2. Cartesian economics and the place of the entrepreneur

Realism in Economics - some observations*

We have taken the framework for our discussion of the distinctiveness of modes of reasoning from Etzioni's project (c.f. Chapter 1). We will be primarily concerned to see if, as Etzioni suggests, Economics can be made more "realistic" by importing sociological theories, data and findings. We do not want to propose, though, that sociological conceptions of social life have a more "realistic" character than anyone else's. Neither do we wish to advocate the in-principled incorporation of Sociology into Economics in order to make the latter more realistic. In our view, it is no business of Sociology's to attempt to decide one way or the other on the "reality" of Economic accounts. That is Economics' own internal affair.

This much being said, what is interesting is that, of late, the reality of Economic theories and explanations has become more and more of an issue within the discipline. This is one of the reasons Etzioni offers for the attractiveness of Socio-economics. The debate has been carried out in two very closely related arenas; the character of Economic models and their specifying assumptions, and the character of findings and explanations. The debate over the propriety of certain orders of assumptions or models has been perennial. In its present form, it appears to be a revolt against the pre-dominance of what is called "Positivist Economics" (after a enormously influential text) and the 'instrumental' defence of the lack of realism in its assumptions which was provided by one of its major figures, Milton Friedman (Friedman 1953). In essence, Friedman argued that lack of realism in assumptions was unimportant as long as the requirements of prediction were satisfied. The major lines of attack on this instrumental justification have been on its logical basis and on the way in which it excludes possible alternative conceptions of economic activities. Among these alternatives are to be

found those arguing for a more social (or as they put it "institutional") conception of economic life.[1]

Alongside this worry over the realism of assumptions is a worry over the realism of findings and explanations. In large measure this is a consequence of the adoption of the instrumentalists concern with the priority of prediction and the use of increasingly sophisticated techniques of appled mathematics and statistics to achieve better and better predictive models. Edward Leamer (1983), for example, has called for a strategy which would "take the 'con' out of Econometrics".[2] This would be achieved first by the explicit recognition that any model which 'predicted' a set of data from which it was derived well would be "fragile" or unrealistic. A fitted function can only be obtained by dispensing with variables and thus reducing exogenous variance. Second, the aim of analysis should be demonstrate the range of fragility which what he calls "families of models" may have, and to indicate the range of plausibility which they can be attributed. Statistical "goodness of fit" would rank very low as a criterion. In his view it is better to have large, weakly predictive but plausible models than tight, strongly predictive but implausible ones.

The worry over realism, then, is a worry which (some) economists share. What seems to be at issue here is the disjuncture between some characterisations of economic activities as they are presented in economic theories and the ordinary sense of economic transactions which anyone has as an actor in every day life. In the theories, activities are seen as patterned, formally analysable, perspicuous, systematically interconnected and elegantly structured. In our daily experience, things are always much more complicated, uncertain, surprising, in a word tangled, than that. That is to say, our feeling is that things rarely if ever work out in the neat, step by step, precise ways which the theories envisage. But why is Economics unrealistic, in this way? What is it that leads economic theories to become so disengaged from ordinary descriptions of economic life that translation appears impossibly difficult? The reason appears to have been the adoption of a particular set of specifying assumptions (those of rational choice) together with the employment of a particular explanatory technology (mathematisation).[3] This combination we will call **Cartesian Economics**.[4] Cartesian Economics is a distinctive (not to say idiosyncratic) way of thinking and explaining economic life. Our first task is to see what its basis is. We will then show how its use in analysisng entrepreneurial activities leads to the problems of realism to which we have just alluded.

Inductive axiomatics

From the turn of the century and what is called "the marginalist revolution" (of which we will say more a little later on), the conventional wisdom has been that Economics is an *a posteriori* discipline. Its theories are inductively validated through being tested against real economic events. Such validating procedures facillitated economic prediction and its testing. Now, while this is the conventional wisdom, such wisdom has by no means been universally acclaimed. Indeed, a small and vociferous band of dissenters under the leadership of Ludwig von Mises and, later, his students, constantly denied that economic theories were testable and hence that Economics was an empirical discipline at all (von Mises 1976, 1978). In their eyes, valid economic theories (note no-one denied these existed) were *a priori* true and not to be confirmed or verified by looking at "how things are". Empirical data, if it is to be had, is

irrelevant to the truth status of economic theories.

The difference between the conventional wisdom and von Mises could not be more thoroughgoing. Both argue that Economics is, or might be, a scientific discipline. Where they differ is over what they take that suggestion to mean. For most Economists, it is the natural sciences, and Physics in particular, which provide the model to be followed. For von Mises it is the mathematical sciences and Logic which are the most appropriate. Put at its simplest, the upshot of these different views was on the one hand the presumption that economic theories are putative empirical generalisations and on the other the presumption that they are deductive inferences premissed in axiomatic systems. Thus for the conventional wisdom, it was how the (economic) world was that determined the truth or otherwise of theory. For von Mises, it was the character of the reasoning - the use of the rules of deductive logic. No-one would expect the mathematician to collect up "data" on various geometrical forms -table tops, box files, pizzas, and cricket balls - to validate mathematical statements about the properties of rectangles, cuboids, circles and spheres. Such statements are about 'mathematical objects' not those we find all around us.

There are two observations we ought to introduce here which upset this neat contradistinction. Many Economists who subscribe to the conventional wisdom agree that Economics is mathematical in character and so also seem to be agreeing with von Mises. However, what they really seem to mean is that Economics deals with quantifiable phenomena, which is not the same thing at all.[5] Second, as we mentioned in the previous Chapter, recent work in the Philosophy of the Natural Sciences makes it far from certain that there is a single unambiguous relationship between theories there and how things are "in the real world". The interpenetration of theory, data, experimental methods, and measurement systems is now well attested to.[6]

The methodology of Cartesian Economics is one which we can call inductive axiomatics. Under this procedure, a phenomenon is defined *a priori* as a pure type with a number of delimited and definitive characteristics. Once the pure type is defined, reference to activities as they might ordinarily be described is secured through a step by step relaxation of the axiomatically defined parameters. To take the most familiar (and general) example, the unitary economic actor acting in the market place (i.e. a single buyer or seller, or a firm acting as a single unit) is defined as a utility maximising device. In positions of choice, an actor will always seek to achieve outcomes which maximise utility. Such maximising behaviour is programmed by a psychology consisting of:

(a) a predisposition to rank preferences in order of utility;

(b) the possession of perfect knowledge of the market situation;

and facillitated by a set of economic institutions which provide a measurement system which can be applied to all economic transactions, and by the perfect liquidity of economic resources. In such an environment, the psychology set out above allows the homunculus called 'the economic actor' to act in economically rational ways.[7]

