CAUSALITY IN ECONOMICS

by

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To Janet, and to the memory of Antiphon who: "... when he was a youth ... studied with great care; though now he devotes most of his time to horses ..." (Plato p.201)
In prosecuting the science of Economics most economists make several assertions about the nature of causality. These assertions lead to at least difficulties over the assimilation of the nature of man with the nature of the economic scientist. The following is an attempt to overcome these difficulties.

We begin by a 'content' analysis of the existing theories of causality which leads us to assert that a causal statement makes a proposition about a relationship in the world whose form is mirrored in the form of the statement itself. The description of the formal properties of such a statement is facilitated by our finding reason to doubt the existence of a consistent logic in which causality can be represented. This leads us to argue that a causal statement is formed in a two-part language.

The concept of causality so implied is quite unlike previously proposed theories for it is understandable only in terms of a mutual dependence between cause and effect with each causal relation being one in a continuum of such dependence. Furthermore, in situations of system and/or observer alteration only such statements which are, by our definitions, causal can be formed.

Now, to change a theory of causality is to compel a change in the theory of action that is based upon it. Hence, we construct a different theory of action based upon the Sartrian concept of freedom. Such construction is facilitated by the criticisms of ends/means theories of actions in general and of the theory of economic policy in particular that we have made. We find that several applications of our theory of action in the science of Economics may be possible, notably in research into the link between 'micro' and 'macro' phenomena.
INTRODUCTION

This thesis was not written as a crusading tract but is a result of certain ruminations on a selection of connected themes. The reason for the study was my perplexity over certain propositions in Economics and my desire to dispel this perplexity. It may be that our efforts merely result in our perplexity being transferred to the minds of our readers. We hope, however, to show that such solutions we have achieved are useful in the science of Economics.

We shall, in what follows, only examine economic analysis in the sense of the so-called 'liberal' Economists surveyed since David Hume in Great Britain and America. No attempt at all was made to analyse the 'historical' school of social science. The reason for this omission was largely my concern to clarify the theory of economic policy, that is commonly employed in the design of economic policies in the so-called Western democracies. Although we have not been able to investigate the question in detail, however, it seems that our results are, in the main, applicable to the more historically-based social sciences.

Our method of study is best described as 'exploratory'. Rather than trying to justify a certain interpretation of the science of Economics by reference to a textual analysis of the subject, we have attempted a different technique. We have sought to set up refutable hypotheses about the nature of Economics and to attempt refutations on the basis of textual quotations. In this way we have tried to study Economics 'as it is'. Furthermore, we have tried, wherever possible, to demonstrate that the pre-suppositions of Economics are derived from a certain philosophical standpoint. This, we feel, is
a course of research more likely to find out the nature of economic argument than to provide another interpretation based upon the enumeration of supporting quotations. If we were to provide such an interpretation, no doubt another could be set up to oppose it and little would be forthcoming that could be used by the science in its purpose of finding explanations of economic phenomena.

Two words of warning may be in order at this point. First, one cannot read this thesis for fixed and definite conclusions and find them. Such conclusions as have been derived must be subjected to rigorous checking by other researchers. It is hoped we have mentioned in the text those parts of the thesis which seem most open to doubt. We would emphasise that much of what follows is still in its infancy.

Second, the reader will find some use made of symbolic logic in the exposition of the argument. No doubt the argument could be reworked into another type of language. We would, however, point out that a knowledge of the rudiments of symbolic logic on the part of the reader has been assumed.

All that remains is to thank my wife, Janet, for suffering the insufferable, my supervisor, Ruth Troeller, for help of every description, the University of Surrey for finance and my friends, John Burton and Tom Loch for a continual supply of stimulating ideas. Without any one of those great assistances, the following would not have been written. I would also thank C.I. Docker and J.D. Watson for their respective books which gave me great pleasure and influenced greatly my thinking.

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On the Subject Matter of Economics

Economics considers man as an agent. Man is seen as an animal that is able to act, i.e., perform actions, that conform more or less to a motivation that is purely economic. We shall not here be considering the definition of ‘economic’. In the following the term will be taken as primitive and so left undefined. Since, too, we mean to conduct a formal analysis of the assumptions of economics we shall not enquire into the nature of economics, except insofar as such an enquiry can help us reveal some of the more deep-lying assumptions of the science of economics.

The concept of the nature of man as an economic agent has changed only throughout the history of the subject in that the concept has been more and more explicitly introduced into economic analysis. As we shall see, prescr
day formulations of the concept are usually in the form of precise statements rather than vague historical generalities. I believe, however, that we shall find the essence of the concept has, broadly, remain unchanged.

Let us peruse the literature on the subject starting with the 'first economist' Adam Smith. He writes:

"This division of labour . . . is the necessary . . . consequence of a certain propensity in human nature which has in view no extensive utility: the propensity to truck, barter and exchange one thing for another."

(Smith Vol. I. P.12).

Hence we are told that in history mankind has engaged in market activities and that presumably that this is common to different nations at different times. As a statement it is clearly refutable. If accepted as valid, it is also a reason for studying how and why these activities are carried on.

Now all economists extol the virtues of economics as a means of understanding 'reality'. It, therefore, follows that they believe what they analyze is, in some sense, part of reality. Thus we must conclude that insofar as economists study 'the propensity to truck, barter and exchange one thing for another' this propensity is real.

It remains unanswered whether the assumption of this reality is limited to a particular social space. Let us, therefore, make some alternative assumptions about these limitations, deduce conclusions of them and then find these limitations which the science of economics asserts.

First let there be no limitations. The propensity is true for all states of mankind in all space/time relations. Thus it is assumed the concept of exchange is applicable to every conceivable attitude into which mankind could conceivably be placed. More specifically, no limitations imply the set of social actions contain no elements which cannot be
called by the name 'exchange'. Yet we read:

"... a society needs more than a market place ..."  
(Stigler P.96)

and

"We each belong to many circles: the United States, the  
Elks, the Samuelson family, the office pool etc. In  
almost none of these relationships is the organising  
principle that of decentralised competitive pricing."  
(Samuelson P. 1425).

Thus we must conclude our hypothesis is refuted. There are limitations  
in that not all social relations are economic ones.

It seems too that, judging by the quotations above, they arise from  
the state of mankind rather than from a particular space/time relation.

Secondly, then, let our propensity apply to a certain state of mankind  
at any particular time. Thus we argue the set of social actions contains  
a non-empty sub-set of actions that can be called 'exchange', at any given  
space/time. We find this supported:

"... the mechanism of the market, the central phenomenon of  
the industrial world ..." (Wicksteed P.13)

or

"Questions directed primarily to maintaining employment are posed  
so infrequently, and are even then so completely dominated by  
allocative questions that for practical purposes they are an  
ininfrequent and minor problem for the citizen." (Stigler P.82).

Thus the set of social actions contains such a sub-set. Two interesting  
questions are raised. First, how big is the complement of the sub-set?  
Secondly, to what extent is it associated with one conception of society?

In answer to our first query we read not only the two immediately
"... the two great forming agencies of the world’s history have been the religious and the economic ... Religious motives are more intense than economic, but their direct action seldom extends over so large a part of life."

(Marshall P.1)

which states generally 'economics matters' yet is supported by:

"Competition ... is only a secondary, and one might almost say an accidental consequence from the fundamental characteristics of modern industrial life". (Marshall P.4).

In other words, economic actions are not the only but are the most important type of relation in society. From this we can form a hypothesis which may help us to answer our second enquiry. We assert that the validity of the propensity to which we referred at the head of this section holds only for an industrial society but is independent of any space/time relations. But before this can be examined we must define more closely 'industrial society'.

We begin this task by examining the hypothesis that: the validity of the propensity 'to trade, barter and exchange' holds only for an 'industrial' society but is independent of any space/time relations. Followers of the historical method will dispute the last clause but since we have excluded them their objections will be ignored.4

As it stands, our hypothesis is not meaningful insofar as the term 'industrial' is not defined. Obviously, it must apply to those countries wherein economists are studying like 'the Western society', but does it include so-called Socialist states or Primitive Agrarian societies? The answer can be sometimes, never or always, or in some places, in no place or in every place or any combination of the six. Hence we have superseded our original hypothesis with another, namely the propensity is true for all times and all places.

At first we can rule out three alternatives simply by noting that
there exists some study of Soviet and Agrarian economies in the light of this propensity. Thus we can say it in not so that the propensity is believed to be untrue for all times and all places.

So what of our original hypothesis? That it is refuted is seen by:

"... there is in fact little contact between the tenets of the economics studied in the Western world and that of the Iron Curtain nations ..." (Samuelson, p. 163)

or

"The 'state-as-end' economist ... sees two states [engaging in International Trade]. He sees therefore a political act. The act may be quite routine and uninteresting and may have purely economic ends. But it is still the act of a sovereign power, and non-economic considerations always come in." (Wiles, p. 4).

Thus we must conclude that the propensity is true only in some times in a socialist system. But what of an agrarian economy?

We read:

"... to cultivate the ground was the original distinction of man, so in every stage of his existence he seems to retain a predilection for this primitive employment ..." (Smith, Vol I, p. 333)

or

"... the opportunity to exchange goods in the world market ... enables the developing country ..." (Johnson, p. 262).

Thus it seems our propensity is present in all mankind to some degree. In some cases it is more constrained than in others. We therefore have to conceive of a given social act which includes actions by one person that bear upon the actions of others. Thus we see that the above-mentioned propensity is seen to apply in all places at least some of the time. Whether the applicability varies over time is not discussed.
How we have some understanding of the extent to which economic man exists, let us again dip into Adam Smith to find what he felt the nature of this animal—economic man—was. He writes:

"It is not from the benevolence of the butcher, the brewer, or the baker that we expect our dinner, but from their regard to their own interest. We address ourselves, not to their humanity but to their self-love, and never talk to them of our own necessities but of their advantages."


Thus man acts according only to his calculation of self-interest. I shall not discuss the prejorative overtones of such a statement but frame the hypothesis that in assessing the benefits of a certain action a man does not consider the benefits accruing to others.

In contradiction to this we read:

"Externality has been . . . central to the neo-classical critique of market organisation . . ..

". . . We define an external effect, an externality, to be present when \[ U^A = U^A(X_1, X_2, \ldots, X_m, Y) \]

This states that the utility of an individual, \( A \), is dependent upon the 'activities' \( (X_1, X_2, \ldots, X_m) \), that are exclusively under his own control or authority, but also upon another single activity, \( Y \), which is by definition under the control of a second individual, \( B \), who is presumed to be a member of the same group." (Buchanan and Stubblebine pp. 199-200).

Thus our hypothesis is refuted. But it remains that unless an individual can control some activities there is no meaning attached to the term individual utility. Hence, we must conclude for him to have a utility function economic man must control at least one activity.

Still, however, there remains a problem. Why does he wish to control any activities anyway? An answer has only been made in terms of social
psychology. Thus we have the following answers to this question:

"The ideal Consumer ... chooses that alternative, out of the various alternatives open to him, which he most prefers, or ranks most highly." (Ricks p.10)

and

"Economic man as a hypothesis ... can be said to 'assume' single-minded pursuit of pecuniary self-interest by employers in competitive industries; and this 'assumption' works well in a wide variety of hypothesis in economics bearing on many of the more phenomena with which economics deals." (Friedman p.29)

and

"All that follows shall relate to an idealised individual ... I assume in the beginning as known, i.e. empirically determinable under ideal conditions, the amounts of a economic goods which will be purchased per unit time by an individual faced with the prices of those goods and with a given total expenditure ..."

"... Thus, confronted with a given set of prices and with a given income, our idealised individual will always choose the same set of goods ...

"... if an individual selects batch one over batch two, he does not at the same time select two over one ..." (Samuelson pp. 4-7).

All three answers to our original enquiry are couched in the same terms. They speak of an individual's desire for his own goods, benevolence nowhere in the reasoning is the concept of behaviour or 'social good' introduced except as an exception, e.g. as in externality theory. Yet we have already refuted the hypothesis that man is assumed to be completely selfish, in that he cannot conceive of this state. As a counterpoint to this view we learn...
"History in general, and especially the history of socialistic ventures shows men are seldom capable of pure ideal altruism for any considerable time together." (Marshall P.7).

Still, however, we have simply shifted the problem back a stage. We still do not know why men hold the kinds of values that they do. This implies two questions. What are the values and why are they held? First we have the concept implied that to have more wealth than less is a 'good thing'. Hence, we could argue that more wealth rather than less is a 'good thing' rather than a 'bad'. In Smith, however, we find:

"Quality ... is so very disputable a matter that I look upon all information of this kind as somewhat uncertain." (Smith Vol. I. P. 225).

The quality of wealth, then, does not enter into Economics, it is given. Hence, we come to the proposition that given that qualities do not change more wealth is better than less.

Yet this begs the whole question of value. We are arguing in fact that provided the nature of wealth does not change more wealth is better than less, yet this leaves completely unexplained why this is so. It is merely an assertion that the value attached to wealth bundles increases as the size of those bundles grows; in this form it is simply a means of obtaining an indication of value. (No answer to our second question is attempted.)

As such, it was used by Marshall who wrote:

"The measurement of motive thus obtained is not ... perfectly accurate; for if it were economics would rank with the most advanced of the physical sciences; and not, as it actually does, with the least advanced."
"But yet the measure, is accurate enough to enable experienced persons to forecast fairly well the extent of the results that will follow from changes in which motives of this kind are chiefly concerned." (Marshall p.21).

The motives themselves are of little concern to economists. An economist is concerned with the effects of these motives in a large sub-set of the social state. It is thus claimed that economics is value-free. It is not tied to a particular value system. Such a claim, as we have seen, is based upon the validity of question begging on this matter; or rather, on the fact that more being better than less is a statement of such universal acceptance that all value systems agree with it. Yet the whole point about values is that they assess quality. Thus the question of value-free generalisations must mean either that the assessment of quality is not an important explanatory device or that economics implies no particular value system. We shall look at these in turn.

First, that value has been placed at the very centre of economists' explanations of actions hardly supports our first alternative. As we shall see in our analysis of action, quality assessment is an important factor in explaining certain actions. Hence, value-free cannot mean this.

Second, recent work on the theory of value, however, has made explicit the motives of an individual acting in society. Let us, therefore, analyse the behavioural assumptions of the model to see if any clue is given as to the value system of the individual. To read:

"The ideal consumer . . . chooses that alternative, out of the various alternatives open to him, which he most prefers, or ranks most highly." (Hicks p.10)

and

". . . if an individual selects batch one over batch two, he
does not at the same time select two over one . . . .”
(Carnap, p. 7).

Now it is clear neither state a system of value for the individual. All that is explicit in a rule which governs the way in which the values are applied to things, i.e. the consistency axiom. We believe, however, there is implied an assumption about value which has again been swept out of view.

In the above two quotations we read “selects batch one over batch two” and “chooses that alternative out of the various alternatives open to him, which he most prefers.” A confusion is immediately apparent. The value of a bundle is synonymous with the action of acquiring or preferring it. Now these are separate.

The separation is seen if one conceives of action performed by a free individual as a resultant of a motive (e.g. to perform the best possible) and a set of values (e.g. an assessment of the best possible). It may be that an individual has no motive at all. In which case the value of various alternatives are not translated into ‘choice’ or ‘preference’ or ‘selection’. Such actions that are then performed are completely random and without reason. Thus economics assumes the motives of the individual are related exactly to a value structure. Given this assumption, we can regard the motives of individuals as an indication of the value system of those individuals.

From the two quotations given immediately above, greater value implies ‘preference’ or ‘selection’ of one alternative over another. Hence better or worse can only be assessed in terms of something else. We may say concerts are better than football matches but this does not enable us to say concerts are the best thing in the world, if the world contains other things.

Now this view of value is particular. It claims that sensation is at the base of all values. The crude form of this view was expressed by
Bentham in terms of his calculus of pleasure whereas more recent thinkers
have emphasized the importance of time preference especially in investment
and production theory. The principle remains the same, however, that
actions are performed because of the 'benefit' that accrues to the performer.
Whether benefit now is exchanged for benefit later does not disturb the
point at hand. The valuation of alternative acts is based upon a sensation
of benefit; there are no values without sensation.

From the above we can make two conclusions before proceeding to our
next enquiry. First, if values are based upon sensations then unless one is
to hold one theory of knowledge for actors and another for scientists then
one must also hold that science proceeds to gain knowledge on the basis of
empirically given phenomena. Now this immediately implies that if economics
is to consistently keep its theory of value collection it must accept the
arguments of those philosophers who have rejected the possibility that non-
empirically based ideas are meaningful. One is thus enforced to accept
ideas of the world that are consistent with this view and so take on a
particular brand of metaphysics.

Second, because as Berkeley showed sensation is relative to the
individual in the broadest sense, all values are given by individuals and
are completely relativistic. Hence there can be no values given by God,
the Good, the Ideal or even the State. Neither can there be values held
to be absolute.

This, however, does not describe the extent of these so-called selfish
acts. For this we conclude that man as studied by economics is incapable
of pure altruism yet constrained in his desire to be selfish. Man's set
of actions is, therefore, strongly associated with a motive that is purely
selfish. We should predict strong positive correlations between the two.

I am not sure if this conclusion can be refuted in the writings on
Economics. All that I can comment is that I have not been able to find
such a refutation and that this conclusion seems to follow directly from
the argument so far presented. Since, however, the above conclusion does not seem vital for the argument that follows I propose to assert it and leave it to others to clarify its validity more precisely than I have been able to.

Having said that, however, several questions arise as to the form this selfishness takes. For instance, do men become less selfish when the objects of their desire are not immediately to hand? Or is their selfishness related to society? To throw light on these matters let us set up the following hypothesis: The less 'tangible' the objects an individual desires the less selfish he becomes. We read:

"The overweening conceit which the greater part of men have of their own abilities is an ancient evil remarked by the philosophers and moralists of all ages." (Smith \(\text{I}\) Vol. I. pp. 95–6)

and

"Such in reality is the absurd confidence which almost all men have in their own good fortune that, whenever there is the least probability of success, too great a share of it is apt to go to them of its own accord." (Smith \(\text{I}\) Vol. II. P.61)

or

"... individuals seek to maximise expected utility ..." (Friedman and Savage P.96)

or

"An action, such as the choice of a portfolio, which must be undertaken without knowledge of its consequences may be said to involve risk. In such a situation it is reasonable to assume that the individual acts on the basis of his expectations."
It seems likely, furthermore . . . that the individual conceives of several possible consequences of his action, none of which may be more likely than others." (Leeper, pp. 58-9).

It seems not to be so. Thus no matter what the character of the goods he is buying our individual remains constantly selfish.

So let us move to another question. Is this level of selfishness related to the society in which he acts? Thus, let us test the following hypothesis: The sub-set of selfish actions is changed if the set called the social space is changed. In refutation we read:

"All for ourselves and nothing for other people, seems, in every age of the world, to have been the vile maxim of the masters of mankind." (Smith, Vol. I, P. 366).

Also

"Money can be liberating. It corrades the cake of custom. Money does talk. Sociologists know that replacing the rule of status by the rule of contract loses something in warmth; it also gets rid of some of the bad fire of olden times." (Samuelson, P. 1412).

Thus, it is society that is changed if the sub-set of selfish actions is changed. Often, too, we read that the market can be used by socialist societies to allocate goods more efficiently, or that the market is not 'ethical'. Such assertions imply that insofar as the market depends upon men's selfishness than that selfishness is common to all men at all times in all social space.

Yet we have already read in Marshall (see P. 4) how religious motives may be more intense but less general than economic ones. Hence we set up another hypothesis the complement of the sub-set of selfish actions in the social space is sometimes, but not always, more important than the
This hypothesis is not directly testable, to our knowledge. Thus, we must attempt an indirect refutation. If it were so, then the statement of market behaviour would not apply to its presumed social space at least part of the time. Hence economics must be by its own assumption inappropriate at least part of the time. Thus, we frame a subsidiary hypothesis: It is supposed that the behaviour predicted by economics will occur in the future and always will. In refutation we read:

"Political Economy . . . "does not treat of the whole of man's nature as modified by the social state, nor of the whole conduct of man in society . . ." (Mill [17 P. 137)

and

"The political economist enquires what are the actions which would be produced by this desire [i.e., for wealth] if, within the departments in question, it were unimpeded by any other. In this way, a nearer approximation is obtained than would otherwise be practicable, to the real order of human affairs in those departments." (Mill [17 pp. 139-140)

or

"The fact that nothing is perfectly accurate should not be an excuse to relax our standards of the empirical validity that the propositions in economics do or do not express." (Samuelson P. 1779)

or

"Before any worthwhile decision can be reached about any particular economic policy in the real world institutional, historical and statistical investigation of the actual situation is an essential complement to the general education in economic principles . . ." (Kendel P. 14).
From this we must conclude that certain of man's actions are non-economic. Alternatively, this can be stated as follows. Some of the actions which economics recognises as actions in the sense of activities performed according to a rational assessment of values in the light of alternative sources of action cannot be explained by the sort of arguments with which we have so far been dealing. Presumably this must mean that certain 'performances' of man in society are not a rational response to values. In other words, the concept of the individual constructed in economic theory is assumed not to provide a completely general explanation of mankind's activities.

Moreover, the reasons for this are unclear. Does the assumption arise because certain of mankind's activities are not actions in the above sense? Or, if not, are they irrational actions? Further, why is it that if economics has no general explanation of man's social acts has not a more general explanation than that obtaining at present been forthcoming? Answers to these questions must, I fear, be the subject of future work and cannot be considered, for all their interest, here.

That we have an individual acting selfishly within certain constraints, this behaviour once introduced into society results in a given set of institutions. So says economics. Sometimes reality appears to behave just as economics says it will but at other times it will not. Now obviously coercion cannot explain a change in the degree of selfishness - only a change in the individual's altruism can do that. Yet if our individuals are selfish there can be no reason for this change internally. Thus errors from this kind of source cannot be explained within economics; they are not introduced as a variable and hence cannot enter our discussion.

Should error, however, be attributed to 'extra-economic' factors then a new hypothesis can be deduced from assumptions and axioms which explicitly include these factors. Only then can the phenomenon be adequately 'explained'.
Thus, we find that selfishness is capable, in certain circumstances, of altering the social space of which it is part but not in others.

We come now to a consideration of the constraints whereby society or other agents can influence the actions of an individual. We shall deal first with those actions by people to people which are a concomitant of their living in a social state. These may be termed externalities. In sketching the form of these we shall not attempt a complete catalogue but merely talk of two types which have general currency.

First we shall identify the 'coercive type':

"The pride of man makes him love to dominate and nothing mortifies him so much as to be obliged to condescend to persuade his inferiors." (Smith Vol. I, p. 345).

We have already seen this defined in Buchanan and Stubblebine on page 6 but we may also read:

"Little room is left for doubt about the extent to which tastes . . . are moulded by social forces." (de V. Graaff, p. 44).

It should be noted that the externalities of Smith are not precisely the same as those of our later examples. We would contend, however, that the distinction between the coercive type and our next category is accepted throughout the history of economics.

Second, the 'social type':

"Wealth . . . is power . . . . . . The power that possession immediately and directly conveys to him is the power of purchasing: a certain command over all the labour or over all the produce of labour which is then in the market." (Smith Vol. I, pp. 26-27)
"... what is important about it [i.e. the doctrine of the Invisible Hand] is the system of checks and balances that comes under perfect competition ..." (Samuelson, p. 1410).

This second type of externality obviously is akin to the concept described by Mill (17) (p. 135):

"... there are certain principles of human nature which are peculiarly connected with the ideas and feelings generated in men by living in a state of society ..."

Thus analysis of society is based entirely upon an analysis of social external effects. How the set of all effects bearing upon an individual is composed of two sub-sets — one containing coercive and the other social effects. Yet to what extent this division is real one and to what extent the division is changed by a different social order remains to be discovered by further research.

Let us first, however, try to analyze this society a little more.

We read on:

"The state or sovereignty in which we have been born and educated, and under the protection of which we continue to live in, in ordinary cases, the greatest society upon whose happiness or misery our good and bad conduct can have much influence. It in, accordingly, by nature, most strongly recommended to us." (Smith 27, p. 95)

and

"The same principles that direct the order in which individuals are recommended to our beneficence direct that likewise in which societies are recommended to it." (Smith 27, p. 95).

It would seem that the society in which an individual lives and under
whose social constraints he is selfish must be the one in which he obtains the greatest reward for his being there. From this we could set up a hypothesis that it is held all individuals live in the societies which they most prefer.

I am not sure if such an assertion has been refuted. Presumably in view of the discussion on factor-price equalization and on migration (e.g. the so-called 'Brain Drain'), our hypothesis stands as unfounded. However, since economic analysis assumes that choosing between alternatives is a costly operation we should suppose a person will only go about finding the most preferred country until £z = £z. Since such a decision is based upon 'lumpy' cost and revenue curves we could argue the person is in a worse than optimal situation.

The nature of the societies between which the individual chooses does, however, have certain general characteristics. Already we have seen that they must permit man to act socially on the lines described above. We have also seen that it is 'industrial' to a greater or lesser extent.

That of social action then? We cannot hold a concept of action unless individuals can control certain activities - this we have already seen. It is also clear that individuals cannot be purely selfish unless the results of these activities accrue solely to these same individuals. Thus, let us examine the hypothesis that the results of certain activities never accrue to the individuals that control these activities. We find this refuted:

"All real economic systems contain some monopoly, and hence some coercive power for particular individuals . . ."

(Stigler, pp. 67-69).

To some extent, then, individuals receive the results of their actions.
It would be consistent with what we have learned above that the extent to which this is not so is limited by the extent of external effects borne upon these actions. As a corollary we can conclude that individuals can stake a property right on certain results of their actions. Hence we have a system of individual property ownership.

In a sense we are examining a system of exchange in individual property rights. We read in Smith (1) Vol. I. P.156 of "... the sacred rights of private property ..." and again in Smith (1) Vol. II. P.79:

"To prohibit a great people ... from making all that they can of every part of their own produce, or from employing their stock and industry in the way that they judge most advantageous to themselves, is a manifest violation of the most sacred rights of mankind."

The system of exchange is, at best, voluntary. Hence, we set up the hypothesis: An individual is compelled to engage in the exchange of property rights. We find this refuted:

"An enterprise system is a system of voluntary contract. Neither fraud nor coercion is within the ethics of the market system." (Stigler, P.67)

or

"The notion is repellant that a man should be able to tyrannize over others." (Scialloco, P. 1414),

or

"Any citizen is permitted to undertake on his own private enterprise the production of any product." (Waado, P. 26).
Having considered the nature of value-systems and of social systems considered by economics, we shall now move on to a consideration of the nature of economic actions. It must be emphasised, however, that all three considerations are intimately interlinked to the extent that any categorisation is bound to lead to certain 'trivestrics'. We hope that the most obvious of these have been avoided.

In the world we have induced above, there can be no apriori imperative statements. If there were, then a particular action would always be better than any other. Let us think of alternative actions as comprising the sum of two sets: conceivable actions and those empirically given. An action chosen from this set on the basis of values will be the best. It will hold an absolute value. If, however, we exclude the conceivable action set, then we can only have relativistic values. (Yet it still remains that there is always a conceivable action which is better than those empirically given but which is impossible since we have excluded it from our discussion of value). Thus, there can be no general precepts for action to be deduced from economics provided only empirically given actions are the object of valuation.

Now if economics is trying to analyse reality, the conceptual world of which it talks, that we have been describing until this point, must bear some relation to the reality it is trying to analyse. It is the same with our conceptual actors. They must not only hold the values and motives specified above but also know that they are relevant to the world the actors are facing and that they can be carried out. If this latter knowledge is missing, the combination of motive and value into action will not, if our individuals are as we describe them, transform itself into action.

This brings us into a discussion of knowledge. Now provided the formation of knowledge is costless and can be done in complete certainty
it is clear our individual will, conceptually, combine motive and value to 'produce' action. But if the formation of knowledge is not costless there will be an optimal level of knowledge for him and motive and value will not uniquely 'produce' a given action.

We are thus forced to argue that in using the traditional concept of an individual economics believes that individuals know that their values and motives are a sufficient guide to the formation of selfish actions. How this implies that the individual conceives that actions prescribed by the combination of motive and value are real in the sense that they are reflected in 'the real world'. In the simplest economics this reflection is one-to-one (see Scolding).

All this can be combined into a somewhat paradoxical form. Let us imagine our selfish individual has set up his system of values and begins to act. He encounters a possibility. He must now check to see if his value system includes this possibility, how it rates the possibility and whether the system is still relevant. As a result, he tests the system. Unless he knows everything about the system and the world, he cannot know if the test has given him a valid result. He will, therefore, test the test and so on. An infinite regress ensues because we assert he cannot know everything. Instead, therefore, of acting as if he were certain of these factors he will act as if the probability of this knowledge being right were greater than the probability of this knowledge being wrong.

In more particular terms this has been considered by some in the problem of feedback as it affects decision-making in economics. Simply stated this means that the knowledge needed for decision-making is given in part by economics, but each decision is part of that economic knowledge. Hence, to make a decision is to alter the system upon which the next system is based.

All the above discussion, however, presupposes that an individual is
Already we have seen that the economic system itself limits the possibilities to which he may react. Hence, our actor is free to accept the particular society or not but once he has accepted it, he is free to 'pursue his own interest'. But we have seen too, that there are situations in which the interests of two individuals may conflict.

Smith writes:

"... those exertions of the natural liberty of a few individuals, which might endanger the security of the whole society, are, and ought to be, restrained by the laws of all governments..." (Smith [1] Vol. I. P. 209)

and

"To hurt in any degree the interest of any order of citizens, for no other purpose but to promote that of some other, is evidently contrary to that justice and equality of treatment which the sovereign owes to all the different orders of his subjects." (Smith [1] Vol. II P. 149)

and

"The statesman who should attempt to direct private people in what manner they ought to employ their capitals would not only load himself with a most unnecessary attention, but assume an authority which could safely be trusted, not only to no single person, but to no council... and which would nowhere be so dangerous as in the hands of a man who had the folly and presumption enough to fancy himself fit to exercise it." (Smith [1] Vol. I pp. 400-401).

The government is thus to look to national security (and presumably other public goods), and to keep entirely out of the workings of society except perhaps to ameliorate the worst excesses of selfish behaviour. I believe this is an adequate summary of so-called liberal economics, which I shall
not illustrate.

It is important to realize, however, that this is not simply a belief for Smith but a description of an actual, if idealized, state of affairs. For our purposes, then, it can serve to illustrate the freedom an individual has in our conceptual society.

As we have seen, a person who cannot link his motive/value complex to his actions may be so unable because he is (1) insane or (2) constrained by (a) society or (b) coercion. We can assume (1) to be deviant behaviour and therefore not general. (2) (a) can also be left aside since such limitations to freedom are accepted by means of the implied social contract. We are thus left with (2) (b).

In our previous discussion of this sort of constraint we found that economics believed they were 'bad things'. Yet if our present argument is valid they can neither be good of bad, for to argue whether they are good or bad is to imply that freedom is a 'better or worse thing' and all constraints upon that freedom are more or less 'bad things'. In other words, freedom can be treated as a good. (Indeed, one could argue that to do anything about them implies a judgment about their worth (whether negative or positive); for if a man, A, can constrain the utility drive of another, B, is it not dubious to at once accept economics in positive and then to assert B could buy off A and gain from the transaction?) Only if we assume B does not like A's constraining influence is this last argument at all relevant. If we assume this it implies that B holds his freedom to be good at all times and so in an absolute.

It could be argued, however, that the economist is simply viewing what A and B do. They act as if freedom were valuable to them. But if we are to be consistent they must view freedom relative to everything else. It must be a good. Yet how can freedom be seen in terms of more or less? Either our man is free to make his decisions or he is not. If he is not,
he is no longer the man with which we started. If he has this freedom from coercion, he must hold it as an absolute for it can be nothing else. But as we have argued, our man cannot hold any absolute values. Hence, we conclude the concept of action that is contained in economics is inconsistent and rests upon a basic confusion; namely either freedom is held to be a good (which is not possible) and men hold relativistic value systems or freedom is an absolute and men can hold absolute value systems (which is inconsistent with the postulates of empiricism).

Now let us take up the question of the form of this industrial society. We have found that our society is one in which individuals are free to be selfish. The degree to which they are selfish is to a greater or smaller extent determined by two types of constraints. No reference has yet been made to the system of this society as such. 11

Let us, therefore, see what can definitely be ruled out. Another examination of Smith yields:


Hence, he believes the social act is made up of three sub-sets and only three sub-sets. It is clear, too, that until these sub-sets are all present we do not have the sort of society Smith is analysing. How given this to be so, we would expect economics to attempt explanations of these phenomena. That this is so, can be seen by the importance placed on finding an explanation of the distribution of income in society.

By the same token, we should expect that the relative shares of rent, profit and wages to be unexplained. Since economic theorising presupposes a society and tries to explain why certain actions take place within it, it would seem it cannot talk within itself of its own pro-suppositions.
This hypothesis is, indeed, unrefuted. As far as we know, there is no general explanation of this phenomenon. Hence, we shall conclude that the society which economics studies is one in which there are three orders (based on rent, profit and wages), there is a relative absence of coercive externalities and the propensity to be selfish is to some extent present.
We have already hinted at some elements of the epistemology implied above. It was noted that our individual was completely free and gained his knowledge of his values from sensations alone. Let us examine each of these propositions in turn.

A person absolutely free to act must have neither constraints upon his ability to act imposed either by outside agents or by himself. We have already described many of the constraints from outside. In general, lack of such constraints must imply that an individual may conceive of an action which can have any possible desired result, given that he accepts society as the best. Thus, he can do anything provided he does not contravene the law or the constitution of the country in which he has chosen to live.

Perhaps more important are the barriers to freedom within himself which we suppose to be absent. First, he can have no preconceptions about the world. His knowledge is based upon sensations which are accepted freely. Thus, we can rule out innate ideas. Second, he cannot know what another is feeling except by what he senses of that person. His life cannot be combined with another in that when someone is hurt he feels hurt, since this places a constraint upon his pleasure. Third, our
individual must have no predictive capacity at all. He can merely say what
has happened in his experience and base a new notion on the assumption that
his experience is a relevant guide to this new notion. In this way he is
not tied to a dogmatic conception of what the world he senses will be.

This enables us to build an epistemology. Most readers, I trust,
will recognize it as that which was propounded by the English empiricists
Locke, Berkeley, and Hume. I shall, therefore, show how these philosophers
incorporated the above ideas into their thought without comment. This will
serve as a useful introduction to the main part of this chapter.

All three begin by asserting that man has no innate knowledge. Neither
can knowledge be acquired by other men's opinion, for "... we may no
rationally hope to see with other men's eyes or to know by other men's
understanding ..." (Locke P.40). Thus, all knowledge is derived in
oneself. For such knowledge to be then it must be presumed that one must
conceive oneself as being.

Now given that man is conscious that he thinks, it follows that the
mind contains some "... phantasm, notion, species or whatever, it is
which the mind can be employed about in thinking ..." (Locke P.16),
i.e. it contains ideas. Those ideas are derived from experience and from
which all knowledge is derived. All these perceptions of the mind can be
according to Hume (P.1) divided into two classes: impressions and ideas.
Impressions are "... sensations, passions and emotions as they make their
first appearance in the human soul ..." (Hume P.1); whilst ideas are
"... the faint images of those ... i.e. impressions ... in thinking and
reasoning ..." (Hume P.1). Impressions and ideas are simple or complex.
Single impressions and ideas are categorical but complex ideas and impressions
may be split into parts. Complex ideas are a combination of several simple
ideas, but our simple ideas "... in their first appearance are derived
from simple impressions, which are correspondent to them, and which they
exactly represent ..." (Hume P.4).
These impressions are derived through sensation or reflection. The first being derived from unknown causes in the soul and the second through the senses and being formed into a simple idea by the memory and the imagination. Memory preserves the original form in which the objects it contains were presented whereas the imagination is not so constrained. Thus the imagination is at liberty to "... transpose and change its ideas ..." (Hume P.10).

Impressions cannot be made without the intervention of a mind. As Berkeley argued the ideas to which the impressions give birth are not connected necessarily with their antecedent impressions but a connection is learnt through experience. Thus, if one conceives of the mind as a blank sheet upon which certain impressions are drawn it must be that the mind has learned a means of interpreting the objects which give rise to these impressions. Hence, we read "... the proper objects of vision constitute a universal language ... of nature ..." (Berkeley P. 92). So the objects which give rise to impressions are analogous to the elements of a language which must be learnt before it is understood.

Now, the complex ideas are combinations of simple ideas formed by means of association by three qualities: resemblance, contiguity and cause and effect. These complex ideas are divided into relations, modes and substances. Modes and substances are merely collections of simple ideas, united by the imagination, which have a name assigned to them. Relation is defined as "... any particular subject of comparison, without a connecting principle ..." (Hume P.14). All reason is the making of this comparison (Hume P.75).

There are seven such relations: resemblance, identity, relations of time and place, proportion in quantity or number, degrees in any quality, contrariety and causation (see Hume P.69). These relations can be divided into two classes: those which are derived comparing ideas, and
those which can be changed without changing the ideas which they contain. It is only resemblance, contrariety, degrees in quality and proportions in quantity or number which " . . . can be the objects of knowledge and certainty . . . " (Hume P.70).

Of the other three - identity, relations in space and time and causation - we read:

" . . . the only one that can be traced beyond our senses, and informs us of existences and objects which we do not see or feel, is causation". (Hume P. 74).

We have seen already that like the school of philosophy mentioned above, the concept of the individual acting in a given state is central to economics. Most economic research begins with this concept. We have already analyzed that even if a particular actor is not defined in such a way that the concept of society used by economists is of such a nature as to imply that concept of a particular actor.

It is also clear that economics, like empiricism, conceives of 'reality' as given to the particular actor. Thus:

" . . . the proposition that it is possible to study economics uniformities without passing ethical judgments or formulating economic precepts seems in fact so little to need proof . . . that it is difficult to say anything in support of it that shall go beyond mere truism . . . " (Keynes P.40).

The economist as an observer, therefore, can and does induce generalizations about the society he is studying from the data (impressions) that are presented to him without passing judgment upon that society. Not only is it possible for him to form ideas from these impressions unimpeded by prejudice but also to associate them in a way which allows ethical and
factual considerations to be kept separate. Ethics and fact are separate
both when simple ideas are being formed and when these ideas are being
combined into complex cases.

We have stated the above in the mode of English apologists because the
judgment process implied by the quotation from Keynes is that of Locke,
Berkeley and Hume. This is shown as follows. If ethics can be separated
from fact, in principle, then a person must be able to discriminate between
the two sorts of judgments. If a person can so discriminate then he must
not be prejudiced in his discrimination, for if he does, ethical considera-
tions enter his discrimination. Now the only way this sort of process is
possible is by means of a mind whose judgments are solely a result of
sensation. Only then can we say that discrimination was unprejudiced. In
other words, we must believe a person does not have innate ideas and that
all knowledge is empirically derived. Yet the assertion that the
separation of 'what is' from 'what ought to be' can be validly made is
difficult to support in social science. For if economics is valid for
all men at least part of the time, it is clear that during the time in
which it is valid our observer must be subject to the social externalities
which everyone else experiences. Thus our economist is by his own
assertion not an unmoved observer of the social space. If economists are
part of the economy they observe then their judgment as to the discrimina-
tion between fact and ethics may change as the influences of society change.
Thus it is necessary for economists to lay down rules of procedure whereby
factual and ethical statements may be kept separate. There are three such
procedures: the 3-twist, 3-twist and Economic. Before we examine
these procedures it is necessary to view the purpose of their application,
or rather that are the uniformities for which economists are to look.

It was Luce's opinion that these uniformities must be causal laws.
This statement is based upon the assertion that causality is the principle by which the existence of one event may be deduced from the existence of another. If observation and/or perception is to be taken as the criterion of the existence of an event and if 'causal links' cannot be said to exist by this criterion then the assertion of causality is the assertion of a constant conjunction between events. In these circumstances a high correlation over time between events is taken to be evidence of causality between those events. Since any relation involves correlation in some form or another, the uniformities of a science accepting the above/being in Hume's sense causal.

Perhaps this last statement requires clarification. There are two strands of arguments implied. First, a statement about extentional entities of the form:

\[ A \sim B \]

do not imply a link \( B \) that is also extentional. The relation simply ensures that \( A \) and \( B \) are associated in the way described by \( B \), and that any extentional link between then takes the form of \( B \). Second, a negative correlation implies a causal link not between the entities mentioned by \( A \) and \( B \) but between these and some other entity which leads to the negative correlation. Zero correlation, on the other hand, is a sign of no causal relations in the uniformity theory of causality.

There are, however, two other arguments in favour of this same point. First, Aristotle argued that if you can find the causes and principles of things then you can find out all you wish to know of that thing. In other words, if one asks 'why?' long enough, knowledge will follow. Such enquiry involves using the conception of causality. That much of economics asks the question 'why?' rather than 'what?' or 'how?' is clear if we recall our discussion of the nature of economic man in which we saw that the 'what?' of man in economics has been analysed by use of the question 'why?'.

Second, we can argue with Kant that the nature of scientific enquiry
is such that the notion of causality is necessary before we can draw from
our impressions ideas which represent the uniformities that underlie our
observation. All three arguments point to the great importance of the
notion of causality in empirical science. This is true of economics as it
is of most sciences. Here we need only illustrate the point:

"... the issue of whether inflation is the consequence of
the upward push of costs or the upward pull of demand on
prices became a lively issue in the late 1950s in the
United States..." (Johnson, pp. 126-127).

This is common and an outcome of the Humean conception of causality.
Another example this time of the Kantian view:

"Causality is an asymmetrical relation among certain
variables, or subsets of variables, in a self-contained
structure." (Simon, P. 29).

From the point of view of economics, then, we are discussing Humean or post-
Humean causality. We have to examine each means of attaining objective
economic knowledge as to what extent these concepts of causality are
consistent with these various means.

First, however, we must examine the various types of causes examined
by economics. If we recall our discussion of action, it will be remembered
that action was an outcome of value and motive. Now these motives must
be the ends of an action. It is, thus, the final cause of action.3
Such final causes cannot be the object of science, since they can never be
observed, according to the tenets of Hume for they are not observable.
Thus, economics may not study the motives which are contained within it,
namely: firms attempt to maximize profits or individuals wish to maximize
utility - these are given.
In the sense that such motives are the ends of actions, they are the final causes of these actions. Yet they are not the final causes of the system in which the actions take place since they may be changed by this system. They are formal causes when viewed in this context.4

An action in a particular situation is really an efficient cause of the resultant event. When, however, such particular causes are generalised, we are talking about efficient causes and so our discourse is of formal causes. For example, a Humean esconced on a great height observing the world of man cannot help but observe the results of efficient causes. There may be cars colliding. If he is a clever Humean no doubt he will find laws or generalised causes that govern the way in which cars, in general, collide. These generalisations are really formal causes. How it seems that such a generalisation is impossible to justify in human terms. Formal causes are derived from the concept of essence which Hume attacked as a collection of simple ideas. Simple ideas have nothing within them that enables us to enlarge our knowledge outside our experience. Since, however, Hume sees causes as complex ideas not simple ones thus it would seem that the concept of causality in economics is inconsistent with the epistemology of Hume that it seems is implied by economics.5

So what is precisely the nature of causality in economics? Again, in the absence of precise statements on this matter, we are forced to resort to the process of stumbling with which we began. Hence, in Smith we read:

"The constancy and steadiness of the effect supposes a proportionable constancy and steadiness in the cause." (Smith [1] Vol. I P. 40)

or in Mill:

". . . the law of the effect is compounded of the law.
of all the causes which determine it . . . " (Mill [2] P. 139).

Thus economists deny the disjunctive plurality of causes or simply the same effect is a necessary consequence of a unique cause. Such an assertion can be seen in the emphasis of economists on causal analysis in the interests of policy-making. Here is one:

"For tackling any major economic problem . . . one must be in a position to understand the causal relationships in an economy. For example, would a reduction of the rate of income tax increase or decrease the volume of employment?" (Nade, P. 13).

Thus we shall accept the asymmetry of the causal relation, referred to above, is an expression of the denial of this disjunctive plurality.

Every economic situation is not, however, uniquely a result of a set of causes. We read:

". . . it is necessary to face the difficulty of regarding the various elements of an economic problem - not as determining one another in a chain of causation . . . - but as all mutually determining one another." (Marshall P. viii)

or

"Every cause has a tendency to produce some definite result if nothing occurs to hinder it." (Marshall P. 26).

Thus we may gather that the causal sequences can best be represented in
certain cases by simultaneous equation systems if the actual causal relations are so complex as to appear to not and react on each other. Such a tendency has continued.

The observability of causal relations in economics is, as we have already seen, open to question due to the subject's prooccupation with formal causes. Such a problem is highlighted by the following:

"Variations in the nature of marginal costs are indeed largely responsible for the well-known fact that those effects of an economics cause, which are not easily traced, are frequently more important than, and in the opposite direction to, those which lie on the surface and attract the eye of the casual observer." (Marshall P. xiv).

Thus economics must observe these hidden causes and test them or invent them and let them be.

Finally, there is a feature of economic causality which is derived from the last two features. It is the absence of the so-called 'crucial experiment'. Because of the difficulty of finding necessary conditions for events due to unforeseen and unknown hindrances, it is often argued that no causal relations can be found except in peculiar circumstances. Causal relations are thus taken to be extremely high correlations between events.

