For Jenny and Martin.

Working For Profit: The Social Organisation of Calculation in an Entrepreneurial Firm

R. J. ANDERSON
Manchester Polytechnic
J. A. HUGHES
Lancaster University
W. W. SHARROCK
Manchester University

Avebury
Aldershot · Brookfield USA · Hong Kong · Singapore · Sydney

Published by
Avebury
Gower Publishing Company Limited
Gower House,
Croft Road,
Aldershot,
Hants GU11 3HR
England

Gower Publishing Company
Old Post Road
Brookfield
Vermont 05036
USA

Printed and Bound in Great Britain by
Athenaeum Press Ltd., Newcastle upon Tyne.

ISBN 0 566 07063 4

Contents

ACKNOWLEDGEMENTS

The Industry and Employment Committee of The Economic and Social Research Council provided funding under Grant No F 00232213 enabling one of us (Bob Anderson) to take sabbatical leave to carry out fieldwork at LTC.

Portions of various chapters were presented at the following: The Boston Institute for Ethnomethodology and Conversation Analysis, 1985; Anthropology Department Staff Seminar, University of Manchester, 1985; Department of Behaviour in Organisations Staff-Student Seminar, University of Lancaster1987; 'Erving Goffman, an interdisciplinary appreciation', York 1986; 'Action Analysis-Conversation Analysis', Maison de la Science de l'Homme, Paris 1987. We would like to thank the participants at these occasions for their many helpful comments.

A number of colleagues both in our Departments at Lancaster and Manchester and elsewhere have helped us with advice, comment and criticism. We would like to thank them all, particularly Steve Ackroyd, Peter Eglin, Gail Jefferson, Fred Kersten, John Lee, Pete Martin, Bryan Roberts and Rod Watson. Gail Jefferson provided most of the baseline transcripts from which the ones we have used were derived. We owe an especial debt to Dave Francis not only for taking on extra burdens during Bob Anderson's leave but also for a close reading of various drafts. His attentions improved the final version immeasurably.

Bob Anderson would like to thank his family for their forbearance and support throughout the period of fieldwork.

We would also like to thank the staff, customers, clients and competitors of LTC but especially Giles, Sandy and Colin for their openness in allowing us access to their daily buisiness lives, and for the help, advice and support which they gave us. Above all else, though, there is Lawrence, without whose willingness to have his very own social parasite around the place, nothing would have been possible.

We would like to thank the following:Select Press and the Editors of *The Journal for Social Behaviour and Personality* for permission to quote extensively from A. Etzioni, 'Socio-Economics: a proposal for a new interdisciplinary field.' *Journal of Social Behaviour and Personality*, vol 1, 1986, pp. 375-82. The University of Chicago Press for permission to quote extensively from F. Knight, *Risk Uncertainty and Profit*. Chicago. University of Chicago Press. 1971. Basil Blackwell Ltd. for permission to use the diagrams on pages 37, 38, 39 and 40 from M. Casson. *The Entrepreneur*. Martin Robertson. 1982.

Entrepreneur:

a. Director or manager of a public musical institution;

b. One who gets up entertainments;

c. A contractor acting as intermediary between capital and labour.

Oxford Shorter English Dictionary.

Preface

In this book, we try to present a coherent argument which moves at a number of different but connected levels. The first and most general concerns the relationships which might hold between different diciplines. In the context of proposals to synthesise the disparate disciplines of economics, sociology and psychology, we argue that analytic disciplines such as these are not only premissed in entirely different conceptions of, among other things, the character of human action and rationality, but also tend to pursue these conceptions in a single minded fashion. Before any proposed synthesis can be put into motion, it is necessary first to demonstrate how the differing conceptions, what we call "theoretical objects", are to be reconciled. Our discussion of these issues and the implications which they might have for investigations is set out in Part One.

The second level of our argument concerns the character of economic activities and the approaches we might adopt to them. Here entrepreneurialism is the central focus. In this discussion, we try to show that a pre-occupation with the function or place of entrepreneurialism in the system of economic activities and the conflation of that activity with a specific kind of economic rationality tends to lead away from the examination of what entrepreneurs actually do. Instead, there is a tendency to opt for global summaries of the outcomes of their activities and idealised versions of the nature of their decision making. This reservation about current disciplinary accounts of entrepreneurial (and by extension all economic) activities is, as we are at pains to point out, widely shared within the disciplines concerned.

The third level is a preliminary description of entrepreneurial work. We present this through the analysis of a case study of one entrepreneurial firm located in a single sector of the economy. In our descriptions of the wide variety of things which this entrepreneur undertakes during his working day and the formal and informal dimensions of his decision

making processes, we try to show what entrepreneurial activities involve at the level of day to day business life and what they mean for those engaged in them. This description is presented in Parts Two and Three.

The case study is, then, an exemplar and no more. Through it we wish to demonstrate the possibilities made available as a consequence of the arguments we set out at the other levels. We are very well aware that the accounts we offer are just first sketches of the field. They mark places for future development. We are also well aware that they cannot constitute a definitive statement of the essence of entrepreneurialism nor of the range of activities to be associated with the entrepreneurial function. We are not concerned here with global summaries and generalised explanations. Rather, what motivates our analysis is what Clifford Geertz (1975) once called "thick description"; that is, the detailed presentation of individual instances and an attempt to convey in any instance just "what the Devil the native (the entrepreneur in our case) thinks he is up to". It follows that what we have to say applies only to this case and can be generalised beyond it only through case by case comparison.

This last point is important, for a number of those with whom we have discussed this work have raised questions about generalisation from a single case and the representativeness of the firm which we studied. Can we be sure that what we say is not biased by the peculiarities of the case we are looking at?

To be fair, the worry over representativeness is understandable if somewhat misplaced. The home ground for it is, as we all know, sampling and in particular the procedures for ensuring randomised samples. Bias is not, then, as it sometimes appears in naive criticisms of sampling methods, a matter of conscious or unconscious distortion and selectivity, but the probability of extreme results. In other words, what this worry is about is the logical basis of measurement and generalisation by induction. This whole concern is crystallised in the set of strict requirements imposed for the use of measures of sample variates as estimations of those variates in the population at large. The use of non-probability samples simply invalidates any such stepwise progression. Precisely the same concern underpins the requirements of inductive testing of models of variables at the various levels of measurement. How confident can we be that our counts or measures reflect the distributions in the population at large and are not, therefore, chance aberrations?

It is important to notice where the worry over representativeness gets its grip. It is in the match between measures or counts in the sample and those of the population. If, and this is the crucial move, the aim is not to offer global or summary measures of some kind, which is to say if the aim is not to engage in inductive quantification and summation, then the representativeness issue simply does not arise. Putting the whole thing the other way round we can say, if it is the particularities of any case which are the object of enquiry rather than some possible generalised features, then whether any single case is a fair representation of the population is of no matter, providing, of course, that we are always careful not to attempt global summaries from the single case. None of this means, naturally, that we rule out the possibility of comparison. We can always compare; but what we compare is the detail of case with case.[1]

Background to the Investigation

The way in which our opportunity to study Leisure Time Catering (LTC) arose was characteristic of the investigation. One of us was having dinner with Lawrence Hunt, the Chairman of LTC. As is the nature of these occasions, a fair amount of gentle (and not so gentle) ribbing had been going on. Lawrence, as always, had taken the standard line on the social parasitism of academics in general and sociologists in particular. They held down part time jobs, had huge holidays and anyway were unproductive and lacked any sense of reality about the nature of world around them. The banter went back and forth as it does, until Lawrence eventually said, "You know you ought to come and see what it is like. Then you'd understand". "Fine", came the reply. "But since you keep saying we're so useless you would't employ us, how do we get the money?" To which Lawrence (fatefully) responded. "Since you are always going on about the research you do, why not research us?" Out of such half offers are research projects built. Eventually, the possibility was realised and the study was undertaken.