It is important to notice that while we can recognise features of our ordinary economic activities in the things the economic actor does, the constitution of the homunculus and its empirical reference is not achieved by collecting up instances of economic activities,

comparing them, and distilling out the essence of economic life. Precisely the opposite strtategy is used. The essence of economic life (utility maximising) is defined *a priori* and then laid against activities which we ordinarily carry out. This procedure is accomplished by step by step relaxation of the stringency of the axioms. Constraints such as the possession of perfect knowledge, perfect liquidity of resources, and with them the impossibility of there being two prices in one market, or a monopoly of supply and demand etc., are set on one side. Second, alongside these purely economic considerations are introduced non-economic relevances. Economic calculations are held to be affected by moral, poilitical and social factors, as well as purely economic ones. If one wants to understand fully how the economic system works, one has to see economic activities as a condensate, a function, of economic, moral, political, psychological, cultural etc. etc. considerations. This additive, building block approach to the description of economic activities which we are calling inductive axiomatics.

It is this general mode of reasoning on which we wish to focus. At its heart is a particular view of the role of theory and theorising. Under this view, the role of theory is to provide the definitions of the pure type and the means by which better and better fit between the pure type and activities as they might ordinarily be decribed, may be achieved. What should be dispensed with and when is defined in the theory. In an economically given world, economic theory is complete. And without Economics we cannot see what economic activities would be like. Thus the tests, the validations, the measurement systems which determine the goodness of fit between 'ordinary conceptions' and 'the phenomenon as defined in the theory' are all rooted within the methodology of inductive axiomatics. Certainly we can imagine the possibility of a 'gestalt switch' enabling the development of different sorts of geometries and different sorts of economics. But what we cannot imagine is a description of geometrical objects which was independent of a geometry. Just as we cannot step outside a geometry to see what geometrical objects are really like, so we cannot step outside the framework of our economics to see what economic activities are really like.

For us, one way in which this exploration might begin is by taking up the whole notion of empirical reference. What does this term encapsulate and how is it envisaged that it might be achieved? These two questions lead immediately to another. What do we know about the activities of the 'real world' referents of the pure types? How do specific types of actors behave in actual economic situations and environments? When things are turned around in this way, what first becomes apparent is the slack in the relationship between the pure type and any of its empirical instantiations. An actual entrepreneur, for instance, corresponds hardly at all to the lineaments of the economically rational actor, even though, in the theory, entrepreneurs are prototypically motivated by pure economic considerations. Furthermore, this disparity is not merely of an ordinal character. True, the entrepreneur may be able to rank only a few of his preferences and on any specific occasion may be unable to determine what his best interests are in a transaction. But equally, the differences are dimensional. Profit may not be treated as a return for risk bearing but as a measure of relative efficiency or relative success. Administrative rationales may override entrepreneurial ones. The upshot of observations such as these could well be the transformation of the logic underpinning entrepreneurial activities in the theoretical accounts or a transformation of our conception of the entrepreneur.

The Marginalist Revolution

The essential components of Marginalism can be briefly summarised as follows:

1. By considering the contribution of marginal increments in value of utility provided by increments in the volume of consumption of a good, it was possible to represent utility as a simple curvilinear function.
2. The total of utility in any exchange system was defined as fixed. As with energy in the Physics of the time, utility could neither be created or destroyed (Mirowski 1984a).
3. Differential calculus provided a procedure whereby the optimal values for any set of transactions in the exchange system could be determined.

The achievement of the Marginalist Revolution was to transform the character of economic reasoning. It provided for the possibility of representing an essentially qualitative phenomenon, utility, as a distribution in two or more dimentions and thus enabled the possibility of its measurement. However, it is important to bring out the underpinnings of this transformation more clearly. To do this, we will turn to an exposition of the marginalist point of view by Frank Knight. In his extremely influential little book, *Risk, Uncertainty and Profit*, (Knight 1971) Knight brings out just what it is that the Marginalists were assuming about the character of economic choice and action in order to be able to specify the form which their analyses were take.

Knight begins with the same considerations which all marginalists departed from, namely the utilitarian theory of rational choice. Utility is defined as the propensity of a commodity to satisfy a need. As such it is a psychic variable and so not directly measurable. Indeed, the supposition that any commodity could be related to an absolute value of utility is quite specious. The utility of any good is only to be determined in comparison with other goods and in terms of a particular rate of consumption. We can quantify the variable only in ordinal terms. Thus, in comparing the consumption of lentil croquettes or beef stroganoff for dinner, we can only say that the one gives more utility than the other.

There are several major considerations which have to be brought into play before the above conception of utility can get any traction. First there is the randomness of wants. Needs and desires are not fixed and determined a priori. Neither can we speak of the possibility of satiation of utility and thus the end to consumption in toto. Our needs constitute what Knight calls a "flying goal". At the same time, the resources which we bring to the satisfaction of these needs are finite. Hence, *ex hypothesi* there will always be a problem of scarcity and with it the problem of the allocation of means to ends for the provision of utility. This is the nub of all economic theory: the allocation of scarce resources among competing demands. For the Marginalists, the most effective way of achieving this was through rational choice. Second, the consideration of utility is inextricably and essentially comparative. The utility which a good endows varies with availability of others. To these, Knight adds a genralisation about the nature of the economic agent. He proposes that in satisfying our wants, we seek to combine choices so that the distribution of utility among available goods is optimised. As

he puts it:

> In the utilization of limited resources in the competing fields of employment, which is the form of all rational activity in conduct, we tend to apportion our resources among the alternative uses that are open in such a way that equal amounts of resource yield equivalent returns in all the fields. (Knight 1971, p.65)

The stories which are provided to exemplify the generalisations and stipulations are all simplified cases. If one of us has the opportunity of growing vegetables in his garden, the rewards for so doing, in terms of net utility, must be equated with the utility foregone in terms of effort expended, opportunities lost, and so on, entailed in the action. Growing a few beans, cabbages and potatoes might bring pleasure, placate the wife and give one a place to hide from the family, but growing all the family's vegetables is likely to be a great deal of hard work. Pleasure may well give way to pain! The point of the story could well be captured in a curvilinear function such as the following.