Now it seems clear that this causal scheme is based upon the concept of constant conjunction between events. It is not, however, clear if this is a complete description of economic causality thus we shall now proceed to a consideration of the concept in the light of the three procedures referred to above. We begin, with the F-twist.

The F-twist is based upon two ideas. First:
"... we cannot ... too carefully endeavour to verify our theory by comparing, in the particular cases to which we have access, the results which it would have led us to predict, with the most trustworthy accounts we can obtain of those which we have been actually realised."

(All \[1\] P.154).

We construct complex ideas from the simple ideas given by our impressions and simply test further consequences of these ideas against what has been observed. This view developed into:

"It is essential to note that the eocnsclist does not claim to measure any affection of the mind in itself, or directly, but only indirectly through its effect ..."  
(Marshall P. 13)

and then to:

"Are motives as important as affects?"  
([9] Stigler, P.96)

or:

"Truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense)."  
(Friedman P.14)

For adherents to the F-twist ideas do not need to be 'real' in the human sense except insofar as they are based upon impressions. Perhaps one can best described the F-twist as tested imagination.

Second:

"Observed facts are normally finite in number; possible
hypotheses, infinite. If there is one hypothesis that is consistent with the available evidence, there are always an infinite number that are." (Friedan p.9)

Hence we must sort out that hypothesis which is most consistent with the facts. This may be achieved simply by statistical means; in such a study it is often a correlation coefficient that is taken to serve as a sign of explanatory power.

Our F-twist procedure is simply then to deduce the conclusions of a given theory and then to compare these predictions with what actually occurred. This may be done for several alternative hypotheses. That hypothesis which yields the most correct predictions as it turns out is then adjudged the 'winner' and is held to be correct until beaten by another hypothesis.

Now this approach I believe rests upon two important confusions. First, the conclusions of a theory are seen precisely analogously to the effects of an action. We shall see that a material implication does not have the same form as a causal statement. Now this is true of conclusions of theories and effects of actions if we limit our means of deduction to mathematical logic. Hence it follows the F-twist procedure may assert an identity between consequences and effects but it may not then make use of the double equivalence relation to argue that that such behaviour (effects of actions) is evidence that people act 'as if' the reasons given for the hypothesis were true. Thus, logically the fact that a consequence of a theory has support from given evidence is not a necessary condition for the assumptions of the theory to be supported.

Second, even if the above argument is to be refuted, the second
confusion is unresolved. Given that for a given set of facts there are an infinity of hypotheses (which would not be admitted into Imre's theory of knowledge) it follows there must always be one hypothesis which is more completely borne out by the data than any other hypothesis. But it is, by definition, impossible to find that most accurate hypothesis for sure. Thus, there is a continual process of successive approximation.

Yet it is assumed the degree to which a hypothesis explains a given set of data is shown by some statistical test. Disregarding the fact that not all hypotheses can be tested, let us look at this assertion. All that a statistical test can show is the probability the given relationship has of occurring by chance. Now, as such, it must be assumed that there is a large number of occurrences and that those occurrences are not interrelated. In those circumstances, we cannot hold a particular exception to our generalisation to be a refutation of that statement. Only if there are several occurrences of this exception can we say our hypothesis is refuted. If, however, explanation is not statistically defined we can refute a hypothesis on the observation of one exception.

Such a confusion is quite unwarranted. If we assert that economics is concerned with the formation of generalisations about men's actions, we may not then say they need not apply to particulars. If they are merely statements about the chance of a given state occurring then they cannot apply to particulars, but if they are of the form of the material implication they must apply to particulars.

The F-twist, then, asserts that the axioms and propositions from which our theorems are deduced need not be refutable even in principle. Yet this implies a causal statement acting as an axiom or a proposition need not be based upon the observation of reality since if it were, it would of necessity be refutable. Thus, causal statements may be pure imagination
in the sense of Hume. But as we shall see, causal statements cannot be
pure imagination since the very concept of cause is empirically derived
according to Hume. It follows, I think, that the F-twist is unable to
analyse human causality since the imagination is not capable of forging
causal links. If imagination is only means by which hypotheses can be
constructed, then no causal statements can be part of an F-twist Economics.
Yet I believe it to be well-known that causal relationships of the sort that
Hume envisaged are the subject of the F-economists study which seems to
indicate a degree of inconsistency.

We can conclude, therefore, at liberty to introduce human causality
into a F-twist methodology if we reject the concept of non-refutable
assumptions.

If, however, we consider the aim of science to be the "... the
development of a 'theory' or 'hypothesis' that yields valid and meaningful
predictions about phenomena not yet observed..." (Friedman P.7) then
it can be argued that our whole testing procedure is an attempt to find
causal statements. Causal statements express relationships between
phomena which hold a necessary contiguous and prior relationship to
each other. Only necessary relationships can yield the sort of predictions
for which F-economists are searching. The whole process of theory
construction can then be seen as a process of synthesis by imagination.
It attempts to construct causal statements which are then checked against
the facts.

But this can only be a valid procedure if our original statements
were based upon observation. If not, then the test we perform to test the
theorems we have deduced must order the data according to the categories
implied by the theorems. In short, our theorems are imposed upon the
world we observe. Thus our predictions must be analytic in the sense that they imply a corresponding observation. Hence, we may never appeal to the facts as an arbiter between hypotheses since our hypotheses are imposed upon the facts we consider.

If this view is tempered to the view that:

"Our intellect does not draw its laws from nature, but tries - with varying degrees of success - to impose upon nature laws which it freely invents." (Popper [1], p. 191)

we must temper our above conclusion. In particular, we must argue that we may construct theories by 'poetic intuition' that may or may not succeed in imposing their theories on nature. Thus, the degree to which our theories are analytic depends upon whether they are more or less irrefutable. The most analytic will be least refutable, in the sense that analytic statements cannot be refuted against the facts and that if one could envisage a degree of analyticity and a degree of refutability the two would, for a given statement, hold an inverse relationship to each other. But since the degree to which they are refutable is only to be found by the process of refutation, there is no way we can find out if a certain hypothesis is refuted because it is made to conform to the facts or if the facts are made to conform to it. A test to make this clear runs into an infinite regress.

Thus we must conclude that we can never know if our theory is refuted against the facts or if our facts are refuted against the theory. Clearly it is important that we can declare this distinction. If we assert the latter alternative, we can merely argue that it is the fact collection process that is at fault in every case the theory is apparently refuted. Without a specific relation between fact and theory this approach will always give us reason for upholding a particular theory in the face of
apparently contradictory evidence.

Let us now turn to the S-twist. It is stated simply in symbolic form. Let, B, be a set of "... axioms, postulates, or hypotheses that stipulate something about observed reality..." (Carnap, P. 1775) and call it a theory. Let, C, be the consequences of this theory and let, A, be called its assumptions.

Now if C is a complete set of the consequences of B it is identical with B (B implies itself and all the things that itself implies). Also the minimum set of assumptions which give rise to B must be identical with B. Thus we have:

\[ A \equiv B \equiv C. \]

If we now suppose that there is a property of 'factual correctness' then this property is shared by all three sets.

Suppose now there is a subset of C, C', and an enlarged set of assumptions, A*, of which A is a subset. We have:

\[ A* \supset A \equiv B \equiv C \supset C'. \]

If now C has empirical validity, we may argue that A and B also have it but not A* unless B* \( \equiv C* \) also have it. Indeed, if there is no evidence for the accuracy of A* - A judgment is reserved and work is concentrated on A \( \equiv B \equiv C \).

Such a prescription bears the same traits as the F-twist. The method is borrowed from deductive logic which conforms to the Husserl concept of knowledge; it conceives of the economic reality as given and 'observable'; and it implies acceptance of the assertion of positive economics.

As stated, we have to conclude that the S-twist is a more consistent statement of scientific method than the F-twist but simultaneously we must point out two corollaries of this approach which limit its applicability.

First, the importance attached to Gokhle's Razor. Samuelson asserts that any statement that can help the derivation of new meaningful theorems and/or make an existing derivation more elegant is to be admitted. To make the derivation of new meaningful theorems possible can only be accomplished through the introduction of new statements with empirical import. Elegance on the other hand, is a property achieved through analysis and is a purely formal property. We should, therefore, be aware that statements that help in the derivation of new theorems are quite distinct from the other type of admissible statements. It is not surprising, therefore, to read:

"My own methodological position is an eclectic one."

(Samuelson P. 100).

Provided the statement can be fitted into the above described general framework, it is to be admitted.

Now let us take an assertion that there is an infinite set of observations. According to Samuelson such an assertion ought to be admitted since it enables the revealed preference theorem to be meaningfully derived. Yet if in general we have an infinite set of observations and an infinite set of theories for each subset of each set, then an infinite subset may be set in one to one correspondence with it. Can Samuelson's procedure identify the relevant correspondences?

Let us assert we have an infinite set of "hypotheses" which are in principle 'observable' and an infinite set of observations. It follows that the hypotheses are in some sense identifiable let us say by assigning each a natural number. Let the same be so of the set of observations. Can the decision technique implied by Samuelson associate the two infinitely denumerable sets? Obviously this is only so if process of identification is precisely the same for both sets and each has same identification number.

Let us assert the first condition holds. To assign the same identification number it must in principle be possible to assign by test the
most appropriate identification to the observations. This, however, is clearly impossible if there is an infinity of them.

If, however, there is only a finite set of observations the Samuelson technique can, in principle, be applied. It is now possible to perform sufficient tests to identify an observation with a hypothesis. The S-twist cannot even perform this. However, since the basis of identifying hypotheses (by prediction) is different from the process of identifying observations (by effect). As we have seen predictions are not logically synonymous with effects and any decision on the basis of the S-twist can lead only to the association of like predictions with like effects. It does not, as the S-twist does in principle, associate theories and observations on the basis of what they purport to describe.

On the basis of the above we must conclude Samuelson must accept the "...methodological straitjacket of working only with a finite number of punch-cards of observed..." (Samuelson, p.103).

Second, let us examine the Correspondence Principle. It is simply the process in which:

"...the dynamical properties of the system are specified, and the hypothesis is made that the system is in 'stable' equilibrium or motion. By means of what I have called the Correspondence Principle between comparative statics and dynamics, definite operationally meaningful theorems can be derived from so simple a hypothesis." (Samuelson [2] p.5).

This highlights another characteristic of the S-twist. It is always possible to make heroic assertions leading to an idealized representation of the real world provided the relationship between what is being analyzed and the analysis is not forgotten. In such a way it is always possible
to, in principle, test any such assertion against the facts.

Yet as we saw in our consideration of the P-twist, such a test is
irrelevant to the nature of Economics. It would be argued that, by such
researchers, that Economics is not interested in the axioms and postulates
of its theories; it is simply interested in knowing what is. If, at the
same time, it can find reasons for the state of affairs then well and good
but it need not.

As we have seen, the S-twist conceives of a theory without stating how
it is derived or the limits to which it is compelled to keep. Thus any
statement from which our theory may be derived must be logically classed
as part of the theory, similarly for implications of the theory. How in
these circumstances the Correspondence Principle is seen as a cornerstone
of such economic analysis is simply because it has implications for economics.
If we look into the P-twist, there is an 'imaginative jump' from the 'data'
to the 'assumptions' of the theory. The Correspondence Principle is, then,
a sort of spring-board; it is not part of the theory as such.

From the above we can see that the S-twist procedure follows more
closely the human example though we may observe that Friedman talks of
'direct observation' which is part of Lasso's procedure. Let us quote the
basic proposition of the S-twist:

"Define a 'theory' (call it D) as a set of axioms, postulates,
or hypotheses that stipulate something about observable reality
... If no conceivable observation can even in principle
refute, confirm, or touch or bear upon the axiom system taken
as a whole, then D is not economics ..." (Samuelson, P. 1775).

Now this probably includes practically every system of scientific procedure
that has ever been enunciated. But what sort of observation and how it bears
upon the axiom system is left unexplained. Thus we are left to discern
answers to these questions elsewhere. We read:

"We must not impose a regularity – or approximate reality – in the complex facts which is not there."

(Samuelson, p. 1777).

The S-twist rejects the previously described F-twist on much the same grounds as we have.

Now it will be remembered from our discussion of Samuelson's utility theory that the data allowed in the construction of the individual value system was observable. It must on this account be directly observable to the individual if it is to directly generate the postulated behaviour. If this data is observable, then it follows that all the data which are relevant for an analysis of the working of an economy must also be directly observable, since the economy is a summation of individual economic actions.

If all the data considered by economics is observable in this way it is clear that all axioms and propositions must be grounded in these observations. How causality in this context may be nothing but pure chance. As such, practically the only criteria for dividing between a cause and an effect is that of priority. When this becomes impossible, as in inflation theory, nothing may be said. Indeed as Samuelson and Solow point out our observation of the effect may be prior to the observation of the cause of that effect (see Samuelson, p. 1342).

Thus, it is clear that the possibility of our identifying cause and effect relations is conditioned by the techniques of observation we have at hand. Some events, e.g. real balance effects, long run consumption, costs in terms of alternatives foregone, etc. are not directly observable, i.e. we may only observe signs of them in observable events. This type of observation is not inconsistent with the Russian direct observation if the
signs of the events are, for certain, precisely 'in tune' with the events we suppose we are observing. But it seems we can never know this. Hence, the 5-twist must assume that the observations it makes are the observations that will be able to observe what, in fact, is being examined.

Yet such an assumption must mean that any implication drawn from it must be correct even if our original assumption is wrong. Thus let, A, be our assumption and B be some other statement:

\[ \sim A \implies B \]

is valid. B may be any statement such as 'the observations I make support our theory'. By the 5-twist, then, if we substitute: 'I am observing what I wish to observe (to support my theory) for A, it follows: 'If I am not observing what I wish to observe (to support my theory) then the observations I make support my theory' is a valid implication which must mean that whatever we observe we may never refute our proposed theory.

We now move to a consideration of the econometric approach. I shall begin with a definition:

"Econometrics may be defined as the social science in which the tools of economic theory, mathematics, and statistical inference are applied to the analysis of economic phenomena." (Cold Berger P,1).

Econometrics is what econometrics does. It aims, we are told, "... to give empirical content to economic theory ..." (Coldberger P,1).

Now we have seen the 5-twist attempts the very same process though not explicitly with the same tools. Hence, we ask what is the unique approach attributable to econometrics? In answer to this question we read:

"Two or more sets of jointly asserted propositions about observable facts (not about mathematics or logic or ethics) are compared to determine which set is, in some sense, in better correspondence with facts." (Korschek in Christ P,vi).
Yet we have seen this comparison is an essential component of the F-twist, which does not consider it important to introduce empirical propositions into economic theory. These differences are a result of the particular notion of the relationship between theory and observation.

Such differences are derived from the statements used by econometricians as against less mathematically sophisticated economists. Indeed this may account for the distrust of the former by the latter to be found in:

"I have no great faith in political arithmetic . . . ."  
(Smith [1] Vol. II P.35)

As Keita has shown economic theory is composed of generalizations (or laws) which are of the general form of a material implication. Yet in the view of Coldberger:

"Economic theory is concerned with relationships among variables."  
(Coldberger P.2)

and

". . . economic theory typically specifies exact functional relationships among its variables."  
(Coldberger P.2).

Now it is clear that those who believe mathematics is simply another language equivalent to (or derived from) logic must also believe there is a statement of material implication in mathematics for these views to be compatible. However, it is clear that an 'if . . . then' statement is different from a functional relation and only with great difficulty can they be expressed as equivalent (see Orcutt, Simon).
Indeed, the assertion that they are, except in limited circumstances equivalent is misleading. Suppose we had two theorems from alternative theories, one of which had the form:

\[ \text{if } A \text{ then } B \quad \text{(a)} \]

and the other:

\[ A = f(B) \quad \text{(b)} \]

Now it is clear that \((b)\) is an equivalence relation and only if certain constraints are applied to it can \((a) \equiv (b)\) (see Simon). This implies a change in \(A\) will always be associated with a change in \(B\), but this is not so for \((a)\). Thus, in our process of comparison against the facts in using form \((b)\) we can rely exclusively upon correlation techniques since we wish merely to find associations between variables. Hence the econometric approach is concerned with deriving associations which are the best that can be found. Is it possible to conceive of causal relations in these terms?

As we have already seen, econometrics is a method involving pure induction as proposed by Mill. It would, however, be unfair to pin the criticisms of Mill to econometricians. Thus, we shall consider the basic approach of econometrics to the problem of causality. For this we shall rely heavily on the writings of Orcutt and Simon. We begin by quoting Christi:

"In our work we try to use the concept of causation that combines the dynamic system and the idea of specific external causes. The reader will readily identify the latter with changes of exogenous variables and the changes (we hope less frequent) in structural parameters."

(Christi P.212).

Thus causes are quite external to our concept of the economy. All that
we can examine are the changes to our specified system wrought by causes that are external.

Two points are of interest. First we are talking of efficient causes which we discovered, are not the subject matter of economics; second, it is difficult to see how we can rule out disjunctive plurality of causes in fact. Although the relation between the system and its exogenous variables must be specified, this does not rule out the possibility of there being more than one cause for a given event in fact. It is simply that it does not find expression in our system.

How then are these causal statements derived? Causal relations are:

"... unidirectional relations which are complete and exact within well defined limits..." (Crockett P.305).

As such they may be expressed in a mathematical set up. Thus we can define causal ordering as:

"Let $B$ designate the set of variables endogenous to a complete subset $B$, and let $V$ designate the set endogenous to a complete subset $C$. Then the variables of $V$ are directly causally dependent on the variables of $B$ ($B \rightarrow V$) if at least one member of $B$ appears as an exogenous variable in $C$." (Simon P.13).

Given this definition we can conceive of an econometrician constructing the following equations:

\[
\begin{align*}
\alpha_{11} x_1 &= a_{10} \quad (1) \\
\alpha_{21} x_1 + \alpha_{22} x_2 &= a_{20} \quad (2) \\
\alpha_{32} x_2 + \alpha_{33} x_3 &= a_{30} \quad (3)
\end{align*}
\]
Solving for each variable in turn and applying the above definition will give us the following causal orderings:

\[(1) \longrightarrow (2) \longrightarrow (3)\]

and \[x_1 \longrightarrow x_2 \longrightarrow x_3\]

(see Simon P. 14).

This concept of causal ordering is, however, not identical with the Humean concept of causality. It is important to realize that Hume believed that causality could be observed as a relation between events not as associations between magnitudes of variables.\(^{10}\)

The difference between these concepts is important. If an event can be expressed as a unique magnitude of a variable then the distinction is not at all clear. For instance if an inflation is seen as the situation in which \[\frac{dP}{dt} > 0\] then provided an inflation always exhibits this, our distinction is nore pedantry. But the problem arises of how to classify a damped inflation which has not yet occurred and so \[\frac{dP}{dt} \neq 0\]. Clearly it is an event and an inflation but cannot easily be defined as such by a single first derivative since nothing, as yet, has changed. Now it seems that a model \(M\) which provides necessary conditions for \[\frac{dP}{dt} > 0\] so that\(^{11}\)

\[M \longrightarrow \frac{dP}{dt} > 0\]

is valid does not provide the same kinds of relationships that Hume termed as causal. In other words:

\[M \longrightarrow \frac{dP}{dt} > 0\]

cannot be causal, from which we argue that an equation summarizing the above relationship such as:

\[\frac{dL}{dt} = \alpha \frac{dP}{dt} + \beta\]
does not necessarily refer to a causal relationship.

Moreover Boe's belief was based upon the exact representation in perception of sensations of events. Now this exact representation may not necessarily be possible in econometrics for two reasons. First, an association between variables such as an equation is the result of a synthetic process in the mind which may not guarantee the representation referred to above. Second in econometrics, in particular, a variable may refer to an event, a representation, a substance or any entity that is variable and whose variability is measurable. A variable then can refer to an event but need not do so.

Associations of variables are distinct from relations between events, also in that such an association is merely a synthesis of a range of magnitudes of at least two variables. Relations between events are, however, a statement that at least two events are part of the same continuum. An association, then, implies a unique relationship between two ranges of variables (at least).

Events, however, are simple ideas or collections of simple ideas. They must, therefore, be unique in the sense above. The case is true for the properties associated with observation. It is clear, however, that since causes are exogenous variables the relation between them is not analyzed. Hence we are considering a closed analytic system that has exogenously determined causal relations. From the point of view of the system the specified causal relations may never be refuted since they are, in effect, unmovable movers of the system.

It is also doubtful in what sense the existence of one event implies the existence of another. If we have argued that the magnitude of \( x_1 = 3 \) and the magnitude of \( x_2 = 6 \) and that these magnitudes alone
determine the uniqueness or otherwise of a particular event, does it make any sense to postulate the same causal relation if the magnitudes are different? Does it not simply mean that we have a curious functional relation which is so it is simply because it is constrained in its applicability?

By concentrating on the asymmetrical properties of the causal relation econometricians have assumed that

\[ A \rightarrow B \equiv \text{B = f(A)} \equiv \text{A causes B} \]

It is clearly not. An exogenous variable takes the form of a many-one correspondence to its dependent variable. This, however, is not so of a causal relation only a one to one correspondence in present. Such a difficulty can be got round by the use of linear techniques. It cannot be however, that \( A \rightarrow B \equiv \text{A causes B} \).

Despite these comments above (or perhaps because of them) we find instances of the solution of matrices on these lines in order to find causal relations, (see [reference]). It is difficult to decide what they mean. In order to incorporate our individual into society we had to assume that he derived a notion of cause and effect in his dealings with the environment and used this notion as a basis of his actions. Yet the solution of the matrix which can be said to exhibit his behaviour made no explicit mention of cause and effect except an causal dependence. How how is this so?

In present-day mathematical economics we analyse the individual on the basis of his having to solve an optimizing problem. Hence, in essence, our individual acts as if he were aware of a feasible set and certain constraints upon him such that he wished to optimize his behaviour on the basis of this knowledge. Thus we are interested in knowing what he wishes to do under given assumptions; in essence we are calculating formal causes of individual behaviour. But these formal causes of individual behaviour
are not causes in the sense of Hume and although there may be a constant conjunction between our calculation and the person's action, we are not interested in analysing them.
We are now in a position to draw conclusions from the argument of the first two chapters that serve to question the theoretical basis of economic science. If our argument is to be accepted, then it seems two questions must be answered before any further research in the science can be carried out. First, if the presuppositions of the subject are inconsistent then how are these presuppositions arrived at? The answer to this question is not as simple as it at first appears for two reasons. Not only can Economics be a purely deductive science since if it were we would expect such obvious deficiencies not to be present but also it cannot hope to explain data in terms of logically coherent arguments. Thus we have no explanation (except in terms of the psychology of individual scientists) of how such general statements as economics has derived, have come about.

Second, we must ask how can the science of economics be changed so that these inconsistencies are eliminated whilst the findings of the science are not destroyed as well? Any answer to this must, to some extent, reject the assumptions of economics in order that different conclusions can be derived. It is this more mundane task that we attempt hereafter. But it must be emphasised that final answers will not be forthcoming and thus
much of what we have written appears to belong to the 'world' of unsubstantiated belief. All we can hope to do is to indicate the questions and try to stimulate others to find the answers. It is now high time to describe the inconsistencies which seem to inhabit the roots of economic science as accurately as possible. To this task this chapter is devoted.

At the outset, however, we must emphasize that the argument of Chapter Two is little more than the assertion that the method employed by Economica derives most of its precepts from the writings of David Hume. Our reasons for this are based largely on the seemingly obvious analogy between the two. It seems, however, unsatisfactory that no categorical proof of this link seems available and so the analogy may be doubtful. We shall leave future researchers the task of settling this question and so if future research were to show our reasons to be unsatisfactory then the arguments and conclusions that follow must be modified.

Further the distinction we have made between the object and the method of Economica may be blurred if concepts have been imposed upon data to any extent. If the assertion of Kant\(^1\) that conceptions are, to a larger or smaller degree, imposed upon the data in true of Economica then it may be that those relationships supposed to be common by the English empiricists are to be found in Economica. Moreover, it may also be true that Economica uses concepts which its method dictates and which are not derived from an examination of data. This, however, is not an important shortcoming since it implies a split between the theoretical and empirical parts of Economica which in, as we shall see, difficult to maintain. Also we can regard the use of concepts, whether imposed or not, as truly part of science. In the study of a science it seems not to matter for the present purposes whether
we separate concepts from data or not.

We shall, therefore, keep the above-mentioned distinction i.e. between object and method in what follows. Thus the science of Economics is seen as an amalgamation of something called the object of study with another called the method of study of that science. If the science of Economics is to be consistent then it must be true that the findings of Chapters One and Two must be consistent with each other. There are two points however in which this is not so.

Man acting in society is seen by Economics as being motivated by selfishness the which is constrained both by society and coercion. The Economic scientist, however, is supposed to be separated from the motivations that govern other men's activities. Their judgments are value-free because scientists are unmoved observers of society. Now this is the first inconsistency. Man qua man is an agent who rationally matches means to ends whereas man qua scientist is forced to leave aside all questions of value that motivate his 'other half'. Man qua man is the object of the scientist's interest and so is studied, (conceptually at least), without interference on the part of the scientist. If man qua scientist were to act in general in an identical way to the object of his interest then there would be little science except through the observation of other scientists and by introspection since, for a scientist, these observations are more easily made than those of man qua man. Conceptually, anyway, the science of Economics would be conducted by a process of introspection and projection. That this has not happened in evidence either that man qua man is different from man qua scientist or that the introspective process cannot yield predictions that are checkable against the facts. We shall examine the latter alternative and find a general reason for our doing so.

Moreover, the nature of scientific man is distinct from man qua man in
Economics. Man qua scientist is seen as attempting to explain society chiefly by means of causal relations between phenomena. It is assumed that this is only possible through the finding of constant conjunctions by an observer who senses but does not affect that which he senses. Man qua man, on the other hand, whilst also finding causal relations does so in the context of society. His purpose is to affect the society so that it conforms to a desired pattern. It is here that a fundamental distinction occurs man qua scientist observes the society of men qua men as an outsider would.

The reason for this view is usually given as the need for science to be objective. At this point we digress for a moment to see how this cannot be possible. Now, objectivity can be achieved either by asserting that a scientist is no ordinary man or by conceding that scientists are mostly ordinary men whose scientific activities are made objective through an arrangement similar to social externals.

First if a scientist were an extraordinarily objective man then few men could become scientists. Such men must be able to transcend the world in which they live. Not only must they be truthful, honest, apolitical, etc. but also they must be aware of their perceptions in relation to the whole of knowledge. If at any time the perceptions they take of men does not have these characteristics, then those perceptions are not objective and are not scientific. On a purely formal level it can be seen that in the manifold of perceptions a scientist can never be certain that the perception he makes is not a result of the viewpoint in which he happens to be at a given time. Hence science can never achieve the absolute of objectivity which this view implies.

The second view of objectivity accepts the above. Scientists are men who are forced, by convention, to act in a given way, namely to be scientific. This is achieved by the constant questioning of assertions and the invention
of new assertions about phenomena expressed so that others can criticize and expand them in new ways. It is the acceptance of this process of 'conjecture and refutation' which leads many scientists to hold a given statement about phenomena to be objectively held.\(^2\)

Insofar as men are acting together to obtain objective statements about society, these men are part of an 'institute' within society. This 'institute' may have as real an economic role in society as a factory or any other employer of labour. It may be analyzed in terms of the very same concepts as an economist may analyze I.O.I. or The Jockey Club.\(^3\) It seems to follow, then, that if economics is seen as objective only because most economists agree about certain answers to certain questions the science of economics must, in a sense, be part of the object of its study.

To return from our digression to the main argument it may be argued that man qua scientist is distinct from man qua man only in that ends to which each acts are different. Man qua scientist acts only to maximize the degree with which he can explain given phenomena whereas man qua man is concerned only to obtain the maximum personal benefit given the means with which he can achieve it. (Indeed it has been suggested that many of the problems of 'methodology' are in essence problems in Welfare Economics.\(^4\))

Now whilst this view is quite consistent with the last concept of objectivity analyzed above it raises more difficulties than it solves. First action is only possible if means are causally related to ends. A man qua man to be sure finds it often difficult to be certain that this is so but a scientist can never be in the least certain if he is concerned with the investigation of as yet unknown facts. But this is not the most important objection, since we are talking really of a difference of degree.

Second, Economics attempts to find constant conjunctions on the basis of a large run of observations. Similarly, it assumes that men qua can form their value judgments and hence their actions on the basis of experience
Economics. Our method will be to examine the notion of causality in Economics given that man qua man and man qua scientist are not separable. On the basis of this it is hoped to re-work the conceptions of nature of man the actor. In this way we hope to present Economics with a theory of action that will not be tainted with the inconsistencies of the present one.
CHAPTER FOUR

On Theories of Causality

The notion of causality is exceedingly interesting. From earliest
times men have been puzzled by its nature yet have not succeeded in invent-
ing a theory of causality that wholly satisfactory. In this chapter we
shall merely attempt to sketch those various attempts and to point to the
major criticisms of each view. We shall also see that certain agreements
appear between the several thinkers we have to consider that will be useful
in the following analysis. Such an analysis rests upon the assumption
that there is common ground which is shared by those who have attempted to
supply an answer to the question 'what is causality?' No doubt good
reason can be advanced for saying this is not so and no doubt the uniformities
we find are imposed unhappily upon the various theories. Nevertheless it
seems fruitful to attempt such an analysis the procedure of which is open
to criticism and thus subsequent improvement.

Other attempts to make the same kind of analysis have simply cata-
alogued the sorts of causal theory and stated the arguments common to each
category. This will not be our procedure. Instead a more fundamental
system of categories will be enlisted into which we can fit the notions
of causality rather than the arguments in favour of notions. Hence we
shall attempt a classification not by philosopher but by what seem to be the ideas of philosophers.²

Let us for the purposes of argument introduce the following primitive terms: 'mind', 'matter' and 'idea'. It is not our wish to dispute on these terms and hence they will be left undefined. All that we wish to assert by this is that they may be treated separately. The different theories of causality can then be seen as a creation of an idea by combining mind and matter in various ways. In this way we hope to obviate the dreary (for no I hasten to add) cataloging of arguments³ that usually accompanies such an exercise.

**Aristotelian**

We begin by considering the most important single contribution to a theory of causality. It is that of Aristotle.⁴ More recent developments are largely centered upon his ideas in that they are either denials or affirmations of particular strands in his argument. We shall not, however, consider the ideas in strictly chronological order but merely as ideas forming a system.

How does the Aristotelian idea of cause come about? Wisdom is seen as knowledge of the first causes and principles of things. There are four types of causes and principles: formal, material, efficient and final. These correspond to four elements that are contained to some degree in all entities; namely, substance, matter, a source of change and purpose. Obviously so stated a knowledge of all these elements must imply a knowledge of everything about the entity which we are considering; in other terms it constitutes an 'essential explanation'.

But if something is one thing it cannot be anything else and so our essential explanation must refer to an unchanging entity if it is to be
true of that entity at all times in all places. If what is true is subject
to alteration it cannot, according to Aristotle, be wisdom or knowledge.
Hence since wisdom comprises a knowledge of first principles and causes
these first principles and causes must be unchanging. This implies two
conclusions. First since it was asserted that nothing could cause itself
to change (except substance) if any change is envisaged there must be a
cause underlying the change, knowledge of which is more fundamental than
of that previously asserted. Hence the universe is, in principle,
reducible to a set of causes which once found need never be changed. Thus
it follows a cause is a substance or is that which is not a predicate of
something else. So we conclude a principle or cause must:

"... exist alongside of the things of which it is the
principle, and must be capable of existing in separation
from them ..." (Aristotle [1] 999a).

Second, since the principles and causes are substances they are not
immediately visible. Thus we can conceive of causes as an abstract entity
which may or may not be mirrored in observations. It follows, then, that
observations cannot affect the idea of causal relations we hold to be true
for truth is found at a higher level than in observation. Hence, we can
argue that the substance of an observation is in turn with our idea of cause.

We see, then, that causality is a universal judgment. It is an
intellectual idea. Yet wisdom is art and art is a combination of
experience (sensation plus memory) and universal judgment. Thus a
knowledge of first principles and causes involves experience. We can see
then that a knowledge of the first causes and principles involves not only
a statement of experience but also a statement about experience. For
example, let us take Aristotle's example of the statue. Cause means four things - first the immanent material of the statue (e.g. bronze), second form or pattern of it (e.g. a warrior), third the source of change (e.g. the process of casting) and fourth the end (e.g. the glory of a soldier's memory). To ask 'what are the causes of this statue?' can be answered by enumerating the above but those in combination form the substance of the statue. Hence, the answer to our question at once refers to experience and ideas that are abstract.

Let us analyse this idea of causality still further. Since first causes and principles are substance, they must belong to a thing by virtue of the nature of a thing. To look for elements of things that exists is to look for elements of being, since things partake of being. Because things can only be by virtue of their becoming through the agency of first causes and principles it follows that investigations of first causes and principles must also be an analysis of being. So when looking for first causes we are examining being as such. Obviously something that is can neither be corruptable nor be able to be split into more primitive elements. Hence being implies unity and vice versa for they are inseparable from the thing and so from each other. Thus cause not only must be but also be incapable of further analysis. It is itself a primitive element.

Causes exist because of progress in time. Thus it is argued that the distinction between cause and effect depends in part upon their temporal relation (see footnote 9). A string of causes is seen to form a finite causal process. Two broad types of causal process can be seen. First that via intermediaries (e.g. boy becomes man) and second via generation and corruption (e.g. water becomes air). In the first causal action is seen as a process of successive mechanical leaps whereas in the second
the occurrence of the effect is dependent upon the corruption of the cause. 9
Such chains of causes cannot be infinite since if they were endless would
be impossible and since the final cause may not be changed the process of
becoming is not infinite. Cause then is the substance of change. Its
various modes are a complete catalogue of the ways of becoming. Something
in, therefore produced.

Now, this element of causality has been stated by Aquinas as follows:

"There is this difference between a cause and an effect -
that whatever is the cause of the cause must be the cause
of the effect, but the cause of the effect is not
necessarily the cause of the cause." (Aquinas **I** P.114).

It is an obvious transition of the statement of causality in terms of
necessary conditions which form the basis of much debate in recent years.

Aristotelian causality, then, had features that were taken up by later
writers. It implied a production of one thing by another (hence priority,
contiguity and necessity) 10 and it implied an activity. It also implied
a simultaneous discussion of a material process and the form of that process
(which, as we shall see, is a point of great importance). Since certain
of these properties have been used as the basis of all theories of causality
we may argue that the Aristotelian notion is quite general. It is,
however, open to certain criticisms. First, if one argues that science
should look for the first causes and principles then, as Bacon 11 pointed out,
it can only be concerned with material and efficient causes. This is
because only these sorts of causes may be observed. Such a remark has
importance only if one denies the importance of Aristotle's 'universal
judgement' and, in particular, the idea that truth is an absolute quality
at a higher level than experience. Given this denial it is a short step
to argue that substances are not part of science and so the causes that
Aristotle and the Scholastics analysed are either metaphysical or nonsensical.

Second, in the same vein it is argued that if we regard observation
rather than the concept of an incorruptible primitive unity as our criterion
of truth there is no way an Aristotelian cause can be said to be untrue.
For since if we have found a cause which can be proved to have a prior
element then we can logically argue this was truly not a first cause and
principle in the first place. Hence science proceeds not by experiment
and/or observation coupled with theorising but by a recourse to the study
of Aristotle's definitions. Such a procedure is less likely to foster a
knowledge of the first causes and principles than to the situation:

"... when men speak such words as put together, have in
their no significance at all; but are fallen upon by chance,
through misunderstanding of the words they have received,
and repeat by note; by others, from intention to deceive
by obscurity. And this is incident to none but those,
that converse in questions of matters incomprehensible,
as the Schools-con..." ( Hobbes pp.62-63).

Third, of one holds a belief in the essential simplicity of the world 12
one must obviously disagree with the studies carried out in the Aristotelian
tradition. In such, enquiries are largely reinterpretations of what has
been written before 13 and tend to be far from simple. 14 Thus for anyone
believing in the simplicity of 'everything' the Aristotelian concept is
suspect.

The above three objections are not purely logical but are the result
of the introduction of new beliefs into man's theory of the way knowledge
was obtained. Indeed this seems to be the major objection. If one
insists that observations are the only way in which truth and falsity can be pinpointed than the Aristotelian concept must be replaced by a theory that does not involve the concept of substance. Yet one may still read passages of philosophers of science wherein scientists are said to find abstract constructions that, at first sight anyway, have much in common with the concept of substance. The idea of an abstract generalization is then a substance born of the Aristotelian assertions about its absolute truth. Hence despite the clearly irremovable concepts of observation and substance the generality of the latter remains an essential part of scientific discourse.

We may conclude that the Aristotelian notion of causality has been abandoned not because of what it said but because the way it said it was not open to observation. Broadly speaking, two attempts have been made to circumvent this difficulty.


dedeg

Our second alternative is commonly called the uniformity theory of causality. It hits upon the proportion that causality was said to have in the Aristotelian analysis and then attempts to formulate a criterion by which causes may be said to exist in purely observational terms. Such a procedure is essential since it was held that causality was the only means by which one could argue from the empirically observed to empirical observations not yet observed.

In this view, as in Aristotelianism, the mind is conceived as separate from that which is observed but unlike Aristotelianism this concept is at the kernel of the argument. The most extreme form of this doctrine states that the mind is simply a receptacle for impressions given quite neutrally from things external to it. Another view is simply that the world external to the mind can only be observed through the mind and hence is a
'creation' of the mind. Yet a third view is that the mind can impose ideas upon 'reality'. In all these views notice that we have thrown over the ancient Greek notion that truth is attainable solely through right argument. Reason is a faculty of the mind but truth is a property of things external to the mind. As a consequence, it is quite impossible to hold the doctrine of substance. Thus causes cannot be substances.

Instead they are simply relations between 'phenomena' (Kant), 'objects' (Hume) or 'ideas' (Locke). Hence, immediately enquiry is concentrated upon the relational properties of causality. Those we found were that a cause is prior in time to its effect, a cause is contiguous to its effect and a cause necessarily implies a given effect. Hence one has simply to set up an criterion by which observations can be said to support the existence of a causal relation. There were broadly three ways in which this was attempted.

First, Locke defined cause and effect as:

"That which produces any simple or complex idea, we denote by the general name cause; and that which is produced, effect." (Locke P,100).

Hence, we have the so-called productive theorists. As many have pointed out the definition is circular. Causality is a productive faculty but to define causality we must define production. Such a definition usually makes use of 'force' or 'power' both of which are equally nebulous. Hence in supplying a means of translating causality into observable terms the productive theory has failed. Since this is the whole purpose of the exercise such a concept is incompatible with our assertions which demanded the downfall of Aristotelianism.

Second the uniformity theory proper, Hume denied the substance theory simply by asserting there was no link between the cause and the effect that could be observed. If it was not observable it was 'reality'. Once that
was argued he had to set up a criterion of necessity that could be derived from experience only. If not, there was no way for anyone to argue theoretically since no inference from the immediate to the not-yet-observed would then be possible in his philosophy. That such inference was possible, however, was evident from the work/scientists.

His solution was exceedingly simple, and was based on the following principles:

"That there is nothing in any object consider'd in itself, which can afford us a reason for drawing a conclusion beyond it; and, that even after the observation of the frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience." (Hume P.139).

These led him to assert that all we can observe of causal relations is a constant conjunction over a long period of time. It is simply this uniformity of occurrence that leads the mind to pass from one object to another and so set up a causal relation between the objects. The connection between the objects then is neither a result of some great final cause (e.g. God) nor of some property of the objects themselves, but of their having occurred in a given way in our experience. On the basis of these relations one may engage in theoretical inference for it is a principle that we have no reason to infer anything about any object which we have not experienced. Constant conjunctions are experience.

This concept has been somewhat refined by later writers. Hill analyzed the concept into antecedent (cause) and consequent (effect) and asserted that:
"... every consequent is connected in this manner 
with some particular antecedent or set of antecedents ..."
(Hill A J P. 213).

Hence an effect is simply the consequent of a set of necessary conditions. 
(Since he talks of both causes and effects as facts we must suppose this 
set is not empty). To cause is simply to take the sum of necessary 
conditions (both positive and negative).

Ducasse has proposed the following definition of 'cause':

"Cause: A state or change X of an object is said to have 
been the cause of a state or change Y of another object, 
if the factuality of X ... was sufficient to the 
factuality of Y.

Effect: A state or change Y of an object, is said to 
be the effect of a state or change X of another object, 
if the factuality of Y was necessitated by the factuality 
of X." (Ducasse A J P.55).

Clearly it follows from the work of Hill. It is difficult in that he 
conceives of causality as 'the relation which obtains between the three 
terms of a perfect experiment.' (Ducasse A J P.16) It is a relation 
between two changes and a state of affairs. Simply there is one change 
C at time T, and another E at an immediately sequent time T+1.

In more recent times work has concentrated on the representation of 
causal statements in logical languages. This follows the work of Carnap 
in the 1930s. If it can be represented in logic there would be a means 
of deciding 'a priori' whether a causal statement is true or false. Thus 
based upon an analysis of conditional statements we have had several 
atttempts on these lines. Notably Durkin's analysis based on the concept 
of causal implication and Carnap's modal logic (see Chapter Five). Hence 
causality is simply a uniformity between two objects of experience of the
if A then B.

Thus we merely have an entailment relation between two objects. Such a uniformity is generally called a causal law.

Discussion of this concept must be clarified by a separation between individual causal statements and causal laws. Let us take particular causal statements like:

John is the cause of Mary's distress.

By the uniformity view we can only make a causal statement for long runs of data. Hence unless John is always constantly conjoined with the distress of Mary we have no justification for making such a statement about one incident. Causality in the uniformity theory then, is really a causal law or generalisation. Only in this way can we posit some kind of unique relation between 'Mary's distress' and 'John', otherwise we could relate 'Mary's distress' to the 'hour of the day' or the fact that an 'elephant ran amok in some remote village in Asia'.

We are dealing with causal laws only in this view. In other words, we are analysing statements of the form:

For all occurrences of John, John is always constantly conjoined with Mary's distress.

As Ussishen has pointed out such statements can not be verified, since we may never be sure that all occurrences of John have been observed. Hence our only criterion by which we can test the validity of the statement is by trying to refute it. Yet in practice this is impossible since we would not have made the statement in the first place unless it satisfied
all our observations. Hence we arrive at the paradoxical conclusion that
if observation leads us to make causal statements, those statements cannot
be verified, but if they can be refuted we are not entitled to make them in
the first place.

It is, however, important to remember that the argument of the
uniformity school is simply against the view of causality as a something we
can observe. However, in this the argument is not wholly satisfactory.
Observation leads, on the basis of a constant conjunction, the mind to
proceed from the observation of one object to another. In this process we
have two movements. First that between objects and second that between
ideas. It is the evidence that two objects are constantly conjoined that
leads us to associate ideas in a causal manner. In this process there is
a hidden dichotomy of thought. There is a relation in the mind and another
in the world. The former is supposed to take the form of the latter, that
in the uniformity analysis it does not is of no consequence. All that
need be pointed out is that the uniformity theorists denied any relation
as causal unless it took, in fact, the form of a conditional but they
refused to go to the extent that a conditional was a causal relation which
was the logical consequence of their denial of 'substantial' and 'productive'
causes, given their view of knowledge.

We now move to the third group of theorists who assert that pure
conceptions of the mind may be imposed upon the phenomena which form their
basis and define their boundaries. If such activity of the mind is possible
then we can assert that causality is nothing but a 'necessary fiction' or that it is an abstraction from given data. The choice of one view or
the other depends largely upon the view of the way the mind constructs
representations. If it can invent any fancy it pleases to explain
phenomena, then one takes the former if fancies are conditioned by
observation, then the latter. We shall deal with the latter alternative here and examine the former in the next section.

Here we shall deal only with Kant's concept of causality that is found in The Second Analogy. If we are able to impose pure conceptions of the mind upon the things we observe then it must be such that the representations contained in our mind correspond to the phenomena which give rise to them. It is clear that to think is to be aware of a manifold of phenomena that is external to the self. Hence, by asserting that pure conceptions exist we have asserted a manifold of experience external to the thinker. Since a manifold is, by definition, diverse we cannot be aware of it as a manifold unless some kind of synthesis takes place whilst we are representing it into our mind. Given that this proceeds the representations in the mind are conditioned by phenomena and are also products of pure thought. It also follows that all we know is that which is unified by this synthetic process. It follows that we can only know ourselves by the same means and so to be conscious is to be oneself and consciousness is simply self-identity.

Given this, we can proceed to The Second Analogy. In representation no empirical cognition is possible without a synthesis in the imagination, (i.e. to associate a with b one must on cognizing b remember a), which is always successive. By this synthesis the order of this succession is not determined. Only in the synthesis of apprehension can this succession be determined "... according to which something necessarily proceeds, and ... something else necessarily follows ..." (Kant C[2] P. 155).

Now if this representation contains a cognition of an event it must be an empirical judgment; hence if a phenomena is posited as antecedent and the
posed consequence did not occur the representation is simply a figment of the imagination. We must conclude then that our holding of the relation of cause and effect between phenomena is a condition of the validity of empirical judgments.