In the broadest sense, we took Lawrence at his word. We went into the business and studied both it and him. What this involved was the full time attachment to LTC of one researcher for six months, followed by visits and days spent in and around the Company catching up with developments (and the gossip. That must be the unwritten rule of research such as this. Never forget the gossip!), tracing lines of enquiry, pulling out files, and generally checking on details of one kind or another.

Coming to the Company in this way with the sponsership of Lawrence had several advantages. First of all, it acted as a guarantee of access. From the very start, Lawrence made it plain by what he did rather than what he said, that he was not at all concerned with what the researchers saw and heard. There were no secrets to hide. And so it was. There were in fact only one or two occasions when the researcher was asked to leave a meeting or turn the tape recorder off because particularly delicate or sensitive matters were about to be discussed. For the rest, there were no constraints at all.

The second advantage of Lawrence's sponsership was the number of research roles which were made available. For some analysts, finding a way to be 'invisible' in the field and hence make no material difference to the activities going on is felt to be a stumbling block to this kind of research.[2] The 'Hawthorne effect' is felt to be both real and debilitating. However, being "with Lawrence" provided a locally recognisable and clearly understandable thing to be doing. In that sense, the research role was fitted into and indeed found in the endogenous social structure. As a consequence, almost no notice at all was taken, once the initial curiosity had been satisfied. If ever an explanation was asked for, and that was infrequently, was that the researcher was "a friend of Lawrence's" and "studying the Company". This seemed quite enough for everyone including outsiders such as business colleagues, clients, and even competitors.

Gaining access through Lawrence had a third, vital advantage. The research was able to concentrate on that arena which as we have already suggested is largely unstudied by sociologists, namely the business life of business executives.[3] The researcher stayed with Lawrence as he moved from meeting to meeting, appontment to appointment. He was there when a number of projects were first introduced, when the Company structure was torn down

and re-constructed. He was there when one senior member of staff suddenly found himself having to account for his time and priorities; when Boardroom rows broke out, were de-fused and smoothed over. He was there when the news came in that a major contract had been lost and the post-mortem started. Being that close to the centre of the business brought to the fore just how fluid, dynamic and evolving the processes of business life are. No business can remain static and hope to survive. You have, as they say, to run just to stand still. What is offered in the chapters which follow is a still life composition, a snapshot, of what was heard and seen, stumbled on and seemed important. Since the fieldwork finished, things necessarily have moved on.

Finally, but probably most importantly, coming into LTC in this way allowed a remarkable degree of movement on both sides. Not only could we do what we liked, but LTC felt it could make demands of us. Thus, as someone unknown in a site, the researcher could act as a shill, objecting to standards of catering or service, just to see what the normal reaction to members of the public might be. He could be sent on 'private tours' of sites and be asked to summarise immediate impressions. He could act as chauffeur, bag carrier, occasional electrician, gofer, and fellow-ruminant on late night drives up and down the motorways. Furthermore, he was also able to offer 'academic services' such as advice on the setting up of a data base for sales, on streamlining the circulation of information. All of these offered involvement. They provided things to do and so access to opportunities and occasions which might never have presented themselves otherwise. It is from these opportunities that the distinctiveness of this research arises.

Leisure Time Catering

Leisure Time Catering (LTC)[4] is a nationally known 'fast food' Company with a turnover in excess of £10 million. It is based in Telford, and has more than 50 outlets or units all over Great Britain. The Company employs over 500 people. At present, the Company is organised along divisional lines, each division reflecting a major trading area. The divisions are CONCESSIONS, AIRPORTS AND HOTELS, and COUNTRY KITCHENS.

The CONCESSIONS Division encompasses outlets such as leisure centres, safari parks, theatres, swimming pools. It is the core of the Company and contributes the largest proportion of profitability. AIRPORTS is exactly what it says. The Company has the contracts to supply catering at a number of regional airports. In the past, the Company had a hotel in South Wales. It now has a part share in one in North Wales. COUNTRY KITCHENS is a chain of salad restaurants in a number of city centres. These restaurants specialise in food with a 'healthy' if not quite 'health food' image. These are the main 'High Street' catering outlets the Company has at present. In addition to these, Lawrence is currently developing a number of projects and associated schemes. They are discussed in Chapter 5. A non-catering enterprise, the franchising of a slimming system from Scandanavia has been abandoned.

The History of LTC

The history of LTC is the history of its founder and present owner, Lawrence Hunt. After what can only be described as a chequered career in a number of areas, Lawrence went into

partnership with a friend, providing concessionary catering in a number of sites, mostly rugby and football clubs in the North of England and in Scotland. Just prior to this, Lawrence had ventured into the High Street fast food business by franchising a 'WIMPY' in Telford in 1971. For a little while, Lawrence ran both the Wimpy and was involved in the concessions catering business. In the end, though, the partnership broke up. Lawrence retained the contracts for the English sites. To run these, LTC was set up in 1973. It was located in the office over the WIMPY.

Sandy Green joined Lawrence at this time, first as Manageress of the WIMPY, and later as his administrative assistant. With her help, Lawrence was able to broaden the scope of the concessions contracts which were won. The big break-through came when LTC gained the contract for the leisure centre at Galashiels, which was at that time the largest in Europe. By this time, the offices had moved out of the WIMPY and Lawrence had sold off his franchise. With the arrival of Giles Davies in 1975, LTC gained the expertise of someone with experience in the hotel and licensing trade. It then became feasible to hedge against possible reversals in public funding for leisure facilities by expanding into other areas such as airports.

The Company grew through the acquisition of more and more contracts. Eventually, it was necessary to move once again to the present site in a converted mill. By the end of the 1970's, the third division had been added. Lawrence had first seen the COUNTRY KITCHEN concept while he was investigating the possibility of opening a hotel. He immediately saw its possibilities as a franchise concern and agreed terms with the originator. The first restaurant opened in 1978 in Birmingham.

In summary form like this, the pattern appears to be consistent and steady growth. However, no one close to the Company will agree that things have been smooth or trouble free. The present success owes more to good luck, hard work and opportunism than it does to long term planning. It has never been easy and at times things have looked decisedly dicey. The Company's expansion has often been very rapid and at times it has almost over-reached itself. Financially, the business is on a much more even keel now due in large part to the association which has been formed with NDS, a company that was once based in a chain of driving schools, but is now much more diverse. As we shall see, entrepreneurial businesses such as LTC expand by a process of discontinuous accretion. Pieces are added and removed in line with short run needs, profitability and, ultimately, the enthusiasm of key members of the business. At times, it is not so much sound business sense and financial planning which keeps the Company together (although these are not neglibile in importance) but energy and speed of foot and car!