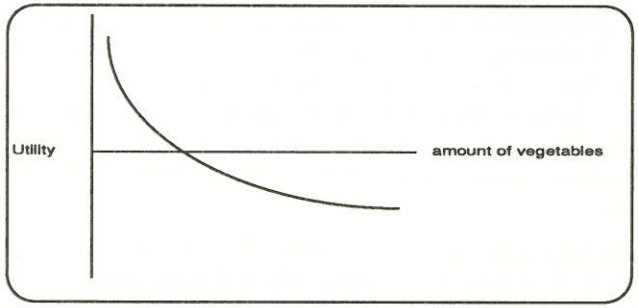

Figure 2.1 The marginal utility function

The reasoning for the curvilinear relationship between utility and numbers of vegetables grown is a direct consequence of the definition of marginal utility central to Marginalism. As the availability of a good increases, the utility each extra unit provides decreases.

Knight is well aware, though, that this representation must be used carefully. The function for the curve cannot be scaled and hence cannot be measured. It is "indefinitely measureable". "Still", he says, "There is a certain feeling of quantitative variability in the degree of preference, and such a curve is not utterly false to the facts of consciousness". (Knight 1971, p.69) He substantiates this in a long footnoted aside. In regard to the indefinite quantificational character of psychic variables, he suggests:

> This may be expressed in technical phrase by saying they are "ordinal" rather than "quantitative"; they are *variable*, but not *measurable*, can be *ranked* but not *added*. The nature of this attribute will lose its mystery is any simple sensation, as a sensation, is considered for a moment. It is easy to tell when one light is brighter than another. Impossible to tell how much brighter. The intensity of light is indeed "measured" by by science, but it is done by a method

> analogous in principle to the discussion of utility above. One light is moved to such a distance that it becomes equal in intensity to the standard, and the distance is measured. Obviously this does not involve the measurement of *sensation* at all. Similarly, a thermometer does not measure the *sensation* of heat or a balance that of weight. (Knight 1971, pp 69-70, fn. Emphasis in original.)

This line of argument for operational measurement is felt to be justified because it is how the natural sciences proceed and because the techniques or procedures are already available. The mathematization which such measurement allows will, therefore, be indirect. We will come back to this notion a little later.

The case we have been discussing is one in which an individual chooses between two valued means to attain a fixed end. Vegetable growing might be compared to reading novels as a way of maximising utility. The market place, the system of allocation for which the model has been derived, is of course much more complex than this. It is, for a start, an exchange system. Many commodities are made available by others, and we ourselves contribute numerous commodies to the general system of exchange. This immediately poses a problem of comparison of value. Robinson Crusoe might be able to value the range of goods which he produces in terms of the time which it took to obtain them, but the rest of us cannot. As a consequence, production or labour time may not be a good metric. Nonetheless, we could imagine a system of exchange based upon the metric of time. We could exchange low valued goods for high valued ones and thus gain surpluses of time which we might invest or trade for something else. We should not fail to notice, though, that there is an element of backward reasoning here. The existence of a price metric using money value is being used to generate an analogy, a metric using time. We can only make sense of the analogy, though, because we are fully familiar with the standard monetary system. The time pricing system is used as a bridge to the monetary system and yet is dependent upon it for its explication. In other words simply because the monetray system is, as a contingent matter of economic history, the system which defines the market place as we know it, this is the scaling system which must be used to measure the psychic variable of utility.

What is at issue, of course, is not how a particular system of valuing exchange arises in a market. That is a matter for Economic History and perhaps Social Psychology. Neither is it the operation of the system of exchanges to produce stability in prices, and hence equilibrium of demand and supply - even though that is the question to which Economics is drawn over and over again. Rather, what we have our eyes on is the way in which the connections between the foundational variable, utility, and the value or measurement system, money equivalance, are specified. Now, this might be achieved by an empirical demonstration that utility can be directly quantified, except that no-one appears to think this is really a viable project. What Knight provides is something entirely different.

> When the process (ie the system of exchange) is finished, the whole mass of commodities will have been reduced to a single homogeneous fund of exchange equivalence or value.....(W)e do not need to concern ourselves with the mode of expressing and handling this fund; in practice it would be inevitable that some sort of standard exchange system would be set apart; but it is immaterial for the

present purposes whether there is some kind of money or as many kinds as there are different commodities. (Knight 1971, p. 82)

The reason we need not bother with this question is because all we need to show is how "an objective and uniform price results from palpably subjective and variable individual preferences." (Knight 1971, p 83). But if that is what we need to do, then we can begin only by presuming the connection in the first place.

To secure the inevitability of a fixed price in the market place, Knight re-introduces the utility function in crucially modified way. Whereas before it was non-scalar and dependent, now it is scalar and independent. Whereas before, the volume of a commodity determined utility, now price (the measure of utility) determines volume. From this move, the establishment of a fixed price follows quite naturally. All that has to be done is to explicate the rationale of the supply or production function along the same lines as the demand function. The intersection of these two graphs represents the co-ordination of their utility functions. Thus price stability is the same as market satisfaction. Once this point has been reached, it is but a short step to move from graphical depictions of the functions to their algegraic forms, and from there to interlinked sets of definitions in systems of differential equations.

The whole tenor of this mode of reasoning is very eloquently brought out by Knights' own summary of his case.

> Cost is the value of the resources embodied in a thing, which is the value of some use for them; it may be an "economic" or a "non-economic (measureable or marketable or the opposite) use, but if there is not a competing attraction of some sort the "resources" will not be "resources" at all, just as if a thing is not wanted somewhere else it will not have an (exchange) value, and we should say not even utility if the word is properly defined. (Knight 1971, p. 91-2)

Having set Economics upon a scientific course, all that is required is the generation of ranges of measurements to be correlated and analysed. Once these are to hand it should be possible to determine how the functions for consumption, production, investment and so forth are interconnected and thus to predict what effects changes in the one will have upon the others. Such predictions will be equivalent to those to be found in the natural sciences. Hence, Economics will be well on its way to becoming a fully fledged, mathematically based science, no different in kind, though different in scope and complexity to physics, astronomy and the rest. The achievement of the marginalist revolution was the redirection of Economics along this course. To be sure, the mathematics of modern Economics is far more subtle and sophisticated nonethless, it is still premissed in the two principles we have enounciated: first, the Cartesian transformations of graphical representations to algebraic ones; and second the use of the price metric as the measure of utility and as the guide for rational choice. The price metric enabled measurement of the observable to stand for the non-observable variable, utility. This is what, in a moment, we will call 'indirect measurement'. Their combination is the cornerstone of Cartesian Economics as a mathematised science.