Causality then is seen as a representation of a relation essential to all statements about phenomena yet a result of a purely intellectual process. It is an a priori conception which is:

"... the principle ... of objective cognition of phenomena, in regard to their relations in the succession of time." (Kant P.155).

As such causality is not related, as a concept, to particular experience.

The above described theory is hardly conclusive, however, for no precise means for determining a causal relation were implied by it. We are simply given a method of referrong our hypothesis:

'on observation of A we predict an observation of B will follow'

to the facts. If the hypothesis is refuted no such causal statement is justified. Notice that again we refuse to accept the link between cause and effect as substantial but merely as a result of testing procedures.

Subsequent discussion on those lines has brought the whole debate very much into line with that previous associated with the previous theory. Thus we read Popper asserting that the 'Principle of Causality' can be restated as a methodological rule as follows:

"... we are not to abandon the search for universal laws
and for a coherent theoretical system, nor ever give up our attempts to explain causally any kind of event we can describe. (Popper [2] P: 61).

As such, it is simply a corollary to the Kantian analysis in that if causality in the basis of all empirical judgments which are the subject matter of science then a notion of causality is essential for the conduct of scientific enquiry.

It is as an extension of this view that Karganou analyses causes as either partial or total. Those, it is argued, refer to the state of a physical system in that a total cause is the state of this system at time \( t \), say \( A \) and total effect is the state of the system at time \( t \), say \( B \). Partial causes are then simply summed together to obtain \( A_1 \). A certain system is then deemed to be causal if the laws of nature governing closed systems do not contain the time variable in explicit form. (Karganou P.405). With this we have a more complex form of the constant conjunction postulated by Hume.

The so-called Kantian view of causality is like the rest, not immune from criticism.

The view that in perceiving phenomena we are engaged upon/synthesis which implies a necessary relation between phenomena can be attacked thus. Synthesis of the sensations of experience to produce representations of the phenomena may be necessary but it is not sufficient, for a given representation corresponding to an empirical cognition. Thus although the cognition of an empirical entity conditions the empirical judgment it does not determine it. Hence, although our representation refers to the relation between phenomena they are not necessarily the case. Only if experience creates representations or representations experience is this so. We can see then that we have turned the whole question of causality away from causality between objects towards a relationship between phenomena.
which are moments of the perceiving mind. As such the main problem of
'what is the nature of causality?' has been converted into 'how can we
know of causality?')

Such a view, however, highlights the whole relationship between the
causal statement and the causal relation between phenomena. The statement
is a representation of an empirical cognition. It takes the same form as
the cognition. Thus the whole form of Kantian argument implies this
double relationship. Unfortunately, the argument does not give reasons for
a one to one correspondence as we have seen.

Taken as a concept, the uniformity theory is not free from objectivism.
It relies upon a correspondence of the theory with the facts for its
truth. Hence we can say simply:

\[ \text{the statement } 'a \text{ causes } b' \text{ is true iff } a, \text{ in fact,}
\text{causes } b' \]

in our criteria for truth. The difficulty lies in deciding whether a
does, in fact, cause b. We may find only that a does not, in fact, cause
b since we can only observe uniformities. Hence, we have a relationship
of 'necessity' between the existence of a and of b that can only be proved
to be non-existent. That this necessity is of a very different form than
that contained in the works of Aristotle is obvious. It follows that we
have changed the meaning of causality and not merely its perceptual basis.
As such the theories of uniformity must always be opposed by the concept
of substantial cause.

But more than this we have seen the establishing such a correspondence
is, in principle, difficult. Thus, although, in principle, we have defined
causality in terms of observables the determination of these observables
is not wholly without conceptual difficulties. For instance to define
not only the observations necessary for such definition but also the net
of objects, events or whatever in which the observations must be made.
If this is done in terms of observables, we have an infinite regress of
observation. Hence, if causality is an observed uniformity between
observables then we must define observables in observable terms if our
observables are to be purely observable. This leads us into an infinite
regress. If, however, we argue a priori to prevent this we are compelled
to admit that causality is not a purely observational concept. Hence, we
must to the extent that criticisms of Aristotle were based on the assertion
that substances were pure imagination, conclude the uniformity analysis
in subject to the same kinds of criticism that were its foundation.

In particular, we must accept the unsatisfactory state of affairs that
everything observed is observed or rather registered as observed by the mind
and as such is purely subjective. We can merely conclude that any causal
statements are simply uniformities which are inter-subjectively held. Truth
simply is this so-called objectivity. It must be clear, however, that the
communication of a causal statement is the implanting of a representation
in another's mind. There is no necessity why this representation should
be precisely that notion encountered by the first observer. Hence, it is
merely the form of a statement not the proposition of the statement that
is held to be objectively true. In those circumstances there is no
certainty that science is a process by which more and more is learnt of
a particular set of phenomena. Instead science may purvey a succession of
arguments whose acceptance is governed by preference, fashion or political
ends rather than the extent to which they conform to cognitions of empirical
entities. Only if propositions (i.e. the meaning of sentences) can be
examined without reference to their form in this means of finding truth or
falsity incapable of this sort of mishandling.\textsuperscript{37} Let us now pass on to the third type of causal theory which has been at various times called the activity theory\textsuperscript{38} or the volitional theory.\textsuperscript{39} We shall term this the phenomenalist theory.

Phenomenalism

It is argued that all observation is impossible without a mind. Not only must sensations be imprinted upon the mind but also those sensations must be interpreted, in order that the observations may be made. Thus observation is, in a sense, the learning of a language;\textsuperscript{40} it is the process by which the mind learns to associate a given sensation with a given idea. Hence, the observation of a constant conjunction must be interpreted before it is called causal. Not only this but also the concept of causality based on observation is an idea of the mind. So far we have much the same sort of argument that Kant put forward.

Given the above and given that the mind has a free agency or will it is possible for causality between phenomena to be simply a mental process. An observer can establish causality by examination of his motivations and desires. Causality is derived from the ability of observers to use constructs in order to understand processes that are observed.

This theory is usually conceived in terms of the relationship between the mind and the body. Since we have a knowledge of ourselves as causal agents our actions can be conceived as being an effect of that will.

Thus it is argued:

"We get our knowledge of causation from our knowledge of motivation." (de Chanc P.9)
If we motivate a certain action, this action usually follows and a deed is performed. From this the notion that nothing that is absolutely quiescent will suddenly change unless something else external to it forces it out of its quiet. From this it is a short step to the principle of causation: namely, every effect has a cause and every cause an effect.

At first light we have arrived at the principle of causation by a simple argument. Against it we may simply argue that we can never be certain of our motivations. There is no way in which we can say that our motivations are not conditioned by our bodies. Hence, if we are not able conceptually to separate motive from action we are not in a position to separate cause from effect.

But let us assume that we can. Since we are not in a position as observer to note the exact links between our motives and our deeds there is no reason for us to assert a link between two phenomena except that one follows the other. Thus we have merely a restatement of the uniformity theory. It is limited, however, in two ways. First causality seen as an analogy between the relationship of mind to body and between phenomena implies that causality is a phenomenon peculiar to organic observers. Thus causality only obtains in this view when an observer is present.

Second the phenomenon of causality is simply a construct of the observer’s mind, a device by which the world is made more intelligible. From this we would conclude that the laws found by science would not necessarily be those found by investigators who have no recourse to causal categories. Now this means that we have no right to assert these kinds of causal statements as laws of physical processes. For it may be that the laws cease to operate when they are not being observed in this manner. Under these circumstances it seems better to hypothesize that the laws engendered by scientific enquiry are universal in space and time rather
than leave the possibility of exception that cannot be investigated or hypothesized.

In this analysis, however, the same general form of causality is underlined; namely as an analogy between a formal abstraction and a relationship between phenomena. A causal statement, then, is simultaneously a statement of causality and a statement about that causality, and this is the major lesson we have learnt from our historical analysis.
In the previous chapter we found indications that causality as a relation could be interpreted simultaneously in two ways. First it could be seen as a link between observables or, at least, objects supposed to be external to the mind. Second it was a statement about that link on a more formal level. We may, therefore, see it as a 'formal language' in intentional isomorphisms with an 'observable language'. As such we shall term causal language a materialised language. It is a natural transition from those considerations to the construction of two languages one as a metalanguage to the other (termed object language). This road has been taken by Burks, Carnap and Barcan Marcus amongst others.

For a number of reasons the so-called conditional statements of the form

'if a then b'

are not suitable for talking formally about causality. These reasons are well known thus we shall not recapitulate them. One important feature of the conditional is simply that it is true if and only if either the
antecedent is false or the consequent is true. Now it is quite obvious that the statement:

'\(a \text{ causes } b\)'

is true if and only if both \(a\) and \(b\) are true or both \(a\) and \(b\) are false.

Hence we may not inter a causal statement in the body of a conditional for we would want to be able to argue that a causal statement is false if:

\[
\neg (\neg a \wedge b)
\]

It has also been suggested that 'a causes b' can be stated as well by:

'a is the cause of b'

Since this is the 'standard logical form' of statements such a formalisation would have great potential advantages. It does, however, suffer from at least two drawbacks. First by the rule of substitution we may write:

'a = c''

If we now have another statement:

'b is the cause of c' or 'b = d''

Only if we make the postulate:

'b = c''

is this logically possible. But this statement says:

'b is the cause of b'

which contravenes the most ancient ideas of the nature of causality; namely, nothing can cause itself.

Second, although in terms of common language 'is the cause of' is equivalent to 'causes' there seems to be a confusion implicit in such
usage in logic. In the phrase 'is the cause of' 'a' is categorized as the cause. It is seen as something different from the effect 'b'. The use to which the phrase is put is the same as 'causes' but that to which it refers is not the case, for 'causes' implies a process in which the states/objects etc. appear. To say 'is the cause of' must imply an axiom which relates 'is the cause of' to a process-like link. At the same time it appears that 'causes' need not have this axiom since 'causes' is the name of a process. If such an axiom is needed, it is clear that the supposed intentional isomorphism implied by the term 'materialized language' no longer exists. Hence we must find that the formulation under discussion is unacceptable.

It appears, then, that ordinary propositional logic is not suited to act as our meta-language. Hence attempts have been made to introduce causality into modal logics. Here we shall follow Burke's formulation of a causal logic. We start with the concept of causal implication which is symbolized:

\[(x) (E_x \land D_x)\]

It simply asserts that the conditions expressed by $E_x$ are causally sufficient to make $D_x$ true. The definition of sufficient conditions is in important respects identical to that of Mill. It runs:

"\[\text{sufficient conditions means:} \] a set of conditions complete with respect to negative as well as positive cases sufficient to cause the state of affairs expressed by the consequent." (Burke P.360)

Apart from 'a' Burke also introduces the concepts of causal possibility $◊$ and causal necessity $\Box$.

His argument is roughly as follows. A strict implication $\rightarrow_3$ of the form $\langle p \rightarrow_3 q \rangle$ is defined as:

\[\langle p \rightarrow_3 q \rangle = \sim_0 (p \land \sim q)\]
This simply asserts that in a strict implication it is not possible that $p$ is true and $q$ is false. No other properties are assumed. First:

$$(p \land (p \rightarrow q)) \rightarrow q$$

Second:

$$(p \land q) \rightarrow ((p \rightarrow q) \rightarrow p)$$

Clearly these properties are analogous to the formal properties of a causal statement:

if 'a causes b' and a is observed then b will follow

and

if 'a causes b' then an observation of b means that a has gone before.

Given this analogy and those asserted between $\Box$ and ordinary necessity $\Box$ and between $\Diamond$ and ordinary possibility $\Diamond$ we arrive at a parallel between modal logic and causal propositions. It is, therefore, concluded that causal propositions have a modal character.

At this point it might be as well to define what is generally meant by modal. Prior has defined it thus:

"To count as a modal logic ... a system must contain a pair of one-argument operators forming statements out of statements with the following properties: The more powerful modal operator $\ldots \Box \Diamond \ldots$ must be such that $\ldots \Box \neg p \Diamond \ldots$ is a stronger form than $p$ and yet not so strong as to be never true $\ldots \ldots$ And the weaker modal operator $\ldots \Diamond \Diamond \ldots$ must be such that the weaker, more non-committal form than $p$ and yet not so non-committal as to be never false $\ldots \ldots$ Finally, $\Diamond p \ldots$ must be equivalent to $\neg \Box \neg p$ and $\Box p \ldots$ to $\neg \Diamond \neg p$." (Prior pp.2-3)."
All this means is that there is a definition of logical necessity such that it is not impossible for any proposition to satisfy it. For example, if logical necessity is taken to mean the intimate relationship implied by the Aristotelian notion of causality, i.e. the idea that water causes water vapour by corruption, then no sentence containing necessity can ever be true. If water necessitates water vapour in this sense then no sentence containing two entities (in Carnap's meaning) can ever be found to be true. This is because water becomes water vapour, thus it not only necessitates the latter but is the latter. Hence, we have simply:

\[ \square (\omega = \omega.v) \equiv (\omega = \omega.v) \]

which contradicts the meaning of necessity here implied? The rest of the quotation seems to be quite straightforward.

Now taking this definition of modality we can immediately show that Burke was not altogether correct in his assertion. Because of the comments on necessity above and because of the nature of causal implication it seems that certain sufficient conditions for causality must be excluded. All commentators agree that the specification of a certain state of affairs is of paramount importance in the definition of a causal relation. Hence, statements like:

'the bronze is the material cause of the statue'

though not usually regarded as causal by the production theorists are seen as statements of conditions necessary for the causing of a bronze statue. But if necessity implies the necessity of truth and the impossibility of falsehood we cannot include statements as above since its truth precludes:

'the stone is the material cause of the statue'

as being true. Here we have two choices - either to agree to exclude such forms of necessity from our consideration of causality or to deny at once that causality is modal. Quite obviously Burke takes the former course.
Thus we must re-specify his definition of causal implication to:

... a set of conditions, complete with respect to negative as well as positive properties sufficient to cause the state of affairs expressed by the consequent provided, the inclusion of a proper subset of that set does not make it impossible for the implication ever to be proved to be true.

Hence, his causal implication always represents a true relation between the respective states of affairs. Now is this is so it is clear the truth values of all the subsets of conditions must correlate. Hence, we can argue if this is so for one cause A it is so for another B if the statement:

\[(A \land B \Rightarrow C)\]

is to be true. Since only if both A and B are true is the statement true we must conclude:

\[(A) \Rightarrow (B \land C)\]

for \(A \Rightarrow B\)

Now this obviously is not so in causal analysis. For example it implies that if inflation is caused by a combination of a lack of monetary stringency and excessive union bargaining power then in times of monetary expansion excessive union bargaining power will cause inflation. Unless there is a link between monetary policy and the bargaining power of unions the statement is obviously false. We have argued, then, that Burka contradicts his own theorem \(F_2\) that:

\[((A \land B) \Rightarrow C) \Rightarrow (A \Rightarrow (B \land C))\]

is false.

Let us now move on to a statement of Burka 'causal logic'. Here we shall not review the system of axioms but merely, by the accepted conventions
of language construction, state the vocabulary, formation rules, definitions and rules of inference which are needed.

**Vocabulary:**

the logical constants \( \neg, \land, (, ) \) and the universal quantifier;

an infinite list of propositional constants and variables;

individual constants and variables and functional constants and variables of all degrees;

the primitive symbols \( \Box \) and \( \Diamond \)

A formula is any finite sequence of primitive symbols. A well-formed formula is any formula which satisfies the following:

1. is a propositional variable or constant,
2. if \( B (\alpha_1, \alpha_2, \ldots, \alpha_n) \) is a functional variable or constant of degree \( n \) and \( \alpha_1, \alpha_2, \ldots, \alpha_n \) are individual variables or constants,
3. \( \neg A, (A \land B), (\alpha) A, \Box A \) and \( \Diamond A \) where \( A \) and \( B \) are well-formed, and
4. no other well-formed formulae exist.

**Definitions:**

\[
\begin{align*}
(A \lor B) & = (\neg A \land B) \\
(A \land B) & = \neg (\neg A \lor \neg B) \\
(A \equiv B) & = ((A \land B) \lor (B \land A)) \\
(A \rightarrow B) & = \Box (A \lor B) \\
(A \land B) & = \Diamond (A \lor B) \\
(A \lor B) & = (\neg A \land (A \lor B)) \\
(\exists \alpha) A & = \neg (\alpha) \neg A \\
\Box A & = \neg \Box \neg A \\
\Diamond A & = \neg \Diamond \neg A
\end{align*}
\]

**Rules of Inference:**

I. If \( A \) and \( (A \lor B) \) then infer \( B \)

II. If \( A \) is an axiom then so is \( (\alpha) A \)

III. If \( A \) is an axiom then so is \( \Box A \)
From this we can prove the following important theorems:

**Theorem 1**

If \( \vdash A \supset B \) then \( \vdash \Box A \supset \Box B \)

**Proof.** First we have to prove a subsidiary theorem.

If \( A_1, A_2, \ldots, A_n \vdash B \) then \( \Box A_1, \Box A_2, \ldots, \Box A_n \vdash B \)

which is proved as follows:

Let \( B_1, B_2, \ldots, B_n \) be a proof of \( B \) on the given hypothesis.

Let each \( A_i \) be replaced by \( \Box A_i \)

Let each axiom \( C \) be replaced by \( \Box C \)

Now \( (a \supset b) \Rightarrow (a \supset b) \) obviously

and \( (a \supset b) \Rightarrow (a \supset b) \) by analogy

thus \( \Box (a \supset b) \Rightarrow \Box (a \supset b) \) by analogy

thus \( \Box C \Rightarrow \Box C \) and \( \Box C \) by I.

Replace any \( B_j \) that is neither an axiom nor a hypothesis by \( \Box (B_j \supset B_i) \Rightarrow (\Box B_j \supset \Box B_i) \)

Thus \( \Box B_j \supset \Box B_i \)

Thus \( \Box B_i \), where \( B_j \supset B_i \) and \( B_j \)

are the premises from which \( B_i \) was inferred in the given proof.

Thus \( \Box A_1, \Box A_2, \ldots, \Box A_n \vdash B \)

Low given \( \vdash A \supset B \)

and given \( \Box (A \supset B) \Rightarrow (\Box A \supset \Box B) \)

it follows \( \vdash \Box (A \supset B) \) which is proved above.

Hence \( \vdash \Box A \supset \Box B \) QED

Let us examine the proposition. It states that in this logic that

if we can prove that if \( A \) then \( B \) we can also prove if the causal necessity of \( A \) then the causal necessity of \( B \). Now the antecedent of the theorem must be true as we have seen and since causal necessity cannot be false so must the consequent. Yet to have any system of logic in which an conditional statement is a necessary condition for a conditional implying causal necessity seems to be flying the face of all that is known about causality. From our discussion of the conditional in relation to causal
statements it is quite plain that it would not be proper for us to say that because we can prove:

$$A \supset B$$

to be true we can show that

$$\Box A \supset \Box B$$

in the case that \(\sim A\) Either these are simply logically true in which case it seems we must forget about causal necessity being different from a conditional\(^{10}\) or the theorem we are considering is nonsensical. Since the latter hypothesis appears refuted, it would seem that Burks has ignored the important property a statement of which began this chapter.

Another form of criticism has been proposed by Henderson. He sets up a standard as follows:

"... wherever conclusions which purport to be validated by strict or causal implication can be validly deduced in terms of material implication, to the extent and in that context the case for the non-triviality of strict or causal implication will be non-proven" (Henderson P.507)

Now an attempt to reduce the logic of causality of Burks to so-called 'material' logic must face the difficulty that if modal logic is non-trivial in relation to truth-functional logic, then any system storable in modal terms will be irreducible to truth-functional logic. This is because inclusion of possibility into a statement of the sort:

$$\left( \Box p \land \sim (p \supset q) \right) \supset q$$

means that the truth value table for \(p \land q\) cannot be completed. Thus we may not interpret the modal logic in terms of truth-functional logic if possibility is allowed into the interpretation. So no integration of
the findings of truth-functional logic into a modal logic is, in general, possible. Hence, to argue modal logic is a more suitable tool to represent causality by analogy (as Durkheim does) is simply to argue the systems are different. It does not mean that causality is adequately represented.¹¹

One wonders, then, if causality is representable in any logic. I believe it is not. The proof (which is as yet only tentative) is given below:

**Theorem II** If a statement in a colloquial language¹² can be represented in any logic then it is impossible to prove by means of that logic that it cannot be so represented.

**Proof.** By assumption ∃f ⟨c, L⟩; f ∈ C, f ∈ L

Hence logic would be inconsistent if:

(∃ψ) Y ⊢ ¬∃f ⟨c, L⟩; f ∈ C, f ∈ L

which implies in a consistent logic we cannot deduce

¬ (∼ ∀f ⟨c, L⟩; f ∈ C, f ∈ L)

which implies that no such proof exists.

**Theorem III** If a statement in colloquial language cannot be represented in any logic then it is impossible to prove by means of that logic that it cannot be so represented.

**Proof.** By hypothesis ∼∃f ⟨c, L⟩; f ∈ C, f ∈ L

By definition there is no such statement in logic that can contain such a function. Thus the set:

F = F \{f₁, f₂, …, fₙ\}

is empty.

If so we may not speak of the postulated representation hence we may assert in a consistent logic:

(∃ψ) Y ⊢ ∃f ⟨c, L⟩; f ∈ C, f ∈ L

though we may never prove that statement.

Thus

¬ (∼ ∀f ⟨c, L⟩; f ∈ C, f ∈ L)

Again we imply no such proof exists.

**Theorem IV** It cannot be proved that a statement in colloquial language
is not representable in any logic.

**Proof.** Let $R$ stand for representable and $P$ for proof of representation

- $R \rightarrow \sim P$  
- $\sim R \rightarrow \sim P$  
- $R \lor \sim R$  
- $\sim \sim P$  

We know that we cannot show by means of logic that that logic is both consistent and complete. Moreover we can demonstrate that a given logic is either inconsistent or incomplete, by means of the arguments that can be constructed within it. Let us refer to this as internal inconsistency or internal incompleteness.

**Thm V** A colloquial language must be internally either inconsistent or incomplete.

**Proof.** If colloquial language can be shown to be internally complete and consistent then there is no sentence which cannot be deduced consistently within it:

$\sim \exists x \sim c_x \vdash s'_x$

But in logic we know:

$\exists y, \sim c_y \vdash s'^y$

Given the axioms and assumptions of both colloquial language and logic are identical then the following equality must hold:

$(c \equiv l) \rightarrow s_c = s_c (s'_1, s'_2, \ldots, s'_n) > s_l (s''_1, s''_2, \ldots, s''_m)$

where $n = m$.

The range of the function $f$ is not the case in both logic and colloquial language. Thus the two 'languages' are not in intensional isomorphism so no such function is possible. This contradicts Thm IV.

But if we assert:

$\exists x, \sim c_x \vdash s'_x$

Thm IV still holds together with the identity between

$s_c (s'_1, s'_2, \ldots, s'_n) \text{ and } s_l (s''_1, s''_2, \ldots, s''_m)$

Hence we conclude that colloquial language must be either internally inconsistent or internally incomplete.


Tru VI

A colloquial language can only be represented in any logic if both it and the logic are internally inconsistent.

Proof:

We shall use a proof by cases:

Case 1: If both languages are consistent then we may not prove that no representation is possible. But simultaneously we cannot know that the ranges of both sets of sentences that can consistently be deduced coincide since in each language there are sentences which are consistent with it but cannot be deduced from it. Hence we may only assert the function within certain unknown limits.

Case 2: If one language is consistent and the other inconsistent, we can argue that for the consistent language the above holds. For the inconsistent language both the above and its formal negation holds. Hence:

\[ \exists f \langle c, L \rangle; f \in C, f \in L \]

for \( C_i, L_i \) where \( i = \) any known relation

and

\[ \neg \exists f \langle c, L \rangle; f \in C, f \in L \]

Hence since both languages must correspond to the best that can be achieved is a function with unknown limits.

Case 3: If both languages are inconsistent, three important follows:

(a) the great advantage of logic as a means of arguing is lost. Indeed the point of trying to represent causality in logic is lost.

(b) We can deduce that we can prove a given representation in causality is impossible and that it is possible.

(c) Despite the fact that our representation in both possible and impossible we are certain of the ranges of both statements and the function in this case. In practice
we can draw up the representation but the criteria of representation (i.e. intentional isomorphism) is probably no longer useful.

Thus it follows here we can know if our representation satisfies our criterion. We cannot, however, be certain that the criterion is still satisfactory.

It follows from this that causality is never truth-functional because unless causality is representable in a consistent logic there is no way yet known that can give the truth value of a sentence from the truth value of its constituent elements. We must also assert that since we can never know that a given representation of causality is adequate by our criterion we are not able to answer questions about causality by appeal to logic alone. It is a mistake, therefore, we contend to attempt characterizations on this basis. Hence we have a basic criticism of those econometricians who have attempted this sort of analysis.

Let us now analyze the reasons for this result rather more closely. We have seen two sorts of attempts to characterize causality. First, we have noticed that it is not simply a relational conjunction and that the conditional is inadequate. It was also impossible, we found, to examine causality in terms of an enlarged propositional logic which included an equality sign. Second, that since causes take the form of verbs modal logic was used to formalise causality. We found that this, too, failed and we could have mentioned the assertions of Vendler, Pain and Langford and Langford in this vein. All rely on an analysis of causal language and all are, therefore, susceptible to the attacks of Shorter. It is imperative to note that although causal expressions appear in common language, they are used loosely and usually without definition. To induct regularities from this usage will never be completely immune from the criticism that
what is induced is artificial (in that it does not reflect the spirit of the language) and hence unjustified.

Evertheless, we must use the tools given us by the (broadly speaking) linguistic philosophers to attempt an explanation of the results we have found. In particular, there seems to be a crucial difference between causal statements and conditionals. For this we shall need two object languages $S_1$ and $S_2$ which are respectively propositional logic and causal language, and a meta language, $\mathbb{M}$, which is a suitable part of English. Now $S_1$ has been formalised satisfactorily. However, we have seen, cannot be so formalised. Indeed causality is usually expressed in colloquial language thus $\mathbb{M}$ contains $S_1$. We therefore have only to deal with $S_1$ and $\mathbb{M}$. In performing this analysis, then we have only to talk about $S_1$ to compare statements in $S_1$ with causal statements which are in $\mathbb{M}$.

In that which follows we must make it plain when we talk about a statement and when we talk ‘within’ it. Thus we shall adopt the following convention:

In talking about a sentence we shall give it a name by including it within quotation marks. In symbolism this will take form of German script.

Now $S_1$ to be complete must contain (1) the vocabulary, (2) the formation rules, (3) rules relating sentences and variables to the description language, (4) rules of truth and (5) rules of ranges. Let us define truth for a sentence comprising a predicate and an individual constant:

"An atomic sentence in $S_1$ consisting of a predicate followed by an individual constant is true if and only if the individual to which the individual constant refers possesses the property to which the predicate refers."

(Carnap, P. 5).
In what follows we shall use the concept of L-truth which is taken to be equivalent to Liebniiz's necessary truth and analytic truth of Kant.

For this we need the concept of state description. This for an atomic sentence in \( S_i \) in a class of sentences which contains for every atomic sentence either this sentence or its negation, but not both, and no other sentences. (Carnap P.9). If a sentence is true if its state description is true then it is said to hold in a given state description. All these state descriptions in which a sentence \( \mathcal{C}_i \) holds is, as a class, called the range of \( \mathcal{C}_i \). Let us now define L-truth.

A sentence \( \mathcal{C}_i \) is L-true in \( S_i = \mathcal{C}_i \) holds in every state description in \( S_i \).

We shall also need definitions for equivalence and L-equivalence.

**Equivalence:** \( \mathcal{C}_i \) is equivalent to \( \mathcal{C}_j \) in \( S_i \) = the sentence \( \mathcal{C}_i \equiv \mathcal{C}_j \) is true in \( S_i \).

**L-equivalence:** \( \mathcal{C}_i \) is L-equivalent to \( \mathcal{C}_j \) in \( S_i \) = in \( \mathcal{C}_i \equiv \mathcal{C}_j \) is L-true in \( S_i \).

Now let us examine the sentences in question. For this we need two new concepts; namely, intension and extension. Simply put we say something has a given extension if it belongs to a certain class of things \( \sigma \mathcal{C}_i \) in the sentence 'A is a cause'. We can argue:

A belongs to the class of causes

hence the extension of A is the class of causes. Intension is simply the possession of something of a given property thus in the above example we could say:

A has the property of being a cause

hence A's intension is the causal property.

On these lines we can define extension and intension in sentences.

Carnap defines them thus:
"The extension of a sentence is its truth value." (P.26)

"The intension of a sentence is the proposition expressed by it." (P.27)

It is clear that we are able to define these crucial concepts without recourse to the causal part of it. Hence we can define for causality concepts which are needed in order to talk about causality in the same language with which we talk within causality; namely, English. This means that language of which causal language is a part forms not only the meta-language of causality but also the object language. That this is not so for a simple formal system $S$, is clear also from the above.

Now let us take the two sentences:

$$A \rightarrow B$$

and

$$A \text{ causes } B.$$

The intention of the conditional is the proposition 'if $A$ then $B$'. It simply says that $A$ is a necessary condition for $B$. Its extension is its truth value, i.e. '$A \rightarrow B$' extension is the truth value that if $A$ then $B$, which may or may not be true. A causal statement has the intention of $A$ causes $B$ and its extension is the truth value/that $A$ causes $B$. In the conditional sentence the proposition if $A$ then $B$ need have no relation to the sentence's extension; whereas the proposition $A$ causes $B$ is related to its extension.

In the sentence:

'\text{A change in the money supply causes a change in interest rates}'

the proposition is simply a posited relation between two changes. The extension of this sentence is whether it is true or false, (or undetermined). Now it is clear that the truth value can only be determined by reference to factual material, whereas the proposition it asserts is presumably deducible from other sentences. Yet the statement is not simply a
proposition but a statement about the world, since this is the only means of defining its extension. Because of this, however, the proposition is only fruitful as long as its extension is known. If the extension is not known, then the proposition itself is emptied of all significance. This is not so of a conditional. The proposition expressed therein is significant or not depending on the truth value of the sentence.

It would seem, then, that causality is a peculiar language. Statements within that language not only make propositions about a state of affairs but also their form precisely mirrors that state of affairs. Undoubtedly this must be derived from the method by which causal statements are constructed. Since causal statements are scientific, or supposedly so at least, this leads us to consider the formation of scientific statements in general.
CHAPTER SIX

On the Nature of Scientific Judgments

Before we can consider the precise nature of the process of causation, we must examine more closely the form which causal statements, if they are to be scientific, must take. It is the purpose of this chapter to analyze the nature of scientific judgments and the form of scientific statements. We shall, therefore, first determine the essence of scientific judgments, second the nature of scientific statements and third the nature of causality. This is the programme of the next three chapters.

Scientific judgments are generally seen in one of two lights. Either they are constructs of the mind which can be seen as 'heroic' generalisations of phenomena or they are simply syntheses that are suggested by an examination of phenomena. We can characterise the difference by equating the set of the first type with the properties of the phenomena plus a generalising agency whereas the second sort are merely an asserting of a general characteristic of all the properties. Generally speaking, the first sort are called deductive judgments and the second inductive judgments. As with all generalisations, this dichotomy is misleading and inaccurate hence we shall try to explain more clearly the distinctions in method.

For our paradigm of the deductive method we shall take Taraki.
explains this very much as if he were constructing a calculus. Ideally, the meanings of every expression and the means by which these are achieved must be clearly exhibited. This ideal can never be attained since to explicate an expression without a vicious circle one needs another expression and so on towards an infinite regress. To prevent this we must begin with a set of primitive or undefined terms by means of these we may use definitions to construct defined terms.

The case is true of statements. There are certain primitive statements or axioms whose truth is accepted. Any other statement in, however, unacceptable unless its validity can be established, such statements which are no acceptable are called theorems. Theorems are thus said to be deduced from other statements. An example may be given as follows. If we set up a vocabulary in which there is an infinity of variable signs \( A, B, C, \ldots \); a set of constant signs \( (, ) \), \( \Rightarrow, \land, \lor \) and rules of 'sentence' formation as follows:

(a) any variable is a well-formed sentence
(b) \( A \lor B \) is well-formed
(c) \( A \land B \) is well-formed
(d) \( A \Rightarrow B \) is well-formed
(e) these are the only well-formed sentences.

Also formulas are defined as combinations of well-formed sentences and the following deduction is valid:

\[
(((A) \land (A \Rightarrow B)) \Rightarrow B)
\]

Given the above primitive vocabulary only certain formulae can be validly deduced by the rule of deduction. These formulae are called theorems. It is hoped it is clear that the example is of a similar form to the deduction of Euclid we presented in the previous chapter.
It is probably unfair of us to use this formulation in the context of the social sciences but it is as well for social science to realise that only:

"The method of constructing a discipline in strict accordance with the principles laid down above is known as the deductive method..." (Tarski §3 pp. 119-120).

Hence we must conclude that unless social scientists are to become unconcerned with facts can social science be truly termed deductive.

It must be seen, however, that much activity in science deals with solely deductive problems. In Economics, for example, we have had many largely deductive debates in the last twenty years, notably the nature of long-run equilibrium in imperfect competition, the nature of the demand for money schedule, what form the consumption function takes, the re-exposition of 'marginalist' theorems in terms of linear programming and the nature of decision under uncertainty. All these debates are concerned primarily at obtaining known results by new arguments. Sometimes, however, it happens that such an argument can be used in quite different fields to explain certain facts. Perhaps the most obvious example in Economics is the use of Welfare Economics to explain certain political activities.

But what of induction? For our paradigm here let us, more fairly, take Braithwaite. Our belief in a theorem of a deduction is obvious since the arguments in favour of the belief are the inferences which led to the theorem. In an induction there is no such automatic justification for it is quite possible that an induction may be false despite the reasons given for it being true. This is simply because an induction is an argument from particular instances to the postulating of a generalisation which encompasses these instances. Such a generalisation is called a hypothesis.
In very general terms induction is the application of inductionist policies. These policies are categorized in two ways. First there is induction by simple enumeration by which an inductive hypothesis is seen as well founded if "it has not been refuted by experience and has been confirmed by not fewer than n positive instances" (P.260). Second, there is the principle of elimination by which our hypothesis is well established if, whilst it is not refuted, other hypotheses are refuted.

This, however, is a very general catalogue. The two principles outlined above underly induction, it is true, yet it is by no means obvious why they should, so let us try to make their position a little clearer.

An induction by enumeration can be done only by citing those occurrences which support the generalisation concerned. No account can be taken of instances that tend to refute the generalisation against the facts. Thus, it is argued, enumeration although suggestive of generalisations it cannot be used to test hypotheses against the facts. Because of this, other inductive policies were developed, notably by Mill.

These policies can be summarised as follows:

1. The Method of Agreement:

"If two or more instances of the phenomenon under investigation have only one circumstance in common, the circumstance in which alone all the instances agree, is the cause (or effect) of the given phenomenon." (Copi P.365)

2. The Method of Difference:

"If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have
every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ, is the effect, or the cause, or an indispensable part of the cause, of the phenomenon." (Copi P. 369)

3. The Joint Method of Agreement and Difference which is a combination of methods 1 and 2.

4. The Method of Residuals:

"Subduct from any phenomenon such part as is known by previous inductions to be the effect of certain . . . Jantecedent circumstancesJ and the residue of the phenomenon is the effect of the remaining . . . Jantecedent circumstancesJ."
(Copi P. 360)

5. The Method of Concomitant Variations:

"Whatever phenomenon varies in any manner whenever another phenomenon varies in some particular manner, is either a cause or an effect of that phenomenon, or is connected with it through some fact of causation." (Copi P. 366)

All have behind them the Principle of Elimination. Method 1 eliminates those circumstances as causes in whose absence the phenomenon can occur.
Method 2 eliminates as causes those circumstances whose effects have been established by previous induction and method 5 eliminates as causes those circumstances which do not vary with the variation in the occurrence of the phenomenon. Our statement of the principle of elimination has been in terms of the elimination of one hypothesis as against another. As such the above account may be modified to fit an experimental science.

For our purposes the above gives a sufficient description of induction.
If one looks at certain econometric and statistical methods now quite normally employed in Economics, it appears (to me at least) that certain of the tests employed try to achieve the kinds of associations for which Mill's five canons also aim. Insofar as the ends of those tests are similar to Mill's principles they are inductive. (Indeed the use of computers could be seen not as a means of achieving deductive breakthroughs but rather as a means for inducing generalizations from banks of data whose size is without precedent. This extremely tentative conclusion is largely due to the fact that models are almost the only kind of theoretic device used in cybernetics and to the speed with which data can be analysed.) But we must remember that the statistical analysis is based upon a theory of probability that cannot be induced since it assumes infinite sets of phenomena. Thus it may be that the simplistic conclusion with which we started this paragraph is merely an evidence of the author's lack of knowledge on these matters.

Note that both methods so far enumerated are extreme versions of how science should be performed and they are described simply for completeness sake. It is worth mentioning that social science (along with all other sciences) because it talks about it, cannot be simply deductive nor is it simply inductive for its generalizations are often quite fanciful in that they are not obviously determined by the data upon which they are based. An example may be the classical theory of the firm which is based upon the exceedingly simply assumption of profit maximisation. Another may be Friedman's theory of the Consumption Function; and yet a third the theory of the demand for money based upon the concept of portfolio selection. Hence most sciences that try to explain data are termed hypothetico-deductive.

The hypothetico-deductive notion of scientific enquiry then has in general been developed because of two arguments. First, the argument that
the only reality that exists is that which is learned through sensation. The basis of all knowledge of 'the world' is then the objects of sensation. This, however, leads to a difficulty in that very reliable generalisations have been deduced e.g. Ohm's Law in this world which cannot really be explained in terms of inductive policies (as Frego showed). Thus the second argument arises through the assertion that certain statements about reality can be invented by the imagination.

In general, science is supposed to find generalisations in the following manner. That part of experience which is sensation is seen to be passive. It is given to our experience without alteration. As such it can be conceptually, at least, separated from the faculties of thought, memory etc. but this separation is a matter of degree in that "an act of perception may be heavily weighted on the side of immediacy" (Karganaki P.55). Now the generation of scientific judgments is seen simply as a movement from experience to language to logic. Thus we are simply talking of the passage from sensory data to orderly knowledge. In this passage the various attributes perceived are synthesised in two ways; first by an integration into a whole view and second by use of memory integrated with other whole views. The movement from data to knowledge has these two moments. The first largely corresponding to cognition of a manifold and the second to synthesis proper which is a process of mental construction.

It is customary to presume that the constructions of the mind (concepts) are not uniquely determined by the rules which led to them. These rules are simply formal descriptions of the different ways in which the above described passage takes place. They are usually referred to as correspondence rules. Now these rules are really arguments by which the transition from data to conceptions is obtained. For example, we may enumerate the cases which led us to assert that firms maximise profits.
They are not intended as the means by which our conceptions are created in that data are simply fed into the rule and a conception immediately follows. Whilst we can see that these correspondence rules are a kind of tie by which constructs are prevented from straying into metaphysical pastures, it is an important point that objectivity can only be introduced by their use. It is in the passage of the rules that it is our way of acknowledging the success of our transition* (Weymann p. 60).

The results of this transition are constructs. In science these constructs must satisfy certain conditions:

(a) logical fertility,
(b) they must have multiple connections,
(c) they must be permanent,
(d) they must be extensible,
(e) they postulate causality, and
(f) they must be simple.

As we have intimated, the nature of constructs is difficult to state precisely. Whether or not a construct is used or not is often a matter of preference for the scientist in that he can decide whether or not to investigate the usefulness of a construct. For example, the choice between Chicago monetarism or neo-Keynesianism as an approach to the problems of macro-economics is largely a matter of preference. Further, the formal properties of constructs are by no means obvious. For although constructs are about data, they are in a sense inductive they are simultaneously capable of purely formal analysis by means of logic and so are deductive. Hence, we are limited to an intuitive description of the nature of construct formation that relies heavily on the rather ill-defined list of desirable properties. The more one construct has of these properties the better it is supposed, ceteris paribus. Not only must they partake of these properties
but also constructs must be verifiable. This latter simply means that in science the constructs are checked against experience as well as being based upon it. The resultant set of statements both in logic and data is termed a theory.

Again the correspondence rules are employed, this time in reverse. The deductions made from the constructs are re-interpreted into the data so that the conclusions of the logical arguments can be checked against the facts. This reverse application of the correspondence rules may take some of several forms. For example, a particular deduction may predict that as interest rates fall unemployment falls. If this association is found in fact then there is prima facie evidence that our constructs are supported by the facts. Let us call these predictive (or retrodictive) correspondence rules. They are by far the larger category of reverse correspondence rules. A second sort of correspondence rule we shall term negative rules. For instance, we can categorically state that Keynesian economic models would not argue that unemployment would increase in an inflationary situation. The finding of such an association is prima facie evidence for the refutation of Keynesian economics. (Our distinction is fairly obviously related to the enumerative and eliminative inductive policies outlined above.)

A precise description of the checking process will not be attempted in this work. It is as well to display a difference of opinion between methodologists on this matter. There are those who argue (with Kragman) that the conclusions of logical arguments can be adequately checked against the facts by the application of a verifiability criterion which merely states the convention that statements that are verified against the facts are to be taken as true. Others (including Popper) see true statements as those which cannot be refuted. The debate can be summarized as follows. Let $A$ be the set of always verified statements, $B$ the set of
possibly verified statements and C the set of always refuted statements.
By the verification criterion statements from A and B will be true whereas
by the refutation rule only statements from A will be so termed. In theory
then the refutation criterion is a more precise measure of support for a
statement though in practice refutation may not be possible.

It is then by a process the conjecture of new explanations and their
attempted refutation that scientific enquiry proceeds. If the conclusions
of our deductions are refuted we have prima facie evidence for our original
constructs to be inappropriate. Thus the whole process begins again. In
this way it is argued that a theory of any society is built up. The
deduced conclusions of a non-refuted theory are termed laws.

Although the above is drawn from Kragman, it will serve as an
exposition in general, of the statements of play in methodology at present.
The most obvious point of disagreement is over the interpretation of the
above ideas. Positivists believe that the above should be a theory of
how science is conducted. Anyone who does not act in the above way is
said to be unscientific. Indeed, some positivists go so far as to see
science as simply the setting up of a scientific language (or purely
deductive principles) the primitive statements of which are inductively
formed. An example of such thinking is the concept of science as a
succession of 'revolutions' in the use of its language. Science is the
search for more and more appropriate terms with which to describe the field
of study. Such a view seems to ignore the process by which the result,
i.e., a language revolution, is achieved. It is not the fact that
scientific language changes that is the concern but how valid (or non-refuted)
statements are constructed within it. This interpretation is opposed by
Kragman and Popper who see the above schema as a statement of the logic
of science. Thus, the rules of science are basically the means which must be followed if statements so formed are to be validly held. It is clear that either interpretation is not testable against the facts which tend to make the positivist interpretation appear inconsistent for if it is a theory of science, it must be testable or it is not science.

If we take the language analogy of positivism a little further, several interesting results emerge. In science we construct a language according to the well-worn rules which among other things contains a set of terms for the bits of experience we have sensed. Each perceptible bit is soon as a 'simple element' hence the formation of a construct can be seen as an addition of 'simple elements' or linguistically as an explanation of an earlier explicant. The synthesis implied by a construct is, then, the sum of the parts of simple elements, although it may well be the result of imagination. Conceptually, then, the world can be reduced to small microcosms which can be analysed independently of outside interest. For example, micro-economics can be seen as the analysis of the microcosms of economic activity while macro-economics deals with the behaviour of an economic system. I do not know of any work on the relationship between these two approaches but presumably for consistency the sum of all micro-economic units should be identical to the macro-economic one of which each is a part. It remains to be seen whether this is so. In the light of the hazy relationship between firms and industry on the problem of price setting, however, it seems any intra-industry models must also be hazy on this point.

Moreover, the view that small microcosms can be studied as representations of larger mechanisms follows directly from the above. For if systems are summations of smaller systems, then the mechanisms within those systems must be uniform otherwise a larger system will be different in kind from a smaller one and so not the result of simple summation. Thus, in the so-called 'harder' sciences the investigation of a given mechanism proceeds
by experiment on those objects which most conveniently display the
mechanics involved. Genetic research uses fruit flies because fruit flies
are easy to keep and are capable of fostering a homozygous population very
quickly. Such an analysis bases its validity upon the analogy between the
mechanisms operating in macro and microcosms. (Examples in Economics that
come to mind are Welfare Economics and voting theory, investment theory
and the theory of Human Capital and portfolio analysis and the theory of the
demand for money.) Insofar as the analogy is valid then we have a simple
enumeration of the instances in which this is so. If, however, we find
that the conclusions of our analogy are refuted against the facts we can
never find out if this refutation follows from our holding the analogy we
do or some other factor.

There is a further feature of this approach to science. It is the
belief that all objects of the world are explicable by the use of reason
in the sense of logical analysis. Any explanation of 'the world' must be
logically consistent before all else. Whilst accepting this we do not
intend to argue that only logically consistent statements are helpful in
the construction of these explanations. Yet as we shall see in Chapter
Eight there seems to be reason for the construction of an inconsistent
logic for use in science. In the above view such a construction is at best
mutatory and at worst worthless.