The Present structure

As we said, the divisional structure was in place by the early1980's and has not really been disturbed since. There has, though, been an almost continuous rotation of staff and responsibilities. The most important recent change was consequent upon the re-organisation of senior personel, and brought Tony Warwick to the Company to run the CONCESSIONS Division. As of to-day, the distribution of responsibilities is:

Lawrence Hunt Chairman: responsible for projects

Sandy Green	Country Kitchens and Administration
Giles Davies	Airports and Purchasing
Tony Warwick	Concessions and Contracts

The list, of course, tells us very little about the relationships between the people. Sandy and Lawrence, for instnce are the key organisational pairing. As we will explore in the Chapter 4, this axis affects almost every part of the business. Giles is somewhat more autonomous. He has his own team running AIRPORTS and tries to distance himself a little from what goes on elsewhere. Tony is so recent the relationship between him and the others has still to bed down.

At the next level of staff is a number of people who relate to the central members of the Company in very different ways. Colin Dunbar is by far the longest serving of these, having been part of the original Galashiels set up. He is a large, bluff, affable and outgoing character. He fronts the sales effort and negotiates the concessions contracts. Colin used to work directly for Lawrence and still feels as if he has a 'special relationship' with him and the others. He now reports to Tony Warwick. This was part of a deliberate attempt to cut down on the demands made upon Lawrence and so make more effective use of his time. Whether it will work remains to be seen. Colin has a small staff of his own and spends much of his time 'on the road', seeing potential and existing clients.

Ros Howarth and Mike Santo are Regional Directors for CONCESSIONS. They too report directly to Tony. They supervise the sites in their areas, the country being divided roughly between them, checking on conditions, and problems, giving support, talking to customers and clients and keeping on the look out for new opportunities. Greg Bailey performs much the same role for AIRPORTS although that is on a national scale. Roger Mulaney is Sandy's major assistant in COUNTRY KITCHENS. He is very much junior to all the others both in experience and in time with the Company. He is still 'learning the ropes' and hence give much less supervisory freedom.

Beyond these are the administrative staff at Telford and the operational staffs in the field. Some of these will feature in the discussions below and so will be introduced then. However, as we indicated, the bulk of our concern will be with the doings of Lawrence, Sandy and Giles.

The Study

The study which we present here is organised around a number of themes all of which express the endemically social character of economic activities. The themes selected for discussion, personal relationships, the organisation of tasks, contractual negotiations, accounting procedures, and so on, are the ones which appear to be significant for the people working in LTC. That is, we have not tried to structure the experience of working in and for LTC other than through the topics of most concern for those people actually involved in it. Day by day, it is the need to keep the books, make the deals, develop the projects, work with one another that these people seem to orient to. This is not, then, an attempt to reconstruct their experience but to construct it.

It is important to remember this simply because the kind of analysis which we will engage

in is not of the usual sociological type. We will focus most often upon the co-ordination and integration of activities rather than on the distribution and filling of functions. Our question is not "Who does it?" but "How is it done?". Again, though, we should not make too much of this in the abstract, for the two cannot really be held as hard and fast oppositions. Most of all, though, what we are interested in is the relationship between 'commonsense business practice' and the grounds of that practice in the businessman's understanding of how thing work and why. This is, therefore, a study in business praxis . The development of an appropriate praxiology is the next step.[5]

In saying this, though, we would not wish to be read as making large claims. We are only too well aware that the materials we discuss have not been subject to extensive and detailed analytic description. We do not offer them as anything more than a preliminary excursus in the area. Our defence of this premature presentation would be three fold. First of all, as we have been told time and time again by colleagues, our materials are of interest in themselves. They offer a fascinating insight into the daily routines of business life in the modern world. As such, we have tried to make the materials and our analyses of them more accessible to those not primarily concerned with the technical detail of the sociological issues which they raise for us and our colleagues. We hope others find in them resources for their own work unrelated to our particular project. It is with this hope in mind that we have designed this volume deliberately to attend to what might be termed the transactions between sociology and other social sciences over the analysis of entrepreneurial activities. It also explains why we have sought to use a form of the ethnographic mode of presentation.

Second, the analyses which we offer are not intended to be read as anything other than working endeavours. We see them as marking out some of the areas and possible approaches to them thrown up by the data which we have collected. We very much have a sense of their character as an initial exploration to determine likely fertililty. The advantage, as we see it, in this is that it may allow others to see how a particular mode of analysis grounds itself and develops in and through the addressing of its materials. In our experience, it is almost impossible for the technically unititiated to locate themselves and their interests vis a vis specialist work in our field.[6] The analytic attitude evinced by the more elaborated and sophisticated analyses, is not so much strange as incomprehensible to many. This is not surprising since it involves a complete re-appraisal of the pre-suppositions of most contemporary sociological analysis. Our hope is that in showing with familiar materials how that re-appraisal is begun, others will come to appreciate some of the purpose behind the line of enquiry exemplified in other, fully fledged analyses.

Third, and this is closely related to the previous one, we see sociology as a collegial discipline; a discipline of friends, colleagues and collaborators. In publishing some of our materials and these very early analyses, we wish to offer others the opportunity to join with us in exploring their possibilities. While we have collected these tapes and transcripts, fieldnotes and anecdotes, we feel no sense of proprietorship over them. They will become, we hope, a resource for others as well as ourselves to explore and exploit.

NOTES

[1] Naturally, we could provide a much longer defence of the approach we have adopted

and offer much more detailed argument on its behalf. These arguments would concern the supposed contrasts between explanation and understanding, the general and the particular, description and explanation, and so on. However, taking them up at this point would lead us far from the topics we are presently addressing.

[2] The literature here is enormous. Both sides are considered and a not quite impartial view taken in J. Kirk and M. Miller (1986). A slightly slightly different argument for its advantages cf Barry Turner (1987) . Kumar offers a vigorous defence of its use in economic contexts is his (1986) paper. Some discussion of relevant issues can also be found in Bryman (1988).

[3] Office life, on the other hand has been exhaustively examined sometimes from positions not too dissimilar from the one adopted here (eg Cicourel 1987, Freeman 1987, Mehan 1987 and Zimmerman 1974). So too have the professional middle classes including managers and executives (Dingwall and Lewis 1983, Dingwall and Strong 1985, Mills 1956 , Dalton 1959). No one, though, has studied the daily business life of the company executive in quite the way which was made possible here. Accounts have tended to be either biographical and hagiographic (Druker 1985, Kantor 1983, Gilder 1986) or have concentrated on the interface of business expertise and socio-economic background (Scase and Goffee 1982, Goffee and Scase 1985, Cromie and Hayes 1988). The work of Mumford and her colleagues while ostensibly concerned with work routines as we are is very different in its orientations. See Mumford (1979, 1983).