Risk taking and the entrepreneur

Frank Knight's explication of the basis for entrepreneurial profits is derived from the analytic

stance just outlined. As with all economic theorising of its type, Knight begins from a stipulated idealisation, that of the economy as a static equilibrium. In such an equilibrium, aggregate supply equilibrates with aggregate demand across all markets, prices are stable, there is perfect competition, no uncertainty. Knight argues his way to this idealisation from the prior set of stipulations concerning economic motivations and the rational character of actions just given. The point is, though, that the idealisation is an idealisation, explicitly and self-consciously so. In any economy at any point in time, there is no perfect competition and no-one has perfect knowledge. If these two stipulations are withdrawn from the idealisation, uncertainty is introduced and with it the possibility of short-run profit (essentially as implied in the neo-classical equilibrium model). The introduction of uncertainty creates the need for, and the possibility of profit provides the return to, a wholly new economic agent, the entrepreneur. The entrepreneur shoulders the uncertainty and takes the profits for so doing. That, in essence, is Knight's argument.

The steps by which this case is built up are as follows:

1. The idealisation of the equilibrium economy is similar to the frictionless plane or the perfectly efficient engine. It is a useful starting point. To ensure a better approximation between the "real world" and the idealisation, we need to introduce something, a variable, which will function like entropy does in mechanics. This is achieved in two ways. First, Knight introduces something equivalent to what to-day we would call "random noise" in the system. This random noise is the uncertainty of projection consequent upon human beings being human beings. It is not consequent upon the character of the economic organisation of activities. Knight quite specifically rules this out.

   > ...we assume a population static in numbers and composition and without the mania of change and advance which characterises modern life. Inventions and improvements in technology and organization are to be eliminated, leaving the general situation.....to remain stationary.....But we shall not assume that men are omniscient and immortal or perfectly rational or free from caprice as individuals. We shall neglect natural catastrophes, epidemics, wars etc., but shall take for granted the "usual" uncertainties of the weather and the like, along with the "normal" vicissitudes of mortal life, and uncertainties of human choice. (Knight 1971, p. 266)

2. With the introduction of uncertainty, it becomes impossible to predict precisely what a future market will do. If it were possible to do this, then the allocation of resources and factors to production and the supply of goods and services would become automatic. Without it, such allocation requires specialist skills or abilities. A group will come to the fore better fitted than others to take on this function since they have the skills. They will be the entrepreneurs. Most importantly, though, not only will this group have the skill to forecast market movements and to organize others so that production for these movements is achieved, they will have "confidence in their judgement and disposition to 'back it up' in action specialize (sic) in risk taking." (p. 270).

3. In this economic Just So story, uncertainty leads to the evolution of two groups

of agents, the entrepreneurs, controlling and organising economic activities, and the labourers. The former are prepared to make decisions, take risks, back their judgements. The latter are more timid and prepared to let others to offer the guarantees, take the risks and hence the profits. There are, then, two elements to the entrepreneur's return: a rent element that is the return for fulfilling the managerial function; and a profit element that is the return for risk taking, or rather for taking non-insurable risks. While it is possible to define these two elements separately, it is in fact impossible to segregate them in any individual case.

4. Now the question arises as to where the profits come from. Knight creates the possibility of residual income by setting imperfect competition against perfect competition. In imperfect competition, no individual knows exactly what the production costs are for every other individual. Innovations of various sorts might give an individual a competitive edge, a lower marginal cost. If, in the short run, demand is fixed, as are prices, then lower marginal costs mean "excess profits", for those with the competitive edge. Since suppliers of factors and labour do not know what the aggregate level of demand is, factor prices and wage rates might be lower than they could otherwise be. The entrepreneur takes the risk that this is so. Of course, he also takes the risk that such profits might be negative. As Knight says

> The only "risk" which leads to a profit is a unique uncertainty resulting from an exercise of ultimate responsibility which in its very nature cannot be insured nor capitalized nor salaried. Profit arises out of the inherent unpredictability of things, out of the sheer brute fact that the results of human activity cannot be anticipated and then only in so far as even a probability calculation in regard to them is impossible and meaningless. (Knight 1971, p. 310-11)

The plausibility of Knight's case turns upon two things. First there is the strategy of inductive axiomatics which he has employed, whereby features of the idealised model are gradually withdrawn, step by step, so that the model successively appears to conform more and more to "how things are in the real world". If, as is sometimes the case, we treat the idealised model as a hypothesised first approximation, then what this strategy involves is re-shaping (or "massaging") the hypothesis to eradicate its lack of fit and thus to provide some degree of empirical reference. This is a standard ploy elsewhere in Economics and Econometrics where it often proceeds under the title "curve fitting". However, this strategy does not make the model any the less idealised. Instead of the idealisation of certainty, we have introduced an idealisation of uncertainty and an idealisation of the effects which it has. There is no attempt to measure the fit between the idealisations of uncertainty now being introduced with the degree and types of uncertainty encountered in business life. In addition, no justification is offered for the characterisation of what it is that the model has to fit. What we all know about real worldly economic life enters the account as an unexplicated but vital ingredient. Second, and equally importantly, there is the element of indirect mathematisation which Knight employs. The conceptual possibility of risks to be taken in predicting market changes is transformed into short-run monopoly pricing by the simply device of seeing an identity between the risk taken and the possibility of a short-fall in costs. The value of the

profit (positive or negative) is the value of the risk. The existence of the profit indicates the existence of the risk. Although Knight does not use graphical representation is this section of his account, nonetheless, it would be quite easy to do in something like the following way.

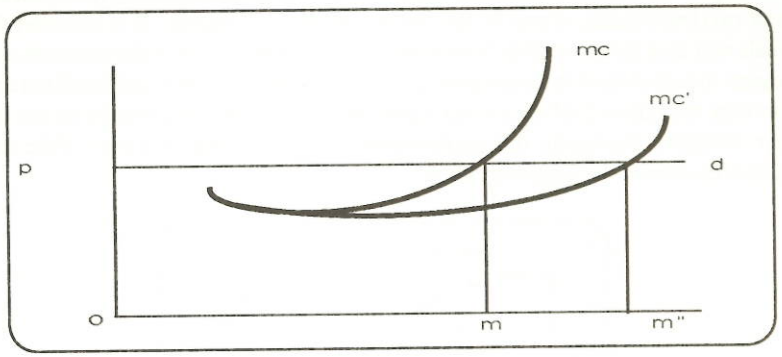

Figure 2.2 The generation of entrepreneurial profits

The short run inelasticity of demand is represented by the fixed price line (pd). The entrepreneuroperating with lower marginal costs represented by mc' makes profits represented by the quadrilateral om" x op. Other entrepreneurs make om x op profits. Since (om x op) < (om" x op), there are "excess" profits to be made. This possibility is only available because of the relative shapes of the marginal cost curves. Thus, it is possible to say that the existence of entrepreneurial profit and with it, the allocation of a function for that profit to be a return to, is a consequence of indirect mathematization. Or, to be more accurate, it is a direct result of stipulating a certain sort of mathematical form for cost curves. This kind of story would not have to be told, if the mode of representation was different. The importance of mode of representation for the analysis that is used is significant for Knight. In Casson's case, it is essential.