Against this view there are the so-called negativist or anti-reductionist
schools. In essence their criticisms of the positivistic-reductionistic
rationality school is based upon the conception of the world as a living
organism. Since we are no longer concerned with a synthesis as above it
is argued that such a synthesis:

"... can yield no complete explanation of the behaviour
of even the most elementary living system." (Yeats P.7)
This, however, is not to refute the view of science previously described but simply to say that a new way of interpreting our sensory perceptions is needed. Of this the process of theory construction has nothing to say, for, as we have seen, the sensations which are the basis of perception are purely given.

Anti-reductionism then amounts to the assertion (with enumerative proofs) that an 'organic' conception of the relationships in a system yield more meaningful statements than the reductionist view. The support of one viewpoint or other seems, however, not to display the willingness to conjecture any statement for refutation that is essential to the conduct of scientific enquiry. It would seem importance to keep both views in mind in the analysis of data.

Having asserted that it must be pointed out that often the only distinction between anti-reductionism and its opponent is the refusal of the former to interpret a conditional from experimental data. Instead of stating if A then B (ceteris paribus) such a statement will not be made without a description of the system which gave rise to it. The emphasis of the anti-reductionist view lies in the importance it places upon the examination of all the interrelationships in a given set of phenomena rather than a selection of these relationships. But the examination under such a view must still face the problem of how far a set of phenomena can be broken down without loss of validity. Anti-reductionist theory cannot be broken down (or reduced) at all; yet unless it can no rule exists by which more 'reduced' systems can be compared with less 'reduced'. Thus anti-reductionism does not prohibit the possibility that a reductionist system is, in its essential respects, as good an explanatory device as the anti-reductionist system from which it could, conceptually, be derived.

That from this we immediately derive the fundamental conclusion that
In science truth is relative is seen as follows. Truth is defined by Tarski as:

"The sentence 'a' is true if and only if in fact 'a'."

A theory is said to be confirmed (never to be true) against the data if its conclusions are borne out by the data and so conform to experience. But experience is unknowable unless it is sensed and so all truth is relative to the experience of scientists. However, the whole argument of science as a kind of formalized process in which the mind is conceptually dissimilar from the objects with which it deals has been open to question. If instead it is argued that the idea of science as a correspondent between the formal and the material is false then the criticism can run in three directions. On the practical way one can argue that there is no way the truth can be found. On the linguistic way one can argue the sentences based on the material are in no significant way different from those that are formal, and from a different conception of perception one can argue there is no necessary difference between the two fields.

Before, however, we present these fundamental criticisms, we shall adjust our view of science in the light of the above debates. We say with Hadamard envision science as the construction of a hierarchical system of languages. Notice that this does not necessarily incorporate the different views we have expressed but is at the kernel of the debate so far. We may be an anti-reductionist or an extreme positivist as we wish but still hold the following to be true. As such it is an expression of the essence of scientific methodology.

To agree that there are things we can sense which are separate from our constructs that are 'things of our mind'. Corresponding to the data we have a data language. It is the most rudimentary in that it
supplies us with simple designators\textsuperscript{10} of things and with which we can form descriptions of things. \textit{Above} this we have the field of constructs in which a so-called hypothetical language is formed. It is to be seen as a kind of meta-language for the data language. On top of this we have the metaphysical level of discourse which is really the language in which the preconceptions of science are couched and are investigated. Science is then a novacent between these languages.\textsuperscript{11} The ability to flit from one language to another is thus a pro-requisite for the scientist just as the need for the continuous use of all three languages is imperative. By this view a scientist is in continuously changing relation to science since the languages are continuously altering as they are manipulated as more knowledge becomes articulated. In such a view then truth becomes simply the correlation of a statement in the data language (which is an expression of what \textit{is}) with a statement in the hypothetical language. We simply take the extension of a hypothetical statement to be decided by the intension of a data statement.

It is not, however, our intent to place undue prominence upon the analogy between scientific discourse and the nature of language construction. Rather it would be preferable if the above could be looked at purely in terms of the fundamental distinction between discourse \textit{about} and discourse \textit{of} a given object. If in this relation which we mean when the above analogy is presented. Despite these comments it is hoped this description is not seen as a ‘straw-man’. Indeed we wish to argue it is not— for not only does it explain many of the arguments which occur in science (for how else can the Great Marginalist Controversy be explained except as a confusion by econoists over whether they were talking of or about the data?) but also it can lead us to deduce criteria for adequacy of statements.\textsuperscript{12} As such it is useful.

Indeed, it leads implicitly to the fundamental distinctions between
'analytic' and 'synthetic' statements. Kant defined them as follows:

"Analytic judgments . . . are . . . those in which the connection of the predicate with the subject is cogitated through identity; those in which this connection is cogitated without identity are called synthetical judgments. . . . the former add in the predicate nothing to the conception of the subject, but only analyze it into its constituent conceptions, which were thought already in the subject . . .; the latter add to our conceptions of the subject a predicate which was not contained in it, and which no analysis could ever have discovered therein . . ." (Kant P.30).

Clearly, statements like:

'Inflation involves increasingly rapid price rise'

is a statement of an analytic judgment whereas the statement:

'Inflation causes social unrest'

has behind it a synthetical judgment. The first is, thus, identified with the theorems and descriptions of science, the second with the product of the movement between languages. Science can then be seen as the means by which true synthetical statements about the world can be produced.

Science then is concerned with the construction of synthetical statements about the world. It is not concerned solely with correct reasoning. Thus, although some statements in a science are of necessity analytic they are useful only insofar as they link the correspondence rules with their reverse counterparts. Further, we have seen the distinction between analytic and synthetical judgments corresponds to the distinction between judgments moving within a language and judgments involving a movement between languages. It is, however, still unclear how the distinction between languages can be expressed in other than the use to which the languages are put. If no
other expression exists than the distinction becomes analytic. Our theory of science is not a theory in the sense that its conclusions are testable but it is a model of the practice of science.

Let us now set about the analysis of this conception of science. We shall, in what follows, simply look at the spirit of the above rather than a textual analysis. For the sake of an overview we are prepared to sacrifice detailed points.

First, let us make some definitions.

Closed system is a finite set of sentences which completely determine the behaviour of the set.

Open system is a finite set of sentences whose overall behaviour may be determined by sentences not contained within it.

Theoretical sentences are those sentences whose propositions are not directly observable.

Data sentences are those sentences whose propositions can be directly observed.

Objectively correct is the state in which the objects of enquiry can be made to determine the extension of a given statement or of the extension of a statement deduced from it. In science then we attempt to make our statements objectively correct. For this we move between our data language and hypothetical language by means of the correspondence rules. But we also need a test of the appropriateness of this correspondence rule. For this the well-known experimental and statistical procedures are used.

This, however, means that the system we are measuring by this test is no longer the system which deduced the conclusion but a system enlarged to include the testing procedure. Our original test cannot test this enlarged system, assuming it was appropriate to the first system, hence another test must be devised. This again enlarges the new system so that this test is now inappropriate.
However, it should be possible to assert that the accretion of tests makes
no significant alteration to the correspondence between the languages. It
should also be open to test but again we run into the same kind of regress
we have seen above. Thus, we can see that a given system will, by this
argument, become more and more altered. We must conclude, then, that a
science is unlikely to be immune from systems alteration difficulties unless
it can include all the tests and data implied by its theorems. Let us call
this the Principle of Systems Alteration.

It implies two important corollaries. First we can never prove a
closed system is objectively correct for a given space-time point. This is
shown as follows. A given space-time point can be given in Hilbert space by:

\[ P = (h, i, j, k) \]

To find if the properties observed:

\[ P' = (h', i', j', k') \]

conform to this we need a test and hence suffer alteration. Thus we can never
show that:

\[ P = P' \]

Second only if certain circumstances are asserted as true can\(^4\) we say our
system is valid. This is demonstrated thus. At a given space-time point
the system has the circumstances:

\[ P = (h, i, j, k) \]

which, let us assume, are observed. (In terms of the above \( P = P' \)) But
in observing we change the system so that new deductions may be possible. Let
the degree to which this latter happens be the degree of alteration (\( \alpha \)). In
using this system again we must believe not only that it has more chance of being
objectively correct than incorrect but also the degree of alteration is less than its chance of being objectively incorrect: \( \rho(O_x) > \rho(O_r) > \alpha \).

It is paradoxical to note that the means by which the above concept of science tests its statements—by comparison with the facts—depends upon the belief that the conclusions of our theory are true (see footnote 4). As we argued above, however, such a belief is scientific. It is only by our inability to refute a statement against the facts that we can argue it is a judgment that is supported by fact. In this process we must believe our statements to be refuted until shown otherwise. If, however, the converse is believed then for consistency a validation test should be applied to our statements. Thus, it appears the belief of 'traditional' science militates it against using the eliminative procedures advocated by Mill.

Now this belief can conceivably be included in the concept of science stated above in the metaphysical language of the 'traditional' concept of science. After all it is this language which contains science's belief terms. This solution itself, however, runs into alteration difficulties. One of the correspondences must be between the two theoretical languages. How to talk about this correspondence another meta-language is necessary which must have correspondences itself. Again there is an infinite regress into meta-languages which is another form of the same species. Perhaps this infinite regress could be seen as a property of science which is desirable in that a particular theory can incorporate (or explain) facts which came to be known subsequent to its original statement. Yet it seems clear that no new facts in the data sense are being investigated in this procedure. We are simply finding out more about our theory. As such we see alteration difficulties as a serious drawback to the conception of truth as a correspondence but it may be accepted in the form of a convention to the effect that we never cease to look for the most general statements possible.
Let us now argue our second line of criticism of 'traditional' science. We have said science wishes to construct true statements of synthetic judgments. This comes about by the movement between languages according to correspondence rules. It is vital that these statements are different from the statements of analytic judgments, for, if they are not, science may proceed simply by the formation of tautologies and hence can never be shown to be incorrect. Since we have seen that science proceeds at least as if it does not take the truth of its statements for granted we must conclude the distinction is of great importance.

We have already seen that Kant felt that causal statements were statements of synthetic judgments. They are, therefore, synthetic. Undoubtedly the formation of a causal statement must involve a synthetic since it presupposes a movement from one language to another. Yet there is a sense in which a causal statement is analytic. It is meaningless to talk of a cause without its effect thus:

"... cause and effect, in however isolated a manner they are taken, are meaningless one without the other ..."


Thus, if identity is to be taken as an 'if and only if' relation we can argue causality is analytic; although it is open to much debate (see Barcan Marcus, Quine, Carnap etc.) The problem can be expressed thus. In the statement:

"If and only if the Government inflates will unemployment fall"
does not imply an identity between the Government inflating and unemployment falling. 'Government inflates' means something quite different from
'unemployment falls' thus they have at least one property (i.e. their meaning) which is not shared. But the point still remains that if the above statement is a true one then the inflation and falling unemployment must be necessarily linked and hence imply each other. Despite these misgivings we shall use this definition. So that if identity is defined as:

\[ x \equiv y \quad \text{if and only if } x \text{ has every property which } y \text{ has and } y \text{ has every property which } x \text{ has.} \]  
(Tarski [3, F.55)

then 'if and only if' is identical with identity if implication is taken as a property and 'if ... then' is a suitable representation of implication. \(^{14}\)

We do not wish to enter this debate and so will accept Tarski's definition. \(^{15}\) Our conclusions, however, thereby be conditioned by the debate on this question.

Unfortunately, I cannot assess the extent to which the following conclusions will be invalidated by the above argument. It would seem intuitively, that the acceptance of an equivalence between 'if and only if' and identity simply facilitates the translation of the argument into symbolic logic. If an alternative logical constant for identity were to be discovered then it could be satisfactorily substituted for the one presently employed. Again we must point out the need for further research into this matter.

At first sight it seems that synthesis is derived in the formation of a sentence whereas analyticity is derived after the sentence is open for analysis. But the supposed difference goes deeper than this. If we can conceive of a mind that is conditioned by, but not determined by, matter then we can conceive of a synthetic formation of sentences since the mind may choose what it includes in the sentences. Let us call this the Principle of Synthetic Choice. If, however, we are analysing and we find that in an
inflation workers generally receive a greater share of national income we may not choose what we include in our conclusions. This let us turn the Principle of Analytic Unchoice. Thus the analytic/synthetic division is based on the two above stated principles both of which are based on the asserted separation of mind and matter.

In social science, however, scientists (the observers) are observing other observers. Undoubtedly one could argue that there is a difference in the type of observation undertaken and also that scientists can be adequately separated from the rest by what they do. This argument would, I think, at most be able to assert a degree of difference and seems incapable of distinguishing a scientist from, say, a football crowd despite his viewing football as a scientific discipline. Thus inevitably the social scientist is part of what he studies.

Insofar as a social scientist is only such, provided he is part of the object of his study there appears to be grounds for supposing that the process of synthesis by which science proceeds is also part of the object of that science. Not only is science both affecting and affected by the world but also inventing, by the process of synthesis, new worlds for science to study. In this schema the synthesis is also analytic in the sense that it is part of the object of science and so can be found by analysis of that object. On the basis of Kant's definition synthetic judgments about society are in truth analytic. If, indeed, synthetic judgments are analytic then no distinction between analytic and synthetic judgments can be made in social science.

Because of this sort of situation it seems quite artificial for the Principle of Synthetic Choice to be asserted for a social science except for well-defined limits. To a large extent when viewed as we have the object of investigation both influences and is influenced by the basic element of investigation. Whether one views society as an organism or as a set of
actors, it is still true that the object of investigation and the agent of investigation itself is precisely the same basic element. Hence quite consistently we have an object which is investigated by the same species of object. To be objective about this process a kind of metadiscipline is needed which leads us into an infinite regress. For example, one can conceive of an economics of the economics of inflation.

No difficulty arises, however, if we can simply state a convention by which we agree to limit this infinite regress. However, as we have stated it our infinite regress is due to the inseparability of the object of investigation and the investigation itself. We contend that in our process of passing between languages there is a two-fold movement. It is this movement which leads us to make our so-called synthetic statements in social science; it is also the same movement which makes them simultaneously analytic. No statement can be constructed in social science unless there is this movement (described above) between the various languages in social science. We shall call this conclusion the Axiom of Constructability. Alternatively stated, the Axiom of Constructability reads:

Statements in social science are constructed by two-way movements between the hypothetical and data languages that that science contains. No statement is to be considered part of social science unless it arises out of this movement.

From this it follows that two languages are involved by the Axiom of Constructability in the setting up of any statement in social science. Now this implies that any scientific statement is to an extent both analytic and synthetic. We would wish to argue that this distinction is meaningless in a social science.

But already we have run ahead of our proposed argument. So far we have criticised the traditional distinction between synthetic and analytic judgments and so have trespassed on to the third section of the argument. Let us revert to the statements themselves. We can broadly correlate
analytical statements with data statements and synthetic ones with hypothetical statements. Only insofar as this correlation is valid can we argue thus: It is not true that co-called observational terms (and hence the statements constructed from them) can only refer to observables. Indeed, observable terms are often introduced by means of unobservables, thus Putnam argues:

"... there is not even a single term of which it is true to say that it could not ... be used to refer to unobservables." (P.243)

Now if we argue that observable terms are only those which refer to observables and not unobservables, there are no such linguistic entities as observable terms. Analogous to this we argue the following.

A statement of an analytical judgment can refer only to that judgment and not to any other. Yet if there is to be an implication between analytical judgments they must refer to the framework in which they are stated as well as the common predicate. Now this framework is a synthesis of several statements. Hence, an analytic statement to be analyzed must involve a synthesis or there is no reason for the analysis to proceed by means of an identity relation. Moreover, unless this analysis is to produce synthetic judgments (which it cannot by definition) the analytic statements joined by the identity relation which result from the analysis cannot be formed by a process of synthesis. The definition of identity thus must be analytic and so must be the relation between it and the analytic statement. If either of these conditions are not fulfilled the judgment becomes synthetic.

We can easily see that Kant's original definition of analytic judgment is a synthetic statement by the relation between a definition of identity and analytic judgment is synthetic by definition. Thus we conclude
the distinction between the various sorts of statements appears too
simplistic.

As we have intimated above, this is due to a basic misconception, on
the part of methodologists, of the form a social science takes. Largely
because of too static a view of the relationship between 'the world' and
'the observer'. In our view, there is no such dichotomy but instead a
process called science is set up because there appears to be a dichotomy
on these lines. As Dooling writes:

"Every human being has an image of a world of time and
space, causality, value and so on in his mind. This we
might call the 'subjective world'. Part of this image
consists of a conviction that corresponding to the
subjective world inside him there is an objective world
outside him to which his subjective image corresponds."
(P.129)

Appearances, however, cannot be the criterion by which mind and
matter are separated. It is the very fact that ideas of the mind need not
conform to any appearance of an extensional object that gives rise to the
distinction in the first place. To then assert this distinction is
meaningful only insofar as testable conclusions can be deduced from it.
It is not meaningful to assert the distinction on the basis of appearances
alone. Further granted the above argument and our comments on the nature
of scientific judgments it follows that mind and matter may well appear to
be separate since by the Axiom of Constructability we know at least two
scientific languages must exist. By the same token we know that this
appearance is the result of synthetic choice and not analytic unchoice.
We contend then that science is better served by disregarding this dichotomy
altogether. This is particularly so of social science for the reasons given
above. Thus we must regard any statement of this process to partake of 
two languages in that it is a statement of the movement between them. Hence 
a statement is the summary of a dynamic transition. It remains now simply 
to particularise these remarks for causality.

We have already seen that there is a causal language and that it is 
especially different from formal languages. From our survey of the 
theories of causality we induced that causality was not only a statement of 
a relation between existences but also statement about that statement. Thus 
on the lines of the above we can conceive of causality as a particular sort 
of movement between languages. If we conceive of a causal language C in 
which there are two sub-languages $C_1$ and $C_2$, defined by their respective 
functions then a causal judgment is simply the dynamic process between $C_1$ 
and $C_2$, which leads to a statement in C to be formed.

As an example, we might think of our asserting:

an increase in demand leads (ceteris paribus) to 
an increase in price.

This statement can assuredly be transposed into:

\[
\frac{dD}{dP} > 0
\]

and so on but it remains that our first assertion is at the basis of all our 
representations. It is clear it is causal. Now, no matter how many times 
we may believe we perceive such a link between these two events no causal 
assertion has been made until we move from the simple statements of 
uniformity in $C_1$ to : cognize them in $C_2$. And in this movement we must 
assert a judgment or the statements in $C_1$ cannot be represented in $C_2$. 
But this implies that $C_1$ and $C_2$ are unchanging. We have found in other 
situations for this not to be so. Thus by asserting a convention that $C_1$ 
and $C_2$ interact we have our conception. In the above example the existence
of certain forms of language influences the way in which the transition from C₁ to C₂ is made just as we have seen C₁ influences the form C₂ takes. Hence, we conceive of our original statement deriving from the process of translation by means of a causal judgment from C₁ to C₂. If this judgment enables an easy translation then it is stated as a causal statement. Obviously, if the translation is impeded, for instance, by our observer not having a concept of cause in C₂, then no causal statement will be made.¹⁹ We shall analyse this movement in greater detail during the following two chapters.

In very general terms, then, it appears that our assertion of the interrelationship between society and the social scientist must lead us towards a concept of science quite unlike the 'traditional' conception. Instead of understanding scientific judgments as based upon the comparison of hypothetical judgments with the facts it appears greater emphasis must be placed upon the interrelation between the two. In particular, account must be taken of the fact that a theory of social science is part of the object of study of the science of which it is part. With this in view, we shall try to describe the process of causation that this view of social science implies. Needless to say our conclusions are as yet ill-formed and so far only tentatively held. It is hoped, however, that further research may uncover meaningful applications for the work.
We have deduced that there are two languages involved in causality. Now assuming the deduction to be valid, let us proceed to a discussion of certain properties of these languages. It follows from our previous discussion that both $C_1$ and $C_2$ are not formalised but are both parts of the English language. From this we can argue several somewhat revealing properties of the relationships that hold between them. Before that is achieved we must outline the process of language construction.

Here, in particular, we are concerned with the construction of terms. This is because we have still to answer the question 'what is causation?' From our discussion of causal languages it seems apparent that no formal language that is logically consistent can be used to represent causality. Thus it seems to follow that we cannot examine causation through the framework of a formalised language but rather a colloquial one. Since no formation rules or rules of inference are known to have been deduced for such a language we are forced to examine the structure of the causal vocabulary. Our attention, is therefore, directed to the structure of causal terms. We have here then to deal with the question of nano relations. It is traditional to assert that a nano relation holds between an expression in a
language and an abstract or concrete 'thing'. This is perhaps the oldest theory of name relations, having been put forward in a crude form by Plato. Its fault is that if every word refers to a 'thing' then if we are to describe the relationship between a word and the thing that relationship must be a thing too. Thus, if we are to limit names in this way we must enter an infinite regress of terms. The expression is generally called a name and the 'thing' named let us term the nominatum. Now according to Carnap, this procedure is based on three principles:

"The principle of univocality. Every expression used as a name (in a certain context) is a name of exactly one entity; we call it the nominatum of the expression.

"The principle of subject matter. A sentence is about (deals with, includes in its subject matter) the nominatum of the names occurring in it.

"The principle of interchangeability (or substitutivity) . . . if two expressions name the same entity, then a true sentence remains true when the one is replaced in it by the other . . ." (Carnap, P.98).

Hence we may have a sentence:

'John caused Mary to become the mother of Jane' — (1)

wherein 'Mary' is identical with 'the mother of Jane' we can by the principle of interchangeability formulate the sentence:

'John caused Mary to become Mary'

which is untrue. As such we have a paradox with which the name-relation must deal.

Broadly, three types of solutions to this paradox have been suggested. First, a distinction between nominatum and sense, where sense is seen as
the way in which the nominatum is given by the expression" (Carnap p.119). In our example above, we must separate the nominatum of the expressions 'Mary' and 'the mother of Jane' from their respective senses. Although they share the same nominatum they have different senses. Now if we regard the sentence:

'Mary became the mother of Jane'

as our 'ordinary sentence' from which our above example was constructed then we can say the sentence from which we deduced the paradox was an exception. When we construct a sentence like (1) we change the nominatum of the original sentence. This, because when a sentence such as (1) is formed instead of using the original nominatum of the sentence we use names which correspond to the sense of the sentence (2). It follows that once this is recognised there is no paradox since we are not truly comparing like with like.

This solution of the paradox leads us into an infinite regress of nominata, which arises in the following manner. If we take any expression it has both a nominatum and a sense (which correspond respectively to its extension and intension) but to talk about that expression we need another expression which too has the same two properties and so on. Hence, if we are to speak about all the possible entities we must have an infinity of names in the object language. We assert that this is highly undesirable.

In particular, this solution seems to deny the premise which is stated at the beginning of this chapter that the language C has two sub-languages C₁ and C₂. If to talk about an object language involved the beginning of an infinite regress, then the one-one correspondence between C₁ and C₂ would be denied (or at least limited to an infinitesely determinable set of terms). Since such a set cannot be found by use of induction, we must rule out its occurrence since we have assumed the formation of C is by means
of the hypothetico-scientific method.

The second solution to the paradox is due to Russell. Instead of regarding sentences as names of truth values he sees expressions as simply notation which does not have independent meaning. Thus names do not occur and hence extensions are not present. By excluding extensions we exclude the antimony. Let us examine the view in more detail. Let us define some more terms:

A description in \( S \) has the form \( (\forall x)(\ldots x\ldots) \).

A description is the entity for which the description stands.

Now a description satisfied the uniqueness condition in \( S \), if there is exactly one individual which satisfies the condition expressed by the scope or if

\[
(\exists x)[(\ldots x\ldots) \equiv (x=\xi)]
\]

is true in \( S \).

Now let us conceive of two contential matrices \( \ldots \gamma \ldots \) and \( \ldots x \ldots \) We construct our description with the second as scope and substitute into the first,

\[
(\forall x)(\ldots x\ldots)
\]

By substitution into \( \ldots \gamma \ldots \)

\[
\ldots (\forall x)(\ldots x\ldots)\ldots
\]

Russel interprets this as:

"There is an individual \( \gamma \) such that \( \gamma \) is the only individual for which \( \gamma \) holds, and \( \ldots \gamma \ldots \)."

Hence (3) is seen as equivalent to:

\[
(\exists \xi)[(\forall x)(\ldots x\ldots) \equiv (x=\xi)] \land \ldots \gamma \ldots
\]

Thus in our definition of desription (3) is seen as the definiendum and (4) as the definitions. Now this process is simply a means of transforming a description (3) into a sentence with the same meaning (4) but without
the description.

Let us grant that a description satisfies the uniqueness condition. Now the description can be regarded as the nominatum of the description but it is clear that a sentence containing the description is itself not about that nominatum. Instead its meaning is given by (4). Since also Russell sees proper names as abbreviations for descriptions neither proper names nor descriptions occur in the primitive notation. Given the above the principle of interchangeability is not applicable. The above means that a statement:

'the mountain is purple'

says nothing about whether there is a mountain. All it says is that if there is a mountain then it has the property of being purple. In other words, the class of mountains may or may not be empty, i.e. there is no presumption about the statement's extension.

If we look at a sentence like:

'John causes Mary to be the mother of Jane'  (5)

thus we can merely assert that the expression 'the mother of Jane' is a description of 'Mary'. Now this description has no meaning in itself. Because of the rule we have stated, however, the sentence that contains it has meaning and this can be expressed without using the description itself. Hence we can see that no concrete 'thing' is contained in the above sentence; there may indeed be no 'Mary', 'John' or 'the mother of Jane'.

However, it is assumed by the philosophers who have examined this question that it is impossible to conceive of a sentence of the form (5) without asserting the existence of the things 'Mary' and 'John'. Causality is an association between two 'existences' (however the latter be defined). Insofar as we have considered the question, then, a causal statement like:
'John causes Mary to be the mother of Jane' cannot have any 'meaning' or 'explanatory power' unless true.

In terms of Russell's method of naming, then, in forming a causal sentence one must assume the sets of 'cause' and 'effect' are non-empty. Hence, although there is no necessary link between extension and description by our convention a correlation is assumed.

Now this is an important result. If a causal sentence supposes that the expressions within it refer to non-empty sets it is clear that the languages $C_1$ and $C_2$ refer to possible existences. Hence the expressions in either language to be held equivalent must refer to the same nominatum but in different senses. We have thus achieved a way round the paradox for causal sentences.

The third broad method of avoiding the paradox is to construct a purely extensional language. In our particular situation this involves showing that any non-extensional expression may be translated into the extensional language. Russell sees this translation as the thesis of extensionality. He asserts that the extensional language must have two properties:

"\[ \exists a \] . . . if a proposition $p$ occurs as part of a larger proposition $q$, the truth value of $q$ is unchanged if we substitute for $p$ any proposition having the same truth value;

\[ \exists b \] . . . if a propositional function occurs in a proposition, the truth value of the proposition is unchanged by the substitution of any formally equivalent propositional function . . . " (Russell \( \exists 2 \) P.261)

Thus (b) implies that if in the proposition:

'since there is a stock of Government Bills money supply can be altered'

we substitute 'there is a stock of Government Bills' with 'there are
Eskimos in Northern Canada, the whole proposition will not suffer a change in its truth value. For causal statements this is not so, since as we found in Chapter Five, the truth of the whole proposition is not truth-functional. Because of this fact it is meaningless to talk of extensions (as defined) in terms of causal sentences thus we are forced to use the Russellian notion of the name-relation.\(^5\)

The terms contained in a causal sentence are thus notations for sets of phenomena which are non-empty. It is not in the nature of the name-relation which accounts for this but it is the way in which the languages \(C_1\) and \(C_2\) are formed from these terms that provides our explanation of causality. Perhaps this point can be accentuated by considering our discussion of the name-relation as the basis of a theory of causal sentences whereas the combinations of name-relations is the foundation of any explanation of causality.

For the latter we need a concept of language formation. Let us for the purposes of argument postulate that the formation of a causal language is a formalization of the process by which an observer becomes a scientist. In other words, if we can exhibit the nature of a causal language then we would expect it to partake of, in its major respects, the main features of scientific method. The description of the formation of the language \(C\) is then also a description of scientific method.\(^6\)

We have seen that a social scientist is an observer of other observers. How this means a social scientist simply non-existent without other observers.

In these circumstances an observer (whether a scientist or not) is concerned with denoting certain relationships as causal. He has to eliminate those relationships which are not necessary as non-causal. How a scientist acting thus when object of his study is a set of beings bent on the same purpose as he perceives the causal nature of his fellow observers. Thus
he in observing a set of beings that are to a greater or lesser extent scientists. In this our scientist assumes the Axiom of Constructability holds. But because the causes determined by the object set are, as perceived, seen as generalizations they are in a sense constructs of the scientist. They are nevertheless also the object of study.

At the same time any object of observation is unintelligible to the social scientist unless he has learnt to interpret that observation coherently. For this he must learn a kind of language. Hence we could interpret our statement of a causal language as a system of representations of the objects of social science observations. The relation of those objects to the language has already been analysed; namely that the languages $C_1$ and $C_2$ are composed of notation rather than words with independent meanings.

It is the combination of language-learning and the interrelationship between scientist and the object of his science that leads to the notion of causation presented in Chapter Eight. The language that must be learnt is the language $C$. This, we have hypothesised, is done by means of the application of a scientific method. Thus although the language $C$ comprises a set of notations these notations are part of the object set of economic science since an economic scientist studies other observers who are also learning at least part of the language $C$. Since scientific investigation assumes that the Axiom of Constructability holds the language $C$ can be conceived of as purely formally or as fact. Thus it appears that the causal language $C$ comprises two parts and so we have an explanation of the fact that the forms causal statements take leads us to infer a proposition about the entities within them. Hence in a causal statement we talk of links between notation like:

'A causes $B$'

but also we can infer a proposition about that sentence from the statement of the sentence. All this agrees well with the conclusions we have induced.
already we have seen one reason for this — the fact that in a causal sentence it is postulated that the sets of causes and effects are non-empty. If this is so, then, of a particular causal statement it follows there must be a representation of the cause in both \( C_1 \) and \( C_2 \) and the case for the effect.

This follows from the fact that a set can be empty because it is impossible to conceive of its having any elements. In terms of our two languages this may be because there is no representation of a particular cause, say, in \( C_1 \) or in \( C_2 \). If there is no representation in \( C_1 \) it means that there are no causes of inflation of the sort:

- "Increases in the unicorn population cause inflation!"
- "Increases in the incidence of cancer causes inflation!"

cannot, on a cause of inflation, be represented in \( C_2 \) but can be represented in \( C_1 \). Obviously, only if an expression appears in both \( C_1 \) and \( C_2 \) in the case context can our assertion of non-empty sets be sustained. Hence, by definition for every expression in \( C_1 \) there must be an expression in \( C_2 \). These expressions are arrived at in the way described above.

Instead of proceeding to the next stage of the argument in which we shall analyse the differences between \( C_1 \) and \( C_2 \) more closely, let us outline a curious feature of causality pointed out by Sir Troeger. This can be easily dealt with in our schema but seems not to be explicable in other notions of causality. Let us conceive of an observer who can interpret certain efficient causes into \( C_1 \) and \( C_2 \). For instance, he may find that as money supply increases by 10%, a general index of prices above a 10% increase. This he finds on many occasions. Let us also
assert that the conditions of observation correspond to the conditions necessary to a 'crucial experiment'. Hence the two associated changes form efficient causal relations. In science, however, our observer is set on generalization hence in the above circumstances he will conclude:

'an increase in money supply causes inflation'.

This statement is a formal cause, however, for it is a statement of "the form of the archetype." (Aristotle $\text{b} 194^b$). In our formulation the above is simply an assertion about the efficient causes but as we have seen this about statement is implied in the statement of the particular efficient causes. Indeed, this is so of all other types of particular causes.

It is traditional to assert that corresponding to particular causes in there is a general causal law which these/particular exemplify. However, we have seen that a causal statement whether general or particular involves the use of both $C_1$ and $C_2$ as such a meta-statement is implied by any causal statement.

Now, although there is prima facie evidence for a causal language $C$ composed of two sub-languages $C_1$ and $C_2$, there is not so far anything except indications as to the precise nature of this language nor of its constituent parts. In this chapter we are concerned with an analysis of the languages $C_1$ and $C_2$. We have seen that any statement in $C_1$ is found in $C_2$ and vice versa. We have, however, not answered the question 'why causality has this particular form?' Insofar as this question is capable of answer in this present work we shall exhibit such results as have so far been achieved in Chapter Eight.
In the above causality has been taken implicitly as meaning the formation of causal sentences. In this chapter we shall present a summary of what we have learnt about this formation and use it to derive a theory of causation. This we believe to be the kernel of any successful theory of causality. Whether, however, what follows is 'successful' is not a matter of opinion but is a matter of the ease with which now meaningful explanations of economic phenomena can be adduced with its aid. Again we must emphasise the tentative nature of our enquiry which is aimed more at stimulating a reconsideration of the nature of economic science than presenting an 'alternative view'.

Now by the Axiom of Constructability we are asserting that causality for it to be scientific must be a construction out of two languages. These we have termed $C_1$ and $C_2$. Yet we have seen that for causality these languages are identical, or, at least, there is reason to believe them identical. Hence, it seems improbable that there can be a process implied by the axioms.

However, we can easily see this is fallacious. A sentence in $C_1$ implies a sentence in $C_2$ and a sentence in $C_2$ implies a sentence in $C_1$. 
Hence it follows to cognize causality in either $C_1$ or $C_2$ implies a
cognition of causality in the other language. How this follows from our
arguments on the logical nature of the concept. It is not an explanation
in the sense that one can translate this argument into properties of a
causal sentence.

Let us, therefore, construct a perfect experiment and attempt to
perform this translation. Let us conceive of a social system that has been
unchanged for two generations which is composed of a set of individual
agents who have remained constant in number, physical characteristics and
temperament for the same period. Let us also suppose that each is an
and economic man unaware of his society's history of societies other than his
own. In such an ideal type, expectations will generally lead one to assert
no new occurrence or shock will occur. Hence, we may rule out speculation.

Let us now suppose there is perfect competition with an advanced
monetary economy (but no speculative effect). Now suppose an individual
were born who could conceive of another way of life than this. Let this
single person plan a great expedition to search for this new way of life,
financed by his own inherited wealth. Now it is clear that a new product
is suddenly introduced to this society; namely expeditions. (Note it is
simply a different combination of the same products used before). This
increases the range of choice of individuals between products. It may
also increase real income and cause a rise in the consumption expenditures
of everyone in the society.

Now in the light of this let us examine the sentence:

\textit{'inventions of new products causes an increase in real income.'}

Let us assert that this, in fact, is so for various reasons. Unless,
however, we can hypothetically link the cause:
inventions of new products'

with the effect:

'an increase in real income'

we shall not be able to call this a causal relation. This conclusion is
due not only to the fact that unless we have an idea of the kind of relation
for which we are looking we shall not observe that relation in the maze of
empirical data but also to the observed relation between the two events.
Thus it seems that the formation of a causal sentence is a result of the
interaction of two types of statement.

To use the exact process involved in causality, we must recall what we
have learnt about causality before. It will be remembered that in describing
the events we are not placing the events in sentences but notations of those
events. Hence, the interaction involved is an interaction between the
analysis of different notations. But we found that these notations were the
same in both C1 and C2. Thus the description of events referred to in
our sentence:

'inventions of new products causes an increase in
real income'

is precisely mirrored by a formal description of that sentence in C2.

It follows that conception of a cause involves the description of a
cause and vice versa. For since a causal statement comprises a series of
notation it does not itself hold any existential import. It is a statement
that if causality were conceivable then no other statement is needed to
describe the form that causality takes. A causal statement itself describes
the form taken by the proposition it puts forth. For instance, the
statement:

'unicorns cause eclipses of the sun'
is a causal sentence since in the compass of the system from which it is
derived the set of ordered pairs of 'unicorns causing' and 'eclipses of the
sun' is non-empty. If, however, we find that on further investigation
the above system is inappropriate in fact we should not state that unicorns
are a cause of eclipses or inflation. In talking about causal statements
the learning of them must be kept separate from their nature. Thus it is
part of the nature of causal statements that once an observer conceives of
a cause he must look to $C_1$ for its mirror image and vice versa. In fact,
if we analyze the statements in $C$ we shall see this is so.

Let us demonstrate this. If $y$ is our individual in $C_1$ and $z$ our
individual in $C_2$ we can show that if $C_1$ and $C_2$ are in intensional
isomorphism then $y \equiv z$.

Now since the three conditions$^1$ for intensional isomorphism seen in
general to obtain between $C_1$ and $C_2$ we can assert that there is an
individual $z$ such that $z$ is the only individual for which 'y' holds and '...y...'

This is because given intensional isomorphism the structures
of languages are equivalent and so their ranges are identical. Now given
that we can construct the following:

\[
(\exists x)[(q)(\cdots q \cdots \equiv (q \equiv z) \land \vdash z \vdash)]
\]

which in our description of $z$ we obtained it thus:

Given $z \equiv q$ and $(\cdots q \cdots) \equiv (\vdash z \vdash)$
and $(\exists q)[(x)(\cdots x \cdots \equiv (x \equiv q)) \land \vdash q \vdash]$
then $(\exists x)[(x)(\cdots x \cdots \equiv (x \equiv z)) \land \vdash z \vdash]$

yet by hypothesis $y \equiv x$ and $(\cdots x \cdots) \equiv (\vdash q \vdash)$
so $(\exists x)[(q)(\cdots q \cdots \equiv (q \equiv z) \land \vdash z \vdash)]$

This simply means that $z \leftrightarrow y$. 

It follows that to analyse $x$ is to analyse $y$. Thus the two are analytically conjoined. Yet we have seen that they can conceptually be separated because it seems to an observer that there is a separation between him and what he observes. Instead, then, of accepting the inseparability of $C_1$ and $C_2$ he analyses them separately. An observer believing in this separability will look at the world from the point of view of a scientist attempting to find support for hypothetical statements in data. Thus he sets up a hypothetical statement in $C_2$ and looks for support in terms of $C_1$, but in doing this he has altered the world he is observing. Hence if he finds the support it will not be support for $C_2$ but support for $C_1$ put forth by the particular observer at that time. Consequently he must find how his hypothesis formation influenced the findings and is led to and fro between $C_1$ and $C_2$. Hence, if such an observer asks 'why' of either '$y$' or '$z$' he will eventually be led back to the other. If this analysis continues, the former notation will come into view again. This is because of the real inseparability of a social scientist from that which he observes. He is led back and forth between $C_1$ and $C_2$ and so constructs an entity which is an element in a given sentence. Here, of course, it is either a cause or an effect.

But in order to explain how causal sentences are formed, we must determine what is that causes the scientist he is dealing with causality and not some other notation. For this we must recall that in causality there are no empty sets. Thus, if this process continues such that no empty sets are found, this is evidence for our notation to be a cause. Yet this is not sufficient until we remember that this process also holds between all other notations in $C_1$ and $C_2$, say between $z'$ and $y'$ and all combinations of notations $z', z''$ and $y', y''$. It is these combinations of notations that are called sentences. Given this, sentences in this language are formed in the same way as the individual notations.
Before we continue, I feel it is as well to clarify the concept of
'non-empty sets'. A causal statement:

'wage increases cause inflation'

is only meaningful if in fact wage increases cause inflation. The set of
ordered pairs of 'wage increase' and 'inflation' is non-empty in fact. Similarly, as we have seen a statement like:

'unicorns cause inflation'

postulates a non-empty set. In learning this statement, however, the
scientist finds that in fact the set of ordered pairs is empty. Thus it
is the 'learning' of the statement by science which decides the criterion
of emptiness to be used. Since we are here analysing the former type
of statements these causal statements imply the relationship between C₁
and C₂ postulated. As we saw in Chapter Seven, the particular relationship
was derived because of the fact that economics examines the behaviour of
other observers who also attempt to find causal statements.

Now since we have found that a causal sentence is not truth-functional,
it is not true that the truth value of a causal sentence made up of two
propositions, is necessarily uniquely determined by the truth values of its
constituent propositions. Hence, we can say that the truth value of a
causal sentence is independent of its constituent truth values. Given this
we must say that a sentence formed in the way described must have no exten-
sion. This involves its not being definable in terms of a class of like
statements or being able to be equated with identical statements. Since
there are no identical statements to a causal statement, save itself, we
have nothing to define causality by except its properties. Thus each
causal statement is, in this sense, unique.
The property which divides all causal statements from non-causal is that which states that causal statements cannot refer to empty sets. This is called the Axiom of Division. On this basis we assert that by definition C is believed to contain causal statements.

At this point it must be made clear that the Axiom of Division is a means of learning causal statements. It is the rule by which statements about part of reality that are causal are separated from those which are not causal. Causal statements are thus divided from other statements about part of reality on the basis of the former's necessary necessity and the latter's lack of this property. So far we agree with Duce, Aristotle and Kant amongst others.\(^3\) Where we do not agree with these writers is in the way causation comes about. Simply stated cause and effect are simple identifiable events on a continuum of events called a process. A cause is undefinable without its effect and vice versa. Thus it seems to view a cause as a rovel of an effect simply on the basis of a spatio-temporal relation between them does not necessarily argue that in fact a cause does have its effect in this way. Moreover the uncertainty of the split between observer and observed may lead to the impossibility of this spatio-temporal relation being identified as postulated. Rather, then, than look for the appearances of causation it appears much more important to look for the continuum which gives rise to it.

Now if this is so, it is clear that a causal sentence:

\[ y \rightarrow y' \]

must imply the following:

\[ z \rightarrow z' \]

It is also clear that the use of the concept causality in this context
implies many things for the relation c. We have seen that a sentence constructed by inter-reaction between notations either \( y \leftrightarrow z \) or \( y \leftrightarrow y' \) is, by the Axiom of Division, causal. It is this inter-reaction which, as we have shown, gives rise to the peculiarities and it is also this inter-reaction which means it is not possible to test for causality by the traditional statistical correspondence methods.\(^4\) In line with the above analysis we can outline the main properties of causality into two categories. These correspond to the languages \( C_1 \) and \( C_2 \).

<table>
<thead>
<tr>
<th>Table 1: The Categories of Causation</th>
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<tbody>
<tr>
<td>( C_1 )</td>
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<tr>
<td>( C_2 )</td>
</tr>
<tr>
<td>( y' ) then ( y'' ) ( \rightarrow ) Priority</td>
</tr>
<tr>
<td>( y' ) and ( y'' ) exist       ( \rightarrow ) Contiguity</td>
</tr>
<tr>
<td>( y'' ) cannot exist without ( y' ) ( \rightarrow ) Necessity</td>
</tr>
</tbody>
</table>

Note in our schema it is the final relation that is the vital one. From the assertion of the Axiom of Division it immediately follows that the other properties are implied by it. This we shall term the Principle of Causation. Thus we are able to deduce the Principle of Causation directly from the Axiom of Constructability.

Now this must mean that the Principle of Causation is a process joining seeming opposites. It has also been found to be deducible from the Axiom of Constructability given the denial of the separability of the subject and object of observation. It seems that causal statements of the 'billiard ball' sort will never be found by the hypothetico-deductive methods in these circumstances. Instead, it seems sensible to regard causal statements as being formed by the above-described process and to accept that as such
causal statements are much more complex than previously realised. Explicitly, it is formed by the movement from one language to another, yet its sentences do not refer to this movement; merely they state the results obtained from that movement. Causality does this but also by the Principle of Causation describes, implicitly, the process by which its sentences were formed. As such, causal analysis is its own method.

It may, at this juncture, be suggested that the process of causation is representable by a variant upon a feedback model. That this is not so can be illustrated thus. The process of feedback involved in such a model is indeed similar to the process we have described but the idea as a whole holds an imperfect analogy with that process. Our whole argument depends upon the notion that the observing agent is altered by what he observes and simultaneously alters the observed. Thus, a feedback model would have to be altered each time a feedback occurs and for a determinate solution must specify its own behaviour in this respect. Now by the Principle of Alteration this is not possible; hence it would seem the model concept is not (at least directly) applicable to the argument.

A causal sentence is built up from 'primitive' variables which appear both in $C_1$ and $C_2$ like 'inflation' and 'redistribution of wealth'. When these 'primitives' are formed into a sentence like:

'Inflation causes redistribution of wealth'

then that sentence in $C_2$ is also found in $C_1$. But if we deny the Axiom of Constructability as well as postulating the Principle of Alteration, then it would seem there cannot be two languages involved in causality. For $C_1$ to be dissimilar to $C_2$ it must have one property which it does not share with $C_2$. Now all we have asserted about $C_1$ and $C_2$ is that both are some convenient portion of the English language. Thus in terms of
'material' properties they are identical. It is then in the interpretation of \( C_1 \) and \( C_2 \) that they differ.

Now the interpretation of \( C \) in terms of \( C_1 \) and \( C_2 \) is derived from a concept of learning based upon the acceptance of the Axiom of Constructability. If this axiom is assumed not to hold then the learning of any statement cannot proceed by a movement between languages. Instead, learning of a statement proceeds through the association of that statement with the cognition that is its basis. Non-causal statements refer to empty sets and so need not be constructed by means of a comparison between data and concepts in any case. But causal statements are of necessity not free from this task.