[4] Here we follow the standard practice of using pseudonyms, although in the interest of ethnographic realism we have retained actual placenames. None of the places mentioned was associated with LTC during the period of fieldwork. No doubt the particularly industrious or curious would have little difficulty in uncovering the real identities of LTC, Lawrence Hunt and the others. What they would have gained thereby we are not sure.

[5] Taking this step is no easy matter, as can be appreciated by a close reading of Lynch (1982, 1985) and Garfinkel's introduction to his recent collection (1986).

[6] One possible measure of this is the number of introductions to this sub-field of sociology (Sharrock and Anderson 1986, Heritage 1984). Another is the degree of technical mastery required before one can even begin to penetrate studies such as Livingston (1987).

PART ONE
THE ECONOMIC AND THE SOCIAL

Introduction

In this Part, we shall be arguing for a different approach to the study of economic activities. We shall suggest that if one begins with neither the problem of how to conceive what is essential to economic activities nor how to explain their distinct form, but rather with the problem of providing a description of particular sets of economic activites as they are carried out in business life, then the prevailing schemas are less than adequate. Indeed, it will be our contention that despite what may be said about their purposes, for one reason or another they resist being brought to bear upon empirically garnered materials. This being so, a novel and distinctive approach such as the one we suggest, is called for. We must be quite clear what we are saying though. We are not suggesting that our proposals and the analyses which are based upon them stand as a refutation of all existing accounts of economic and social life and the relationships between them. Neither are we claiming that the investigations we detail provide the basis of a fully fledged analytic paradigm or disciplinary matrix which can be slotted in to replace those presently in use. What we offer are explorations of a possible line of investigation and analysis together with some documentation of the distinctive slant which it provides and some of the advantages which it confers.

From what we have just said, it would seem obvious that we feel a new way of approaching economic activities is required simply because the existing ones do seem to resist being given what we might call empirical reference, especially as that is usually provided within sociology . That is to say, they do not seem designed to encourage their use as the organising framework for descriptions of actual economic practices in business life (and, indeed, elsewhere). Two obvious questions arise at this point. The first is "Why do we think they do that?". Isn't there any way in which conventional accounts of economic activities could be given an empirical basis? The second is "Why do that anyway?". What is the purpose/value/

point of bringing actual business practice under the rubric of theory? Answers to this second question can be pitched at a number of different levels and take a plethora of different forms. A vast number of economic and sociological treatises have been written outlining and defending them. We will offer two related and very simple answers. The first is, straightforwardly, that this is what these theories claim about themselves . Even the most analytically pure exposition has at least part of its purpose in what it might tell us about actual economic behaviour. The connection may be very indirect and, even, obscure, but some sort of link will be envisaged. The second draws on the place and distictive purpose of the case study in the social sciences. As explanatory disciplines, particularly causally explanatory disciplines, the social sciences are somewhat underdeveloped. In large measure, we feel, this is the consequence of a lack of attention being paid to descriptions of naturally occuring social activities. Whatever the reasons why this might be so (and they are numerous and inter-connected), it remains our view that progress towards the goal of explanation can only be achieved (if at all) through the utilisation of theoretical terms and concepts in the description of cases.

This much being said, what of the first question? Why do the prevailing theories resist empirical reference? Here our answers are more complicated and, perhaps, more contentious. What we are concerned with are ways of characterising what are usually thought of as the economic and social aspects of activities. Thus, to take a familiar example, the purchase of a piece of kitchen equipment might be seen as having economic and social aspects (or factors). These are, of course, conglomerate terms which could be further analysed out. Thus there is the calculative rationality of weighing utility against price. There is the perception of needs or desires and the availability of goods in the market place to satisfy those needs and desires. Then there are the economic mechanisms by which the transaction is facilitated. Alongside all of these are placed social and peer group pressures for conformity; class and socio-economic backgrounds as determinants of expectations; and, of course, the relationship between the economy as a social structure and other social structures such as the state, the family, law and so forth. As the various strands in this skein of relationships are teased apart, the question of the relationship between economic and social aspects of particular activities turns into a question of relating social and economic explanations of those activities. That is, it turns into a question of relating Economics and the other Social Sciences. Each of the disciplines is associated with, or defined in terms of a cluster of relevant aspects and their explanation.

When considered in this light, the proposal which often follows has a sort of natural inevitability about it. It seems intuitively obvious that what is required is a rapprochement between the disciplines, a synthesis which will enable both a full, complete or more extensive description of the activities in question and hence will encourage a closer empirical tie between theory and real world activities. It will be our contention that at least in the case of the organisation of economic activities, this intuition is badly misleading. Further, we will claim that the degree to which it is misleading can be gauged by the numerous attempts at synthesis and integration which have been already offered. We will argue that it is only by disregarding what we might term the internal logic of the disciplines, their modes of reasoning, that any attempt at integration or synthesis can be achieved. The result is unified but emasculated theories.

Because we can see no way just now to achieve the unification within a viable synthesis of

theories extracted from Economics, Sociology, Psychology, and elsewhere, we have been drawn to the conclusion that if we want to describe real world economic and business activities, we have to begin in some other way. The possibilities are, of course, endless and each provides its own set of interests and orientations; things to look at and things to disregard. The choice between these problematic possibilities cannot be simply arbitary, a matter of whim or fancy. Instead, it must be governed by some methodological or investigative principles. Without such principles no claim to systematicity or rigour could ever be made. But it also follows that no such choice could ever be justified as logically neccessary or foundational or more realistic. They are each equivalent. The principle which we have chosen is visibility in the materials we have gathered. That is, we will begin with what those who engage in the activities are concerned with: what matters to them and what they orient to, as far as that can be determined from what we have learned as fieldworkers about the setting and what can be seen in the materials we present. And, in our view, this means accepting that while actors engaged in business life are concerned with economic (i.e. calculative) rationality they are also concerned with what we will call organisational relevances. The interplay and interpenetration of calculation and organisation provides the place from which we wish to start. The analyses presented in the second part of this book are the exploration of what is made available when investigation is formulated with this as the departure point.

1. Socio-economics: A self defeating project

We fully recognise that the sketch we have given in the Introduction to this part is less than satisfactory.* We have made a number of claims about the self defeating character of one prominent line of work in the social and economic sciences. In this chapter and the two which follow, we will attempt to detail the thinking which lays behind these claims. In a nutshell, it is that analytically pure disciplines such as Sociology, Psychology, and above all Economics tend to be extremist.[1] They take ideas and see how far they can be purified and extended. Hence the methodology of these disciplines militates against mutual incorporation and mutual adjustment. The upshot of this is that putative syntheses turn out to be even less satisfactory than the original, analytically pure disciplines.

To demonstrate how this comes about, we will take a single example, that of the recently developed Socio-Economics associated with the work of Amitai Etzioni.[2] Having set out Socio-Economics' goals and the ways in which it is envisaged they may be achieved, we will delineate why we find the proposal to be misguided. In brief, our claim will be that without an explicit theory of the relationship between disciplinary phenomena or objects, all we have to fall back on is a constancy hypothesis. Under this hypothesis, when Economics, Sociology and Psychology describe and analyse instances of economic choice, for instance, they are describing "the same" phenomenon. We will find the constancy hypothesis untenable and hence its implicit use as the basis for synthesis unsupportable. The demonstration of the untenability of the constancy hypothesis is contained in our discussions of the modes of reasoning underpinning what we will term **Cartesian Economics** and its application to entrepreneurial activities which is contained in the next chapter.