MARK CASSON AND MARKET MAKING

In Knight's account of entrepreneurial risk, the gamble which the entrepreneur takes is against the operation of Adam Smith's 'invisible hand' in the perfect market. Casson (1982) somewhat reconstructs this definition. The entrepreneur takes a position vis a vis others in the marketplace. What he is gambling over is the possibility that a market could be set up which would generate profit, what Casson calls "market making". In the equlibrium model, the allocation of productive resources is automatic, as is the articulation of supply with demand. Casson suggests this is a static conception (as did Knight). What the entrepreneur does is co-ordinate resources and make markets for them, thereby acting dynamically in the market place. Profit is the reward for foregoing the opportunity costs (what Casson calls the transaction costs) of market making. Thus profits are permanent features of the economic system, and are not ground away leaving the entrepreneur to receive simply a rent for administration.

For Casson, what the entrepreneur does is make a market by freeing resources, seizing

opportunities, capitalising on innovations. There are, therefore, elements of both arbitrage and brokerage in what he contributes. Finding markets and bargaining prices are the essence of entrepreneurial activities. To demonstrate the possibilities, Casson makes use of a standard representational device, the Edgeworth Box. In the box are sketched the indifference maps of two individuals, A and B, for two complementary goods. It is assumed that both indivuals will seek some weighted combination of the goods, hence the maps are convex to the origins. It is presumed in the simplified first approximation that A and B will not exchange - either they are ignorant of each other's preferences or existence, or they do not trust one another. Whatever the reason, third party intervention is required. The logic of the story falls out of the features of the Edgeworth box.

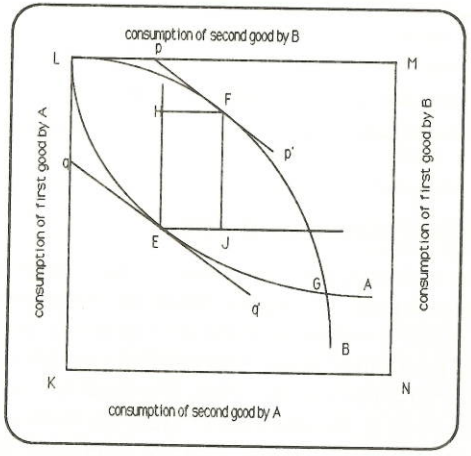

Figure 2.3. A transaction map (from Casson 1982, p. 61.)

The box KLMN is an indifference map for A and B with regard to the two goods. The sample indifference curves LA and LB show particular orders of combination. Since no exchange is at present taking place, the transaction point is L. The space LFGE contained by LA and LB contains the set of transaction possibilities. Within it, both individuals would be on higher indifference curves. The entrepreneur's indifference curve is represented by cc' drawn relative to an origin at E. The task of the entrepreneur is to move A and B to points where he maximises his advantage, satisfying their requirements and yet his own at the same time. To do this he has to move A along LA and B along LB until the maximal set of trading possibilities is made available. This occurs where the tangents qq' and pp' are parallel. If the entrepreneur can negotiate so that the bundle of exchanges E is offered to A and F offered to B, a surplus represented by EH and EJ is realised. Achieving this requires the entrepreneur to do two things, make a market for A and B to exchange in and bargain with them so that they arrive at the required points.

Of course, this simplified model will not do. It presumes that at least one agent, the entrepreneur has perfect knowledge. If we withdraw that assumption, what then? In Casson's view, all that happens is that the set of trading possibilities becomes more circumscribed, simply because the entrepreneur will seek to acquire some, albeit limited knowledge by means of offer proposals.

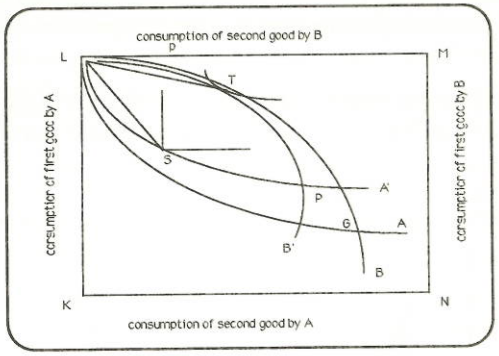

Figure 2.4 A transaction set (from Casson 1982, p. 64.)

The space LSPT represents the transactor's offer set bounded by the offer curves LA' and LB'. The entrepreneur's optimal mix is the pair of points T and S where the slope of the offer curves is the same as that of the indifference curve.

The proposal that the logic of the analysis is the logic of the Edgeworth Box is not a critical observation. Nothing is implied about the statements which Casson's makes about the putative behaviour of the idealised individuals under the conditions set out. Indeed, given the formal character of the analysis, there is no doubt that the propositions are true. However, the truth status of such propositions is not dependent upon anything actual entrepreneurs might actually do. Hence we do not know if the analysis tells us anything about how entrepreneurs operate in the situations in which they find themselves. The connection between the model and the economic activities of actual entrepreneurs remains formally undecidable and its empirical reference unknown. This is still the case even after Casson has modified and elaborated his model in the light of considerations he feels to be needed, such as the importance of bargaining, the addition of multiple actors in the system, the nature of risk taking and so on. This is because, in Cassons' theory, what is motivating the entrepreneur is the logic of choice, negotiation, maximisation and exchange defined within the parameters of the Edgeworth box. If the analysis tells us anything novel, and here we are not competent to judge, it can only be about the deduced properties of the functions defined within the box. That is to say, the discoveries will be mathematical, in some sense, and hence formal not empirical.

The same could be said of Cassons' account of the entrepreneurial market, that is the movement of individuals into and out of entrepreneurial activity . Here is a summary of the analysis.

> Although in the short-run the reward to the entrepreneur is a monopoly reward to information, in the long-run it is simply compensation for time and effort: namely, for the time and effort spent in identifying and making judgemental decisions. The equilibrium reward is greater, the greater the demand for entrepreneurs, and the smaller is their

> supply. The demand for entrepreneurs depends upon the pace of change in the economy. The faster change occurs, the greater will be the demand and the higher the reward to the entrepreneur. (Casson 1982, p. 337)

Here we have a relatively simple market explanation which differs from the standardised explanations in only a few (albeit crucial) ways. The explanation can be represented as follows.