Or rather this appears so until we remember that if the Axiom of Constructability does not hold the distinction between concept and data is dissolved. Thus even if we wished causal statements could not be constructed by the comparison referred to in the previous paragraph. Instead all statements would refer to non-empty sets. Any statement that could be constructed would be causal since simply stating a proposition would imply the truth of the proposition. For such a language to be formalised, an inconsistent logic would have to be employed yet this seems so far off that we need not bother with the possibility.

Yet we must face the fact that often the Axiom of Constructability is violated in social science, i.e. passage between languages is rarely constrained. In this passage in traditional science there are needed correspondence rules. Now in economics these rules are non-existent in certain cases. Perhaps we could illustrate this by reference to simple theory of the firm. It has been found by various investigations that:

- Firms do not know their marginal costs
- Firms do not try to maximise profits
- Firms are to an extent ruled by decisions made in committee
firms can influence price
firms can influence costs
and
firms try to influence government decisions.

Now these, and other, statements about the supposed facts surrounding the firm have been made. As such they are unremarkable. Notice that none of these statements are purely factual; they all contain theoretical elements. They must, therefore, be a link between 'fact' and 'theory'. Hence, they are performing as a summary of a correspondence rule. But as such the correspondence rule is constrained. Never can there be a definition of a 'firm' that is adequate hence never can such a term be used adequately in rigorous deductions. Thus it seems the reverse correspondence rule need not exactly be equivalent to the first.

But further no such reverse correspondence rule can be found to correspond to:

'firms influence price',

we can say that if

\[ \frac{dP}{dF} > 0 \]

then our statement is verified. We can watch the price-making process, we can use any sort of correspondence but never arrive at a crucial condition for the statement. The reason for this is simply that 'influences' implies changing circumstances from what they would be otherwise. Hence, any test of like statements must imply a knowledge of what has never been which is not possible in science. Yet this statement is axiomatic to practically every shopping decision made in this country (I assert). The usual resort is to say that \( \frac{dP}{dF} > 0 \) means 'influences' in science, hence that is what it is for science. This is perfectly valid provided one does not exchange one for the other, as is usually done. Either then social science must give up all pretence to analyzing propositions of this sort by its
method or it must accept them as unscientific primitive statements. It may not have it both ways.

From what we have said, it is clear that social science, if it is to make scientific judgments in the traditional way, must keep the Axiom of Constructability unassailed. Yet this is in the nature of social science impossible. Science is itself part of the subject matter with which it deals. It is at once talking about and talking within society. Hence not only are the propositions it asserts conditioned by that society but also they condition society itself. If a scientist consciously makes a theoretical statement, then immediately that statement is part of the society in which he lives and it may well be that he will have to treat that statement as an observation on a future occasion. (Perhaps the expectations led wage increases in Great Britain 1969-71 are the result of trade unionists learning about real incomes!) It seems, therefore, very artificial to impose a separation of sentences on the basis of a single property; namely, whether it is or is not about constructs. This is so especially when other more clear cut criteria are available; for instance, the causal/non-causal distinction. But if science is to be about that which is beyond the scientist and given the scientist is in the situation described no longer can the Axiom of Constructability be held with complete faith. Instead another sort of process of sentence formation may need to be envisaged which will take into account this bassiness.

In this context both System Alteration and Observer Alteration lead us to argue the following.\(^7\) Let us assume that society is changing steadily. This means that our scientist is also changing. Suppose now that our scientist wishes to find the 'cause' of inflation, in the which investigations he recognises that he can only find necessary conditions for a cause in the best human manner. He proposes to use the hypothetico-deductive
method. He thus wishes simply to show a uniformity between inflation and the occurrence of some other variable over time. Now if in this experiment he envisages himself as a stationary system watching succeeding events in a changing one then it may well be that since he relies upon the changing system to give him his idea of succession he will not get the sequence in time correct. This is because the succession he observes may well be the result of succeeding alterations in his own viewpoint and not in the point at view. He may be viewing the same object in a system that is changing the view of which gives him an impression that the object has changed whereas in fact his way of representing the view has changed. We conclude, therefore, that because of this effect he may not even be able to ascertain priority amongst events.

Now this we regard as undesirable - hence we should seek to find a means by which Systems and Observer Alterations are built explicitly into the theorems of social science. Quite obviously we usually assume that both the observer and the observed change at the same pace and in the same way over time and hence we can hold with Kant that the idea of succession is vital to cognition. However, such an assumption is made without any justification whatsoever. It would seem that the following may well prove more justifiable. Let us say that we have obtained the probability of

\[ p(x_t, y_t) \]

of being being true by statistical analysis:

\[ p(x_t, y_t) = \alpha \]

Now the higher this probability the more likely we are to believe it hence the more unlikely we are to make similar assertions in precisely the same manner as we did the first. The same is true of low probability values for \( y \). Let us call the probability of observer alteration \( B \). Clearly, \( B \) will be lowest when there

\[ B = f(|os - p(x_t, y_t)|) \]
is neither a strong probability for or against. Clearly then if:

\[ \alpha > B \]

our temporal relation is unlikely to be wrong. Since this depends upon the value of \( f \). The graph for all values of \( f \) as a function scalar such that \( \alpha = B \) is approximately as follows:

\[ \alpha \sim 10 \]

\[ f \]

As such this equation is the boundary of permissible alteration. Areas to the left of the curves indicate more permissible areas. Thus \( \text{III} \) is better than \( \text{II} \) which is better than \( \text{I} \). Only if we are in sector \( \text{III} \) can we say that \( \alpha > B \) from the above a simple characterisation of the Area of Permissible Alteration should be possible.

If we look at \( f \) as a weight that is added subjectively to our frequency ratios, we can see that the condition, called for above, gives us an indication of whether we believe the view of the world, with which we have analysed the data, is a reasonable one to hold. How this concept is of great importance in economics where small changes in assumptions give rise to great discrepancies in analysis. It is clear that only insofar as \( \alpha > B \) can we say the Axion of Constructability is not irrelevant. Hence, we may hold our traditional view of economics within these limits. Notice that the limits are not set by any objectively given factors but by the value for \( f \) which is subjectively evaluated. In a sense \( f \) is an indication of our belief in the relevance of the analogy between the scientific procedures adopted and the observer/observed relation. Hence \( f \) can be called the Analogy Factor.
Now it is clear that the Area of Permissible Alteration is associated with low Analogy Factor and low positive probabilities. Hence it seems reasonable to assert that in our analysis we should attempt to set constraints on this process. Let us say a probability of less than 0.7 is intolerable. Given that constraint and our Analogy Factor we can compute the difference:

$$|\alpha - B|$$

We would then be able to choose between hypotheses on this basis.

It must be recalled, however, that such analysis is useful only if we can truly regard our variables as variable. There are, broadly speaking, two conceptions of a variable. First one may conceive of it as a summary of a process. Obviously, this is analogous to Newtonian 'fluxions'. This concept fits well with the Aristotelian concept of causality in that it is a generation/corruption cycle. Second, we can see it as a synthesis of space-time points. A variable is seen not as something continuously variable but exhibiting different values at different times which are discrete. The changes between values are then seen as attainable by a process of continuous change. Obviously, statistics is capable of performing the latter but not of describing the former, except insofar as it conforms to such a synthesis.

Hence, it may well be that statistical analysis depending as it does, on the finding of correspondences between 'fact' and 'theory' is unsuitable for the investigation of causality.

All the above tend to conspire, I think, to cast grave doubts on our ability to investigate causality by the traditional means. Now this is of fundamental importance for our present purposes. We are faced with a choice between approaches, since either we investigate society as we have until now, or we look at society in the light of alteration and keep our concept of causality. This choice should be open to test and it should
be allied to our Analogy Factor. The higher the Analogy Factor, the nearer to \(0.5\) in ... \(B > \alpha\) then we should use an analysis which takes alteration into account. If we are in the Area of Permissible Alteration then, as we have outlined above, statistical results are justifiable.

In those circumstances, then, we may be able to characterise a causal sentence according to very strict statistical criteria whose derivation may be based upon the forerunning discussion. At first, it seems that those criteria would not be very different from the constant conjunction criterion put forth by Hume and discussed in Chapter 4. We should, however, point out that this criterion is simply a proxy for a more complex process which, for want of better statistical theory, is analysed in this way. A correlation is 1.0 for all time is not causality but it is the only part of causality which can be measured in the present state of knowledge. Moreover, such a constant conjunction is not the 'most primitive' idea we can have of causality and so it may well be that deductions from the proxy may lead to different and less empirically relevant conclusions than deductions from the ideas that have been expounded above. This we shall attempt to indicate in following chapters.

In conclusion we note that grounds for supposing that the so-called analytic/synthetic distinction is irrelevant in any discussion of causality have been given. No doubt criticisms can be made of the above argument whose importance will only be demonstrated by more work on the lines of the above. Causality is at once analytic and synthetic. For an extension of this argument into some problems in the Theory of Action we shall now move.
CHAPTER NINE

On Freedom and its Relation to Causality

We now begin the task of demonstrating that the theory of causality set out above, albeit imperfect, can form the foundation to a solution of the problem that we posed in Chapter Three. At the same time it is hoped that the importance of this theory will be indicated.

Let us define freedom, negatively, as the absence of general laws which determine a person's behaviour. The discussion of freedom, then, becomes a discussion, in essence, of the so-called mind/body problem.1 Such a discussion runs as follows. A determinist will argue it is the world that conditions people to desire, reason, act, etc. in certain ways, as such all human behaviour is explicable in terms of general laws without reference to a 'ghost in the machine'.2 Whereas the human efficacy view will argue that man can be an unmoved mover of the world of which he is part.3

It must be noted that we shall not in this Chapter, nor elsewhere, examine the problem of God as an unmoved mover of human agents. Instead we shall suppose that no God (or like agency) exists and examine the general characteristics of freedom assuming this to be so. Our justification for this procedure lies in our finding that, in Economics,
no such absolute affects the behaviour of economic agents (see Chapter 2). If, however, further research into this matter were to discover that some absolute is a central concept to Economics then the following analysis must be drastically modified accordingly.

Given the above, we characterise the two camps by:

(a) Determinist: 'No man consciously will change, is changing or has changed the world he inhabits,' and

(b) Human efficacy: 'At least one man has altered consciously the course of world events'.

Neither of the tenets stated above are susceptible to empirical test or hypothesis. The determinist view has two objections. First, as stated, all men must be examined before we can claim to show support for the statement in the data. It may be argued that one may use sampling techniques to get over the problem of monitoring an infinite set of data (since it is postulated no man has ever acted as an unmovable mover) but the second objection remains. There is no way we can show that changes which have been observed in part of the world can be said not to have occurred if man did not exist in that world. As far as we know, there is no test involving the principle of elimination which can be performed on data which it is impossible for a human observer to collect. The human efficacy view shares the latter objection with the determinist together with the difficulty of testing for whether a certain state of affairs is consciously brought about or not.

Since we have found that those parts of the determinist and human efficacy views which appear to be testable hypotheses are not in principle testable, we have to resort to the use of argument to decide the matter. The human efficacy view may uphold that if a person believes he changes the world, then he must be free to do so, whilst a determinist must say
that that belief is determined by a general law since it is part of the world. Yet this answer itself must involve a form of consciousness. Thus we are immediately faced with the question of whether it is possible to formulate judgments without that formation being governed by general laws of behaviour. An alternative formulation of this might be:

\[
\text{Is it true that } \ldots \text{ even if men are not free, they cannot help behaving as if they were.} \quad (\text{Caws P. 79}) \]

It is in this vein that the whole question will be examined.

Seemingly then the whole question cannot be settled by argument or by empirical investigation and so it is often considered that freedom be treated as a primitive term and its existence or non-existence as an assertion. But this solution is hardly suitable. It is important for us as scientists to know what we assume man can and cannot do. In science it is clearly impossible for this question to be investigated given the above assumptions. Hence a different approach is called for.

Let us examine the deterministic notion in more detail. It has been summarized as:

"Given the view that ultimately all the principles of behaviour will be reduced to law-like physical relationships between a few simple variables \ldots, the relationships which are apparently most satisfying are those which come to us from physical mechanics."

(de Charms P. 53)

Hence we may look upon a deterministic notion of man as seeing man as part and parcel of a closed system. As such he can have no possibility of being the unmovd mover. All behaviour of the system can be explained
in terms of these relationships. If freedom is defined as the impossi-
bility of such an explanation then clearly it is absent by hypotheses.

It is also absent of necessity. For freedom to exist we must show not only that the abovementioned explanation does not exist but also that it cannot. The statement:

"there is no explanation possible in terms of a closed system of laws"
must always be true. Now we have seen that in science the correspondence between observations and theoretical assertions is of necessity disturbed by both systems and observer alteration. Thus it must be true that there are certain sorts of behaviour that are not explicable by a closed system since systems are generally opened as explanation proceeds. However, this does not rule out the possibility of our hitting on the required explanation. Because such an explanation may be found we can never show that the above statement is always true. Thus freedom in this sense is totally absent from any explanation in terms of law-like relationships.

Now what of freedom itself. Since we have defined freedom albeit tentatively as:

the absence of general laws which determine a person's behaviour,
we must conclude that no explanations of a person's behaviour in terms of law-like relationships are acceptable if we suppose that person to be free. In this circumstance we cannot explain the difference between behaviour that is observed and the relevance (or significance) of behavioural laws in the explanation of this behaviour by recourse to the positing of a free agent. The positing of a free agent precludes our use of law-like explanations of that agent's behaviour. Freedom then, is not just the degree of indeterminism with which a law-like statement is asserted for if it were, one could correlate free will with a dustbin variable whose value could be calculated according to statistical laws.
In such a case freedom is supposed to be determined by some kind of law just as indeterminism may be. Thus, whilst indeterminism may behave according to law-like relationships, freedom does not, by definition, and so the two should not be equated.

I believe it follows that we must give up all notion of generalising about human behaviour if one is prepared to accept the concept of freedom defined above. Indeed it seems that the very act of describing behaviour is not freely done. Since we must obey certain conventions to make our description intelligible it appears that it is not without the law-like relationship mentioned above.

Not only does freedom seem impossible but also it seems complete determinism is also impossible. It has been shown by Gödel that for a closed system to be complete it must be inconsistent. Thus it follows if we are to insist upon a complete explanation of behaviour from our system we cannot obtain it. More formally, since explanation implies consistency and since the law-like statements contained in our system must explain observed behaviour, such that all the propositions of all the statements are borne out in the data, it follows that since some of the statements cannot be consistently deduced, the system cannot be completely borne out in behaviour. As such, it is not a complete explanation of behaviour. It is an argument such as this that leads economists to conclude with Kekta:

"The free-will is, therefore, free but, . . ., it is not cent per cent free." (P.57)

From this it follows that the will is undetermined by the laws which govern the behaviour of things to a greater or lesser degree. To the extent that the will is so governed, however, it is similar to any body
that is part of the world. Now, provided we can regard the body as so
governed, we can see that the will is separated from the body because it
does not obey the laws that a body does. It is its freedom which provides
a criterion by which the will is separated from the body of which it is part.

Yet to assert this criterion is not to imply that there is a separation
between the mind (or will, or soul) and the body. Only if it is assumed
that the body is subject to law-like relationships will the separation be
implied. Though this is not explicitly stated in the statement above of
our criterion, I believe it to be generally accepted. Hence, it seems to
follow that in following Leibniz we must accept the distinction between mind
and body. (Indeed it may be argued that any reasoning on the question of
personal freedom defined as the absence of any law-like relationships that
determine behaviour is but an illustration of the so-called mind/body
problem.)

The body then has a spatial context. Its parts and their inter-
relationships can be investigated by anyone; bodies are subject to
mechanical laws. Minds, on the other hand, are not in space. Their
workings are not easily analysable by anyone nor are they subject to
mechanical laws. For our purposes, it is important to realise that such
a dichotomy asserts that whilst the body (or things) are subject to cause
and effect, the mind (or the will) is not; since if it were the dichotomy
would scarcely hold up as described above. As it stands, the doctrine
sees a person as akin to a clock that contains a dozen which by sheer
wilfulness causes the clock to behave in an unpredictable way. The mind
then 'fills the gap' between observed behaviour and an asserted law-like
explanation. Such an idea has been criticised as follows.

Ryle has argued that to speak of the split between mind and body in
the above mentioned terms is to commit a category mistake. A category
mistake is essentially the ascription of a concept to be equivalent to a
type of thing to which it is not logically equivalent. It is akin to the concept of The Average Taxpayer (to use Ryle's analogy) of which Fred Bloggs may be a replica. His wife may well hate, love (or whatever) Fred Bloggs but she cannot do any of these things to The Average Taxpayer. If she were to state her love for The Average Taxpayer, one would look in vain for this person. It is the same for the demon in the clock.

We look in vain for this demon acting in the same terms as the clock itself for in doing so we are making a category mistake similar to that made by Mrs Bloggs. Mind and body are of different logical types. It follows that we should assert that the mind exists in quite a different sense from that in which the body is said to.

Thus we conclude:

"As the human body is a complex organised unit, so the human mind must be another complex organised unit, though one made of a different sort of stuff and with a different sort of structure." (Ryle P.20)

Yet this 'solution' does not help to answer the question with which we started this enquiry; namely, is it true that even if men are not free, they cannot help believing as if they were? This state of affairs is because Ryle's argument denies that the question of personal freedom can be posed in mind/body terms. If the mind is a different thing from the body then whether or not it is not subject to law-like explanation has nothing to do with its relationship with the body. The 'solution', therefore, changes it into two separate questions:

'what is human behaviour?'

and

'is it compatible with that expected from a free agent?'

No doubt any behaviour can be construed as emanating from a free agent, yet as such we have not faced the basic question which was to find
reasons for or against the proposition:  

"even if men are not free, they cannot help behaving as if they were."

An answer to Ryle's solution can at least give us a necessary condition for this to be borne out in the facts we can observe. Since it is equally likely that a determinist answer can also be concocted, we are no further forward.

We thus have to resort to an empirical investigation which will be inconclusive. If we find that the assertion of a free agent is borne out by the facts to a greater degree than the determinist theory, one can easily argue that this is determined by the environment in which the test took place. In order to assess the value of this argument, it is essential not to assert that this view too is determined (since this merely lends support to determinism) but to investigate the relationship between test results and the environment. It is paradoxical to note, however, that such an investigation can never find such a relationship. For if it is not there, it will not be found and if it is, it will merely support the original hypothesis in favour of a 'free agent' theory. There appears to be no way in which we can design a test for the hypothesis:

"this hypothesis is biased"

that can be conceived. Ryle's argument, then, leads us to the conclusion that if we can set up the problem of freedom in a testable fashion it cannot be tested.

An alternative approach, though connected, to the dissolution of the problem is to assert that a person may well believe he is behaving freely in a situation when an observer can clearly indicate certain laws to which his behaviour conforms. Hence it is common in economics to assert laws about the behaviour of free agents in the economic system. The mind/body
problem then becomes simply a statement of the point of view of the observer or the agent. If one is an agent, one believes oneself to be free, whereas if one is an observer one sees people on the average as not free. Stated thus, we commit a category mistake of the Average Taxpayer kind. But granted this solution we find it has the following shortcomings.

In observing society an observer must also be part of that society. This is particularly so of an Economic scientist, who may have to devote a significant proportion of the resources of the economy he is studying to the study of that economy. Hence in the case of a large research project the scientist must act as an agent within the economic system. Now this means that in investigating the system the scientist also changes the system in precisely the same manner as the agents he is studying changes it. Moreover, if the generalizations which the scientist uncovers are not more or less part of the will of the agents of which they are about then a collection of free agents will continuously refute those generalizations unless it was them in the construction of actions.

It would also seem that to dissolve the problem by recourse to the analogy between science and meta-statements, does not work because scientific statements are not precisely mirrored in the data as meta-statements are in their primary language. Scientific statements are linked to the data by correspondence rules which do not necessarily lead to the constructs employed. Meta-statements must have this necessary link which is essential if arguments within say a logic can be precisely represented in a meta-language.

The third kind of 'solution' is to see the problem in the same sort of terms as the second. It is different in the sense that freedom is seen as essential to determine laws of behaviour. Usually it is present in terms of an analogy. Suppose a person is arrested for a criminal
offence and is sent to trial. The Court proceedings are designed so that only the facts of the case are used to judge the guilt or otherwise of the person. His attitudes, beliefs, intentions, etc. are considered irrelevant to the conduct of his case. All that is needed is a decision of whether or not in fact a law has been transgressed. Let us suppose all the facts bearing on this decision are found and presented. It is possible then to show simply by recourse to facts the fact that a law was or was not transgressed. 9

Low it is argued science cannot be simply this when human beings are studied. Because they are not inanimate objects it is important to realise that their mental states have an important bearing on their behaviour. We are to accept then the demon in the clock but if the social scientists develop:

". . . a quality of mind that will help them to use information and to develop reason in order to achieve lucid summations of what is going on in the world and of what may be happening within themselves."

(Wright-Kills P.11)

it is possible for him to know how the demon acts in various circumstances. Thus though introspection and empathy the social scientist can formulate law-like statements about human beings in general. It is through our finding out more about the mind that the dichotomy between mind and body can be removed. The demon can be studied but by different means from that of the clock, so that its behaviour in relation to the clock can be described which will enable law-like statements to be made of the mind/body complex.
Such a view is a thorough-going denial of determinism for it pre-supposes the existence of a free will. We may, however, argue that the knowledge that can be grasped of this demon is determined by the environment in which the scientist works. Indeed his very concept of introspection may be a consequence of some childhood disaster or whatever.

Moreover, if we assert that simply by the play of the human intellect we can overcome the difficulties associated with the mind/body problem we have what appears to be an inconsistency. The intellect, it is supposed is free of determining laws. Yet it must be that, given other faculties of the mind are determined by environmental and other factors, an explanation exists for why the intellect is never so determined. Such an explanation seems to be similar to a law-like explanation. It would seem that this view assumes the intellect to be determined by a law-like relationship to be free. It is at once free and not free.

If, however, we deny that the freedom of the intellect is not an exception amongst the mind's faculties we have returned to the assertion of the demon in the clock. For if the mind is undetermined in this way, then no amount of empathy or introspection can find out its nature. We shall always by virtue of the fact that it is not governed by laws have our demon inside our clock.

The three arguments for the dissolution of the problem stated above are not satisfactory. Hence, it remains uncertain whether our original question must be answered affirmatively or not. There seems to be one alternative solution to the cul-de-sac we have argued into. It is our belief that the whole debate is a non-debate not because either view is true or false nor that the debate does not exist but that, as such, the debate is incapable of a solution which would imply the existence of the debate.

Freedom has been defined above as the state in which a can can change
part of the world in which he exists. A person must, therefore, be conceived as being able to act to some extent as an unmoved mover in at least part of the world. He must also be relatively immune from influences emanating from the world of which he is a part. A person is free insofar as he can alter the world not in response to an influence from outside his person but as a result of influences totally belonging and acting from within himself. This must be either believed or observed. In all cases this resolves itself into a simple conditional relationship between a subset of a state description and the state description itself.

Or, in other words, a person can only be conceived as free in the sense that the existence of a given state within him is followed causally by an ability to alter the state description of which he is part.

Stated in this way, however, it is tautological. For a person to experience something new is to change the state description of which he is part. But since we have stated a causal relation, it follows that the concept of freedom mentioned above is conditioned by the idea of causality we hold.

If we hold a different view of causality from that implied by the above discussion on freedom (as we must if our critics are right) then our concept of freedom too must be different. Instead, then, of seeing freedom in terms of an unmoved mover at the beginning of a causal sequence it seems much more fundamental to argue that freedom is the acceptance of the principle of causation by the agent concerned. It has been pointed out to me that this idea is identical with that of Sartre. (No doubt parallels will also be found between his "cause-intention-act-end" and our formulation of the nature of action.) Now this is derived from a person intuiting himself in terms of the world. If, as we are positing, a person can only conceive of freedom in terms of the principle of causation and he suffers no illusions about his place in the world, then
it is clear he cannot hold a concept of freedom that is consistent with a concept of causality that is at variance with our assumption of a person being an observer of other observers. Since we found other theories of causality suspect in this respect, we must conclude that it is inappropriate to hold the concept of freedom that is consistent with these. It is, therefore, necessary to analyze what freedom can mean in terms of the principle of causation we have expounded above.

We have argued freedom is the acceptance of the principle of causation. It is imperative that we try to make our reasons for this more explicit. A person in perceiving the world makes use of the language he has learned to associate the perceived representations with reality. In learning this language he experiments under the assumption that he is free to perform such experiments as he sees fit. It is the results of these experiments that enable a state description to be built up. But because they are experiments they cannot answer questions like 'Is a person indeed free to learn in this way?'. Our person learning this perception language clearly cannot also find out if what he perceives is 'reality' but he proceeds as if it were. It is this penchant for experiment that leads, I believe, men to the notions of freedom and causality that we have expressed. Since causality itself is the result of an experiment as is freedom, the arguments in favour of one must rest upon the same principles as that of the other. It follows that not only is it important to only associate concepts of freedom with like concepts of causality but also freedom and causality are derived by the same process. Now, since causality is derived from the principle of causation by analogy there must be similar process for the derivation of freedom.

If a person believes he is free then it means his experiments show that he can do certain acts when he desires. There is a causal relation between his, say, desires and his actions. The absence of freedom can
only be intuited through a constant denial of this process. Hence, freedom can be intuited only in terms of causation whereas determinism is intuited only in terms of its absence on a subjective level. Hence, we come to the paradoxical conclusion that a person can be considered free only insofar as he is part of a causal sequence.

In traditional analysis he is the unmoved mover of the sequence. But we have argued that he cannot be unmoved by changes in the state description of which he is part. This follows from our concept of the experiment. In order for the process of learning to take place, it is necessary for the results of the experiment to be assimilated by the experimenter. It is the motivating force or the pre-requisite for the learning of the principle of causation. As such we can see that a free individual is free to experiment only insofar as the principle of causation is not infringed, otherwise he is able only to be not free which is the notion of freedom. Freedom then is simply part of the process of learning about the state description of which the individual is part.

Clearly this has important implications for the concept of action. But before that subject is approached, we must mention the question of teleology and determinism.

In most of economics it is supposed that freedom (in the sense of an unmoved mover) is exercised towards an end. In the Great Marginalist Controversy, for instance, we were told that provided people ‘maximized something’, marginalism was to be upheld. Thus the validity of a supposedly scientific theory was pinned to the concept of teleology. We wish to assert that such reasoning is common amongst economists. Support for this assertion can be found in the following:

"... the manner in which a man reacts to stimuli is determined not so much by the properties, physical and
chemical, of the material body as by the subtle mind that has sway over it. And this mind is the instrument through which the inner self (whatever its precise nature) tries to satisfy its urges, its wants. The body and mind are both instruments in the hands of this inner self . . . . If the reactions of the body to external stimuli can be controlled (within limits) by the mind . . . , it is not difficult to see how all reactions work in harmony to subserve some ultimate object." (Nahta pp. 64-5).

Because of the assertion of mind and body, it is clear that we are forced, to avoid an infinite regress, to postulate some end towards which the mind is able to affect changes. Otherwise we shall have to argue that there is no possibility of regularity of behaviour and thus of science.

There are, however, certain well-known difficulties associated with this resort to teleological explanation. In the following Brown's analysis will form the basis of our comments. Suppose behaviour is directed towards an end. A given behaviour pattern that is required to bring an end about will not, of itself, bring it about. It is necessary but not sufficient for the achievement of that end. Thus to explain behaviour in terms of teleology one must postulate either necessary and sufficient or sufficient conditions for the achievement of an end.

This is all very well provided the end is achieved but let us imagine it is not. If we have postulated necessary and sufficient or sufficient conditions then we can argue one of the following:

1. The system is not functioning properly; or
2. Teleological laws simply give necessary conditions; or
3. Teleological laws are goals towards which behaviour is directed.

All three are unsatisfactory. First the system's behaviour depends upon the end. Hence if No. 1 is put forward it follows that the behaviour of the system is not fully specified in terms of the properties of it. Thus
there can be no necessity of our finding causally sufficient conditions for breakdown within the system itself. Hence we can never show how or why the system has broken down. For instance, if we assumed firms tried to maximize profits and, for some reason or another, we knew that firm A did not do so then there is nothing within our original system to help us point out why this should be so. In order to find out the cause of the breakdown, a new system incorporating perhaps a mark-up pricing policy must be invented.

Second, if the end is not achieved, we cannot explain in terms of interpretation no. 2 why the individual behaves as he does. Formally this means if \( T \) then \( A \) is a valid implication where \( T \) means a teleological law and \( A \) means the achievement of an end. But if \( \sim T \) then \( A \) is also valid hence whether \( T \) or \( \sim T \) \( A \) still occurs. Yet if, as we suppose, if \( T \) then \( \sim A \) we have a false implication and so we cannot explain in terms of \( T \) why \( A \) did not occur. Thus a macro-economic policy-taker may use a Keynesian Consumption Function to predict the rate of economic growth in an economy. If all other targets necessary for the achievement of the growth target are achieved then should the growth target not be achieved he will not know how to explain why people did not consume according to the postulated consumption function.

The Third interpretation implies that to explain by teleology is to mention goals towards which behaviour is directed successfully or not. Such an interpretation because it mentions final causes of behaviour is not capable of description in terms of necessary and/or sufficient conditions. Thus, desire and needs are seen to be causally related to the end or action which is committed. Only if in fact desire for a state of affairs does not produce its action can we then say that no such law exists, but this is clearly an extremely difficult test to run. It
is this interpretation which is most common in Economics, its grave drawback is its untestable nature. Perhaps the unrefutable nature of such an explanation is itself an explanation for the 'maximisation' models common in all economic decision theory when the institutional evidence seems to be against such models.

But perhaps more importantly it is not that it is impossible to show that a teleological explanation can never be shown to be incorrect. We say it is true, encompasses any findings at all with the concept of a 'something' maximiser. The above arguments of Brown demonstrate the redundancy of the whole concept; if there seems to be good reason to doubt a teleological explanation than that explanation is no help in finding new explanations. As such, it is lacking an important property of scientific explanation. Thus teleological explanation is always valid because it is an explanation of which it is impossible to show how, why or when it is invalid. It is not because of any positive evidence in its favour that teleology is accepted but because no test can, as yet, be devised which could conceivably be capable of refuting it. In other words, although correspondence rules can be constructed for teleology from the data no reverse correspondence rules can be constructed that permit the Principle of Elimination to be used. Insofar as a teleological explanation is induced, then it appears to be purely enumerative.

The reason for this property of teleological explanation lies in a confusion between causes and states. If we accept our view that causes and effects are really only identifiable states in a continuum of events then it becomes clear that a teleological law is not truly a cause as such. Rather it is some final resultant of a particular process. It is an end state incapable of change. For this reason, one cannot use eliminative techniques since one can always argue that the process which leads us
towards the end is yet to be completed and so any test is invalid.

There is also the point that such an end state need not produce the action required to achieve it. This is simply because if a person is free then unless there is a causal relation between his action and its desired end (assuming for the present action is intentionally oriented) then he will not perform that action. But as we have seen, an end such as this cannot be a cause and so cannot be the motive-force of a given action. Thus, although an action may be the one that leads towards an end, it may not be performed simply because the end cannot cause the action to be performed.

I feel that irrefutability is an undesirable property of scientific statements. Instead I propose that the whole teleological concept of freedom be abandoned. With this abandonment many of the difficulties apropos the question of determinism are dissolved. Only one need really concern us here.

To study a free agent in terms of ends provides science with a justification for answering, in the affirmative, the question posed at the beginning of this chapter. A person seems to obey given laws but to obey them with a free will. But such a conclusion does not help us in the face of our other conclusion that a person is free only insofar as his behaviour is conditioned by causality. Only if we are to admit final causes (in the Aristotelian sense) to our analysis can these conclusions be made consistent. Since this is freely admitted to be the denial of an empirically based science we must conclude that either much of economics must be abandoned or that its pretense of empiricism must be dropped.

The solution to this case seems to be as follows. Let us conceive of freedom as part of a process leading to action. As such, it is undirected towards an end but is the outcome of our being aware of the world and the changes we can bring and our awareness of the world causing changes in us. There is no world without us nor us without the world.
Hence, the question with which we started our chapter (namely, is it true that even if men are not free, they cannot help behaving as if they were?) is quite meaningless. People are free since they are engaged in this process thus there is no possibility of separating the influences upon people and by people.

Now this result is of great importance for economics. It means that economics based upon this concept of freedom is neither grounded in a particular type of psychology nor in a particular type of determinism but rather that many such particulars can be, in principle, placed within the format to form specific models of economic activity. As such, it could be argued that our attempt to answer our original question has led us to uncover a fundamental characteristic of economic argument. It is not expected that it is anything more than one category of all arguments and it is certainly not valid to argue that all economics must conform to this criterion if it is to be classed as 'economic'.

The foregoing does, amongst other things, assist us to clear up a certain disagreement over the purpose of economic generalisations. If we assume that such 'laws' are causal then we can delimit two points of view. First there is the determinist view. This states that economic generalisations show us how human beings in an economic system will behave at a given space/time relation. It asserts that once all the laws governing the system can be known we can explain, in general, the behaviour of the vast mass of actors in that system. Second we have the interventionist. He would argue that these laws show what happens without there being any conscious attempt to alter the system. If thus shows that action to change the system must proceed along lines given by the nature and influence of the causal laws. Clearly the one accepts a full causality without imputing freedom to individuals and the other accepts freedom whilst denying full causality. Because of the nature of either view we have seen that the one is not strong enough to provide a sufficient refutation.
of the other. Hence any debate on these lines is not, in principle, capable of solution.

If, however, the definition of freedom we have proposed as the free acceptance by the individual of the Principle of Causation the reason for the lack of a solution becomes clear.

Both schools of thought concentrate on but one part of the causal relation and between man and his environment. The determinist sees man as determined by his world but forgets that his world does not exist without man, whilst the interventionist sees man determining the world and in forgetting that part of that world is other men, fails to realise that man can be influenced by the world.

Once this conclusion is taken, several interesting conclusions follow. It is hoped these will become clearer in the rest of this work. But before such an exposition can be contemplated certain other points must be cleared especially with respect to the notion of decision in economics. It is to this task we shall now repair.
We are told by Lord Robbins that:

"Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses." (Robbins P. 16)

Now, by two other authors, we are informed that:

"The theory of action is a conceptual scheme for the analysis of the behaviour of living organisms. It conceives of this behaviour as oriented to the attainment of ends in situations, by means of the normatively regulated expenditure of energy." (Parsons and Shils P. 53 footnote omitted).

It seems that Robbins is stating that economics is about the analysis of action in much less formal terms than Parsons and Shils. Despite recent criticisms of the Robbins Definition¹ it is, I believe, fair to assert that the vast makeweight of economic concepts is based upon the formalisation of an economic unit as an actor in the above sense.
For instance, individuals are seen as welfare maximisers (with weak or strong ordering), or risk averters (to a greater or lesser extent), or satisficers; firms are assumed to maximise profits, or sales, or growth; governments are conceived to guarantee a certain desired target such as full employment or to reduce diseconomies of an economic activity, and in all these cases the economic problem is seen to arise because only a few of the necessary techniques exist to achieve all the desired goals at a given space/time. We can characterise this conception as action proceeding towards a desired order. Action, then, is the process by which the desired order is to be achieved. 2

It must, however, be emphasised that action is not the only behaviour process that can be conceived to exist nor is said to exist by sociologists. It is seen, nevertheless, as the primary mode of behaviour of free human beings. Thus, free action is seen as the conjunction of five conditions:

"... the agent:

(1) intends to achieve a given end by it,
(2) is not restrained by another agent,
(3) has suitable resources of energy and material at his disposal,
(4) has sufficient information about the state of the world at the time of the action, and
(5) knows the laws governing the behaviour of that world and hence the consequences of his action."

(Cass P.100)

Any behaviour which does not include all of these notions is thus seen as not free action. But clearly if any of the conditions is missing the behaviour cannot strictly be classed as action either. This is seen if
we omit each condition in turn. There can be no quarrel over (1) and (3) for these are implied in the concept of action anyway. Conditions (2), (4) and (5) if dropped would lead the actor not to be able to achieve the goals he wishes. As such it is very difficult to see how the theory of action can be used as an explanation of behaviour in these circumstances, for, as we saw in the previous chapter, it is extremely difficult to see what use a teleological explanation is if the ends it posits are not achieved. Thus action is necessarily free action.

We have said that action is the process by which a given order is achieved. As such, we can explain action in terms of intentions, motives etc. but these explanatory devices are not essential to the central concept of action set up in the first sentence of this paragraph. There have been four ways in which action has been analyzed. First by equating acts with the use of verbs. Second, by seeing a human act as ascribing responsibility for a particular deed; third by defining action in terms of an antecedent and, fourth, by asking if actions are events which it makes sense to qualify in some way. Let us examine these ways in turn.

The first way is suspect because the relationship between the use of a verb and the mention of an action is imprecise. The second type of analysis assumes that:

'a is responsible for x'

is equivalent to:

'a did x'.

An answer that a is responsible need not imply that a does, has done or will do anything at all. The analysis, too, must proceed by ascribing a rule of responsibility which would lend a degree of arbitrariness to the whole analysis. And the fourth way of examining action does not try to help us with the proportion that action must have which is, after all, the point of the exercise.
We thus come to the third concept of action.

This sort of analysis can be conveniently divided into two. Either one supposes that an action is a necessary consequence of an antecedent or that the antecedent is only contingently related to the action. As to the second category, it is always logically possible for the action to be performed without the antecedent. Hence, if we say:

'a rate of inflation of 10% p.a. requires a budget surplus'

the running of a budget surplus may have nothing whatsoever to do with an inflation of 10%. (It is crucial to note that conjunctions like 'requires', 'desires', etc. are not necessary for the performance of the action.) Granted this an antecedent which is but contingently associated with an action cannot logically provide us with a view of the nature of action.

Hence, in our examination of action as preceded by an antecedent we must consider that antecedent as necessarily related to our action.

This view has been expressed by Hobbes as:

"There be in Animals, two sorts of Motions peculiar to them: One called Vittally; begun in generation, and continued without interruption through their whole life . . . to which Motions there needs no help of Imagination. The other is Animal Motion, otherwise called Voluntary Motion as to go, to speak, to move any of our limbs, in such manner as if first fancied in our minds." (Hobbes P. 39 italics omitted)

or by Austin:

"Certain movements of our bodies follow invariably and immediately our desires and wishes for the same movements . . . These antecedent wishes and these consequent movements are human volitions and acts . . . Our desires of these bodily movements which immediately follow our desires for them are the only volitions; or . . ."
the only acts of the will . . ." (Austin quoted by White P. 5 italics omitted).

Acts then are a necessary consequence of 'the will'. Bodily action is then seen to be a kind of emanation of 'the will' into bodily movements. Again, we are faced with a demon in the clock. The demon is essential to the clock performing any movement other than simple reactions to environmental stimuli. Thus the mind is able to cause bodily actions. At this logical conclusion then we see that:

"Our will causes our bodily actions in the same sense, and in no other, in which cold causes ice, or a spark causes an explosion of gunpowder." (Hill §2 J P. 232).

Desire then causes actions. No account need be given of where the desire is derived since we are enquiring into the nature of action.

Such is the general nature of action that is supposed by economics. If we recall the quotation from Robbins at the head of the chapter on our analysis of action in Chapter One of the present work we can see this is so. In particular, we assert that however preferences are constructed a rational being (free in the sense of undetermined by laws of behaviour) will act according to them. Insofar as this concept of action is dependent upon the distinction between mind and body it is thoroughly consistent with the 'undetermined' concept of freedom which, as we found in Chapter Nine, was also based upon this distinction. It must, therefore, be subject to the criticisms made in that Chapter. There are, however, other criticisms that can be made of this view of action.

Here we shall concentrate on two.

First it is argued to conceive of action as the necessary consequence of 'the will' is to neglect the distinction between an act and a voluntary act. Acts are not necessarily voluntary for they may be obligatory or they may be forced upon an agent. For instance, it is argued, the
contract between buying apples rather than pears or buying apples because there is no alternative good available in a contrast between a voluntary and an obligatory act, whilst the distinction between resignation and dismissal is that which lies between an act performed voluntarily and one forced upon that person. Acts then are not purely voluntary. Also, it is argued, that to see all acts as voluntary is to assert that a voluntary act is necessarily preceded by a volition. But if we see that voluntary implies the assurance that an actor has an alternative to the action he performs then some acts are not voluntary. Rather they may be obligatory (in which no alternatives exist) or unintentional in which alternatives are not even thought of.

I believe this criticism to be groundless. Action cannot ever be obligatory since the actor always has the alternative of not performing the action. Thus when one hears of the producer being forced to put up prices it is because he is not willing to contemplate bankruptcy. Moreover, if a situation could be conceived in which action were obligatory then the situation (which is either the result of voluntary action or some kind of externality) is the reason for that action being termed obligatory.

There appears to a confusion in the argument over the term obligatory. We read that:

"what I do non-voluntarily I do because I am obliged to do it, that is, because in the circumstances all alternatives other than . . . [the action performed] . . . are closed to me either physically . . . [legally, morally, or in some other way]." (White, p. 6)

In no sense can these be termed obligatory in the sense that no alternatives exist. A person may bow to the physical force, act illegally, immorally or whatever. But the different degree of obligation implied by these
constraining factors at different times to different people seems to indicate that those who would hold that obligatory acts are not by definition impossible are not consistent in their definition of obligatory. One would suppose that physical, legal and moral sanctions are in that order of decreasing effectiveness in creating obligatory actions.

Moreover the manipulation of the environment may well lead to voluntary actions being performed that conform more to a desired obligatory act than actions that the sanctions designed to impose that act are able to cause. An example may be the 'carrot and stick' regional development policies in Great Britain. It would, therefore, seem that the distinction between voluntary and obligatory acts is a matter of degree not of type.

We now consider the question of unintentional actions in which no alternative is envisaged because none is sought, but if no alternative is sought then the action becomes more or less a reaction to outside stimuli such as wincing in pain or shivering with cold, for it cannot be obligatory nor voluntary but must 'belong' to the agent. Such a reaction can scarcely be classed as an action on the part of the actor. It appears much more the result of an action performed by someone else or of a change in the environment upon the actor that reacts unintentionally. Although it is performed by a particular person it is not truly his act for he cannot control it and so does not 'belong' to him in the sense that a voluntary act does.

With the acceptance of these comments it would seem that the first criticism does not hold logical water. Thus the theory of action under consideration does not separate act and voluntary act because no such separation is possible.

There is, however, a second criticism of this view of action. The idea that an action is something proceeded by 'the will' or a volition has difficulties unconnected with the first line of criticism.
itself, is a concept which is difficult to characterize precisely. Thus we read:

"We . . . have to admit that while we know the general character of that to which we refer when we use the word 'willing', this character is sui generis and so incapable of being defined, i.e. of having its nature expressed in terms of the nature of other things."

(Friderich P.61)

Hence it is something of which everyone knows but which no-one can precisely describe. In particular, it does not have the properties of an act for if it did an act of volition could be described separately from its effect. For example, if I say I bought some apples because of an act of volition on my part then when asked 'why did you buy apples' all I can answer is 'because I wanted apples'. On further questioning I may answer 'because I willed the wanting apples' and so on.

Such a difficulty arises for two reasons. First, the concept of willing is itself undefined because no definition is possible in terms of other like ideas. This means that if we are to search for a reason for an action in volition no reason can be stated, except those given in the previous paragraph, because the postulated reason (i.e. volition) has not been defined in the first place.

Second the division between mind and body implied by the volitional concept of action leads to grave difficulties on the nature of the relationship between 'the will' and the movements of the body. This arises because the mind is seen as made of different stuff from that of which the body is composed. It is, therefore, inconceivable that a 'willing' can cause a 'movement' in the same way that excess demand can cause a price rise. The root a 'willing' can cause is a desire which is but the first in a long line of intermediate states of mind such as
resolution or intention which link 'the will' to bodily movements. But no such link can exist all the while mind and body are assumed not to be contiguous. Yet this is the very assumption that we found in Chapter Nine to be unfounded.

It would seem, then, that this second criticism to be in part at least well-founded. Hence we must revise our concept of action in the light of the above and of our findings in Chapter Nine.

There is one point arising from the immediately preceding analysis which, although anticipating our later argument, seems best mentioned here. Our second reason for the weakness of the view of action presently being discussed involved the lack of contiguity between the mind and the body. Now this can only be a reason for such a weakness if volitions and movements are causally linked and that the notion of cause here implied involves contiguity. We are immediately reminded of Ruse's list of proportions that a causal relation must have. Thus we seem to have a prima facie case for arguing that the volitional/movement view of action presupposes a human causal process.

In criticising the above view of the nature of causality, care must be taken to avoid the description of the analysis as an explanation of action. It seems that in stating the principle that all actions are derived from volitions involves us not in explaining those actions in the sense of stating precise conditions for their occurrence but in looking at actions from a particular point of view. For instance, it is not sufficient to explain an act, say, consuming apples, by resort to the statement 'I wanted to consume apples'. By this we cannot explain why apples and not bulldozers were consumed, why the consumption of apples was at the level it was or why apples are not consumed all the time by this individual. To facilitate such explanation, therefore, a statement of the nature of the article (a definition of a good), a statement of the rule by which
decision in zero (prefer more to less), a statement of what may precipitate a change in consumption (an inverse relationship between demand and price) and a statement of the end to which the process proceeds (welfare maximization). That such an apparently complete explanation of action has been constructed in Economics is, I believe, clear. That, however, its nature is of the volitional/movement type of causality means that it is subject to the sorts of criticism we have added above.