Socio-Economics: the new synthesis

In recent years, Amitai Etzioni has been attempting to build an "interstitial discipline" which he calls Socio-Economics. The aim of Socio-Economics is to provide the basis for the mutual compatability and elaboration of findings in a number of contiguous disciplines. In Etzioni's view, Socio-Economics

> .....may require combining contributions from psychology, sociology and political science, hence the reference to socio-economics. While psychology may provide much of what is necessary, other social sciences may need to be drawn upon in the study of macro elements such as the relation between groups (as distinct from those between individuals and groups) and that of the polity. These in turn may need to be combined with economic concepts which recognize the role of supply and demand and prices without the neo-classical economic assumptions. (Etzioni 1986b, p. 479)

To begin with, then, Socio-Economics is a discipline which combines elements, approaches and findings from already existing ones. However, its very existence and outlook will, of itself, generate novel areas for investigation and require the interconnection of newly developed theories. Etzioni has himself enumerated several: consumer behaviour and the theory of rational choice; utility theory; perfect and imperfect competition; organisational decision making; and of course the area of our primary concern entrepreneurship. [3] In each case, it is imagined that the introduction of the broader perspective and a wider range of investigative methods will have much the same consequences.

> Introducing greater reliance on induction, drawing upon experiments, observations and attitude surveys, may turn out to be the single most important corrective socio-economics will provide to the highly deductive approach of neo-classical economic behavior. Those who do not regularly follow neo-classical economics may find it hard to believe how far it is removed from empirical testing, at least in the way that this term is understood in many other sciences, and all other social sciences....(Etzioni 1986b, p.480)

As an example of the kind of broadening effect which the new discipline provides, Etzioni discusses some recent work in the "economics of crime".

> Neo-classical economists in recent years have attempted to show that crime is a rational choice behavior by ordinary individuals rather than the result of distorted personalities, poor socialization, deviant peer cultures and such, the lines of interpretation many psychologists and sociologists follow. Cost (punishment) and benefits (size of the loot) were shown to correlate with the levels of crime, even crimes of passion such as rape and murder. However, most recent work shows that such "economic" factors account for only about one third of the

> variance, while factors traditionally studied by psychologists and sociologists, such as the capacity to defer gratification and moral socialization, also account for about a third of the variance.....hence the value of systematically studying both kinds of factors in one framework. (Etzioni 1986b, pp. 479-80)

What Socio-economics provides, then, is not so much a picture which "corresponds to reality" but an account which incorporates and integrates our developing knowledge, thereby contributing towards the theoretical unification of the social sciences and hence the possibility of a better fit between "the real world" and our theories of it. At the end of an outline of the methodological basis of his new discipline, a basis which he deliberately labels "Kantian", Etzioni offers arguments which are these days virtually conventional in Sociology and most of the other social sciences.

> In short, it is not enough for neoclassical economics to give up its imperialism, its attempts to understand non-economic behaviour in rationalist, egoist terms. The role of moral commitments and the factors which shape them must be taken into account in studying economic behavior, including subjects such as saving, incentives to work, behavior of markets and productivity. An integrated paradigm of social factors (among which only moral ones were explored here) and economic factors is to be tested is to be tested by the same criteria by which neoclassical economics is judged: the ability to predict and explain, parsimony, and the ethical implications of the paradigms for those who view the world through its framework. (Etzioni 1987b, pp. 37-48)

What is clearly the major pay-off of the synthesis is the way in which it will allow the empirical grounding of analyses in what we have called "real world economic and business life".

Obviously we do not have the space, nor is it necessary to explore all of the possible topics which Etzioni proposes are ripe of the socio-economic approach. We will focus only his account of entrepreneurship. Here what he suggests is that the "facts" with regard to the psychological predispositions of entrepreneurs are relatively well known and the economic rationale for their activities is more or less clear. What is not so easily pinned down, though, are the reasons for the relative success or failure of particular lines of entrepreneurial activity. Here, he says, we have to turn away from the individualistic conception of the entrepreneur and look towards a **social** conception of it.

> It is commonplace to observe that entrepreneurship is (a) often not an individual undertaking but a team task (although much of the literature focusses on entrepreneurs as individuals), and (b) that much entrepreneurship work is not an ad hoc improvised activity (as implied in many of the personal accounts by and of entrepreneurs) but a routinized activity in societies that legitimate entrepreneurship, especially those that rank it as a highly approved activity. (Etzioni 1987a, p.187).

Rather than seeing entrepreneurs as sort of "randomised" responses to changes in the economic environment, their success or failure can be patterned along lines of legitimation.

> Entrepreneurship is studied here as the force which promotes social reality testing. Societal patterns (institutions, organizations, rules, etc.) tend to ossify, lagging ever more behind constantly changing environments. Entrepreneurs, by promoting new patterns, help bring society and its component units in touch with reality. Unlike many discussions which focus on entrepreneurs as individuals, exploring their traits or personalities or decision-making styles, the focus here is on the contribution of entrepreneurship to the society at large and to the economy embedded within it. (Etzioni 1987a, p.175)

This conception builds upon the analysis of the function of entrepreneurial behaviour offered by Schumpeter and others[4] The way in which entrepreneurs are able to perform this adaptive function for society is through their exploration and exploitation of profit opportunities created by the differential rates of socio-economic change to be found within the system. However, the permanent existence of a dis-articulation between patterns of activities and their consequences and the sets of normative and value orientations by which such activities are guided, make the possibility of entrepreneurship more feasible in some areas rather than others. The dynamism of social, economic, politico-legal, psychological and other forms of cultural change is such that opportunities for the pursuit of short-run profit are not randomly but socially distributed. They occur in some places rather than others, and for clear cut reasons. Forms of entrepreneurship are tied to forms of social structure. As "reality testers" entrepreneurs enable the adaptation of socio-economic patterns of activity to changes in the environment in which they are found. This, in essence, is the Schumpeter line (Schumpeter 1961). What is different is that Etzioni proposes that in some societies, entrepreneurship has been routinised and legitimated. He connects up, then, economic relationships and patterns of moral, ethical and legal ones.

This interconnection of patterns of economic activity with patterns of political, ethical and legal justification enables Etzioni to suggest that to understand **successful** entrepreneurship, we have to look to the capacity of such entrepreneurs to mobilise political resources and utilise political rhetoric. This is because those with vested economic interests in the existing but relatively inefficient economic structure (as seen from the point of view of adaptation to reality) will themselves be able to deploy such resources in defence of their positions.