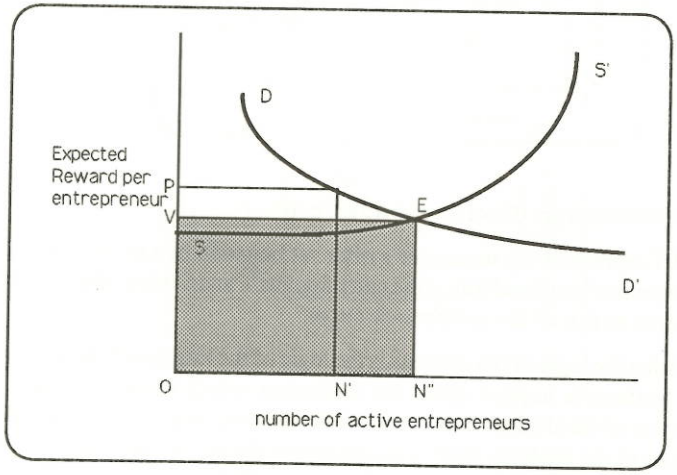

Figure 2.5 The market from entrepreneurial risk taking (from Casson 1982, p. 336.)

At N', the opporunity cost of entrepreneurship (ie the level of real wages) is exceeded by the reward to entrepreneurs (OV < OP), and so more are drawn in. In line with standard analysis, as supply increases, returns to supply (expected rewards to entrepreneurs in this case) fall. E represents the intersection of demand and supply and hence an equilibrium point, with N" entrepreneurs each accruing OV reward. The total reward to entrepreneurship is the space OVEN".

Casson rationalises the model's explanation of the changes in the market for entrepreneurship in the following way.

> .....the expected reward to entrepreneurship would exceed the opportunity cost, and additional entrepreneurs would be drawn in. They would be drawn in first from non-entrepreneurial employment and then from leisure. (Casson 1982, p. 337)

Unfortunately, neither we nor Casson have any real idea why entrepreneurs are drawn in and out of the market (c.f. Little 1985). Certainly, we have absolutely no idea whether this rationalist utilitarian reconstruction is a reasonable summary of what actually takes place. Further, we have no idea what the relevant preferences are. Does the entrepreneur see his

entrepreneurial activities as an alternative to leisure? to employed work? Or is this entrepreneurial project set against that one? We don't know. Nor can we know from the model. It specifies its choices in terms of commonsense utilities and simple dichotomies. The analysis "works" because of its mathematical basis and its superficial plausibility, i.e. the way the mathematical relationships are fitted into a story we can all recognise. Nothing in the analysis turns upon the way the system of economic activities in the world actually operates.

This might seem somewhat obvious and more than a little irrelevant, until we begin to think about its consequences. To begin with, we have to be able to distinguish generalisations which are premissed in the formal mathematical properties of the analytical machinery utilised from those which are not. Take, for example, the following:

> An individual's decision upon whether to become an entrepreneur will be based upon a comparison of the expected reward to entrepreneurship and the reward to the best alternative use of his time. (Casson 1982, p. 335)

Is this an empirical generalisation? It certainly looks like one. But what is its basis? Who (or what) are the individuals to which reference is made? Are they business men or what Alfred Schutz (1962) once called "homunculi", economic agents or actors constructed solely with the motivations and preferences of the theorist? The obvious answer is the latter.

> What it is important to get the public, and the social functionaries whoc control action, as well as the ""scientists", to understand if economic theory is ever to play any useful role in the world is that "of course it is unrealistic". (Knight 1940, p. 462)

In similar vein, Rothbard cites extensively from Schutz's writing.

> ...once we do turn our attention to the subjective meaning of a real individual, leaving the anonymous "anyone" behind, then of course it makes sense to speak of behaviour that is atypical - atypical in relation to standardized economic goals. To be sure, such behaviour is irrelevant from the point of view of economics, and it is in this sense that economic principles are, in Mises words, "not a statement of what usually happens, but of what necessarily must happen". (Schutz, 1967, p. 245, quoted in Rothbard 1973, p. 339)

What we have, then, is a stipulative motivational psychology, the psychology enshrined in the market as laid out in the above figures and, of course, distantly related to the utilitarian psychology of Marginalism. Short of knowing why individuals actually do become entrepreneurs, and short of understanding the grounds of their judgements with regard to market choices, we will not know how far the idealisations required for the indirect mathematisation of the logic of the Edgeworth box and the market mechanisms reflect actual features of the economic world. And unless we can do that, we will never know by how little or how much such models do approximate to and explain the logic underpinning entrepreneurial activities in daily life.

Kirzner's counter-position.

The sticking point with both Knight's and Casson's account of the entrepreneur is not, for us at any rate, that they are 'formal theories', for there is nothing wrong with formality in theory and description. The difficulty arises in trying to see how far they are only formal theories and hence what status to accord them. As we saw just a moment ago, this difficulty is most pointed in the discussion of the motivational psychology of entrepreneurship. In both of the cases we have discussed so far, this has been a market psychology of rooting out, searching for opportunities to maximise profits and a willingness to take on risks to do so. In the work of Israel Kirnzer, both of these properties are missing. Kirzner does not seek a formal theory; neither does he envisage the market process as equilibrating, with entrepreneurs chasing short-run risky profits. While they do seek profits, they are not risky ones. Entrepreneurs do not take risks at all. They back their hunches: but then they could do nothing else anyway.

There is, then, in Kirzner's work (Kirzner 1973, 1979, 1982, 1983)(Rothbard 1985) an attempt to break with Cartesianism and build a theory that is responsive to how entrepreneurs actually operate. To achieve this, Kirzner feels it is necessary to set aside the basic component of Cartesian Economics, namely the conception of the market as tending to equilibrium. With that conception goes the possibility of construing economic activities as a system of simultaneous equations. As a consequence, mathematical modelling becomes not so much a distraction, as emblematic of a wholesale misunderstanding of the nature of economic life; a sentiment which Kirzner seems to have taken over from von Mises.[8] In place of a fully determined set of functional relationships summarisable in systems of equations, Kirzner wants to put at the centre of his analysis the notion of human agency. This functions in precisely the same *a prioristic* fashion as the utilitarian model does. However, this time the theory is designed to be non-calculative, non-deterministic, with the emphasis on the importance of the non-rational in human affairs. This turns virtually everything upside down.