The various difficulties we have found over the concepts of value and action in Economics (see Chapters One to Three) apparently are derived from the nature of the concept of action employed. Action we have argued is a bodily movement performed by an organism which can perceive an alternative to that action. We have also free action which is identical to action hence we must suppose that our organism is free in the sense of accepting the Principle of Causation. (So is only free insofar as he can cause something to happen which can only be possible by his intervening in the mechanisms of the world not by constraining them but by turning these mechanisms to his own use.) If we are to remove the difficulties contained in the notion of economic action then it would seem that we must build up a theory of action that is non-volitional using these ideas.

At first sight we have constructed a contradiction for in traditional analysis freedom is taken to mean the absence of laws governing an individual's behaviour but we have argued that to be free the agent must be part of a causal process. That this is not a contradiction is seen if we think of action as the attainment of a state of affairs rationally designed. Only if the end of action were causally related to action that leads to its attainment could the agent be free in any respect. Thus traditional analysis by defining freedom as it does rules out the possibility of a rational agent ever being free.

The nature of action, then, is that of a process which is performed by rational living organisms. As such it is formally identified to
the process of causality. The process which is called action can be characterized as follows. An organism insofar as it can conceive of the ends to which it is formulating its action acts rationally. A rational organism thus selects a given end and then proceeds to employ the most suitable means of achieving it. Now in this process an end cannot be conceived as directing action unless it can be conceived of being achievable. This is clearly conditioned by the organism's ability to perform the means required of it. At the same time, no means will be employed that will lead the organism away from its desired goal. Thus in action one cannot conceive of the end without having conceived of the means that will achieve that end nor can one conceive of a means without having conceived of the end to which it is directed. There is no possibility of conceiving in a meaningful sense of end or means in isolation from each other.

In particular, the idea that a free, rational organism can examine what he wishes to do in isolation from the possible means of achieving that aim seems unfounded. Only if, in fact, his aims or ends are causally related to the means of achieving them can action be performed. Thus instead of conceiving of the individual as trying to match market data (as he sees it) to his primary desires, the economist must see the preferences of individuals as causally connected to market data.

If this separation is not possible then the concept of rationality as the extent to which an individual's actions conform to his stated aims appears also to be impossible. For if a person cannot compare the results of his actions with separable ends he cannot assess the rationality of one action as against that of another. To him all actions will seem equally rational. Thus we must revise our concept of rationality as the basis of selection. Instead we shall assert that an individual is to be seen as rational if we see him experimenting with what he can
perform. Not only is he seen as unsure of his ends but also of the means he can use to get those ends. Hence, he experiments. We can regard all of life indeed as the succession of experiments to find out these sorts of facts. In particular we suppose our organism is especially interested in finding relationships between these sorts of facts. As such there are no ends and means nor even the functions of ends and means but rather a particular sort of experiment being performed at a given time.

Such a process would run as follows. One might set up an end as a hypothesis say welfare maximization. An organism can then be said to use this as an end insofar as it is seen to attempt to achieve Pareto optimality. But since it is logically impossible for a person to consume more than one good at a time it is quite impossible for a person to ever achieve a welfare optimum of this sort, if there is more than one good open and if new goods appear for his consumption. Thus the nature of the optimum must change. Given this a person, if he is rational, will not attempt to achieve an impossible end and thus his end will be modified. Let us say he tries to achieve a level of welfare higher in this period than in the last. With this end in view, he sets about finding means which will help him to this end and so on. But at the same time not only are his ends changing but also the means by which they are achieved in more and more experiments are performed. Hence, the form of the experiments will alter largely on the circumstances impinging upon the experiments that are performed. Not only this, however, for in this process of succeeding experiments the individual will be changed by the new information, sensations and so on that occur. Human activity is then a succession of experiments.

If we are to equate this activity with action, then it is clear a different concept of its nature from the traditional one must be looked for. To behave with freedom is to act. But to behave with freedom in
also to behave according to the process of causation. Now this leads us
to define activity as the process wherein organisms act upon, and in, the
world. In this process ends are achieved and means are performed but
these are not the causes of the activity but merely symptoms. The cause
is that man is in continual experiment to assess his place in the world
and the world's place in him. That he has recourse to explanation of
his behaviour in terms of ends and means is a result of his assessment
that he is able to be a mover of the world on the basis of desires not
determined by that world. But since the world contains men it seems a
little inconsistent to then argue he may not be altered by the world.

On the most analytical level it seems such a conception overcomes
many of the difficulties contained in the traditional concept of action.
For instance, the argument over voluntary, obligatory and unintentional
action is seen as misleading. In using the concept of activity there
are no distinctions of this sort to cause difficulties. Instead that
since all are forms of action performed by free organisms it follows that
they are different only insofar as each interaction is different.
Voluntary action is of the sort described above, obligatory action is
simply an interaction whose ends are not those of the agent performing
the means whilst unintentional action cannot be performed by free
organisms. It is the result of an accident. As such the process of
action is precipitated by accident but the process itself is not accident
for it is impossible for one accident to cause another if one defines
accident as an uncaused phenomenon. Thus we would argue no distinction
between these sorts of action is at all basic to their natures.

It must not be thought, however, that the above amounts to our saying
everything depends on everything else. Instead, we believe we have
argued that only certain events are related in the above way. Rather we
would argue social scientists are mistaken if they contend that relationships of the form:

'if A then B'

will ever be found in society. The social relationships are fundamentally different from those contained in say the analysis of molecules. Not only is it possible to study a molecule separate from its environment and hence to subject that molecule to the separate influences of its environment but also one can envisage that the experiment can be repeated for other separate influences. In social science, on the other hand, it is impossible to conceive of the object of study as not part of the world for the world is a creation of the object of study and is as much the object of his attention as he is under its influence. Hence, it is only possible to conceive of experimental social science in very narrow limits and also quite impossible to perform the same experiment twice. In those circumstances not only does it appear that investigations to find 'if . . . then' generalisations are difficult but also they are likely to be misleading if found.

Until now we have concentrated our discussion on the nature of action for it is here that the difficulties associated with its description and explanation seem to be founded. It should not be taken that these latter difficulties are unimportant ones. It is merely our feeling that light can be thrown on this darkness once we have clarified the nature of action in general. In what follows, then, we shall attempt to discover a description of and an explanation for, a particular act within the framework of our previous analysis.

In the description of an act there is no single mode of description. This simply because acts are as diverse as 'things'. So far as we know there is no one description valid for all things thus we should not expect to find such a description for all particular acts. Acts may be
described physically (buying a good) or by reference to its agent (an act of Government policy) or by being of a certain type. Included in the latter are moral acts (unjust acts), psychological (desperate acts), legal (theft) or institutional (homicide). We do not propose to enter the discussion on this matter to any great extent except to note that given we class acts by the moral/legal/psychological/institutional catalogue only certain categories of acts are studied by Economics.

Moral acts are excluded entirely from our so-called value-free science by assumption. Economists are not supposed to talk about unjust acts insofar as they are unjust. But unless one is aware that actions do have value-laden implications by virtue of their circumstances or intentions it may well be that a particular action is better analysed as moral rather than as legal for instance. This is a subject upon which more research is needed. Again we must call this to the attention of other researchers and pass on.

For our purposes, we need only point out that only institutional acts are analysed by economics. Whilst these acts may have moral, psychological or legal aspects it is the institutional nature of them that interests Economic Science. For instance, if the Government acts so as to try to change the interest rate that policy will only be described in its institutional context by an Economist. The circumstances (unemployment), its consequences (increased investment) its intention (reducing the numbers out of work), and so on, will be stated in institutional terms and the total description will change as each of its components changes.

Let us now consider the question of the explanation of action. Since we are not concerned with the philosophy of action as such but how that body of thinking can help us to elucidate everyday life we shall not be concerned with a detailed study of the explanation of action. We shall not, for instance, be considering the forms of the various types of
explanation or how these types are related since we shall argue there is only one complete explanation of action. Our starting point will again be the volition/movement model of action.

In this model the explanation of an action is given in terms of a motive. Motives or inclinations (Ryle) or ends (Ebbinghaus) or targets (Tinkham) are propensities which govern a person's behaviour. Apart from this, admittedly loose phraseology, it is difficult to describe a motive except in negative terms.

For instance a Government may act in a certain way because it has a motive to achieve the end in which that act results; it may want to keep unemployment at the 2½ level. If a motive is to be an explanation of an act it must be both necessary and sufficient for the performance of that act.

If we examine this example a little more closely we can, I think, draw the following conclusion. The motive here is to keep unemployment at the 2½ level. How this implies two procedures. First a statement of a general propensity which governs the Government policy. It is a statement induced from several occurrences of Government policy on unemployment in the past. Second there is implied that in the future the Government will act to keep unemployment at the 2½ level should it tend to differ from it.

Granted this argument then it appears that a motive is induced by a constant conjunction between a given action (say Government unemployment policies) and some fact or state (say 2½ unemployment). Hence a motive is a kind of cause of action. This together with the postulate of what has happened before will, ceteris paribus, happen in the future seems to guarantee the necessity and sufficiency of a motive explanation. Furthermore, granted that freedom in the acceptance of the process of causation by the actor and given that actors must be free, action is only
possible insofar as the motives of action are causally related to the movements performed. Thus, it seems that motive explanation is a form of causal explanation.

Let us now consider various drives to see why they are not motives in order to clarify the concept of motive by examining what it is not. A psychological state such as a feeling or a thought is not a motive. For example, the mere feeling that apples keep no healthy is neither necessary nor sufficient for my purchase of apples. Either is a disposition such as cruelty a motive for a Government cannot be said to act out of cruelty if nothing is done when unemployment is very high. Either, it is the fact that a Government does not reduce high levels of unemployment that would lead some to conclude it is cruel. A disposition, then, may be sufficient but cannot be necessary for a given action.

Desires are also not motives as can be seen as follows. To desire to do something is not necessarily to perform an action. Rather one should regard the desire of an end of an action as preceding from the concept of an undetermined will. For if the will is the source of the drives which lead to action then a 'free will' will generate only those drives which originate solely in the individual. Such a drive is a desire. Whilst, in this concept of freedom action must be performed only if it is desired, not all desires are translated into action. A desire, then, is sufficient but not necessary for the performance of an action.

Under our concept of freedom, however, there is no recourse to the will. Instead we have construed freedom as the acceptance by the individual of causal nature of the world upon which he is acting. Presumably, the concept of 'the will' is not essential for a statement of this view. Hence insofar as desires can be defined in these terms it would appear they do not influence the nature of action and so cannot
be used as an explanation of action.

Finally, let us examine intentions. To intend a given action does not mean that one performs that action neither does it mean that a given action was a result of an intention. It would seem that intention implies a predilection for a future action. As such, it is neither necessary nor sufficient for the performance of an action and so would seem to be a kind of prediction that a given action will be performed given the existence of the motive for that action in the future.

It would seem that motives provide the only complete explanation of action so far advanced. This, however, is not to show that motives are the only possible explanation of action. We shall not investigate this matter but merely state that any explanation of action which gives necessary and sufficient conditions for that action will be termed a motive. Already there seems to be support for the view that motives and actions are causally related. Although such a relation will guarantee a motive as a complete explanation it may well be that the apparent similarity is unfounded. It is to this task we shall now turn.

Apart from our proposed definition of a motive others have been advanced, for example:

". . . the anticipation of a change in affect that is associated with a behavioural sequence under certain stimulating conditions." (de Charms, P. 162)

where an affect is an emotional response. It is thus a purely subjective condition that is correlated with a given action. As such, it stands in causal relation with the action. However, this is not the usual philosophical interpretation of motive. White gives as an example of a motive:
"... a desire to blacken a rival's character ..."
(P.15)

Now this is a motive for the performance of action only if the motive is both necessary and sufficient for a given action, say, rumour mongering. If that motive does not lead to the individual indulging in rumour mongering then it is not a necessary condition for that action and so not a motive.

It seems then that only if a given motive were thought to be causally related to an action would a certain action be performed. For instance only if I think I can watch the Derby will I go to Epsom racecourse on the first Wednesday in June, otherwise my going to Epsom racecourse would be totally unexplained. Sometimes I make mistakes such as going to Epsom on the second Wednesday in June to watch the Derby but such mistakes are due to my acting as if a causal relation did exist between my motive and action when in fact it did not. It is thus due to my lack of knowledge or insufficient investigation of the matter. Thus, it seems in order to act freely I must investigate the causal relationship between my actions and their results. There seems, then the reason to equate motives with causes and actions with effects.

Yet to show that motives are causes we must demonstrate that no other relationship between them and actions will provide us with a necessary and sufficient explanation of that action. Failing a detailed proof of this proposition we rely on the following argument. For a motive to be both necessary and sufficient for an action it must be uniquely related to that action. Also, neither can refer to empty sets of phenomena. Further a motive cannot be described without reference to its action, nor an action without reference to its motive, just as we found the description of causes and effects was similarly interrelated. Given these similarities it seems that a proof similar to that given in Chapter Five could be
constructed to show that motive/action statements could be represented only in an inconsistent logic. The basis of that proof being the distinction between a colloquial and a logical language. Since motives like causes are apparently mentioned in a colloquial language the statement of such a proof may not be difficult.

It may, however, be argued that such a view is suspect because a motive does not explain an action as exactly as a cause explains an effect. For instance, to explain an action, say, breaking a window, by the motives of the schoolboy who threw the stone is asserted by some to not explain as exactly the breaking of a window by research into the causal interaction between the window and a stone hitting it. This distinction is derived from the fact that the schoolboy need not have thrown the stone in the first place. Yet to argue thusly seems to imply that the stone had to hit this particular window in the research into the causes of windows breaking. This is not so. A causal relation is until it is experimented with only potentially a fact which is true also of a motive/action relationship.

Furthermore, although we may by experiment separate cause from effect by certain criteria such as priority, etc., this separation is only to facilitate the description of that causal relation. As we saw in Chapter Eight causal relations are best conceived as stating the relationship between the two events in a continuum of events. Given this the separation of cause and effect is false in that there is a two-way interrelationship between the two events that have been identified. Now the above arguments would suggest that the case is true of a motive/action relation. A certain motive implies a given action and vice versa.

Now this implies that all action is explicable in terms of causal relationships. Against this argument are strong rebuttals. Here we shall mention two since we are keen to avoid linguistic niceties in
argument both for and against the above.

It is argued that reasons (motives) are distinct from causes. A reason for action must contain a reference to the action or to the means of achieving it. But if a motive is to be a cause then it must be logically separable from that which is its effect. In other words a cause must observe Hume's rule:

"That there is nothing in any object considered in itself, which can afford us a reason for drawing a conclusion beyond it." (Hume P.139)

interpreted as being able to describe a cause without reference to its effect. Clearly, then if causality is to be taken as a 'billiard ball relationship' a motive cannot be a cause. It is not our purpose to dispute upon this interpretation of Hume but we hold it to be wrong.

Since our theory is not, in this thesis, dependent upon human causality we can ignore such rules set up for theories which are.

Thus, from what we have said about causality, such a criticism is unfounded. Causality is based upon the concept of a process between two events. That because the events are interdependent does not mean that they are not separable as events nor does it follow that the process itself is indeterminate. All that can be said is that without reference to its effect a cause cannot be described as a cause. This criticism does not demonstrate that motives are not causes.

The second sort of objection is related to the above. Let us imagine that there is a sealed envelope on the desk of the Governor of the Bank of England addressed to the Chairman of the National Westminster Bank. He does not know but the envelope contains orders that if carried out will spell disaster to the nation's economy. In his curiosity he sends the letter and disaster duly befalls the nations. It is argued that in this
example the motive or reason for the Governor sending the letter may
well have been his curiosity to see what happened if he did but this was
not his reason for devastating the British economy. On the other hand,
one could argue that it was the Governor's curiosity that caused the
devastation. Thus, it is argued, reasons are not causes for if they were
then we could say that the Governor's curiosity was the reason for the
devastation of the economy.

Leaving aside the difficulties over whether the Governor was best
advised to send the letter in his curiosity rather than open it appears
that this argument amounts to arguing that reasons are not transitive as
causes are. This is best shown symbolically. If we say the reason for
sending the letter was the Governor's curiosity but this was not the
reason for economic disaster we may express this as follows:

\[
\begin{align*}
C & \rightarrow r \rightarrow S \\
S & \rightarrow r \rightarrow D
\end{align*}
\]

where \( C \) is the Governor's curiosity, \( S \) is the sending of the letter, \( D \)
is economic disaster and \( r \) is the reason for. A causal relationship
between the same occurrences would run:

\[
\begin{align*}
C & \rightarrow c \rightarrow S \\
S & \rightarrow c \rightarrow D
\end{align*}
\]

where \( c \) is the cause of. In the second argument \( C \rightarrow D \) is valid but in
the first \( C \rightarrow D \) is not valid. Only if \( c \) was a transitive relation and
\( r \) was not would this be so.

Such an argument appears to ignore the nature of causality as we
have described it. In a world of 'billiard-ball interactions' it may
well appear that the first interaction is the cause of the latter
interactions. But the nature of causality described in Chapter Eight is
not limited to those kinds of interactions as causes. Instead we would
see each interaction of the billiard balls as an event in a continuum of such events. Thus each strike would if looked at in isolation to the others would be the result of a cause and an effect which are interdependent. It is not then that each particular cause is transitive in the sense implied but the continuum of which it is part makes it appear that the first cause is the cause of later strikes.

So it is with a motive or reason. Whilst a particular reason is only a reason for a particular action it is part of a process whereby a given state of affairs is brought about by action. It would seem then that arguments such as the one above involving the Governor are based upon the comparison of a particular motive with a causal process. As such they cannot show that motives are not causes.

Actions, then, are causally related to motives. They are properly regarded, therefore, as a process by which a given state of affairs is brought about through human activity. But having stated that end having explained why a given motive leads to a given action, we have not explained why action 1 and not action 2 was performed. One such explanation is decision theory. That it is very prevalent in economics will, I think, become clear as we proceed. If action is grounded in the will and if people are rational, free beings then to perform an action must involve a problem of choice between alternatives. Of this choice we can ask two distinct questions; namely,

'why is action 1 chosen rather than action 2 ?'
and
'how is this choice process performed?'

In economics these questions are invariably muddled.

To specify an explanation of why action 1 rather than action 2 is performed is not to give necessary and sufficient conditions for that action 1 unless an explanation of why a person should perform actions is given. Thus to give a theory of decision is not to explain why a
particular action is performed, but to describe the process by which motives are selected. Hence, decision theory is really only a response to the second question asked above.

Decision theory does, however, attempt to answer to the first question. It states that a person knows the alternatives open to him, he can value each alternative against every other, he wishes to obtain the best combination of alternatives and he rationally follows this aim. He will thus, it is argued, tend to choose those alternatives which lead him most nearly to his avowed end. The precise logic of this process is taken as well-known. But so far we have merely set up a conceptual model which, if it bears on reality, might be used to give necessary and sufficient conditions for an action to be undertaken.

That such an theory does not constitute an answer to our first question is seen as follows. Given that each action chosen from the (at least two) alternatives open to the decision-taker is related causally to a motive for its performance then the decision-taker must clearly obtain a clear indication of the relative merits of each motive before action can be performed. But this is possible only if motives or reasons for action can be considered distinct from those actions. Yet as we have seen, motives are causes of actions thus it would appear that to hold a motive immediately implies that one performs the action which it causes. It would seem then that no calculus of motives implied by decision-theory is possible.

To answer the second question a further assumption must be made to the effect that the logic of decisions is used by people in their decision-taking. This implies two further assumptions that people do in fact take decisions and that the logic of decision is useful to them in this.
Two points must be made here. First a decision is a human action. It must in the traditional analysis serve an end. But decisions are performed (or taken) because the individual must take them. It is not, therefore, a voluntary act for the only alternative is not to decide. Hence our individual is not free in the sense that he is undetermined by law-like generalizations. Since this is the definition of freedom implied by decision theory we appear to have found a confusion in the theory.

Second, even if we could find that decisions are voluntary actions then if we are to be consistent the individual must first decide whether or not to take a decision. But since this is a decision in itself it must be based upon another decision and so on, to an infinite regress of decisions. Thus our individual must continually be deciding whether to decide and cannot act on the basis of decision. Decision as an explanation of action, then, either contradicts the postulates of the action theory of which it is part or implies it is impossible for the individual to perform actions.

Given then that action is process by which certain motives are transformed into states of affairs and given that a free individual (in the sense of being undetermined by law-like relationships) cannot in terms of theory ever decide on which motive to hold we seem to have shown that the description of the process of choice implicit in the theory of action is not that described by the theory of decision-taking. In particular, the theory of decision does not attempt to analyze how the motives between which the individual must choose arise for consideration by that individual. We cannot explain why a decision is necessary, by means of this theory. If, however, decision-theory were adapted to include an explanation of the necessity of decision-taking then decision theory would not be merely a statement of how best actions are to be adapted to motives. Motives are causally related to actions thus if
we can explain by decision-theory the motives held we can explain what actions will be forthcoming.

Thus an answer to the question:

'Why did A do B?'

would not be in terms of the most efficient achievement of A's ends but of the form the motive for action B arose because of some factor x. Whilst we can explain perfectly well why action B was performed we cannot throw light upon the decision process as such for we have no criterion by which to choose x rather than some other factor y. Thus it would appear that either the theory of decision explains the criterion of choice without explaining the necessity or can explain the necessity of choice without being able to give a criterion by which it should proceed.

It seems then that one can conceive of action without decision, i.e. the statement of some criterion of choice, but not without choice as such. It seems, therefore, to follow that action is logically possible without decision. People behave with freedom without deciding what to do. Hence it follows a theory of free behaviour does not rest upon, nor is even explicable in terms of, a theory of decision. It also follows that economics in dealing solely with the concept of the decision-taker has drastically restricted itself to a particular sort of action that which is voluntary but ruled by decision.

It is at this point that we would begin our presentation of an alternative theory of action to that so far presented (to be continued in Chapter Twelve). At the outset we would try simply to purge the confusion of decision with choice from our discussion. Already we have given reasons for not regarding the two concepts as equivalent in connection with the theory of action. Our task may be assisted if we can spell out more general distinctions between the two.

As we have described it, decision comprises the comparison of motive
and action in order to obtain the most efficient combination of the two. It then implies that only an organism which can alter one or the other or both can make decisions. We would not, therefore, talk of a rat in a maze deciding to enter corridor 1 rather than corridor 2. It would, however, be proper to say the rat chose corridor 1 rather than the other. His choice may well have been determined by environmental factors such as heat, light or food but it is still a choice.

A choice then appears to be a statement that alternatives 1 and 2 were open and 1 was in fact taken. A decision, on the other hand, is a statement to the effect that alternatives 1 and 2 were open and 1 was taken because of a criterion which it satisfied more precisely than the other. It is the setting up of a criterion for choice which is crucial to our distinction. Unless we can explain why a given criterion (such as maximisation of welfare) is the basis of decision then it appears we have no reason for saying choice proceeds by decision rather than in some other way. How in the case of welfare maximisation we do not have this explanation so why is it assumed that choice does proceed by means of decision in Economics? We would perhaps answer that people act as if they decide. But then rats enter corridor 1 rather than corridor 2 as if they decide upon the former so why should we assume humans do? Moreover, an explanation in these terms is, like all teleological explanation, irrefutable. A theory of choice however may well be refutable in that the choice predicted is not taken.

We would suggest many of the problems associated with the explanation and analysis of human behaviour can be overcome along the following lines. Men are free experimenters. They are supposed to act according to their perceptions of causality in forever searching for new information about the world and themselves in relation to it. How in this they are performing an activity. It is this performance which allows us to
conclude that men or antelopes are not inanimate objects but living organisms. Clearly in this activity causal relationships are sought for by subjecting perceptions to the Axiom of Division which separates causal from non-causal relationships.

Now from the Axiom of Division it follows that only sentences referring to non-empty sets are to be considered causal. Thus an activity is only considered one such if at a given space/time it refers to a non-empty set. In other words the activity must be open to the individual at a given space/time for it to be performed. Since the individual can only perform one activity at any space/time it follows he will only perform that activity shown by his use of the Axiom of Division.

Potentially, such a theory has several advantages over the one it is supposed to replace. First the statement of a law-like proposition such as the Axiom of Division obviates the infinite regress of explanations implicit in the use of 'the will' to explain the basis of action. Second, such a theory views man as obeying law-like relationships and so does not resort to the 'demon in the clock' explanations of actions. Only if we can view man's activities as obeying law-like relationships peculiar to man can we ever hope to find these relationships rather than just relationships analogous to those found in mechanical physics. Third, we should be able to explain why actions are performed which is not possible in the presently accepted theory.

Before, however, we begin our re-statement of action theory we shall present some more detailed criticisms of the decision-taking concept in the form of a case study. The case we have chosen is the Tinbergen code of economic policy.
The nature of 'the' economic problem has been seen by some as partaking of deciding whether a certain course of action is to be performed rather than an alternative. This principle has been taken up by economists working on problems of the international economy in the light of the findings of Keade, Tinbergen and Mundell. Such workers see Governments acting in the world as a more or less rational agent with ends and means that are in principle separable at any one time and space. The ends are given externally to the selection of the means with which they are pursued. Now this kind of theory is but a variant of the theory of action we have examined until now. It has been characterised by Suppes as a normative theory of decision-taking since it is based upon the positing of a set of subjective standards by which the alternatives may be assessed. If seen in this light the theory of Government economic policy is basically a matter of positing a process by which a decision of choice between alternative courses of action can be made. And if Governments decide ultimately the nature of the individual macro-economics in the world then international monetary economics theory can be seen as the theory which predicts those actions that will be taken by Governments.
together with the interrelations that will exist amongst them.  

Such a theory has been explicitly stated by Tinbergen. He defines economic policy as:

". . . the deliberate manipulation of a number of means to attain certain aims." (P. 5)

The parallel with the "Robbins—definition" is, I think, not open to doubt. In general the policy-maker observes undesired states of affairs in his domain he wishes to cure them. That is his aim. It is usually characterised by a set of desired values of certain variables. This is usually called a target and the variables target variables. The means by which he achieves these target values are instruments. Conceptually anyway, all the policy-maker has to do is to obtain those values of the instrument variables (which are unknown) that will enable the target (the values of which are known) to be attained. Characterised as above, the problem of economic policy is seen simply as determining the values of the instruments that will lead to the target being achieved; it is a matter of setting up an equation system that is simultaneously satisfied.

The procedure that was specified by Tinbergen to carry out this programme runs as follows. First, the actual state of affairs must be found since it is by comparison between that and a desired state that a programme for action can be decided upon. Second the comparison is made and if a divergence is found the third stage begins. An estimate of the effects of alternative policies must be made. Fourth a choice between alternatives must be made and fifth the policy must be executed. Here we shall not be concerned with the fifth stage though it is of paramount importance. The relationship between instruments and policies pursued may be remote, e.g. between the theory of controlling prices and incomes and a prices and incomes policy. Further other policies pursued in other
directions may counteract the economic policies designed in the above manner, e.g. deflationary measures may be counteracted by an expansionist housing programme financed by foreign sources. Nevertheless we can postulate that a Government will tend to behave in the above way if it is rational. But then such a hypothesis is rather self-confirming since Governments have been advised to take on this format of decision-taking. Hence, insofar as the advice has been accepted, the above is a description of the process by which Government action is decided upon.

Now let us examine the above schema more closely. In general the above is useful in determining the desirability of 'quantitative' policies such as small changes in interest rates rather than 'qualitative' ones such as the dissolution of monopoly power. This is simply because the mathematical tools employed are not easily adaptable to changes of the latter sort. In practice, however, the distinction is not so clear. For example, the execution of a Government budget deficit may lead to large-scale structural changes within a given industry by the consolidation of monopoly power, or whatever.

The first stage of the Tinbergen procedure implies obtaining a knowledge of all the relationships that would exist in the economy in the absence of the now Government policy. This essentially consists in finding the behaviour of institutions and individuals in the economic situation, which is represented by relationships or equations. Then looked at together these relationships form an economic model. A model is a simplified picture of reality in which the assumptions made about the nature of this behaviour are made explicitly. Such exact specification and its attendant simplification are necessary for:

"... on the basis of such specification only can the economist put precise questions and try to give precise answers." (P.27)
Thus all policy questions are looked at in terms of such a model.

At first inspection this concept of a model which is essentially a precise or quantified expression of an intuitive theory appears to be at odds with the concept used in logic. This latter concept is defined as:

"... any model of the axion system (of a theory) satisfies all theorems deduced from these axioms." (Tarski, p. 123).

In other words a model is formed by a set of variables and constants if and only if this set satisfies the axioms constructed from the primitive terms of the theory. For example, if we set up the two following axioms:

Axiom I For any element \( x \) of set \( S \), \( x R x \)

Axiom II For any elements \( x, y \) and \( z \) of the set \( S \) if \( x R z \) and \( y R z \) then \( x R y \).

From this follows:

Theorem I For any elements \( y \) and \( z \) of the set \( S \) if \( y R z \) then \( z R y \)

Proof. Sub. in Axiom II 'z' for 'x'

for any elements \( y \) and \( z \) of the set \( S \) if \( z R z \) and \( y R z \) then \( z R y \)

But \( z \equiv z \) by Axiom I

Theorem II For any elements \( x, y, z \) of the set \( S \) if \( x R y \) and \( y R z \) then \( x R z \)

Proof. Sub. in Axiom II 'z' for 'y' and 'y' for 'z'

for any elements \( x, y, z \) of the set \( S \) if \( x R y \) and \( z R y \) then \( x R z \)

But if \( z R y \) then \( y R z \) by Thm. I
Thus for any elements $x, y$ and $z$ of the set $S$ if $x R y$ and $y R z$ then $x R z$.

Here $R$ and $S$ refer to the fact that the relation $R$ holds a certain relation with the class $S$. Axiom II expresses a given property called $P$. By this means proofs are generalized. Let us say that the relation $R$ is reflexive and has the property $P$ in set $S$ then we can say $R$ and $S$ form a model of the axion system of the theory. In the above we have merely stated the particular way in which a relation of a certain kind is related to a class. In the process of model building in economics precisely the same relation holds. The setting of economic relationships into a solvable mathematical system is only possible if these relationships have the property they hold in the class of solvable systems in that pointed by the axioms of that system. For example, a Keynesian model is so termed because the relationships it contains are consistent with axioms constructed from the sentences that Keynesian economists have written. Insofar as the axioms determine the derivation of valid theorems then models are simplifications of a fully fledged axion system; insofar as the original theory was imprecise and intuitive a model is a precise statement of the underlying analytical properties of the original theory.

In economic policy taking however, models are constructed from an analysis of data, thus it may be thought that, in economics, models are not related to a theory. But this argument ignores the nature of economic investigation which is based upon the attempted refutation of hypothetical statements. As much it is theory construction and the 'models' derived therefrom are really quantitative theories of less exact intuitive theories. It must, however, still remain that any set of variables and constants that satisfies a given set of axioms is a model hence our theory is a model of itself. As we have seen before this property of economic models has important and well-known drawbacks. But more of this later.
We are now in stage two of policy design. Here we have to set our model of the actual state of affairs against the state of affairs we desire. It is here that a welfare function of some sort or another has to be established. Now since we are analysing Government policy this welfare function must to some extent reflect a social welfare function of some sort. The difficulties associated with this concept are legion and well-known. For our purposes, however, it is sufficient to report Arrow's impossibility theorem.

The axiomatisation of welfare theory proposed by Arrow is as follows:

Axiom I  For all \( x \) and \( y \) either \( x \, R \, y \) or \( y \, R \, x \)

Axiom II  For all \( x \), \( y \) and \( z \) if \( x \, R \, y \) and \( y \, R \, z \) then \( x \, R \, z \)

Defn. 1  \( x \, P \, y \equiv \sim \, y \, R \, x \)

Defn. 2  \( x \, I \, y \equiv x \, R \, y \) and \( y \, R \, x \)

Thm. 1  For all \( x \), \( x \, R \, x \)

Proof  In Axiom I let \( y = x \)

Thm. 2  If \( x \, P \, y \) then \( x \, R \, y \)

Proof  By definition 1 and Axiom I

Thm. 3  If \( x \, P \, y \) and \( y \, P \, z \) then \( x \, P \, z \)

Proof  If \( x \, R \, y \) and \( y \, R \, z \) then \( x \, R \, z \) by Axiom II and Theorem 2

If \( \sim \, y \, R \, x \) and \( \sim \, z \, R \, y \) then \( \sim \, z \, R \, x \)

But \( \sim \, z \, R \, x \equiv x \, P \, z \) defn. 1

Thm. 4  If \( x \, I \, y \) and \( y \, I \, z \) then \( x \, I \, z \)

Proof  \( x \, I \, y \equiv x \, R \, y \) and \( y \, R \, x \)

\( y \, I \, z \equiv y \, R \, z \) and \( z \, R \, y \)

Defn. 2

if \( x \, R \, y \) and \( y \, R \, z \) then \( x \, R \, z \) by Axiom II

also if \( y \, R \, x \) and \( z \, R \, y \) then \( z \, R \, x \) by Axiom II

Thus \( z \, R \, x \) and \( x \, R \, z \equiv z \, I \, x \).
Theorem 5. For all $x$ and $y$ either $x \leq y$ or $y \preceq x$

Proof. From defn. 1 and Axiom I

Theorem 6. If $x \preceq y$ and $y \preceq z$ then $x \preceq z$ by theorem III

Proof. If $x \preceq y$ then $x \preceq z$ by Axiom I

Definition 3. $C(S)$ is the set of all alternatives $x$ in $S$ such that for every $y$ in $S$, $x \preceq y$

Theorem 7. A necessary and sufficient condition that $x \preceq y$ is that $x$ be the sole element of $C(\{x, y\})$

Proof. $\{x, y\}$ is the set of two alternatives $x$ and $y$

If $x \preceq y$ then $x \preceq y$ and $y \preceq x$ by Thms. II and I

Thus $x \in C(\{x, y\})$

But $x \preceq y \equiv y \preceq x$ Defn. 1

$\sim y \preceq y$ Thm. 1

Thus $y \notin C(\{x, y\})$

Definition 4. A social welfare function is a process or rule which, for each set of individual orderings $R$, ..., $R$ for alternative social states, states a corresponding social ordering of alternative states, $R$.

With these basic axioms, theorems and definitions we can construct a social welfare function. Arrow, however, now proceeds by placing certain restrictions on the form of the social welfare function. First if an admissible set of ordering relations is a set for which the social welfare function defines a corresponding ordering, it is reasonable to assume that some possible orderings are not admissible in this sense. A government may be quite equivalent to the plight of foreign exchange speculators who make a capital loss in a devaluation.
Condition 1:

Among all the alternatives there is a set S of three alternatives such that for any set of individual orderings $T_1, \ldots, T_n$ of the alternatives in S there is an admissible set of individual orderings $R_i, \ldots, R_n$ of all the alternatives such that for each individual $i$, $x R_i y$ iff $x T_i y$ for $x$ and $y$ in $S$.

Second it is reasonable to assume that individual and social orderings are positively associated.

Condition 2:

Let $R_1, \ldots, R_n$ and $R'_1, \ldots, R'_n$ be two sets of individual ordering relations, $R$ and $R'$ the corresponding social orderings and $P$ and $P'$ social preference relations. Suppose for each $i$ the individual ordering relations are connected as follows: for $x'$ and $y'$ distinct from an alternative $x$, $x' R_i y'$ iff $x R_i y'$ for all $y'$, $x R_i y'$ implies $x R_i y'$ for all $y'$, $x P_i y'$ implies $x P_i y'$. If $x P y$ then $x P' y$.

Third it seems that irrelevant alternatives must be ruled out. Thus the social welfare function must indicate a choice between alternatives in a given environment chosen only upon the alternatives in that environment.

Condition 3:

Let $R_1, \ldots, R_n$ and $R'_1, \ldots, R'_n$ be two sets of individual orderings and let $C(S)$ and $C'(S)$ be the corresponding social choice functions. If for all individuals $i$ and all $x$ and $y$ in a given environment $S$, $x R_i y$ iff $x R'_i y$ then $C(S)$ and $C'(S)$ are the case.

Fourth the sovereignty of citizens must be guaranteed. Thus we make the condition that:

Condition 4:

The social welfare function is not to be imposed.
A social welfare function is said to be imposed if,
for some pair of distinct alternatives \( x \) and \( y \), \( x \mathrel{R} y \)
for any set of individual orderings \( R, \ldots, R_n \),
where \( R \) is the social ordering corresponding to \( R, \ldots, R_n \).
Fifth, we assume there is not a dictatorship. Thus:

**Condition 5:**
The social welfare function is not to be dictatorial.

**Defn. 6** A social welfare function is said to be dictatorial if
there exists an individual \( i \) such that for all \( x \) and \( y \),
\( x \mathrel{P} y \) implies \( x \mathrel{P} y \) regardless of the orderings \( R, \ldots, R_n \)
of all individuals other than \( i \) where \( P \) is the social
preference relation corresponding to \( R, \ldots, R_n \).

Let us now assume a social welfare function satisfying conditions
1-5 and that the entire universe is the set of three alternatives men-
tioned in condition 1. Let \( V \) stand for a set of individuals, \( V' \) a set
of one individual and \( V'' \) the set of all individuals.

**Defn. 7** The set \( V \) is decisive for \( x \) against \( y \) if \( x \neq y \) and \( x \mathrel{P} y \)
for all sets of admissible individual ordering relations
such that \( x \mathrel{P} y \) for all \( i \) in \( V \).

**Thm. 6** Let \( R, \ldots, R_n \) and \( R', \ldots, R_n' \) be two sets of
individual orderings such that for a given distinct
\( x \) and \( y \), \( x \mathrel{P} y \) for all \( i \) for which \( x \mathrel{R} y \). If \( x \mathrel{P} y \)
then \( x \mathrel{P'} y \) where \( P \) and \( P' \) are the social preference
relations corresponding to \( R, \ldots, R_n \) and \( R', \ldots, R_n' \).

**Proof.** Let \( z \) be the alternative distinct from \( x \) and \( y \)
for each \( i \) define \( R_i'' \) such that:

\[ x \mathrel{R_i''} y \iff \text{either } x \mathrel{R_i} y \text{ or } x \neq z \text{ or } y = z \quad (1) \]
and for each \( i \) define \( R_i^* \) such that:
\[
x R_i^* y \iff \text{either } x' R_i y \text{ and } x' \neq z \text{ or } y' = z \quad (2)
\]
From (1):
\[
x R_i'' y \iff x' R_i y' \text{ for } x', y' \in \mathcal{L}_x, y, \quad (3)
\]
\[
C(\mathcal{L}_x, y) = C''(\mathcal{L}_x, y) \text{ from (3) and condition 3}
\]
By hypothesis:
\[
x P y
\]
Thus \( C(\mathcal{L}_x, y) \) contains only \( x \) by Thm. 7
Thus \( C''(\mathcal{L}_x, y) \) contains only \( x \) or:
\[
x P'' y \quad (4)
\]
From (1) and Defn. 1 for all \( i \)
\[
y P_i'' z \iff x' R_i y
\]
From (2) and Defn. 1 for all \( i \)
\[
y P_i^* z \iff x' R_i y
\]
Thus if \( x' \neq x \) and \( y' \neq x \) then \( x' R_i'' y' \iff x R_i^* y' \)
\[
x R_i^* y' \quad (5)
\]
Also \( x P_i'' z \) and \( x P_i^* a \) for all \( y \)
for all \( i \) such that \( x R_i'' y \) then \( x R_i y \) by (1). since
\[
x' \neq z
\]
By hypothesis \( x P_i y \) thus \( x P_i^* y \) by (2).
for all \( y' \), \( x R_i'' y \) implies \( x R_i^* y \)
\[
(6)
\]
for all \( y' \), \( x P_i'' y \) implies \( x P_i^* y \)
\[
(7)
\]
The hypotheses of condition (2) are satisfied by (5), (6), (7) and (4) Thus
\[
x P_i^* y.
\]
From (2) we proceed as above to find
\[
C''(\mathcal{L}_x, y) \text{ thus } C'(\mathcal{L}_x, y) = C'(\mathcal{L}_x, y) \text{ thus } x P' y.
\]
Thm. 9 If there is some set of individual ordering relations
\( R_1, \ldots, R_n \) such that \( x P_i y \) for all \( i \) in \( V \) and \( y P_i x \) for all \( i \) not in \( V \) for some particular \( x \) and \( y \) and such that the corresponding social preference relation yields the outcome \( x P y \) then \( V \) is decisive for \( x \) against \( y \).

Proof. \( R_1, \ldots, R_n \) is a set of individual orderings such that \( x P_i y \) for all \( i \) in \( V \)

\[ (3) \]

By hypothesis that \( x P_i y \) for \( i \) in \( V \) and \( y P_i x \) for \( i \) not in \( V \) then:

\( x P' y \) if \( x R_i y \)

Thus \( x P' y \) by Thm. 8.

Thm. 10 For every \( x \) and \( y \) such that \( x \neq y \), \( V'' \) is a decisive set for \( x \) against \( y \)

Proof: Sub \( x \) for \( y \) and \( y \) for \( x \) in Defn. 5

Thus by condition 4

\[ \sim y \equiv x \quad \text{or} \quad x P y \quad \text{by Defn. 1} \]

(9)

Let \( R_1, \ldots, R_n \) be any set of individual orderings such that:

\( x P_i y \) for all \( i \)

(10)

for all \( i \) such that \( x R_i y \) then \( x P_i y \) by (10)

\( x P' y \) by Thm. 8 and (2)

Thus by definition of \( V'' \) \( x P' y \) for any set of orderings such that \( x P_i y \) for \( i \) in \( V'' \), \( y P_i x \) for \( i \) not in \( V \)

(for no \( i \)). By Thm. 9 \( V'' \) is decisive.

Thm. 11 If \( V' \) is decisive for either \( x \) against \( y \) or \( y \) against \( z \), \( V' \) is decisive for \( x \) against \( z \) where \( x, y \) and \( z \) are distinct alternatives.
Proof: Assume \( V' \) is decisive for \( x \) against \( y \). We must prove \( V' \) is decisive for \( x \) against any \( z \neq x \).

Let the individual in \( V' \) have the number 1. Let \( R_1, \ldots, R_n \) satisfy the following:

\[
x 
\begin{align*}
p_i & y & \quad \text{---(11)} 
\end{align*}
\]

\[
y p_i z \text{ for all } i & \quad \text{---(12)} 
\]

\[
z p_i x \text{ for } i \neq 1 & \quad \text{---(15)} 
\]

\[
x p_i y \text{ for all } i \text{ in } V \]

Thus \( x \ p y \) by Defn. 7 \quad \text{---(14)}

\[
y p_i z \text{ for all } i \text{ in } V' \]

Thus \( y \ p z \) by Thm. 10 and Defn. 7 \quad \text{---(15)}

\[
x p z \text{ by Thm. 3} \quad \text{---(15)}
\]

But \( x p_i z \) for all \( i \) in \( V' \) from (11) and (12) \quad \text{---(16)}

(13) is rewritten as

\[
z p_i x \text{ for all } i \text{ not in } V \quad \text{---(17)}
\]

Since the hypotheses of Theorem 9 are satisfied by (15), (16) and (17) it follows that \( V' \) is decisive for \( x \) against \( z \).

If, however, \( V' \) is decisive for \( y \) against \( z \), let the following conditions hold

\[
x p_i y \text{ for all } i \quad \text{---(18)}
\]

\[
y p_i z \quad \text{---(19)}
\]

\[
z p_i x \text{ for all } i \neq 1 \quad \text{---(20)}
\]

As above (16) implies \( x p y \) and (19) implies \( y p z \) thus:

\[
x p z
\]

But \( x p z \)

which by (20) and Thm. 9 shows \( x \) as decisive against \( z \).
Thm. 12  For every pair of alternatives \( x, y \) and every one
member set of individuals it is not true that \( V' \) is
decisive for \( x \) against \( y \).

Proof. Assume the contrary: Let the member of \( V' \) be designated
by 1. Let \( y' \) be an alternative distinct from \( x \) and \( y \).
By hypothesis \( V' \) is decisive for \( x \) against \( y' \). This
is true for \( y' = y \).

Thus:

\[
V' \text{ is decisive for } x \text{ against any } y' \neq x \quad (21)
\]

For a fixed \( y' \neq x \) let \( x' \) be an alternative to \( x \) and \( y' \).
\( V' \) is decisive for \( x' \) against \( y' \) by (21) and Thm. 6(1)
If \( x' = x \):

\[
V' \text{ is decisive for } x' \text{ against } y' \text{ provided } x' \neq y',
\quad y' \neq x' \quad (22)
\]

Choose any \( x' \neq x \) and a particular \( y'' \) distinct from \( x \) and
\( x' \).

Since the choice is possible (22) still holds.