> Entrepreneurs, in effect, join the political challenge, by providing new, knowledge-based reasons to discard the old patterns, and above all, point to what new patterns to switch.....now, founders of new Hi Tech industries seek political allies in supporting young "yuppie" candidates, to overcome the resistance of politically entrenched old industries (such as steel and auto) to modify tax laws to allow for more R&D and to keep making international trade freer. (Etzioni 1987b, p. 181)

In being able to change the legitimated structures of values and norms in this way, entrepreneurs are able to effect the distribution of revealed preference, the allocations of

resources by the polity and many of the other resources and constraints in terms of which they operate. But these transformations, once they have occured, tend to be subjected to the law of diminishing marginal effects. That is to say, the legitimation wears out. As a consequence, given permanent change, existing legitimations exert a weaker and weaker control over patterns of activities. Etzioni proposes that in socio-economic systems such as those of the advanced industrialised nations, such atrophy of legitimation has brought about the routinisation of entrepreneurship. This in turn means that a wholly different conception of entrepreneurship associated not with the individualised economic actor but with the entrepreneurial team has to be developed. In the work which we will discuss in later chapters, we will have a lot to say about the routinisation of entrepreneurship and the operations of an entrepreneurial team.

The problem which Etzioni is addressing is, as he says, the Kantian one of the analytic constitution of reality. Each separate frame of reference offers a partial depiction. The task is to find some synthesising frame which integrates and co-ordinates all of them. This is an exercise in theoretical reconstruction. What the surface plausibility of this reconstructionist approach fails to bring out are two underlying premisses. The first concerns the requirement of analytic separation. To put is somewhat differently, Socio-economics is offered as a solution to a problem generated by a particular methodological strategy. In this strategy, what we encounter and experience of our own and others' activities is, under the aegis of scientific analysis, decomposed along lines defined by the rubrics of the different analytic disciplines. Having taken our experience apart according to its economic, social, psychological or whatever "dimensions", we face the task of re-constituting the experience, of synthesising the frames of reference. And, of course, this has proved impossible, at least if we use the recognisability of those activities to those who engage in them as a criterion for success. Analytic descriptions precisely because they are so to speak "category-driven", remain inescapably vague, abstruse and 'theoretical'. Furthermore, even by its own lights the strategy has proved to be unsuccessful. The aim in decomposing activities was to isolate the essentially social, economic and psychological (to name but three). What has been found is that the closer you look, the more there is to see. It is rather like an incompetent watch repairer taking a watch apart to see which parts kept the time. Having found that it was the watch which kept the time, or rather having found that no particular parts alone kept the time, we have to put the watch back together again. And we don't seem to be able to.

The problem with Etzioni's synthesis is that it is constructed around a contrast between explanations which apply at the level of 'micro' behaviour (that of individuals) and those which apply at the "macro" level. He proposes that with an appropriate theorising of the "macro behavioral", a unification will be possible. The individual economic actor (for instance, the entrepreneur) will be located in a social structure. Unless and until we can provide this, the ineradicably individualistic character of entrepreneurial genius will always stymie us. But this is just another instance of the decomposition strategy. We tear experience into 'the individual" and "the social" components and then try to put them back together again.

The second premiss relates to the character of the experience which is being decomposed. Here the presumption is that we know what, for example, economic activities consist in as practical matters for those that engage in them. That is to say, we know what the reality of economic life is to which we bring the decompositional method. But, this is a very contentious presumption. To be sure we know what, again to use the example of entrepre-

neuship, entrepreneurial activities are like from the point of view of rational actor theory, preference theory, decision theory, organisational theory, sociological theory, and now socio-economic theory. But do we know what they look like from the point of view of those who are pragmatically engaged in them? That is to say, do we know what they look like from within? This is a question to which we will return at a later stage in this discussion.

The Constancy Hypothesis: an unrecognised problem

If all that Etzioni was saying was that economic activities are located within social life and have to be understood in relation to the rest of the activities which we engage in, then this would be neither remarkable nor novel. It was precisely the implications which were supposed to follow from agreement on just this view which led Talcott Parsons and Frank Knight to debate the relationships between Economics and Sociology almost half a century ago (Parsons 1940, Knight 1940). Again, it has been a major theme in the **institutionalist** tradition in Economics stemming from Veblen and others (Mirowski 1987a). No, what Etzioni is very self consciously offering is the basis for a completely new and unified discipline not simply a re-hashed version of tried and tested theories. Socio-Economics will build upon but be distinctive from Economics, Sociology, Psychology, etc.

This raises a crucial methodological question. How is the unification to be achieved? There are at least two ways in which this could be done. One could seek to build a new discipline out of elements of the superceded ones by a method of accretion or extension. Etzioni himself expressly rejects just this possibility when he pours scorn on the "silliness" of attempts to offer one-sidely social or economic accounts of phenomena (Etzioni 1987b, p. 43). Whether the concepts being used are economic or sociological, the net effect is an unwarranted imperialism and colonisation. The second line of attack might be to form a wholly novel set of concepts out of the debris of the discarded theories and disciplines, somewhat, say, as Marx formulated his concepts out of the debris of German Idealist Philosophy and classical Economics, or Talcott Parsons, to whom we have already refered, forged his concepts out of the "theory of action" he saw inchoately emerging in the work of Pareto, Marshall, Durkheim and Weber. The new concepts have a broader remit than their progenitors. This certainly appears to be the strategy which Etzioni favours. We are a little diffident here simply because exactly what Etzioni is attempting to do is never outlined explicitly. In all of the programmatic statements, methodological considerations are subservient to substantive ones. We are repeatedly told how different approaches and findings to particular topics must be taken into account. What we are not told, though, is how to do this. And yet this methodological issue is crucial.[5]

What is at stake in all of this has been very well summarised in a recent paper by Hirsch, Michaels and Friedman (Hirsch et al. 1987), although we would not follow them in all of their diagnosis of its origins and certainly not in the remedy which they offer. They set up the contrast as follows

> Microeconomics became the paradigm for the discipline as a whole. Thus contemporary economics exemplifies a highly abstract, deductive approach to social science. Its style is characterised by the development of models based on deliberately, vigorously, and rigidly simplified assumptions. The elegance of the models, their "parsimony", is prized and the intent is that they be predictive.....Sociology on the other hand tends to value description or explanation over prediction. That is, the realism of the concepts and propositions used, their resemblance to the perceptions and meanings of participants is highly valued. (Hirsch, Michaels and Friedman 1987, p. 318).

The first consequence which they see following from the outlook of Economics just described is, of course, exactly that castigated by Etzioni, namely a lack of interest in empirical reference, at least in the way that sociologists conceive it. If it is achieved at all, it has the general character of what Ryle (1949) called 'retrodiction' and results from what are, in essence, "curve fitting" exercises. The second relates the the character of theorising in the two disciplines. Both are built upon a conception of human action. The sociological is predicated upon the plasticity of human response, values and behaviour. It follows that sociological theory has to remain, abstracted, generalised and abstruse in order to encompass the myriad of social forms to which it will be related. The economic conception of human action is much more fixed and determinate being predicated in the universality of a calculative ratiocination. The psychology of Homo Economicus is a narrow set of axioms from which the theorems of macro and micro economics can be formally deduced. These core assumptions are, as Hirsch and his colleagues suggest, expressions of a preference for a certain "extremist" style of theorising; one in which primary assumptions are pushed to their logical conclusions. In Sociology's case, this results in a welter of contradictory theoretical positions all contending over the ground made available the central conception of the socially determined character of human nature. In sociological accounts, the social actor could, therefore, be dubbed *Morph the plastic man*. In Economics, what emerges is a unity of style and outlook, by and large, within which technical expertise, sophistication and professional skills, have come to dominate through general agreement over the features of *Algy the Ready Reckoner*.