Kirzner begins with the foundations of the classical theory. The entrepreneur, like all economic actors, is defined as maximising profits in the context of given means and ends. What entrepreneurs do is achieve a finer and finer tuning between the means available to achieve the end of maximum profits. However, in Kirzner's view, such activity would not be entrepreneurial in the least. To use his example, the fisherman who improves his rod and line, or who learns to chart shoals by the activities of sea birds, or who stays at his line a little longer than others would not be an entrepreneur. The reward for the expenditure of the effort and time will have been anticipated. Hence, such activity is a factor of production. But entrepreneurship is not such a factor. Entrepreneurship arises simply because we do not know how to maximise utility, value, profits, or whatever. Any recipes we might have for doing so are bound to be erroneous. What the entrepreneur achieves is a complete re-valuation of ends and means, a gestalt switch, which would not be possible if we knew how to maximise value. Thus the entrepreneur is the one who decides that he would be better off building a boat and sewing nets rather than improving existing rod and line technology. Turning to new means achieves a re-valuation of the old means and the ends to which they were directed - in this instance extracting value from the activity of fishing. The entrepreneur is both open to opportunities of this sort and able to bring off the gestalt switch. What distinguishes the entrepreneur is the vision, the imagination, and the alertness for opportunities to use it. Such alertness is not a factor of production, about which choices can be made and opportunity costs

scaled. Neither is it reducible to an algorithm of calculability. The entrepreneur is driven on by his vision of the possibilities.

> Entrepreneurship is not, thus, something to be deliberately introduced into a potential production process. It is, instead, something primeordial to the very idea of a production process awaiting possible implementation. Entrepreneurial alertness is not an ingredient to be deployed in decision making; it is rather something in which the decision itself is embedded and without which it would be unthinkable. (Kirzner 1982, p. 22)

If alertness does not have an opportunity cost, it cannot be given a value. There is no sense to the notion of the entrepreneur choosing to follow a perceived opportunity or not on the basis of a calculation of comparative utility.

> One who possesses lumber and potential labour time may decide to build a boat with them. It is his hunch about the future that inspires this decision; his hunch is never an ingredient involved in the deliberations that control action. One does not decide to use or not to use ones hunches concerning the exploitation of a pure profit possibility; after all, to decide not to use a hunch would be to reveal that the hunch simply did not exist. One does not refrain from exploiting a truly perceived opportunity for pure gain. (Kirzner 1982, pp. 84-5)

If rationality is to be defined in terms of the employment of systems of calculation, then it is the non-rational (note not the irrational)which lies at the heart of the economic system. If the entrepreneur is the dynamic force in the system, such dynamism is not to be accounted for by recourse to rationality of means and ends. In Kirzner's view, by focussing on just such a rationality and then representing it within the ambit of mathematical models, economic theory has misconceived the character of the entrepreneur. By strapping the analysis of the entrepreneur to the procrustean bed of maximization theory, economics doomed any attempt to understand entrepreneurial activity to failure.[9] Again we come back to the question of how to mount investigations directed to the study of entrepreneurial activity *in media res*, in the midst of the market process, spotting, seizing and creating market opportunities, speculating against competitors, and backing hunches. What sort of studies might we undertake which were consistent to this vision? What sorts of materials might they provide? Here Kirzner offers little guidance. What is plain, though, is that however it is to be done, such studies will require a break with the individualising, narrowly rationalistic conception associated with Cartesian Economics.

Conclusion

In bringing this discussion to a conclusion, we want to turn back to the general grounds from which marginalists departed. One of these, it will be remembered, was the presumption that any proper scientific method should follow the model of mathematical Physics. Furthermore,

this presumption incorporated a view that theories in Physics, and by extension in any proper science, should a priori be mathematical in form. Physics was neccesarily mathematical. In adopting this view, Marginalism was in line with most of the Philosophy of Science of its time. However, studies of scientific development in the 16th and 17th centuries (Koyré 1968)[10] have suggested that 'the mathematisation of nature' was part of a metaphyical shift which took place among a small number of intellectuals and has since percolated through to the rest of Western European culture.

Probably the clearest summary statement of this novel outlook is Galileo's purported suggestion that "the book of Nature is written in geometrical characters". With this shift came the end of the Aristotelian tradition of classificatory science.

> If you claim for mathematics a superior status, if more than than that you attribute to it a real value and a commanding position in physics, you are a Platonist. If, on the contrary, you see in mathematics an abstract science which is therefore of lesser value than those - physics and metaphysics- which deal with real being; if, in particular, you pretend that physics needs no other basis than perception and must be built directly on perception, that mathematics has to content itself with the secondary and subsiduary role of mere auxiliary, you are an Aristotelian. (Koyré 1968, pp. 36-7)

For Koyré, Galileo was a Platonist. Whether Galileo would accept this description of his views is neither here nor there. What is certainly true is that if you define Platonism in the narrow way Koyré does then Galileo can be described as one, for he was certainly committed to the mathematization of science and, indeed, one could go as far as Husserl (1970) and say that work done following this conception is, in fact, Galilean Science. But, this does not mean that Galileo invented this conception. Because many of the forces and phenomena "discovered" by this new Physics could not be directly experienced, and hence directly measured, what Gurwitsch (1974) calls an "indirect mathematisation" was necessary. The development of systems of indirect mathematization owes more to Descartes and Huygens than to Galileo, even though we talk of the Galilean Revolution in science. As Gurwitsch points out

> Indirect mathematization of qualities requires that they be correlated with occurences which, because they are describable in spatio-temporal terms, are capable of direct mathematization. (Gurwitsch 1974, p. 54)

Descartes' analytic geometry, of course, provided the means by which spatio-temporal representations could be cast in mathematized forms. The net result of this accomodation of qualitative phenomena within the mathematizing schema was to extend the scope of Physics. It became the method by which all of Nature (and from there all phenomena) was to be described and explained. The principle became established that

> Nature as it really is (in contrast to its perceptual appearance) is a mathematical structure, perhaps a plurality of such structures, and it matters little whether the structures are comparitively simple, as in the early phases of modern science, or extremely complex and

> abstract, as in contemporary physics. (Gurwitsch 1974, p 55).

Indirect mathematization was achieved in Physics through the use of the Cartesian analytic geometry. Spatio-temporal geometric representations could be cast into algebraic form, abstracted, formalised and compared, and eventually integrated into general laws. Just the same strategy was on view earlier in the transformation of utility functions into the price metric. With such a transformation, the crucial step on the way to formulating an economic science was to have beeen achieved. However, the Galilean proposal about the essentially mathematical character of the laws of nature, and hence of natural phenomena, is not a discovery. It is a stipulation. As a stipulation about the essence of natural phenomena, it constitutes a metaphysics. Only if we lose sight of this feature would we feel free to be able to generalise the mathematisation of nature to the mathematization of social life. It might very well be the case that the institution of modern science is predicated on the application of mathematical procedures as a means of furnishing generalised, formal descriptions of its phenomena. But the success which it has had in providing such descriptions is not, of itself, a guarantee that the strategy is exportable to other arenas and phenomena. The possibility of mathematization might be a contingent and not necessary fact about the "natural" world. (Let us leave to one side just for the present the difficulty of disentangling mathematical innovations from the use to which those innovations have been put in the natural sciences.) If it is a contingent fact of nature that natural phenomena are relatively easily to describe in mathematical terms, then we can infer nothing at all about social phenomena from that fact. We need either an argument which shows that social phenomena and natural phenomena are essentially mathematical in form, or an argument which secures their direct isomorphism. In the absence of either argument, we are left to wonder why we should suppose that social and economic activities are even adequately rendered in mathematical terms. As we said earlier, the invocation of an assertion that they deal with quantified values is certainly insufficient, if only because the induction of quantified variables into economic and social theorising (as opposed to their common or garden use in daily economic and social life) was a consequence of indirect mathematization, not its precondition.