Sub \( x' \) for \( x \), \( y'' \) for \( y \) and \( x \) for \( y \)
\( V' \) is decisive for \( x' \) against \( x \) provided \( x' \neq x \) by Thm. 11

11

Writing (23) and (22) together

\[
V' \text{ is decisive for any } x' \text{ against } y' \text{ provided }
\quad x' \neq y' \quad (24)
\]

(24) implies

for all \( x' \) and \( y' \) \( x P' y \) whenever \( x' P; y' \).

This is a dictatorial social welfare function by Defn. 6
and no condition 5 is contradicted.
We come now to the climax of the discussion which is called the General Possibility Theorem. This is stated as follows:

"If there are at least three alternatives which the members of society are free to order in any way, then every social welfare function satisfying Conditions 2 and 3 and yielding a social ordering satisfying Axioms I and II must be either imposed or dictatorial." (Arrow P. 59)

Proof: Let $S$ be the set of three alternatives (see Cond. 1)

For each possible ordered pair $<x', y'>$ there is at least one set of individuals which is decisive by Thm. 10.

Choose any set, $V_1$, which is both decisive for some $x'$ in $S$ against some $y'$ distinct from $x'$ in $S$ and which has the fewest members.

Let $z$ be the other alternative

Let the members of $V_1$, number $k$ designated 1, ..., $k$

Let the remainder of individuals be designated $k + 1, \ldots, n$

Let $V'$ contain individual 1, let $V_2$ contain individuals 2, ..., $k$ and $V_3$ individuals $k + 1, \ldots, n$.

$V_1$ is decisive for $x$ against $y$ by construction — (25)

Thus:

any set which is decisive for some alternative in $S$ against some other alternative contains at least $k$ members — (26)

Since $V_2$ contains $k - 1$ members:

$V_2$ is not decisive for any alternative in $S$ against any other alternative in $S$ — (27)
By Thm. 12,

$V'$ is not decisive for any alternative against any other alternative. (28)

Let $R_1, \ldots, R_n$ be a set of individual orderings such that:

1. For $i$ in $V'$, $x P_i y$ and $y P_i z$ (29)
2. For $i$ in $V_2$, $z P_i x$ and $x P_i y$ (30)
3. For $i$ in $V_3$, $y P_i z$ and $z P_i x$ (31)

From (29) and (30), $x P_i y$ for all $i$ in $V$

From (25)

$x P y$ (32)

where $P$ is the social preference relation corresponding to $R_1, \ldots, R_n$

From (30)

$z P_i y$ for all $i$ in $V_2$ (33)

From (29) and (31)

$y P_i z$ for all $i$ not in $V_1$ (34)

Now suppose $x P y$

From (33), (34) and Thm. 9 it follows $V_2$ is decisive for $y$ against $z$ which contradicts (27).

Thus we must say $\sim z P y$ or $y R z$ (35)

where $R$ is a social ordering relation corresponding to $R_1, \ldots, R_n$

From (32) and (35)

$x P z$ (36)

From (29)

$x P_i z$ for all $i$ in $V'$ (37)

Whilst from (30) and (31)

$z P_i x$ for $i$ not in $V'$ (39)
By Thm. 9, (38)-(39) imply that \( V' \) is decisive for \( y \) against \( x \). This, however, contradicts (28). Thus, if conditions 1-5 are taken together they are contradictory, or if conditions 1, 2 and 3 are satisfied then either condition 4 or condition 5 cannot be.

If this is so for three alternatives it must be so for \( 'n' \). Hence we have to conclude that a social welfare function built up from individual valuations is impossible unless a measure of imposition or dictatorship is present. The welfare function of our Government decision-maker cannot, therefore, be said to be 'democratically' arrived at. Instead it must be constructed on the basis of the preferences of the Government decision-maker. To discover the welfare function of the decision-making process one does not ask the proverbial 'man in the street' but those who manipulate the instruments which are used in policy execution (see Tinbergen pp. 17-18).

Further than this the form of the welfare function, either imposed or dictatorial, can be deduced. Tinbergen describes the relationship between the policy-maker and the citizens as:

"If the preferences (underlying a decision) are consistent, they may be represented by some central, all-embracing, concept in the minds of the policy-makers, which we usually call welfare or utility of the economy to which the decisions refer. This welfare concept will largely, but not always completely, coincide with a certain representative individual welfare concept. In other words, the welfare function according to which the policy-maker acts will depend, among other things, on the quantitative and qualitative elements that also enter into these individual utility functions. In addition, 'collective preferences' will come in, that is,
preferences taken into account by the policy-maker because of feeling himself responsible for the economy in a collective sense." (Tinbergen P., 11)

Now, as we have seen, such a welfare function is in part dictatorial since our policy-maker can decide what to do despite the preferences of the citizens. However, since the policy-maker is concerned about the individual welfare of the citizens he is not entirely dictatorial. From our definition of dictatorial we can see that Condition 5 must be violated by the above concept. Thus it follows a welfare function can be constructed for collective decisions.

A difficulty arises immediately. We asserted that choices could only be made between alternatives that exist in a given environment. Thus a policy-maker can only rank a little more unemployment against a little less inflation, say, if they both exist. Hence, it seems difficult to envisage how Condition 5 cannot be violated as well. A policy-maker is choosing between alternatives that could be attained in an environment which he has in part, anyway, constructed. Thus, alternatives are not given but are, themselves, the outcome of previous policy decisions taken by the policy-maker. Hence since all orderings are potential it seems that to know the ranking of \( x \) against \( y \) is to know which the policy-maker’s decision. It may be that another variable may influence the choice actually made since it may be indicative of reasons why \( x \) should not be preferred to \( y \).

Moreover, it is supposed that any given policy has certain side effects in which the policy-maker is not interested. The policy-maker is thus supposed to make his decision on the basis of his ranking of the values of the target variables only. Now this is, to an extent, tautologous since target variables are only those variables in which the policy-maker is interested but one important result can be obtained. This
is really an rigorous statement of Condition 3 above. However, because of uncertainty we have argued that 'irrelevant' variables may be relevant as a guide to policies. Also because there is no 'given' environment in the sense of alternatives laid out like commodity bundles we have reason, I think, to doubt that Condition 3 can be said to hold for our policy-maker. In other words, our policy-maker's decisions do not rest solely on his preferences.

It seems, prima facie, that the concept of a welfare function enunciated above is not the simple collective welfare function that Tinbergen supposes it to be. Not only does the above seem to throw doubt upon the ability of the policy-maker to compare desired states with actual states on the basis of individual welfare but also questions whether he can in fact perform the fourth stage of policy taking; namely the act of decision. But before this doubt is analysed more deeply let us consider the third stage of policy-taking that of estimating the effects of alternative policies.

We shall not here look at the various means econometric or otherwise of estimating the effects of various policies but we shall attempt our analysis of the nature of this estimation. In terms of Tinbergen's procedure this stage is essentially the finding of causal relationships, by means of our model, between changes in the instrument values and changes in variables which we have termed targets. Since we are concerned basically with two changes our causes are efficient causes in the Aristotelian sense.7 (It must be emphasised, however, that these relationships are not looked upon as causal but are merely correlations between changes in values in a given equation. We would contend, however, that unless this correlation is supposed to be causal then it is of little use to the policy-maker who must be sure that a given
Hence, we wish to assess the extent to which it is true that, say, an increase in money supply will cause a fall in the rate of interest.

Now this is essentially the same problem we faced before. We have set up an agent whose behaviour is only evinced in terms of the changes he can effect in a given situation. At the same time this agent must act within that situation in order to find out what his behaviour can be. We have seen that causal analysis in this situation ends up very differently from that set out by traditional action theory. In particular, we have found that the assumed analogy between 'conditionals and causality is invalid. In these circumstances it seems to be a mistake, on the part of policy-makers, to look for relationships in which this analogy is apparently upheld, for if the analogy is not fully borne out there is no reason to suppose that simply by altering an instrument's value one can alter a target's value without altering the instrument's value and so on.

Furthermore, since we have seen that targets and instruments are causally related not only is the definition of one meaningless without the definition of the other but also the process of causation would I think predict that neither the observed target changes nor the related changes in instruments are anything but two stages on a continuing process. Hence to assert a causal relationship on these terms one can hardly use the concept of a target which one can hit more or less exactly but rather a state through which it would be desirable to pass at a given space/time. Clearly, it is important to achieve this state in a way that does not prejudice against one's ability to pass through even more desirable states later on. When looked at in this way, the Tinbergen model by using the concept of target, whether moving or stationary, ignores the fact that policy is a continuous process. It is not simply the setting of target variable values and achieving the instrument values that will achieve them.8
We have characterized the third stage of economic policy as the finding of efficient causal relationships. How those are relationships which link the source of a change with the change that occurs. Hence, in terms of economics, we are concerned primarily with flows over time rather than stock levels at a particular time. In particular, it is common to assert that a ceteris paribus interest rate increase will cause an inflow of short term capital. Yet it is clear that this response is really the result of moving from one stock equilibrium to another on the part of holders of international portfolios. Once the latter equilibrium is reached clearly the inflow of capital will stop, thus a continuously changing interest rate is necessary to cause a continuous inflow. Our concentration on the analysis of efficient causes precludes our studying anything in terms other than flow analysis, for it is only in those terms that changes in the values of the two variables can be interpreted. In terms of efficient causes, it makes no sense to talk of the level of the stock of money causing a given price level for instance since there is nowhere implied a relationship between changes. It follows, then, that the Tinbergen-type procedure cannot take account of stock variables except in flow terms.

In the light of the above we shall now consider the problem of the separability of targets and instruments. Broadly three lines of criticism of the supposed separation have been raised. First, targets cannot be specified because of difficulties over the social welfare function, second the relationship between instruments and targets may be unquantifiable and third the statistics may be inferior and so not permit a choice to be made between different models. Since, like Tinbergen, one may say that the procedure outlined above is useful only where the aims of policy are clear cut and the relationships well known and separable the above arguments do not constitute a serious criticism of
this sort of policy making.

Instead we shall attempt the distinction and then draw some conclusions from it. Mundell quotes Polak in stating:

"... targets refer to variables for which we 'care', instruments to those for which we 'do not care'."

(Mundell P. 202; reference omitted)

Now it cannot be that targets are desired except in isolation from the instruments which can lead to their achievement for if both are desired together then we cannot differentiate between targets and instruments on the basis of more or less desired. Yet targets are not targets in effect unless there exists an instrument which is capable of bringing them about. Thus one cannot consider targets separate from instruments. Yet if the separation is not possible apriori no variable can be compared with another in order to discover whether or not it is a target; to say x is preferred to y supposed that x is separable from y.

An alternative approach is to classify ends and means on the basis of their functional relationships. Thus:

"... the class of means which ... are of a quantitative character and are used for frequent changes ... will be called instruments ..." (Tinbergen P. 5)

and

"Quantitative policy in our sense ... will be directed towards the attainment of changes in quantitative aims, that is changes in certain economic variables to be called target variables or just targets." (Tinbergen P.9)

Clearly the targets are simply the ends of quantitative economic policy and as such are stated in the policy maker's welfare function. The
Instruments are simply the means by which these targets may be reached. Related in this way it is possible to classify instruments as causes and targets as their effects. It must be pointed out, though, that targets are merely a sub-class of all effects and are selected on the basis of policy-maker's preferences. But since to prefer implies separation we have the same problem as before.

It is conceivable that targets and instruments could be separated by assumption. One could, for instance, envisage targets as the primitive terms of a calculus and instruments as vocabulary introduced by those terms. In this case there would be no reason for holding one primitive target rather than another ad desirable. There would also be the problem of being able to make one term primitive at will; if this is so one must explain why this happens. It seems one's only recourse is to desires and to the difficulties outlined above.

An alternative approach would be to look at the above procedure as an ideal type against which actual policy making may be analyzed. The targets would then be separated by hypothesis. They would be a set of postulated ends towards which the policy-makers are supposed to act. One can frame predictions in the light of the goals. Insofar as this interpretation is just the economic policy outlines above is part of economic theory. But not only do we read:

"The logic of finding the best economic policy, that is, of finding the extent to which certain means should be used in order to achieve certain aims, is, in a sense, an inversion of the logic to which the economist is accustomed." (Tinbergen P.9)

But also nowhere do we find an explanation of why this sort of procedure should be adopted rather than another. Moreover in the above the policy
maker is envisaged to be able to articulate his targets at will whereas in economic theory the end is seen as sort of prime mover of all economic activity. Nowhere are there suggestions that policy-makers have given ends which are perfectly general. Instead the insistence is upon an 'ad hoc' use of the above considerations and there is no question of using these models for this purpose. We would, in addition, argue that such use is impossible anyway.

For a set of sentences to be used as a theory to either predict or generalise upon the world, it must satisfy two criteria. First, it must by correspondence rules refer to the world it is analysing, and second it must yield consequences that can be tested against the data. Since the process of testing correspondence rules must be used in reverse, it is sufficient and necessary for a set of sentences not to be a theory that they do not and cannot contain correspondence rules. This we shall attempt to prove for Tinbergen type models.

It is both necessary and sufficient for a correspondence rule that the Axiom of Constructibility holds. That is we must assert the existence of at least two languages, a data language and a theoretic language. Thus, if we show that in the Tinbergen-type of model one or the other language is excluded then we have shown that the Axiom of Constructibility is violated and so no correspondence rules can be forthcoming.

Let us assert that the sentences of our model are purely theoretic. In this case the policy prescribed is simply that of equating a given trade-off between aims with the relative costs of those aims. As such it is no different from the theory of consumer demand developed by Marshall, Slutsky and Hicks. That is impossible to associate with data because the substitution effect is simply a residual. Let us now assume our model is composed of purely aggregative equations induced
from a given data. In such a model, as is well known, "the theoretical terms can only be defined by means of observable properties on condition that the theory cannot properly be adapted to new situations." (Braithwaite P. 76). Thus we can envisage two languages but only insofar as it applies to the observable proportion by which the theoretical terms were originally defined. This is simply because the theoretic terms are not generalizations as is implied by the axiom of constructability but merely terms for a set of observable properties.

Finally, let us view the process of economic policy outlined above in more general terms. We read of trade cycle models:

"The policies suggested will be no better than the models from which they are derived, and if these models do not present a fair approximation to "what happens during business cycles", the policies likewise fail to provide a proper starting point for the practical policy-maker." (Matthews P. 170)

From this we may argue that in setting up a model it is imperative to find that the Axiom of Division holds. Now as we have seen the methods of analysis used in economics assume that the Axiom of Constructability holds. But as we have seen, it is impossible to satisfy the Axiom of Division and the Axiom of Constructability. It seems, then, that one or the other must be discarded. But if we discard the Axiom of Division, we must discard the notion of causality and as we have seen, the notions of action and decision. Since this whole theory is based upon these notions, this is not the course that will be taken. Rather we shall assert as above that the Axiom of Constructability does not hold.

Thus we must assess the Tinbergen-type models as to their effectiveness in verifying the Axiom of Division for a given relationship. We
shall recall that the Axiom of Division separates causal from non-causal relationships and so for it to be effective it must be possible for non-causal relationships to be formed. This, however, is not possible in a model. Only tautologies can be deduced from a model and so conclusions are 'attached' to premises analytically. If conclusions are to be of use then they must correlate with an empirically based 'entity'. But this means that the conclusions are never refutable since in a consistent deduction only an expression or its formal negation can be deduced (but not both). Thus if Tinbergen-type models can incorporate the Axiom of Division they cannot manipulate it effectively since non-causal relationships would be impossible to find.

Moreover, the Axiom of Division cannot be incorporated into a logical framework for if it could, causal sentences would then be truth-functional. Yet certain success has been achieved in axiomising decision theory in logic, and so we must conclude that the Axiom of Division is not incorporated into the sort of models we have been considering.

This conclusion implies two corollaries. First, the sentences analysed by the above models are analytic only and non-causal, because of the absence of both the Axiom of Division and the Axiom of Constructability. Second these models cannot delineate activities for a particular agent. The actions that are prescribed are no doubt correlated with empirical entities and so far as this is so, refer to non-empty sets but this non-emptiness is not derived analytically from the very nature of the relationship. Rather, it is imposed as a meaning on the symbols in the calculus. Hence, instead of a necessary relationship between a causal sentence and a perceived process, a correlation is supposed between a model of behaviour and behaviour in fact. This correlation
is not, however, the result of a synthesis since the Axion of Constructability is absent but is a result of the use of 'misplaced concreteness'.

We find, then, that the traditional concepts of economic policy are not only incapable of deriving activities but also are inconsistent with the kind of scientific methodology to which they aspire. It is, however, of importance to make clear these conclusions in the light of modern Economic theory.
On a Revision of the Theory of Action

It is a conclusion of the theory of action that action is governed by the principle that ends and means are logically consistent. This implies that not only must the means of achieving a given desired end be logically consistent with each other but also an individual must choose consistently between various ends/means combinations. The individual must, therefore, be consistent in the construction of the means by which he values the various combinations and consistently apply those values in the assessment of various actions. If the latter requirement is guaranteed by some form of decision-making process the principle of consistency can be stated as:

"... if an individual selects batch one $\overline{c}$ or policy one or alternative one $\overline{f}$ over batch two $\overline{c}$ or policy two or alternative two $\overline{f}$, he does not at the same time select two over one." (Samuelson P. 65).

Such a principle allows action to be subjected to logical analysis. It is also possible by equating rationality to the consistency of ends and
means to equate the rationality of an individual with the ability of that
individual make the selection of given alternative actions a matter of
logic.

If we ignore the criticisms made of such a theory in previous
chapters then we should expect the complete axiomatisation of the theory
of action to be possible. But such research seems to ignore the point
that the theory of action is not simply a matter of the logic of choice,
whilst it implies many such elements, e.g. in the selection of motives,
it also implies others, e.g. the matching of motives linked causally
to actions. It would seem, then, that to base the whole of a theory of
action upon the principle of consistency is to ignore other important
facets of the explanation of action.

In particular, two features of actions are completely swept aside
in the Consistency view. First, no account is taken of its causal nature
and, second, the concept of a free agent as being of necessity, bound to
use those causal relationships that exist in the world is ignored. It
in those two ignored facts which we shall use to revise the theory of
action into a more general theory. Let us take them in turn.

The causal nature of a motive/action combination implies, from our
discussion of the nature of causality, that both motive and action are
occurrences in a continuum of similar combinations. Thus, if one
combination occurs it must mean that another combination will also occur
at a later time and so on, provided nothing else occurs to change the
continuum. A particular action then once taken implies another action
in the same continuum. How this could explain the phenomena described
as follows:

"... in many cases there is a virtual 'compulsion'"
to perform additional investment in the subsequent years, on pain of not being able to derive proper benefit from past investments; such phrases as 'we must either expand with the growth of the market, or sacrifice what we have already invested' we frequently not in the course of our interviews, and applied whether or not the business was earning good profits up to that date." (Rodda-Wolff, P. 25)

It seems that firms embark upon a policy which leads them to take further policies in the same vein. As far as I know, the theory based upon consistency can explain these cases only in terms of uncertainty or mistakes in the decision-making machine. But it seems, by Ockham's Razor, that if the same phenomena can be explained more elegantly by different means those means are to be preferred.

If this explanation were accepted then we must give up all notion of explaining action without reference to actions committed in the past. We cannot ignore these actions by arguing:

"Historical costs have powerful sway over untutored minds. The Internal Revenue Service insists that corporation assets be so valued. The public utility commissions consider historical costs a relevant or even decisive item in setting rates. Men incur additional losses trying to "get their money" out of a venture. They all fly in the face of a basic principle of rational behaviour, 'By-gones are forever by-gone.'" (Stigler, P. 104)

The key to a criticism of this concept is that rational behaviour at a point in time implies the body of theory based upon the principles of consistency. But if we consider an alternative use of the term consistency, we can see that Stigler's assertions do not carry great weight.
It is common to term a person as consistent if he holds the same views or performs the same actions over time. We shall examine this use of the term hereafter to show that although under special assumptions it can imply the view expressed by Stigler it is not constrained by that view's logical perspective.

Furthermore, we would argue from anecdotal evidence (such as a perusal of the rules by which bureaucratic organizations are run) that most of the decision procedures of large sectors of the economy are in fact based upon the latter interpretation of consistency. It seems much more satisfactory for Economics to accept this fact (if in fact it is such) rather than criticising the procedures on the basis of an irrefutable model such as perfect competition theory. Moreover, those theories of the firm most recently developed by Frencoe and by Harris emphasize the processes within that firm rather than conceiving of a string of decision isolated from each other by the past.

If a free agent is only free insofar as he can use causal processes at his disposal than for him to act he must examine the world he inhabits with a view to finding these causal relationships. As a beginning, let us assume that a person will conduct an experiment on the lines of a 'controlled experiment' to find out the causal relationships he can manipulate. He will proceed by arbitrarily making an action in order to find the state of affairs that action causes. We could conceptually regard our individual as proceeding on the lines suggested by Orcutt for the investigation of causal relations in the economy.

\[ \text{TABLE 2} \]

\textbf{Actions and Observed Changes in Objective Variables}
In the above table (Croust P. 306) the experimenter can perform certain actions from the motive of finding causal relations. He can also observe the behaviour of certain objective variables each time an action is performed (a response in the respective variables with a given action is indicated by a cross in the matrix). Thus in the above we find that none of the actions 1-4 enable the actor to control, by action, a response in both $x_1$ and $x_2$ and so a new action, 5, must be performed to enable the control of $x_1$ and $x_2$ to be found. It is by this process of 'successive experimentation' that the performance of action continues. Action, therefore, occurs because the agent must discover causal relations or he is not free.

The above, however, is a schematic description of the process which we are discussing. It must be qualified in two respects. First the causal relations which can be found by this method are not the motive/action combinations we discussed before but are the results of those actions. In other words they are an indication of the action which
follows from the action now being performed and so give an indication of the continuum of which the latter action is part.

Second implied in our statement of the process of experimentation is the separability of each action whereas as we have remarked each action is to be regarded as an occurrence in a continuum of action. Instead of performing arbitrary actions, therefore, we would expect groups of actions to be performed in succession once the continua have been identified. For instance, a person on finding that the price of bread rises as he buys more and more of it may then purchase other goods to see if the same is true of them.

We have not, it will be noticed, tried to answer the question: 

'why is experiment 1 rather than experiment 2 performed?'

It may well be argued that if we did precisely the same difficulties would arise as we found to be present in the concept of 'the will', as the basis of the theory of action. But such a question is not relevant to our study since we are concerned here only with a general statement of our position. The precise experiment at a given time will be given by the state of affairs at that time and those that preceded it. Yet this answer may well lead to a supplementary question:

'what is the nature of the choice between alternative experiments?'

Given that the particular experiment at a space/time point is uniquely explained by the above the choice then appears to be not between alternative experiments at that point but between performing the prescribed experiment and doing nothing. Doing nothing will not affect the experimenter's view of the continuum upon which he is set and so will serve merely to delay the performance of the experiment prescribed as above.

It seems, however, that in certain cases choices between experiments
are taken. We would argue (see below) that this is in reality a choice between continua determined by what has been learned from experiments in previously held continua.

One could also argue, correctly, that very few people in society can conduct the kinds of experiments we have been describing. Such experiments have all three properties listed below:

1. The technique of holding other things constant;
2. The control of relevant variables, and
3. data suitable for analysis.

Uncertainty theorists would, no doubt, point out that 2 and 3 are never present in most actions. Actions always have undesired repercussions and data is usually too scanty or too expensive to collect to enable truly rational actions to be made. This criticism holds just as much for traditional theory as for the present analysis.

More far reaching is the statement that by the Principle of Alteration 1 can never be present. Again we must admit the criticism but point out that it is this very Principle which led us to argue that causality could only be perceived as a two-way relation between something known as a cause and another known as an effect. For by continuing to use the kind of experiments outlined above we argued the Economist is looking for an entity displaying priority, contiguity and necessity which cannot be found in conditions of alteration. Thus we propose, failing a notion of experiment to accord with these comments, to assert that individuals experiment in the above described way because its nature seems to be derived from the concept of a free agent we advanced in Chapter XI.4.

If the foregoing is now borne in mind then we believe the following argument will become clearer. We now state the basic notions of our
revised theory of action. First, let us define some terms. The terms action, experiment, knowledge and continuum will be taken as explained above or primitive (i.e. undefined). The term continuum is here seen to be the process of causality over time. It may well have observable instances along it but it is not itself observable. For instance we can see that if sales of bread rise, the price of bread will rise but we cannot observe the process by which this happens. Consistency will be taken to mean that the results of an action are logically equivalent to the desires of an actor. Continuity will be construed as the property of actions which involves their being causally related to other actions in the same continuum.

Let us now imagine that our individual experimenter is pitched into a social milieu say by being born. He performs experiments not because of any innate idea or drive or because of a desire to obtain knowledge but largely as a process of action and reaction. Do doubt some of the experiments he performs will also be a result of a process of imitation. In this way he learns about the continuum of which his actions are part. Formally, we can look at this process as the construction by the individual of a causal system of his environment. This system is unique to him for it is derived from his perceptions of himself in relation to the world he inhabits and so can only be represented in someone else’s perceptions.

Simultaneously with this finding out process is the performance of actions at a point in time. It is only by this performance that an experimenter finds out. We, therefore, imply two ways of analysing action. First, one could see it as the process of successive actions over time and second implied in this view is the idea of discreet action steps in this process.

When looked at from the point of view of succession, the causal nature of action (in the sense of a constant conjunction between actions)
becomes clear. But say we identified a causal relationship between motive and cause during an experiment. An action has thus resulted in another action to be performed. For instance, if our experiment were the sale of particular product, then it may well be in the course of our experiment a new type of product (previously not experimented with) is noticed. We should, therefore, experiment with this latter type in order to assess its place in our environment. It is in this way that actions lead to other actions over time.

If, however, we examined the same actions as unconnected with each other, we should be forced to assert that at each point in time two motive movement combinations caused the action of selling the original product and later the experimentation with the product never before experienced. There is, however, nothing in this interpretation which precludes the alternative view. All that we are arguing is that at a point in time motives and actions are the immediately obvious factors in an individual's experiment. He is, after all, relating actions to certain responses in the world he inhabits and so may well equate motives with the responses he finds. In this way the causal nature of a particular action at a given time is made clear.

Since our notion of an experimenter seems to imply both the concept of consistency over time and consistency in time we have reason it seems for supposing that this notion of action based on the principle of Continuity is a more general notion than that based upon consistency. We shall, therefore, state the Principle of Continuity as the basis of all rational action such that all action will be performed only insofar as it is related causally to actions which an individual has performed previously.

At once, however, we are faced with a limitation to this view. Not only does it appear that choices between experiments rather than just
between an experiment and its non-performance exist but also it appears that we have taken on the determinist viewpoint which we attacked in Chapter Nine. As stated a person acting according to the Principle of Continuity will continue to learn more of the continuum in which he acts until, given no changes in his environment, he has formed a system that serves to completely explain, to him, his position in the world. Explanation here is in purely personal terms. It is not in terms of necessary or sufficient conditions for these are formal conditions unconnected with the kinds of experiments we are considering.

In the circumstances, we have postulated a person can only act according to the Principle of Continuity if in fact there are experiments that can be performed to uncover new causal relationships in the same continuum. This implies that an action aimed at this uncovering must be causally linked to a previous one and that the system is not yet complete in the sense described above. It seems that the crucial determinant in the degree to which the system is a complete explanation. If it is complete, then there can be no other actions possible in the same continuum. Hence his actions are not linked causally. In these circumstances action is not caused in the way postulated and so the person is not acting in continuum or system but, for want of a better term, in a dissystem. His actions may then be termed accidental. They are unintentional in the sense that the results of them cannot be found due to the absence of system.

A person in this situation is in much the same position as he was when he was born. In other words, he must set about experimenting to find a new system. Thus, by the growth of knowledge the person can start action upon a different continuum. Such a change of continua can also occur by a change in the environment upon which the causal relations are dependent. For instance, the blocking of a street may lead to new
For instance, a change in the law such as the prohibition of patent rights may lead to new experiment patterns which lead to the sale of new products never before experimented with.

Whether such switches between continua is possible through active decision processes is much more doubtful. To decide in to choose between two equally available alternatives on the basis of a criterion. Given that we have this criterion such a decision would involve the simultaneous discovery of two systems. Under our present theory this is quite impossible unless a particular action causes two actions one in one system one in the other. This is ruled out by the concept of causality.

It may, however, appear that at a point in time such decision-led choices have been made. In a time of discourse the accidental actions which lead to a new continuum being struck because one action struck upon caused another action could be interpreted as evidence for desires emanating from 'the will'. We would argue, however, that such an interpretation is unnecessary in that it is inconsistent with our view of rational behaviour in particular and freedom in general.

This then is our tentative and informal description of a theory of action which is to replace that presently in use. We are aware that it is as yet an infant and beg the reader's indulgence but it is hoped it can form the basis of more meaningful models of human behaviour than those that exist at present. As a conclusion to this chapter we shall try to illustrate why a consistency model is implied by a continuity model which shares with the former identical structural assumptions. As a basis for this exercise we shall take Hicks's illustration of an optimizing approach to the decision of the appropriate policy mix for the obtaining of targets in an open economy under fixed exchange rates.

It is a two target \((y_1, y_2)\) two instrument \((x_1, x_2)\) model. A social welfare function (see Chapter Eleven) is postulated together with
a target frontier. This latter:

"... specifies the maximum value of any objective which can be obtained for given values of other objectives and thus determines the set of efficient policies." (Wieland P. 097)

By solving the system we get:

\[ y_1 = y_1(x_1, x_2) \]
\[ y_2 = y_2(x_1, x_2) \]

Now to obtain the target frontier we maximise \( y_1 \) for given \( y_2 \). A necessary condition for maximum is:

\[ \frac{\partial y_1}{\partial x_1} / \frac{\partial y_1}{\partial x_2} = \frac{\partial y_2}{\partial x_2} / \frac{\partial y_2}{\partial x_2} \]

which are essentially analogous to the maximising conditions in production theory.

Now, if we let \( y_1 \) be output, \( y_2 \) be reserves and make \( x_1 \) (exogenous) money supply and \( x_2 \) public expenditures. We can see that the LHS of the above equation is the slope of an iso-output curve and the RHS the slope of an iso-reserve curve. For maximum, then, a tangency solution exists between the two curves as shown in figure 2. The most efficient policy mixes between Government expenditure
and money supply is given by the curve A-A. If this latter is now plotted onto target space we get the following target frontier upon which are plotted social indifference curves. If in figure 3 is the optimal policy. The trade off between targets must thus be given by:

\[ \frac{d_2}{d_1} = \frac{\partial y_2}{\partial x_2} = \frac{\partial y_1}{\partial x_1} \]

which is the slope of the target frontier.

Our continuity model of the same state of affairs is not so easily translatable into mathematics as a consistency model so we shall rely upon the use of verbal reasoning to show how much a consistency theorem could be deduced from a continuity model. Let us take A to be the action performed and C0 be a combination of two instrument variables. Let B be the result of the action A and C0 be a combination of two target variables. Let the structure of the economy be that shown by figure 2. Let the performer of the action C0 have no knowledge of the possibilities which confront him at the outset. Let there be no change in the environment.

If we suppose our actor to be in the above situation, he will perform any action say that given by point J in figure 2. This leads inevitably to a combination X, B of targets, say B in figure 3. B is then a new environment for action. From B another action will be performed.
with a given result and so on until in the end the target frontier is reached. Notice that with prior knowledge the target frontier would have been reached by means of one action. If we were to plot a path of the policies achieved and taken by our actor it would be of the form:

![Figure 4](image)

provided we made allowance for the continual change in the point from which policies were taken by use of some weighting factor.

As we argued in our theory of action that the results of an action B lead to new actions being performed. Thus, if we look at figure 4 we can see that some policies are more effective than others. If we compare points K and V then we can see if an action which leads to V is performed the effect of that action is greater in magnitude than that which led to K. Thus we would expect V to be perpetuated longer than simply because the other policies taken are less effective and so will change the effect of V to lesser and lesser extents the less effective they are. A position on the target frontier once struck on will not be wholly changed by causally less efficient policies taken before or after.

Since motives for actions are the targets for a given policy we can see that given the above argument at a point in time the trade-off between targets will be given by the slope of the target frontier. Hence, equation (2) must hold. It would seem that there is reason to believe that if actors are governed by the principle of Continuity then at a point in time they will be consistent.
We shall in the next chapter attempt to draw the reader's attention to other applications of the Principle of Continuity in the analysis of International Economics. This chapter, like the present one, it must be emphasised, must be regarded only as tentative suggestions for future research. It is hoped, however, that we can thereby indicate the possible fruitfulness of our argument.
CHAPTER THIRTEEN

On Some Possible Applications of the Revised Theory of Action
to Some Unsolved Questions in Economics

There are three types of problems in economics which the above discussion may be helpful in solving. First, there is the problem of tâtonnement, second the problem of apparently contradictory behaviour in the short and long runs and third the relationship between 'monetary' and 'real' theory. We shall in this chapter try to suggest the form of these solutions not so much to present hard and fast conclusions as to suggest avenues of future research.

Let us take the first problem. Tâtonnement has been described as:

"... start with any arbitrary set of prices — Walras'
"prix criés au hasard". In general, it will not be an equilibrium one. That is, at this set of prices there will be some markets with positive amounts of excess demand and others with negative amounts. Prices will then rise in the former markets and fall in the latter, bringing us a new set of prices ... and so the process will go on. It is by this continuous groping — tâtonnement — that the economy ultimately finds its way to the equilibrium position." (Patinkin pp. 30-39 italics omitted)
How such a process accords well with our general description of the
process of experimentation. In the case of tâtonnement, however, we
have no reason for the process beginning in the first place. Indeed, one
would expect there to be great risks involved in setting the first set of
prices, for since such a set bears no relation to an equilibrium process
the person that sets these prices is likely to 'over-value' or 'under-
value' goods. That person would find, no doubt, that the risk of loss
is great enough to persuade him not to set these prices. As far as I
can see, the same argument will apply to a number of individuals setting
of arbitrary sub-sets of this set of prices. Yet the theory of prices
fluctuating in response to excess demand upon which the process of tâtonne-
ment depends, is itself implied by some theory of action as a response to
personal gain in some form. It would seem that the process of tâtonne-
ment implies that that process will never begin.

We would suggest that such an apparent contradiction is a result of
the theory of action implied by tâtonnement. If we were to conceive of
action on the lines discussed in Chapter Twelve, then it should be possible
for us to see tâtonnement as the outcome of a particular system at a
given time. For instance, we may think of a situation in which the
volume of trade is growing but irregular in which bookkeeping is very
rudimentary and in which barter is the means of payment. Such a situation
may force traders to keep a store of goods that can be easily exchanged
since they will probably find they have no goods to exchange just as
large shipments arrive for their purchase. In effect, they are forced
to set up a kind of partial money - money which serves only as means of
exchange or a store of value or a unit of account but not all three
simultaneously. Now not only can they see by this store how much profit
they are making but also the relative values of goods. In this way,
we could envisage commodity money arising not because of convenience but
because of the causal relationships which a trader must use in his work.

Once the structure of the market is established the étatement will continue. It will start simply because in our theory of action an experiment will be forthcoming according to the prices set up in the previously described manner. If individual traders are assumed to act according to the Principle of Continuity, then by experiment a system is set up by them to place themselves in the environment. In this system a trader may conceive of himself as reacting to prices that fluctuate according to excess demand or whatever. It is an admittance of the fact that any action in society is conditioned by the causal relationships in that society. For instance, the relationship between excess demand and relative price can be seen as a causal relationship between any action and the world in which that action takes place to the effect that a particular action implies changes in the environment that may lead to changes in that action. One could suppose that the nature of excess demand rather than acting through the desires of an individual acts through his stock of commodity money indicator. If he feels that the growth of that stock is insufficient for him to continue trading between shipments, he will raise his prices to increase this growth rate. Also such a price rise will mean others run down their stocks of commodity money more quickly and this will cause a drop in 'demand' for his products.

One could state this whole notion in terms of a person wishing to continue to be a trader. But our contention is that such a recourse to 'the will' is unnecessary. Rather, one can more consistently explain actions by use of the concept of an individual as a free agent where freedom is defined as the acceptance of causal relationships.

The second type of problem we think is able to be elucidated by our notion of action is that of apparent discordances between short and long run behaviour. A general statement of the problem is as follows:
Particularly important is the fact that the explicit time-horizon of each successive choice is much shorter, in principle infinitely shorter, than the span of time covered by the dynamic process as a whole. Thus while each step, being determined by a conscious set of choice, satisfies maximising conditions, the sequence as a whole does not. (Leontieff P. 1041 slightly amended)

For instance, that a particular person's welfare position satisfies partial maximising conditions at each point in time is no reason to suppose that over time he will maximise welfare. This may be because his behaviour at each time point could yield greater welfare if a different pattern of behaviour were followed. Such a situation arises because the individual is supposed only to rank alternatives given him. Since he is not given different continua (or life styles?) as alternatives, he makes the best of what he can.

So we have a serious deficiency in Economic theory in that a person by pursuing welfare maximisation policies at each point in time does not necessarily maximise welfare over time. If a person cannot so maximise long run welfare then although we may be able to explain short run behaviour by such a theory, we cannot explain long run behaviour. An illustration may be the theory of imperfect competition. In figure 5 D→D is the demand curve facing the firm before entry whereas D'→D' is the demand curve after entry; NR and NR' are marginal revenue curves derived respectively from the demand curves. Now if the firm is assumed to maximise profits one must be careful to make clear whether these are long run or short run profits. In figure 5(a) short run normal profits are earned but in the long run NR > NR' thus abnormal long run profits result. If, however, long run normal profit is earned (P, q, in 5(b))
then in the short run $M<NC$ no short run losses accrue. If we assume a person maximises long run profits we cannot explain how he would continue in business in the short run and if short run profit maximisation is asserted we cannot explain where abnormal profits arise. In neither case can we explain the whole behaviour of the perfectly competitive firm by recourse to its consistency to one motive.

In principle this and other similar difficulties may be solved by stating precisely the conditions under which experimentation may take place and postulating the Principle of Continuity. If we can prove that under certain conditions such a principle leads to short run consistency
but need not imply long run consistency we would have reason to suppose this is so. To illustrate this, let us suppose perfect competition. How under profit maximisation assumptions short run production will be set where $MC=MR=AC=AR$. Under the Principle of Continuity, however, the same will occur. The only variable our entrepreneur directly controls is the level of $MC$ at which he produces. A change in production thus implies a change in the level of $MC$ but causes a change in total revenue proportionate to the change in output. Any level of production such that $MC>AR$ will mean that the firm's total profit is less than normal. Since short run profit is the means by which firms continue in business to perform further experiments, our firm will go out of business in the long run. Similarly, if $MC>AR$ at current output then losses are incurred and so again our firm must go out of business. Those firms that survive will, then, have had to produce where $MC=MR=AC=AR$ in the short run.

In the above we have no recourse to asking what firms want to do but what they can do given the circumstances in which they are experimenting. By this means we argue that in the long run a firm behaving according to the Principle of Continuity will survive not if it decides to maximise but does survive because it happens to hit upon that output which does maximise profit. Short run profit, because of this, we are at liberty then to argue that given the firm wishes to survive in the long run, then it must produce a certain output that maximises profit. A causal relation exists between its survival and short run profit maximisation and this is the explanation of the firm's behaviour. To construct survival as a chosen end is to unnecessarily take on the theory of action which we have already criticised and its attendant 'billiard-ball' causality.

So to the long run. If long run costs are constant then clearly short run profit maximisation implies long run profit maximisation. But if we could show that in the long run costs cannot remain constant (perhaps because of externalities) then short run profit maximisation will lead to
long run losses, if external diseconomies, or long run abnormal profit, if external economies. The average cost curve may not appear to change from short run to short run but in the long run extra costs (such as the depletion of a fishing ground) or extra profits (such as increased wheat yields due to the planting of trees) will be found. Here consistency cannot be guaranteed by profit maximization. Rather we must see long run behaviour in these circumstances as a result of past actions performed according to the Principle of Continuity.

For example in the case of long run diseconomies. We can explain political action to obtain control of fishing grounds by indicating that there are gains from trade in everyone restricting his profit maximization. Otherwise it would be incumbent upon a profit maximiser to exhaust, for his own benefit, the fishing grounds, before everyone else. Yet how can we explain the concern over the conservation of resources, which is clearly an economic action if no one person can reap a gain from it?

The answer, I believe, lies in part in the Principle of Continuity. A set of fishermen on finding signs that the fishing grounds are becoming exhausted will find evidence of this in their actions. That set will, therefore, begin to find doubts about the future of their continuum. Since this will mean the fishermen can no longer perform actions we must conclude they will be led to actions which experiment with the preservation of the fishing grounds. Such preservation will be contingent upon the degree to which the latter experiments restrict fishing and if successful one could in the means/ends theory of action argue that the experiments (political lobbying, civil disobedience, etc.) were an outcome of the desire for survival. But we would argue that fishing survived because a given experiment succeeded in restricting fishing.

It is here that the so-called 'free rider' problem arises. It runs as follows:
"If a person expects another person, or persons, to provide him with benefits in any case, he will not voluntarily imitate action on his own. Especially if the number of persons with whom he interacts is large, the individual is likely to consider that his own behaviour in no way influences the behaviour of others." (Buchanan \[P. 114\]).

Now we do not have a solution to this problem except to show as above how it may arise. If our set of fishermen decide to restrict the level of fishing then some 'free rider' will find it profitable to not restrict his fishing. (A similar occurrence occurs in the theory of cartels.) Clearly such a fisherman is acting on a different continua than the other fishermen. Such a 'free rider' is only a problem because of the form of the restrictions with which the other fishermen are experimenting. If such restriction does not affect the behaviour of the 'free rider' then it is ineffective in that it will not result in the survival of the fishing ground. Any effective restriction must, therefore, bear upon the actions which are common to both types of fishermen although they may belong to different continua. We would, therefore, expect restrictions as to the way in which experiments are conducted to be much more effective than restrictions on the size or number of those experiments. In the case of fishing restrictions on the size of nets in the means by which grounds are protected rather than limitations on boat size or territorial fishing rights. Such conclusions could perhaps, also be used to advantage in the evaluation and design of income policies.

Another field involving the interaction of long and short run behaviour patterns wherein the principle of Continuity may be usefully employed, in principle, is the theory of the Consumption Function. It is well known that in the U.S.A. since 1944 a divergence has been found
between long run and short run Average Propensities to Consume. In
the short run (1-3 years) the APC > MFC whilst in the long run APC = MFC.
Such behaviour has been explained in a variety of ways by ratchet effects
(Duesenberry), by a permanent income hypothesis (Friedman), by structural
shifts (Smithies), or by a lifetime consumption hypothesis (Fodigliani-
Brunnberg). All have a similar feature in that present goal achievement
influences future behaviour.

We may explain such a phenomenon by supposing that an individual acts
according to the Principle of Continuity such that at a point in time
APC > MFC. Let us suppose that such a relationship is caused by the
fact that the action of working harder (i.e. earning higher incomes) leaves
less time for consumption activities to be performed. In this case, if
we now look at the individual's behaviour over time, we shall see that
APC = MFC. This is because a synthesis of each time point will yield
a relationship between consumption and income since at each time point
consumption activities and work activities are performed. Hence, a given
APC is attributable for each time point. That the relationship between
income and consumption must be constant is seen if we remember an
individual is acting according to the Principle of Continuity if he is
acting in the same continuum. Such a continuum could be summarised as:

\[ C_t = \alpha Y_t \]  \hspace{1cm} (1)

in which \( \frac{C_t}{Y_t} = \alpha \) and \( \frac{dC_t}{dY_t} = \alpha \)

It is then the nature of the single action in a continuum of action which
ensures the Consumption Function to take the form that it does. We do
not again need to invent special hypotheses of human behaviour to take
account of it.

On a more general level we would hope that such applications of the
Principle of Continuity by policy makers in the setting up of their action systems will enable them to not only predict the direction and magnitude of the employment of instruments but also the timing of policies. Since we have built action over time explicitly into our theory of behaviour such determinations should be possible once they have found the nature of the action continuum. Insofar as this hope is justified many of the problems that face such policy makers which are a matter of timing, e.g. growth policies, stabilisation policies, anti-speculation policies, would be soluble.

Let us now move on to the third type of problem in the science of Economics which we feel may be capable of solution by means of the above described perspectives. The problem may be conceived of as the conceptual split between 'macro' or 'micro' analysis or the influence of trade patterns on the economy in general (such as the relationship between trade and growth). Although some progress has been made recently in the study of an optimal trade in a growing economy (see bibliography in Bhagwati), it is still common to find the above dichotomy in static analysis. Here we wish to show that 'macro' and 'micro' analysis are intimately linked and indicate a method of analysis that may be used in the analysis of this relationship. As our base for explanation we shall take two theorems in International Economics: the Heckscher-Ohlin theorem and the theory of the Balance of Payments.

The assumptions of the Heckscher-Ohlin theorem have been stated as follows:

"(1) We have a 'double' model. So we have two countries, which we shall label A and B; commodities, which we shall denote by X and Y; and, factors, which we shall call C (capital) and L (labour) . . .

"(2) There is, in A and B, perfect competition in both markets . . .

