and the C-Sector *can* grow. And because of the differences in the nature of the two sectors, it is possible also that the determinants of savings in the two sectors may not be the same. This can have some bearing on growth policy. Since growth can take place in both the B-Sector and the C-Sector, growth policy must be such as not to affect growth in either sector. It is here that the Lewis-type models are poor guides to policy for the development of overpopulated underdeveloped economies. To take but one example, in the Lewis model since the growth of the capitalist sector corresponds to the growth of the economy, and since inflation contributes to the growth of the capitalist sector, inflation is recommended as a device for growth. But in our model, even granting that inflation may be a stimulant to the C-Sector, it is doubtful if it will encourage savings in the B-Sector. It is even likely that some policy measures that encourage savings in one of the sectors may retard savings in the other.

All these aspects call for further studies—theoretical and empirical. The purpose of our experiment has not been to discover all the properties of the kind of economic system that we have confronted. Many more experiments will have to be conducted before we begin to have a fairly coherent picture of such a system.

My attempt in these lectures has been to establish that many pressing practical problems may remain unsolved and partially understood till they are illumined by theoretical insights, and that substantial reformulation of theory may be necessary before theory can enlighten practical problems. In a rigorous study of the economics of underdeveloped countries we may discover the underdeveloped state of the science of economics also !

# INDEX