Now we can pull out the conclusion which Hirsch et al come to.

> *What all this means for productive dialogue and collaboration between the two fields is that it cannot be managed via a simple transformation of one or another aspect of one field or the other* (Hirsch et al. 1987, p. 333 italics in original)

This is because each field defines problems, poses questions, evaluates results and designs research in entirely different ways. But if it is not to be a simple transformation, what would a complex one be? It would, we suggest, be a fully worked out theory of disciplinary objects and relations; that is, a theory of how the alternative conceptions of theoretical objects given in the disciplines (rational choice, utility, preference, entrepreneurialism, for example) could be aligned and made compatible. In the absence of such a theory, and as we have indicated with Socio-Economics, at the moment there is such an absence, all we have to fall back on is the naturalistic assumption that since these terms all refer to "the same thing" there must be something in common ("the same") for them to refer to.[6] Each is a partial, distorted, imprecise or whatever rendition of "how things are in themselves" - to invoke the noumenal/phenomenal distinction which clearly Etzioni has in mind. This naturalistic presumption, we

will call "the Constancy Hypothesis".[7] While this hypothesis might have great practical efficacy as one of the guiding principles of daily life, it is untenable as even an implicit presupposition for scientific or theoretical endeavours. Even if Socio-Economics were to be able to formulate a theory of objects such as the one we have indicated is required, we cannot see how, on its own terms, it could do so without recourse to some version or other of the constancy hypothesis. This being the case, we would argue that Socio-Economics, and perhaps all such syntheses, are inherently self defeating. We will now show why.

The character of the constancy hypothesis

It is quite clear from his avowedly Kantian stance that Etzioni does not think that scientific or other facts "speak for themselves" in the sense sometimes associated with what is to-day called 'naive realism'. Understanding, comprehending, **perceiving** the world involves judgements and interpretations of some kind. The world-as-perceived is constituted through such judgements. This much is both philosophically and theoretically uncontentious. What are a great deal more contentious are the implications drawn from this agreement and the theoretical moves which are premissed within them. On the one hand we might want to say that while objects and phenomena appear in different guises when we look at them from different points of view and in different lights, in reality, as they are in themselves, they do not change. They are, to put it more formally, constant across different transformations. If we now suppose that, for example when dealing with a physical object, while it may look, feel, or sound different under varying conditions, its physical constitution really remains the same, then we are grounding the examination of perception, and hence Psychology as an investigative discipline (and perhaps, the other social sciences too), in Physics. This form of "naturalism" and the assumptions on which it is built are for us part of our Western rational-scientific ethos.

> In these terms, a psychologist, to explain perception, starts from the universe conceived by physics and then considers the human organism as a physical system acted on by physical events.....(I)n the course of his elaboration the psychologist progressively substitutes physical systems for perceptual things. (Kersten 1971, p. 525)

As Kersten goes on to put it, " access to the world" is seen to be provided by the naturalistic assumption of constancy of objects.

The point is, of course, that the presumption of a constancy of objects is a presumption of practical life in the commonsense world. Without it, it would be impossible to trust our senses. This suspension of doubt is part of what Kersten following Husserl (1970) and Gurwitsch (1974), calls "the natural attitude" (hence the use of 'naturalism' above). To transfer that presumption across from the sphere of practical activity in the commonsense world to that of theoretical activity in science requires a "bridge-theory" of the relationship between the disciplinary constitution of appearances. While it might be natural to presume that the reality of objects as they appear ordinarily to us is underpinned by theories in Physics, grounding that would require us to provide a theoretical account of the relationship between objects as they appear to Physics and objects as they appear to commonsense, a theoretical account independent of and logically prior to both Physics and commonsense. If it were not independent in this way then the argument would simply be circular. Were it not prior, then

there would be no grounds for treating it as other than a further, equivalent account. In the absence of such a theory, the constancy hypothesis serves. It achieves the desired relation by presuming the grounding of our ordinary perceptions (and hence their psychological explanation) in Physics.[8]

Etzioni's Socio-Economics is not seeking the reduction to or substitution of social scientific accounts by physical ones. Nonetheless, it faces much the same problems as a reductionist Psychology faces. It has to show theoretically that objects and relations as they are conceived across the social sciences are **essentially** the same. This could be done by a version of reductionism, by saying, for instance, that the psychological underpins the sociological and the economic aspects. As we have seen, Etzioni eschews this. No one account is foundational for the others. This version of naturalism is not open to him. On the other hand, he could demonstrate that phenomena as they are constituted by the theories of Economics, Psychology and Sociology are in fact grounded in a set of categories or concepts which is independent of and transcends them all. Such a transcendental argument would provide for the possibility of synthesis. The category of rational action might, as Parsons (1968) suggested for instance, be a contender for this role. Without an explicit theoretical account, we have nothing to justify the synthesis other than the constancy hypothesis. Because Psychology, Sociology and Economics all have theories of entrepreneurial activity, those theories refer to the same underlying phenomenon. Amalgamate the partial descriptions which are given in each of the disciplines and the essence is revealed.

In Gurwitsch's discussion, the constancy hypothesis appears in relation to perceptual assymetries. The sort of cases he has in mind are those where, for instance we are picking out carpets to go with our furnishings and you insist that a particular tone is "jade" and I insist that it is "aquamarine". We look at a colour card and see agree the colours are different. You still say the carpet in view is jade; and I still disagree. Or again, we are looking out of the window and you say there is a greenfinch on the bird table. No it is not, I say, it is a female sparrow. In both of these cases we accept that what we think we see can be affected by lighting, speed of movement, posture, contiguous colours or whatever. That is, we accept that what we are looking at at any moment is seen against a ground from which it is picked out. The whole constitutes a gestalt and variations such as the ones listed can affect the way in which we constitute the gestalt, as a collection of objects in view.

But familiar and interesting as these examples may be, they are not paradigms for the sorts of cases we have in mind. These are more like those of the "Compton-effect". The Compton-effect describes the behaviour of light under certain (experimental) conditions. When light is scattered by forcing it to collide with electrons, the resultant patterns could be described both as the fragmentation of a beam of particles (photons) and as the diffusion of a wave of energy. In the Physics of the time, these two descriptions were incompatible. Resolution of the contradiction came through the unification of the two incompatible descriptions within Quantum Mechanics. That is, it came about through the provision of a further theory. But this further theory was not an amalagamation of the rivals. It departed from completely different postulates and marked a complete break with the premisses for the two rival theories.

As far as we are concerned, two important things have followed from this. First, contemporary Physics is now almost completely detached from commonsense understand-

ings of the physical world. The constancy hypothesis has to be invoked because the two sets of theories cannot be alligned. Second, there is the related meta-theoretical issue, namely consideration of what it is for something to be an object for Physics has, perforce, become a matter for metaphysical consideration. Part of this has involved raising problems of the relationship between modern physical science and the commonsense physics we all utilise in our ordinary activities, with its reliance on determinacy, continuity, spatio-temporal co-ordination, cause and effect, all of which are thrown into doubt by the statistical character of Quantum Mechanics. Such issues of ontological commensurability have much pre-occupied philosophers of science.