"(3) Then, and if, trade is opened up between the two countries, there will be no costs of transport and no barriers of any form to trade . . ."
"(4) There is no mobility whatever as far as movements of factors are concerned between the trading countries. But there is complete mobility of the factors internally . . . .

"(5) We can make certain assumptions as to the "production conditions" existing in both countries:

(a) It is meaningful and possible to measure the physical amounts of each of the productive agents possessed by each country. Then we are able to compare the factor-endowment ratio of the two countries in the isolated state.

(b) The techniques of producing identical goods in the same in both countries . . . . and the goods or products are, or can be, classified according to their factor intensity . . . .

. . . . Each production function is subject to constant returns to scale.

"(6) Once we have postulated using assumption (5) (a) what one of the countries' factor-endowment ratio is, we can directly infer from it what its relative-price structure will be . . . .

"(7) Labour and capital are of identical quality in A and B.

"(8) There is a fixed supply of the two factors in A and B."

(Ford pp 3-4; italics omitted, punctuation slightly amended.)

Let us assume X is relatively capital intensive and Y relatively labour intensive in both countries at all levels of output. If in A capital is relatively cheap and in B labour is relatively cheap then A can produce X relatively cheaper than B, while B can produce Y relatively more cheaply than can A. Hence we conclude that:

. . . . each country will export that commodity in whose production a relatively large amount of its relatively
It is a rather more difficult task to find the assumptions of the theory of the Balance of Payments stated as explicitly those for the Heckscher-Ohlin theorem. We shall here try to sketch out the main points of the argument based upon Johnson's analysis (see Johnson, pp. 153-160). In an open economy payments by residents to residents of that economy are identical to receipts by residents from residents, \( P_r = R_r \), so:

\[
B = R_f + R_r - P_f - P_r = R - P \tag{2}
\]

The balance of payments is, then, the difference between aggregate receipts and aggregate payments. A deficit so that \( R < P \) implies one of two alternatives. Either cash balances fall or the cash balance fall is counteracted by credit expansion on the part of the monetary authorities. Johnson then distinguishes between a stock deficit (which is due to a once and for all change in either \( P \) or \( R \)) and a flow deficit (which is due to a continuation of the cash balance effect). If cash balances fall, then the deficit is likely to be only temporary since rising interest rates will tend to diminish the cash balances available for making payments. If, however, cash balances are continuously replenished then interest rates will not rise and the deficit can continue as long as the money instruments available for such expansion are not exhausted.

If we now assume that there are no international capital movements and no intermediate transactions then:

\[
B = Y - E \tag{5}
\]

Both \( Y \) and \( E \) are valued in units of domestic output. Given this let us now return to the Heckscher-Ohlin theorem. We know that \( X \) will be exported from \( A \) and \( Y \) exported from \( B \). If we take the case of \( A \) then if \( X \) and \( Y \) are produced there we can take \( A \)'s balance of payments to be:
\[ B_A = (X + Y) - Y_A + \frac{Y_B}{(X + Y)} + X_A \]  

where \( Y_A \) is the \( Y \) produced and consumed in \( A \) and \( Y_B \) is the \( Y \) produced in \( B \) but consumed in \( A \). If \( Y \) is to be valued in terms of \( A \)'s domestic output it must precisely equal that output which was devoted to the production of \( X \) in \( A \) that was exported to \( B \) in exchange for \( Y_B \). Hence \( B_A = 0 \). In the Hecksher-Ohlin theorem no balance of payments deficit is possible. It would appear, then, that the world implied by Hecksher-Ohlin is not the case world as that implied by Johnson. Since monetary institutions are the only difference between the two we must argue that in International Economics money is not a veil.

But before we can try to refute the belief that in this context Principle of Continuity is useful we must find a means by which the flow of goods and services over time can be stated in the form of a stock balance. In Economics this is usually achieved by valuing a flow in terms of money or some numeraire, such as domestic output, so that one can convert a flow into a change in the corresponding stock. In the present case the balance of payments corresponds to our stock concept and the trade pattern to our flow concept.

It is usual to convert a stock at a given time, \( Q_t \), to a flow by considering the change in that stock over time as:

\[ \Delta Q_t = Q_t - Q_{t-1} = S_t - D_t \]  

where \( S_t \) and \( D_t \) are respectively ex post supply and demand. We have, thus, a definition of a stock at a given time that is dependent for its behaviour over time on a net inflow or outflow from the stock. Theoretically, then, if one could measure the stocks at each point in time, assuming no wastage, flows could be calculated. But such measurement must depend upon there being a common unit of measurement such as 'market prices' or some indication of the purchasing power of money. It would appear that such flows and stocks will have to be measured in
units which are common both to the theory which explains them (such as continuity) and to some convenient yardstick obtaining in 'reality' so that these theories can be tested. Usually stocks and flows are measured according to the money flows with which they are associated.

For the so-called measuring rod of money, however, such a procedure is dubious. It raises what has been termed as the Index-Number Problem. If money is here equated with a numeraire good which is one of ‘n’ goods in the economy, then the problem can be stated as:

"It is abundantly clear that alternative sets of prices cannot ordinarily be considered independently of the tastes and habits of the persons involved when we rank purchasing powers of money. This can be stated . . . . Thus, if the price level is defined (where the price of numeraire good is unity as

\[ P = W_1 P_1 + W_2 P_2 + \ldots + W_n P_n \]

The \( P_i \) show the quantities . . . of the numeraire good (here the \( n + 1 \)th) that must be surrendered in order to obtain unit quantities of the first, second, . . . , and all other goods . . . . The \( W_i \) are weights \( W_1 + W_2 + \ldots + W_n = 1 \). We have seen . . . that . . . \( P \) depends on the \( W_i \) as well as the \( P_i \)." (Burstein P. 62)

It would appear, then, that to equate money dispensed with a real flow of goods and services is improper if one is to assume that such a procedure gives a standard measure of stocks and flows. Despite its convenience, however, a new measure of economic activity is needed.

It is here we introduce the new concept of subjective time. We shall here assume that actors have learned to compare phenomena by means of a clock. Whether such a clock is related to one's pulse or to physical time such as the time it takes to boil a pot of rice need not concern us. This clock is assumed to be unique for each individual and
will be used by the economist to measure the degree of continuity implied by given actions. This may occur in the following way. A person acting by the Principle of Continuity performs a given action which leads to another. The continuum in which he acts guarantees this. An Economist can only perceive of this continuum over time hence in order to compare two continua a description of each must be envisaged. Such description we shall assume is made possible by the assumption of a subjective clock which determines the intervals between different events. In all that follows it must be remembered that this assumption is not about the nature of the individual but a means by which the Principle of Continuity can be measured.

It may well be that an actor other than the individual whose behaviour we are studying must predict what the individual will do in certain circumstances in order to be able to construct his own system. We would suppose that such prediction is essential in bargaining, oligopolistic and scientific environments. The predictor, we assume, having made several experiments into this problem comes up with the idea that other actors act according to the Principle of Continuity. He will, therefore, envisage other actors as acting along a continuum. In predicting behaviour he cannot directly observe this continuum and so we shall assert he will employ some rule for its identification which is a necessary condition for that continuum. Such a rule is the Principle of Least Time which runs as follows:

An agent will perform one action rather than another in a given system at a given time if that action takes a shorter time for its completion than the other.

Although each subjective clock taken individually could be measured in terms of some standard clock to which each could be co-ordinated, we must be aware that such measurement is not objective. For if we set up an experiment in which there is a standard clock in a stationary system
and two subjective clocks in a changing system then the following may be true. Let us assume that we have two subjective clocks A and B which are moving in relation to each other. If we observe from the point of view of the standard clock A and B are synchronous but if we observe A from the point of view of the moving system it will not be synchronous with B. Einstein argued that it was the experiment for testing the synchronity of clocks that gave this result; in our terms we would argue that it is the Principle of Alteration which leads to this result. For not only are the clocks moving relative to each other but also relative to the observer that is part of that system. Because, then, our observer is part of that system no categorical test for the synchronity of the clocks can be made. Inevitably our observer will conclude first that A's time is not comparable with B's time and second that no absolute measure of priority exists within the system. (The first conclusion is analogous to the assertion that inter-personal comparisons of utility are impossible and the second leads from our argument on the nature of causality and can be seen in such statements as 'everything depends on everything else'.)

Given the above, we should be able to perform the first step towards an integration of 'trade' and 'monetary' theory in International Economics. We shall assume a two country, two good, two factor world. Let the countries be A and B, the factors C and L and the goods X and Y. We assume all trade is carried on by two international firms $F_A$ or $F_B$. We assume A has a subjective clock as does B. We assume A and B act according to the Principle of Least Time. We assume that both A and B have their own monetary institutions.

Now our international firms are able to perform either X or Y in the other country. If one country, A, is to rank its own activities by its
own time then clearly it must also rank the activities of \( Y_B \) by the same time. The same is true for \( B \). Let the following be the rankings in the two countries:

<table>
<thead>
<tr>
<th>A-TIME</th>
<th>B-TIME</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Country</strong></td>
<td><strong>Activity</strong></td>
</tr>
<tr>
<td>A</td>
<td>X</td>
</tr>
<tr>
<td>Y</td>
<td>15</td>
</tr>
<tr>
<td>Money</td>
<td>12</td>
</tr>
</tbody>
</table>

From the above we can argue that the total of activities performed by \( A \) in \( B \) need not equal those performed in \( A \) by \( B \). This is because although \( A \) will tend to export \( Y \) and import \( X \) and \( B \) will tend to import \( Y \) and export \( X \) the valuations placed upon each activity differ. Thus if we take the current account balance of payments for \( A \) the imports from \( B \) in terms of A-time will not necessarily equal the exports to \( B \) in terms of A-time, nor will the balance of payments necessarily be zero if some kind of exchange rate were imposed upon the above system. The balance of payments need not balance.

The balance of payments of a country, \( A \), excluding monetary movements can be conceived of as the difference between the time spent in \( A \) by \( B \) less the time spent in \( B \) by \( A \). This time is spent entirely by international firms thus:

\[
B_A = t_A - t_B \tag{6}
\]

If each perform one activity, then one can see that \( A \)'s valuation of \( B_A \) will be different from \( B \)'s valuation of the same variable. If \( A \) imports one \( X \) and exports \( Y \) then \( B_A \) in A-time is (15–40) whereas \( B \) values the
same transaction as (20-20). In terms of Economics this is an illustration of the argument stated above.

Now the above illustrates the nature of the problem of observation in Economics. It is essentially relativistic. In order to obtain some sense from the above technical relationships it is essential that some kind of static co-ordinates are set up by the observer. In the science of Economics such a co-ordinate is 'the measuring rod of money' but analogous to the above the same money has different values in different countries. Not only is money held as an asset but also it is not a store of absolute value but of relative value. As such, it has properties very similar to our concepts of A-time and B-time. It is for this reason that any measure of value which is itself part of the system may well find relationships which are not part of that system. It would seem then, that a pre-requisite of any integration of 'trade' and 'monetary' theory must set up a coherent set of co-ordinates by which the two theories can be simultaneously measured.

Hence, it appears we must look at any integration of 'trade' and 'monetary' theory from a more general point of view. We must impose a standard clock on the subjective clocks which each individual uses. Such a standard clock may be one based upon physical time such as the time in seconds it takes for a unit of money to circulate a 'standard' economy. Thus, we would suggest that unless some sort of standard clock is obtainable no such integration is possible. This is so until we realise that a standard clock of this nature is also part of the moving system which is being studied hence, depending on whether we take a standard time, A-time or B-time our notion of actual continua will be altered. It seems then we cannot take proxies for continuity. Instead we have to represent each continuum in such a way that they can be combined so as to provide some resultant. At present we know of no mathematical or logical
theory which can give us the desired mode of analysis.

It would seem, then, that another perspective is called for. We would suggest that the following analogy may prove fruitful. Trade flows could be seen as a continuing process over time. The action of exporting by an international firm could be regarded as an intra-firm transfer of property rights which proceeds by the Principle of Continuity. Whether such a transfer represents a balance of payments surplus or deficit to the countries concerned is not relevant to the form, or continuation, of this process (except insofar as stabilisation policies may directly affect it). Further the degree of deficit or surplus must depend upon the precise valuation placed by the countries concerned on this process which may not equate with the valuation placed upon it by the firm. (An example may be the sale of engines to U.S.A. by Ford subsidiaries in Europe at less than cost price being valued apparently at market price by the German and British Governments.) Thus in practice the balance of payments at each point in time may not necessarily correspond to the flows of 'real resources' involved.

But more than this we should not expect this to in theory. The measurement of the Balance of Payments is one action (albeit an unimportant action) in the economic system which gives rise to the trade flows. It influences, admittedly to a limited extent, the trade flows which it is supposed to be measuring. It is, therefore, part of the same continuum. Or rather is one event on the continuum of which the trade flows are also part. Hence, we should expect the accounting of a Balance of Payments surplus or deficit to be causally related to some other action but not necessarily so related to the trade pattern which also part of the same process. Instead, then, of looking at the theory of the Balance of Payments as a theory of measurement of trade flows we wish to construct a continuum such that a deficit on balance of payments is the outcome of a
certain trading pattern both of which are part of the same continuum.

Instead, then, we shall assume that the continuum can be determined by the assumptions of the Hodgechor-Chalin theorem changed so as to obviate the necessity of employing ends/means descriptions of behaviour. Now if we can describe the stock situation at a point in time without referring to a valuation process, we shall achieve our objective. We would suggest that since by assumption (7) both capital and labour are of identical quality in A and B they can form the basis of our stock measurement. Let us take labour. The pattern of trade could then be summarised by reference to the Marxian concept of 'labour embodied' in a given good. For instance, suppose the following:

**TABLE 4**

<table>
<thead>
<tr>
<th>Country</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>A</td>
<td>30</td>
<td>50</td>
</tr>
<tr>
<td>B</td>
<td>40</td>
<td>30</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Activity</th>
<th>X</th>
<th>Y</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>30</td>
<td>40</td>
</tr>
</tbody>
</table>

Under classical theory trade will continue until the trade off between commodities is identical. Thus, if A has 150 units of embodied labour and B has 200. Now A can produce 4,500 X or 6,000 Y, B can produce 10,000 X or 6,000 Y. Let us assume internationally 1X is exchanged for 1Y. If A exports Y and B exports X we can see that (if A only produces Y and B only produces X) then 4,000 Y may be exchanged for 4,000 X. Now this means in terms of labour embodied A gives 100 units of labour embodied in exchange for 80 such units from B. Since these units are of identical quality A losses in the proportion of 80/100 in such trade.
Though if we were to calculate our balance of payments by previously mentioned means it would be zero.

This bears upon the discussion of international firms. Two firms $F_A$, $F_B$ acting according to the Principle of Continuity will, therefore at a point in time be transferring labour embodied from one country to another. Clearly this will have an effect upon the patterns of investment, employment and the level of real income. In the country which gains labour embodied these except the first will tend to rise whereas in the other country a process of labour substitution will take place causing a rise in investment. We would, therefore, propose that the balance of payments be looked upon as a 'real' phenomenon rather than a 'monetary' one on the lines given above. There is, however, no intention of trying to re-assert the importance of the Labour Theory of Value; instead we are like Keynes making use of a labour unit as the basis of aggregation. Such a procedure is, however, so open to dispute that, although on the basis of the assumptions of the Heckscher-Ohlin model, it may be valid we shall not use it here. Two units of labour-embodied may be identical although they have different contents of capital and labour and different histories. This alone means that a unit of labour embodied is not uniquely determined as to its nature as the unit of a numeraire good is. Moreover, it is possible that if one were to measure economic phenomena by this unit significant differences in capital/labour ratios would be clouded over.

Traditional attempts at an integration of monetary and trade theory employ the supposed analogy between balance of payments as a stock and trade flows.) Let us propose an alternative approach. Suppose we have two countries A and B such that A habitually has a balance of payments surplus and B a deficit from their trade with each other. Suppose now
that A incorporates B. Immediately there is no measured balance of payments but the trade relationship between the two former nations still exists. In terms of goods and services there is a balance of payments which could presumably be measured according to some standard. This example indicates that the relationship between trade and the balance of payments is essentially one of measurement. Trade is a measure of the balance of payments at a point in time whereas the balance of payments can only be seen over time. For example, in our commodities X and Y then trade could be seen as the relationship between $P_X$ and $P_Y$ such that $P_Y = \frac{X}{Y}$. If, in the course of time, payments in the ratio of 2X for 5Y had been made per unit of Y. Given the assumption that the balance of advantage is zero over time we can argue that 2X is equivalent to 5Y. 

But if we assume the balance of advantage is not zero, e.g. because we believe trade is not 'fair', then our relationship between prices and payments breaks down.

Yet the Principle of Continuity asserts that although trade at a point in time will obey the principle of consistency this not necessarily so of trade over time. In terms of the balance of payments this implies a denial of the necessity of the balance of advantage assumption.

Trade, according to the Heckscher-Ohlin theory, then, will be part of a continuum set by the nature of the payments flow. We may illustrate this notion as follows. A person acting as a trader-experimenter in our simple economy can only assess the continuity of his firm by the stock of goods he has available for exchange at a given time. Only if this stock is positive will he continue in business. If we assume trade is carried on by such traders acting internationally, then we can see that depending on stock rises or falls so the terms of trade will change.

If we now add all stocks together at a point in time and compare that result with a similar aggregation at another point in time, we shall
obtain an idea of how trade flows have changed and how the balance of payments has altered. The balance of payments thus is interlinked with trade flows to the extent that a change in one alters the way in which the other is measured at a point in time.

If we are to explain, either the balance of payments at a given period we cannot use trade flows. Thus we would propose that a theory of International Economics must indicate by the Principle of Continuity the pattern of exchange stocks over time. If a trader faced a constant inflow of goods he would continue in business if he had sufficient exchange goods to allow his purchase of the next batch of imports. If, however, traders face irregular imports then only those traders who keep large stocks of exchange goods will remain in business. Hence if we suppose trade proceeds at a constant rate over time balance of payments over time will be zero, the direction of trade will be determined by relative factor endowments and the relative prices of the goods will not change. If, however, the rate at which the trader experiments varies (perhaps because of the variability of imports) then the balance of payments need not be zero for, depending on the period chosen, stocks of exchange goods may be building up ready for the next irregular import. This is despite the fact that trade may still be determined by (unchanged) relative factor endowments and prices are unchanged. We would argue in conclusion, then, that any integration of the 'monetary' and 'real' processes in International Economics must be based upon an investigation of the behaviour of international companies and that this investigation must be according to the Principle of Continuity.

It is hoped that this chapter has indicated some of the promise of our revised theory of action. We hope that further research and thinking along these lines will assist Economics to overcome some of the criticisms we have aimed at in this thesis, whilst performing the exciting task of
investigating social behaviour on a more general and deeper level than
has been possible until now. Unfortunately, it does not fall to us to
carry on the analysis into the investigations of data at this time but
we hope to have that opportunity in the future.
It is with trepidation that I now embark upon describing those conclusions which, I think, it is safe to draw from the above thesis. That which is here presented will be a subjective selection from our thesis and could be interpreted as a guide to the above work rather than any advertisement for it.

If I were to select that part of the above which contains our main thesis, I would pick Chapters Eight, Nine and Ten. Chapters One to Seven form a prelude to, and Chapters Eleven to Thirteen an extension of, our central argument. We would suggest that such conclusions as may be forthcoming from our study will be found in these three Chapters and so we shall point out what might be learned from them. Perhaps the arguments presented in these Chapters could be grouped under two heads: those connected with the theory of causality and those connected with the theory of action. In each case, it seems that our argument is better highlighted if we start by summarising the traditional view as it is contained in the science of Economics. We begin by considering
the theory of causality.

The theory of causality presupposed by the science of Economics was found to be the Humean constant conjunction theory. Given that all knowledge is, at base, empirical in the sense that sensation is at the base of all experience then Hume argued that causality cannot be observed as such. All that a scientist can induce is that causal relationships are learned from the observation of constant conjunction of events in a large run of data. An Economist in this sense as observing data, quite neutrally, in the sense of not altering the data he is observing in the course of his observation. Furthermore, the causal relations so induced are to be divided from other relations on the basis of the constant conjunction criterion.

Such a view could be criticised we found on two main grounds. First we argued that an economist must be part of the society which he is observing. He cannot, therefore, in general assume he cannot influence the data of which he is a part. In particular, we found he cannot make the neutral comparisons between events implied by the constant conjunction theory. A person may observe what seems to be a constant conjunction but he cannot be sure that such an observation is not due to his point of view rather than any 'real' relationship to be induced from the data. We summarised the tragedy of the observer of society in the Principle of Alteration (see Chapter Six).

Second, we found certain arguments that lead us to believe that causality is not the simple relational entity that Hume believed it was. In Chapter Four, by reviewing the major theories of causality, we came to the conclusion that a causal statement was one whose form was a precise mirror of the actual relationship between phenomena to which the statement as a whole referred. Not only did a causal statement
refer to a relation between two events, but also implied by its form
the nature of that relation. This led us to state that a particular
causal statement could not refer to empty sets of data. Clearly, a
causal statement is not simply a statement of relation but has
extensional import.

Further we were able to deduce (in Chapter Five) that causality
could not be represented in a consistent logic. This implies that a
causality statement is not truth-functional and that only inconsistent
logics are useful for our purpose of symbolising causality. Since we
are unable to investigate the problems associated with the formation
of an inconsistent logic, we left the symbolic problems of causality
aside. Moreover, we found that by examination of the process of
scientific judgment that in a situation without alteration the Axiom of
Constructability must hold before science is possible (see Chapter Six).
This implies that, at least, two languages must be at the disposal of
the scientist before he can make scientific judgments.

From these latter three results, we argued as follows. Let us
assert there to be no alteration for the present. Let us also assert
science tries to formulate causal relationships between phenomena. In
these circumstances, two languages must be involved in the formation of
causal statements, by the Axiom of Constructability. One language is
used for talking in and the other about the data. A causal language
is thus composed of two sub-languages. Now it is the relationship
between the two sub-languages which provides a clue as to the nature
of causal statements.

Causal statements are peculiar in that a given variable representing
a cause or an effect may not be replaced consistently by another with
the necessity of the truth value of the statement being unaltered. Such
statements are termed oblique. Now, of the three methods proposed to solve the problems of reference in such statements (description/sense formulation, the extensional logic and Russell's theory of descriptions) only Russell's solution was found to be adequate (see Chapter Seven). This means that a causal statement was a description of a relation between phenomenon. It did not itself have extensional import though unless it was a description of some causal relation it would not have been formed in the first place.

It was on the basis of the above that our notion of the Process of Causation was derived. This begins by our noting that neither can a cause be described without its effect nor can an effect be separated from its cause. Coupled with the relationship between our causal sub-languages deduced above implied that the two causal sub-languages precisely mirror each other. If now we assume alteration to be present so that the Axiom of Constructability does not hold, we find that only statements constructed in precisely the same manner as causal ones are part of science. Hence, in alteration the only scientific statements possible are causal.

But, more than this, the statement of causality is to be seen as just one made at a point in time and is the outcome of phenomena at that time. It is, however, implied in the process of causation that phenomena are linked over time. This relation, we postulate, becomes apparent because of the continuous process involved in causality, such that events appear along that continuum. It is this latter process which Hume concentrated upon but it is dependent upon the formerly described process of causation for its form.

From these arguments two conclusions for the science of Economics seem to result. First, if data are observed and analyzed under the
assumption of the Axiom of Constructability then if alteration is present it will appear that 'everything depends upon everything else'. For example, if an observer tries to find causal relations on the basis of priority whilst he is part of the system he is observing then any time he takes will be altered by his movements in the system. This may be to such a degree that causal relations of the 'billiard-ball' type are not observed as such but rather as loose associations in which there is no clear priority (see Chapter Thirteen). Second, if causality is examined whilst alteration is assumed to be present, the resulting un-directional relations are immediately interpretable. They can be construed as the intimate two-way relationship between cause and effect at a point in time which is just part of a causal continuum. If this continuum is investigated, then no doubt the precise relationship between the causes and the effects will be uncovered.

We shall now move on to review our arguments upon the theory of action. In Economics it is usual to define a free individual as one who is able to perform actions undetermined by any behavioural laws. If we are to keep our notion of knowledge as based upon sensation then the individual must possess some agency of his own which can initiate and perform actions. This primitive mover of action is usually termed "the will". The will is seen to be able to design the ends to which actions are performed and also to so manipulate the various means available to him in order to achieve those ends. The traditional theory of action depends upon there being a separation between mind and body. Ends are designed independently of the observation and use of the means by which they are achieved. Since actions performed by free men are voluntary the performance of an action involves a process of choice by 'the will'. This is usually construed as a process of decision in that it involves
the selection of that action which will most nearly conform to the ends prescribed by 'the will'. The nearness or otherwise of the results of actions to ends is adjudicated on the basis of some value system that 'the will' is supposed to possess.

Three strains of argument can be criticised here. First, the conception of 'the will' is not wholly without criticism because it acts like a demon in a clock. Just as we could attribute the behaviour of a clock that did not conform to a law of clocks to the presence of a malevolent demon in the clock so we see 'the will' as ensuring the freedom of the individual. In other words, although the body is seen to operate by means of mechanical or chemical processes, the mind is seen as inexplicable in these terms. But it is 'the will' that causes bodily movements to be performed. For this in terms of the human theory of causality, mind and body must be contiguous and not separable in the way postulated (see Chapter Ten).

Second, two sorts of reality are asserted: one 'in the mind' and another 'outside the mind'. In the case of our economic observer, who is also an actor, this amounts to our being able to separate him from the society of which he is part by virtue of his observing it. But this argument we found was invalid by the Principle of Alteration. And, third, we found the definition of freedom implied by the traditional concept of action gave rise to more problems than it solved (see Chapter Nine).

Instead a definition based on that of Sartre was conjectured which involved a man being free only insofar as he accepts the causal relations in the environment of which he is part.

Furthermore, we found that motive and movement which together form action are causally related. Hence, if we are to change our conception of causality in the light of our discussion on that matter, we shall
need to change our theory of action as well. If we accept our comments on causality then a different theory of action must be forthcoming if we are to have a consistent analysis of economic behaviour based upon the actions of free individuals.

It was because of those considerations that we were led to look at action not in terms of consistency between ends and means at a point in time but from the point of view of a continuous process. We looked at the individual as performing actions at a given time which were part of a continuum of such actions. Our individual is seen to experiment in his environment not according to his 'will' but according to the Principle of Continuity. By this, one action leads on to another action and so on along the action continuum. Hence, any action not only affects the present but also all future presents. Any action must be looked at as having two components: an appearance at a given time and an appearance over time (see Chapter Twelve).

Such a conception can only be accepted by the science of Economics if it leads to the formation of new meaningful explanations of economic phenomena. In Chapter Twelve we found that this revised concept of action implied one typical consistency theorems whilst establishing a new theory of action over time. Hence, there seems, on grounds of elegance, reason to admit the continuity concept. Moreover, in Chapter Thirteen a new method of integrating 'trade' and 'monetary' theory in International Economics was suggested on the basis of the Principle of Continuity. We would suggest that advances in the fields of economic policy (especially with regard to the problems of timing), of consumer behaviour theory and of welfare theory, may also be forthcoming from the Principle of Continuity.

As to suggestions for further research: some suggestions have been made from time to time in the text. Rather than list these, we would
simply remark that our thesis is still in its infancy. Further research along the lines suggested by the point of view expressed above seems to be essential before this idea can bear its full fruit for Economics.

* * * * *
Chapter One:

1. See Robbins, Krupp, etc. on this.

2. It should be remembered that our study involves only a perusal of Economic literature in Britain, U.S.A. and the English world. For the large part Continental, Russian and Development economists have been excluded through want of time and because we are concerned with questioning accepted dogma by showing there is reason (perhaps not sufficient reason) for it to be changed. It must be emphasised that others must follow along this path before any concrete results obtain.

3. Social space can be defined as the set of occurrences in which the postulated relationships can be said to be valid (see Papandreou chapter 6).

4. We are here asserting that economics denies there is a historical law which can explain society at a given space/time. The classic formulation of this is 'bygones are forever bygone'. This is an important denial of the use of historical method as tools of economic analysis (see Introduction).

5. One could reasonably object that we are being too harsh on Economics here. If one stated the motives in terms of tendencies then consistency will not be impaired.

6. Buchanan has apparently come to a similar conclusion.

7. Man always tends to be selfish but is always constrained by society (the means by which he can behave selfishly) in his actions.

8. 'Reforms' in the Communist countries of Europe are seen, by some, as evidence of this.

9. The distinction we make here is a very general one. It is not consciously related to the production/consumption dichotomy usually analysed in this context (see Arrow and Solow for references) though future research may reveal that it is. Really it is an attempt to list the kinds of influence that affect the individual merely because he lives in society.

10. Again we must emphasise the generality of the following.

11. Apart from the early political economists few economists have considered these problems rather they take society as given and then analyse how people act within it. Yet we have seen that action determines society and so we feel it is a little inconsistent to view the social system as given whilst actions make their varied interplay.
Chapter Two:

1. Often the idea has been characterised as the 'blotting paper' concept of learning. The mind is like a piece of blank, clean blotting paper at birth and is, through life and the agency of the person's senses, gradually filled out. Everything that is sensed forms an impression on the blotting paper.

2. To ask 'why?' need not, however, imply an answer in terms of efficient causes only. As Aristotle showed, there are other sorts of cause than the type with which Hume was principally concerned. Thus the argument here may well be irrelevant since Aristotle was concerned with a wider definition of causality than Hume.

3. In the Aristotelian sense (see Metaphysics 983).

4. I am indebted to Mrs Ruth Troddler for this point.

5. This theme is enlarged in Chapter Three.

6. It is interesting to note that Marshall acknowledges his indebtedness to Hegel in this matter. The concept of mutual causation that he hints at has not, to our knowledge, been taken up until now (see Marshall P. viii).

7. See Popper (1) pp. 217-220.

8. This is the basis of Samuelson's criticism of the P-twist. The latter asserts that the original assumptions of a theory may be wildly inaccurate descriptions of the phenomena. All that this formula indicates is that it is logically impossible for a proposition with a given property (say empirical validity) to be derived from assumptions that do not have this property.

9. In the S-twist/P-twist debate it seems we have a choice in stating either the 'why' of this is unimportant or the 'what' of thing is unimportant.

10. I suppose each magnitude of a given variable is a unique event or at least is a proxy for a unique event. Yet there does seem to be a distinction between the occurrence of a moving train at a given space/time and two moving trains at the same space/time.

11. Say a simple Keynesian model with a demand for labour function L

Chapter Three:

1. Kant P.12.

2. See Popper (1) pp. 218 ff.
Chapter Three (Cont'd):

3. The growth in the expenditure of the Brookings Model to over $20 billion (U.S.) p.a. illustrates this whole paragraph rather graphically.

4. See Friedman on the role of imagination in science.

5. The same kind of difficulty was raised by Hume.

Chapter Four:

1. e.g. C.D. Broad ch. II, M Dunge chs. 1-4.

2. This may well be illegitimate for most purposes since the idea of causality is intimately connected with the solutions to other problems that a particular thinker may wish to solve and so we are lifting ideas out of context. For our purposes this is not an important criticism since we are concerned with causality in the abstract and not with describing a system of philosophy for Economics. One could look at it as follows. We are trying to answer the question 'what is causality?' whilst holding all other 'philosophic variables' constant. Of course this is artificial but we trust the solutions obtained are justification for this admittedly syncoblochial procedure.

3. e.g. A.C. Dwing ch. 1, C.J. Ducasse (1)chs. 1-4.

4. See Aristotle (1), (2).

5. See Levinson A.B. and Thalberg I.

6. Here substance is used to indicate the origin of a chain of causes like a God-head.

7. See Aristotle (1) 1015a lines 24-35.

8. Note that on reading progress in time it must be understood that unique progression is not implied. For instance, a formal cause is like a law of nature unchanging over time whilst in final causality the last discovered event is in fact the cause of a previous event. I am indebted to Mrs Troollar for these points.

9. This division was continued by the Scholastic philosophers. Different terminology was used by the essential distinction remains, (see Acquinas on exemplary and relative causes and
Chapter Four (Cont’d):

Ockham on essentially ordered and partially ordered causes.) Indeed, one still finds a distinction between 'mechanical' and 'chemical' types of causal interaction.

10. See Hume pp. 71 ff.
11. See Bacon Bk. II, VII.
13. To an extent much debate in Economics is of this nature. See for example the literature on marginal cost pricing (refs. in Friedman footnote 13, p. 15). Also the literature on the Assignment Problem which are largely arguments about definitions or the consequences of different definitions for the basic model.
14. See Ockham on causality.
15. See e.g. Marganau ch. 5, Pepper (1) p. 217 ff.
16. See Mill (2) p. 213:

"The notion of Cause . . . is . . . the root of the whole theory of Induction."

and Hume p. 89:

"The only connexion or relation of objects, which can lead us beyond the immediate impressions of our memory and senses, is that of cause and effect; and that because 'tis the only one, on which we found a just inference from one object to another."

17. See Locke.
18. See Berkeley, Hume.
19. See Kant, Introd.
20. Because Locke sees everything of the mind as an idea and since those ideas are deposited in the mind by sensation in conjunction with memory we could argue that causality is productive in that it is produced by a simple input of sensations.
21. Strictly speaking Kant tried to find the conditions under which a unique ordering of events was cognised. He did not explicitly examine the problem of priority as such.
22. It is as well to draw a distinction between the contiguity – the inseparability in time and space – of two events and
Chapter Four (Cont'd):

21. their contingency - the possibility that they exist together. Necessity is the opposite of the latter but perfectly consistent with the former. See Hume p. 75.

23. Those are still common. For example, Dunge's final definition smacks of production.

24. Hume p. 166.

25. Mill (2), Russell, Ducasse (1), etc.

26. Hume himself faced this difficulty.

27. We shall deal later with the proposition that all particular causal statements must be derived from causal laws (see Davidson (2) ); for our present purpose it is sufficient to point out that the uniformity theory has certain drawbacks but also useful features.

28. Ducasse (1) p. 17 goes some of the way when he asserts:

"... if I were confronted with an object and were to hit it with a hammer, and the object broke into fragments; and in addition I happened to know that the hammer blow was the only change in the circumstances of the object at that moment, then I would also know from this alone and without the least need of inquiring whether or not those two events or others like them have been conjoined before or ever will be again, that the hammer blow caused that smash." (footnote excluded).

29. Note Kant assumes they must be thought it seems intuitively more correct to follow Popper in asserting that they may be so imposed.

30. See Vaihangor. Also see Russell (1) p. 223:

"The word 'cause', in the scientific account of the world, belongs only to the early stages, in which small preliminary, approximate generalizations are being accosted with a view to subsequent larger and more invariant laws."

31. See Kant pp. 149-160. Being contains an analysis of a different proof which is neither conclusive nor as widely used as the one we have reported.

32. Kant p. 182:

"... phenomena ... are ... objects of a possible experience."
Chapter Four (Cont'd):

33. The three types of synthesis are 'the synthesis of apprehension in perception', 'the synthesis of reproduction in imagination' and 'the synthesis of recognition in concepts'. All three are essential if a representation of a manifold is to be made.

34. It seems doubtful if a summation of partial causes can in fact supply an essential explanation see Levison and Thalberg.

35. See Fwing pp.82 ff.

36. The medium is the massage?

37. This discussion bears upon the distinction often made by linguistic philosophers between use and meaning, or use and mention. There is no way of ensuring that in fact that the meaning of a sentence is being tested rather than its use.

38. Broad ch. II.

39. Mill Eik.,III, ch. V. Sec. 11.

40. See Berkeley.

41. See Broad ibid.

42. See footnote 26 above.

Chapter Five:

1. See Bunge and Lopper, John for an analysis of these reasons.

2. This appears to be so of other formulations such as 'is caused by' and so on.

3. See Quine on the distinction between 'use' and 'mention' especially pp. 97-98.

4. For an introduction to modal logics, see Lewis and Langford ch. V and Carnap ch. V. The view we here represent is seen as a direct descendant of Levin's system of strict implication.

5. This work has a useful detailed analysis of the relationship between causality and various implications.

6. Lewis and Langford p. 124 equ. 11.02.
Chapter Five (Cont'd):

7. This is seen as follows:

\[(\omega) \Box (\omega \land \omega) \equiv (\omega = \omega)\]

substituting:

\[(\omega) \Box (\omega \land \omega) \equiv (\omega = \omega)\]

Again:

\[(\omega \lor \omega) \Box (\omega \lor \omega) \equiv (\omega \lor \omega)\]

But this contradicts:

\[(\omega) \Box (\omega \lor \omega) \equiv (\omega = \omega)\]

Since \(w\) also necessarily follows from something else yet \(w, \lor w\) only follows necessarily from \(w\). Hence we have altered the hierarchical system of necessities which Aristotle implied by his definition.

8. Presumably defined a la Tarski.

9. Together with most logicians.

10. I mean really that causal proposition is not simply a logical one. It is both extensional and intensional whereas Burks seems to have concluded it is analyzable in terms of pure logic. It is true that part of what a causal statement proposes it about itself and hence analyzable in terms of logic via a meta-system as this. But it also appears that not all the properties of causality can be so analysed. The implication:

\[(\vdash A \supset B) \supset (\vdash \Box A \supset \Box B)\]

clearly implies that once we can prove a conditional relation between propositions we can prove a similar relation between causally necessary propositions. Undoubtedly this is so logically but I wonder if the same could not be said for any linguistic entity we may choose. When we talk about propositions Burks is correct but he is not talking within causal analysis.

Burks analysis thus appears to be superfluous since causality is its own meta-language.

11. If one takes a different criterion, say Henderson's non-triviality criterion with 'material' reading 'strict or causal' and vice versa. In which case we must show that 'material' implication can deduce statements which strict and causal implication may
Chapter Five (Cont'd):

11. not. Now, here, we may find certain causal expressions better expressed by truth functional logic.

12. In the sense of Tarski (2).

13. By Godel's theorem for a simplified statement of which see Newman and Nagel.

14. See Tarski (1).

15. See Davidson (1).

16. See O'Carroll, Simon etc.

17. Note economists have asserted that conditionals are what economics is about. For a statement of this view see Kehoe.

18. For example see Kleene or Tarski (3).


20. It should be made clear that proposition does not mean here sentences together with their meaning. Here proposition is taken to be linguistic entities that are themselves extra linguistic but which can be expressed by sentences.

21. Truth value here does not imply causality in truth-functional but simply that it can be said to be true or false.

22. Perhaps the distinction between extension and intension can be characterised as follows. Extension is the truth of the sentence whereas intension is the proposition expressed by it. Thus in the sentence: 'That man is bald' the extension is truth value that the man has no hair, whilst the intension is the proposition that the man is bald. The first (extension) can only be found by reference to a state description whereas intensions can be found by analysis.

Chapter Six:

1. Him made great use of arguments under this heading.

2. Seen here as the so-called Vienna Circle - Carnap, Schlick, Franck and von Mises.
Chapter Six (Cont'd):

3. It does not matter whether we take verification or refutation as our criterion. The principle in both cases is identical.

4. See von Mises p. 50 ff.


6. See Samelson, Marganau, Popper etc.

7. c.f. the classical theory of micro-economics.

8. Note that from this many of the unscientific allegations bandied about by positivists and anti-reductionists are immediately translatable into political dogma.

9. Tarski (1) p. 156.

10. See Carnap.

11. Madsen would, however, probably not wish to say this.

12. See the work of Suppes, Beth and Van Fraassen.

13. See Popper (2) p. 74:

"... a system of analytic statements ... cannot be regarded as a system of empirical or scientific hypotheses ... since it cannot be refuted by the falsification of its consequences; for these too must be analytic."

14. See criticisms of Quine on this.

15. It should be made clear that logicians have drawn distinctions between identity, equivalence and equality (see Carnap) which again is a debate that is needless for us to enter. We should, however, be aware of the fact that weaker forms of the identity relation have been introduced (see Barcan Marcus pp. 77-81) following Carnap's discussion of equivalence.

Chapter Seven:

1. Much of the following is based upon Carnap and Russell (2).

2. '(7x)' is interpreted as 'the one individual x such that' and '(... x ...)' is a sentential matrix with 'x' as a free variable.
Chapter Seven (Cont'd):

5. 'x ≡ z' means 'x' is the same individual as 'z'.

4. Meaning here is to be understood in the sense of designation meaning. See Carnap p.6.

5. Note that Carnap has suggested that the definitions could be applied to the first method to circumvent the necessity of inventing an infinity of labels by using extension and intension. But since we have found causality is not truth-functional, this is obviously inappropriate.

6. This does not, however, involve the conclusion that the only object of science is the search for causal laws. The assumption is an attempt to cut through the difficulties surrounding this aspect of methodology, which seems undecided as to how scientific languages are in fact invented. All we do is to look upon language formation as a scientific activity.

Chapter Eight:

1. See Suppes (2) p. 260-271. It appears that the necessary functional relation between the two sub-languages looked upon as sets of notation is possible.

2. This is impossible we have argued.

3. The same may also be true of other kinds of statement by the Principles of Alteration.

4. This comment may be doing an injustice to certain observers. For instance, Marshall writes:

"He [i.e. Cournot] taught that it is necessary to face the difficulty of regarding the various elements of an economic problem, not as determining one another in a chain of causation... but as all mutually determining one another. Nature's action is complex, and nothing is gained in the long run by pretending that it is simple, and trying to describe it in a series of elementary propositions." (p. (viii)).

5. Perhaps this point is better illustrated with reference to the assumption we made in the previous chapter. There we assumed that the formation of a causal sentence proceeded in precisely the same manner as science was conducted. This implies that the Axiom of Constructability holds. Now we have assumed that it does not hold.
Chapter Eight (Cont'd):

6. The following is only tentatively held.

7. Figure 1 represents a sketch of the graph.

8. For this we may use a measure of versimilitude as suggested by Popper (1) p. 228 ff.

9. See, for instance, research into the demand for and supply of money.

10. For discussion of this see Harris (appendix).

Chapter Nine:

1. For an excellent introduction to this see C.D. Broad "The Mind and Its Place in Nature" R and K.P. 1925.

2. This phrase is taken from Ryle upon whose discussion much of the following is based.

3. 'Humanist' and 'determinist' are here to be taken as categorizations of opposing positions. They are not intended to reflect the two philosophical positions more usually characterized by those names, though it may well be that they do.

4. 'Complete explanation' can be regarded as analogous to 'essential explanation' for which Levinson and Thalberg should be consulted.

5. The parallel between this idea and that of Caws is clear.


8. See Chapter Three for a fuller statement.

9. Perhaps one could look at the differences between the F-twist and the G-twist in this light. The F-twist does not ask why a person broke a given law whereas the G-twist would be. Again I am indebted to Mrs Treoller for this point.

10. i.e. uniformity-type causal relations.
Chapter Nine (Cont'd):

11. For the following let us suppose that experiment here means a perfect experiment despite the difficulties associated with alteration.

Chapter Ten:

1. See Lopper, John.

2. From the above it is clear that we are speaking very generally about economic concepts. Despite certain movements towards linear programming techniques (e.g., Lancaster) the conception of an actor as a goal-oriented agency is as strong as ever.

3. See White Introd.

4. e.g., the verb 'to skate' in the sentence 'I skated across the pond after John pushed me' does not denote an action for 'I' since 'I' did not intend it, whereas in the same sentence the verb 'to push' does denote an action.

5. See Aristotle 1026b.

6. For those see White Introd.

7. This paragraph seems to me to be inconclusive. In particular, the linguistic distinction between 'reason' and 'cause' upon which the whole argument rests is not dispelled. Let us hope further work will reveal that our argument is not too misleading.

8. For an exposition, see von Wright.

9. For a similar conclusion derived linguistically, see Dower.

Chapter Eleven:

1. Suppes (2) p. 106.

2. Micro-economic decisions can also be seen in the same framework. Here, however, we shall concentrate on the macro-economic theory and leave it to the reader to interpolate suitable micro-economic analysis.

3. See above Chapter Ten.
Chapter Eleven (Cont’d):

4. For a more rigorous statement consult Fox, Thorbecke and Sen Gupta.

5. For an excellent discussion of models see Suppes (2). Also consult Lepor, John for a simplified and imprecise analysis.

6. The impossibility theorem is quoted in full. No attempt to abbreviate or alter it has been made except minor notation and a certain amount of editing.

7. The reader may be puzzled by the sudden use of the term 'cause'. I believe it is obvious if we remember the conclusion that all activity is causal and so the linking of 'ends' and 'means' must be by means of causes.

8. See Leontief.

9. For discussion of this and related points, see Willet and Forte.

10. See Popper (1) pp. 72-75.

11. See Suppes (2).

12. This is a term invented by, I believe, Whitehead for the phenomenon of supposing that observed phenomena will conform to formal calculi. An example may be that Pareto Optimality should be the aim of Government policy when in fact Paretoian Welfare Economics is a theory of consumer and producer behaviour. In essence the term implies the supposition of conformity between a theoretical analysis and the perceived world.
AUTHORS CITED


Burgo, Maria: "Causality"; Meridian, Cleveland, 1963.


Burostein, H.L.: "The Index-Number Problem"; pp. 61-64 in Clower.


(2) "Actions, Reasons and Causes"; pp. 79-94, White.


Ducasse, C.J.: (1) "Causation and the Types of Necessity"; Dover, New York, 1969.