The question to be asked, therefore, is not whether it is possible to represent economic activities in mathematical terms but what is to be gained by so doing? Do we understand economic activities any better? Or do we find the mathematical character of the descriptions becoming "disconnected" from their putative empirical base? Are the mathematical discoveries, consequences and implications of more interest and value than what can be said about 'real world economic agents'? In a commentary upon general equilibrium analysis of the market system, Loasby (1972) offers the following observations.

> Having failed to establish that the results of the formal analysis apply to the system being studied, theorists are liable to recommend alternative methods of resource allocation. The specification of these alternative methods is of a standard that would be universally derided in a general equilibrium model. There is rarely much consideration of how they might work, or whether there is any reason to expect them to get anywhere near Pareto optimality, even assuming Pareto optimality is desirable. If the motivations and structure of a market system do not produce these results, one is entitled to ask what alternative structure might do so with the same motivations, and

> why such a structure might come into existence; yet such questions are barely considered. Which of the available imperfect methods of resource allocation is to be preferred is a question of great importance, which general equilibrium theorists make no attempt to answer. (Loasby 1976, pp. 48-9)

The apparent success of Galilean science cannot of itself be taken to be indicative of the generalisability of the principles on which it is based. What it counts as a success, its opponents do not. But this is a sociological and not a critical point. It is a social fact about our society that the Galilean conception of science, predicated as it is on the indirect mathematization of nature has come to dominate. It has permeated the modern consciousness. Part of this permeation has been the way in which the mathematizing principle has been taken over by other disciplines in their search for rigour and scientificity. One such annexing of the principle was the marginalist revolution in Economics. Qualitative phenomena, for instance utility, were represented in spatiotemporal terms, as curves on graphs, which were then subjected to mathematical transformations and indirect measurement. The increasing sophistication of these mathematical transformations and the degree of precision of the predictions made on their basis should not blind us to the metaphysical commitment which such a Cartesian Economics undertakes. It is a commitment which cannot be validated by appeals to "the data", just as Galileo's conception of the equivalence of motion and rest could not be validated by appealing to experimental results. What is measurable, what is discoverable, what is an economic phenomenon in the first place, are all defined within Cartesian Economics.

As we said at the start, misgivings about the consequences of considerations such as these, have begun to be voiced within the community of professional Economists. In all cases, it seems, the issue becomes most sharply focussed in determining what it means to call Economics an empirical discipline. From what we have said in this chapter, it would seem that, of itself, mathematization is not sufficient. This might mean one of two things. It might be that von Mises was right all along, and that Economics is a deductive science with no direct connection to the pursuit of economic activities in our daily lives. Its relationship to them is the same as that which Logic has with the grounds on which we frame and evaluate arguments in daily life. Both are "pure" sciences not "descriptive" ones. Or it might be, as the critics of instrumentalism seem to be saying, that we would to re-think the commitment to Cartesian Economics and its metaphysics. One way in which we might begin to determine which of these alternatives is the more likely, would be to see if it were possible to re-conceptualise economic activities outside the conventionalised framework. If different models provide ranges of alternative variables described in appropriate measurement or other systems, then the beginings of comparative evaluation might be possible. When we have the basis for viable comparison worked out and laid down, we will then be in a much better position to see if the disjuncture from which the argument over realism in Economics derives, must hold *a priori* or if it is a feature of a form of theorising at a particular moment in the history of our understanding of economic activities. In particular, we will be better able to determine if, in order to gain a firmer understanding of economic activities and processes, we have to relate them to the social environment or context in which they are found, and , of course, what that suggestion might mean. It is this task to which the rest of this book is seen as a very preliminary contribution.

Notes

*A version of some of the arguments underpinning the discussion in this Chapter are contained in Anderson, Hughes and Sharrock (1988).

[1] See for example, D. McCloskey (1985), P. Mirowski (1986b), P. Mini (1974), and P. Deisings (1971) summary of the debate. Unfortunately, P.J. O'Sullivan's (1987) discussion became available to us too late for an extensive examination of its claims to be included. Although O'Sullivan, like us, wishes to incorporate lines of thinking derived from phenomenology into economic analyses and descriptions, we are almost entirely at odds with him over what their import and implications might be.

[2] See also E. Leamer and H. Leonard (1983). Other papers of similar ilk are C. Sims (1980), and M. McAleer et al. (1983).

[3] In a series of papers, Mirowski has suggested that the Marginalists took over the mathematical apparatus used in the analysis of "energetics" by 19th century Physics without fully understanding it and applied it willy nilly to the phenomenon of the production function. P. Mirowski (1984a, 1984b, and 1986a)

[4] Our use of this epithet does not quite square with Mirowski's. In his (1987b) discussion, he refers to the combination of mathematical formalism and calculative rationality as a "Cartesian vice" (p. 70).

[5] This suggestion is made in W. Stanley Jeavons (1967).

[6] A review of these issues can be found in Ian Hacking (1983) and Churchland and Hooker (1985).

[7] This term is used by Alfred Schutz in his classic (1962b) paper.

[8] L. Von Mises (1963)(1976) (1978) . Recent reviews of "Austrianism" can be found in Reekie (1984) and O'Sullivan (1987) . What is surprising is the lack of attention given to the possibilites inherent in the Misean scheme. Some of these are picked out in Felix Kaufmann's (1933) brilliant paper . Others are briefly discussed in parts of Schutz's (1962a) Collected Papers . Reviews of these general issues are in Gunning (1986) and Prendergast (1986)

[9] See I. Kirzner (1983). The volume in which this piece appears is a useful sourcebook for a number of differing approaches to entrepreneurs.

[10] A. Koyré (1968). Some doubt has been cast on Koyré's interpretation of Galileo's notebooks and memorabilia by M. A. Finocchiaro (1980).