In the face of the possibility of a radical disjuncture between "scientific" and "commonsense" physics such as that just articulated, philosophers[9] have proposed the adoption of a strategy of "ontological relativity". By this they mean that it should be for Physics to determine what for it the world of physical objects might be. Such ontological relativity implies not only that "scientific" physics might constitute the world differently to "commonsense" physics, but that within the former radical discontinuities would be possible as well. As Ian Hacking (1983) and Nancy Cartwright (1984) among others point out, the key to the possibility of such discontinuities is variability in the kinds of studies made and hence in the investigative interests to hand. What "the world of physical objects" is like depends on what it is treated as and what it is investigated for, that is, the mode of investigative reasoning used. The descriptions (and, for Cartwright, the laws as well) which physics provides are phenomenological in character.

Precisely the same considerations could be brought to the problems posed by the relations between economic, psychological and sociological theories of business life with which Etzioni is concerned. At first blush they look to be ontologically incommensurable, and this, too, may be the consequence of the use of differing forms of investigative reasoning. Unless we can provide a theoretical account which unites these forms, all we can fall back on is the constancy hypothesis, a hypothesis which even on its own home ground (commonsense and scientific physics) is far from self-evidently secure.

Etzioni does not provide a the kind of theoretical account we have suggested is necessary. Nor, to be fair, could he. The modes of theorising, the processes of reasoning, characteristic of the disciplines we are discussing are fundamentally at odds. It is not simply that Psychology is experimentalist and Economics is mathematical while Sociology is neither. Neither is it simply that Sociology is collectivist in its explanations while Psychology and Economics tend to be individualist. Rather, it is that all three are methodologically extremist (to use Hirsch et al.'s phrase again) in different ways and for different reasons. In our view, their extremism makes them increasingly centrifugal rather than centripetal. Our task now is to show why and how. In the next Chapter we will look at length at the nature of economic reasoning and how this outlook is applied to the problem of entrepreneurial activity. We will then turn to how Psychology has approached what is ostensibly the same topic. Having demonstrated the nature and the magnitude of the differences is as we have claimed, we will be able to see that the presumption of the applicability of the constancy hypothesis is unwarranted. The consequence of this line of argument is the proposal we laid out in the Introduction to this Part. If the development of a unifying theory is likely to prove intractable, we will have little choice but to begin investigations in an entirely different way. The exploration of this way will

occupy the rest of this book.

NOTES

* It is hoped a version of this Chapter will appear in a special issue of *The Journal of Inter-disciplinary Economics* (forthcoming)

[1] This expression is used by Hirsch, Michaels and Friedman in their (1987) paper. We will be discussing this below.

[2] Etzioni's general statement in contained in his (1986b).

[3] C.f. Etzioni (1985a)(1985b)(1986a)(1986c)(1987a)(1987b).

[4] C.f. the discussion in Chapter 3.

[5] In one sense, the most serious task of all is only addressed *en passant* in Etzioni's programme, namely the requirement to mathematise sociology so that the formal descriptions provided by both disciplines could be aligned and made consistent all the way through. (See Etzioni1985a). If a synthesis is to be achieved, such an alignment has to be a *sine qua non*. It is because we do not underestimate the difficulties of moving beyond initial programmatics that we regard the work presented here as exploratory only.

[6] In a discussion of a similar order of problem, Schutz called this presumption "naive" and thoroughly offended Talcott Parsons thereby. C.f. R. Grathoff (1978). He meant only that it was a natural, **pre-theoretical** one.

[7] The "constancy hypothesis" is discussed in A. Gurwitsch (1964) and applied to the social sciences by Fred Kersten (1971).

[8] The classical empiricist philosophical psychology of "sense data" might serve were it not for the developments in bio-physics. It remains to be seen whether the popular interest in computational and other functional models will be any more effective.

[9] See W.V. Quine (1969), N. Goodman (1978), H. Putnam (1975) and (1983).

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.

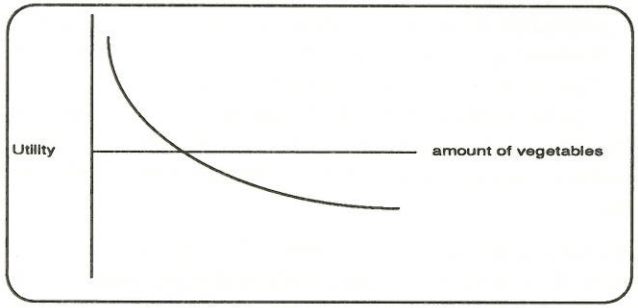

Figure 2.1 The marginal utility function

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.

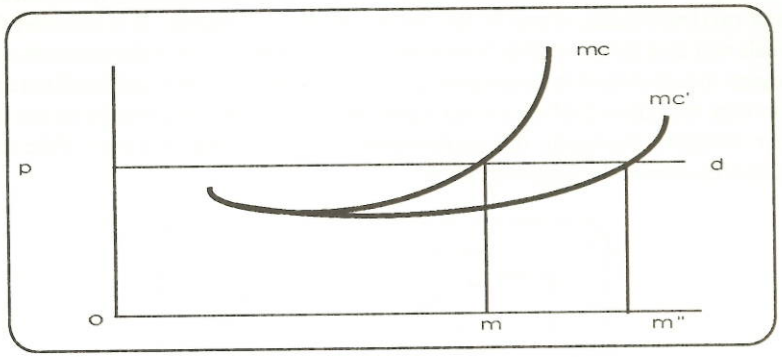

Figure 2.2 The generation of entrepreneurial profits

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.

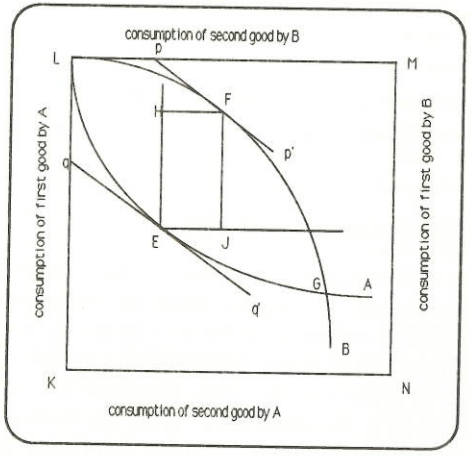

Figure 2.3. A transaction map (from Casson 1982, p. 61.)

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.

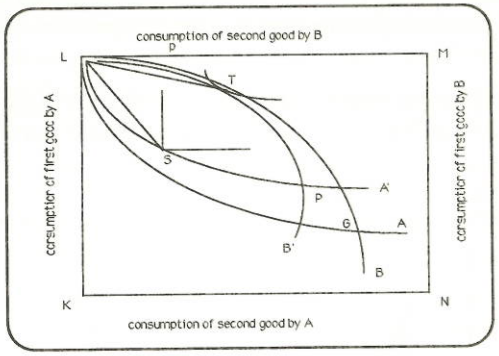

Figure 2.4 A transaction set (from Casson 1982, p. 64.)

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.

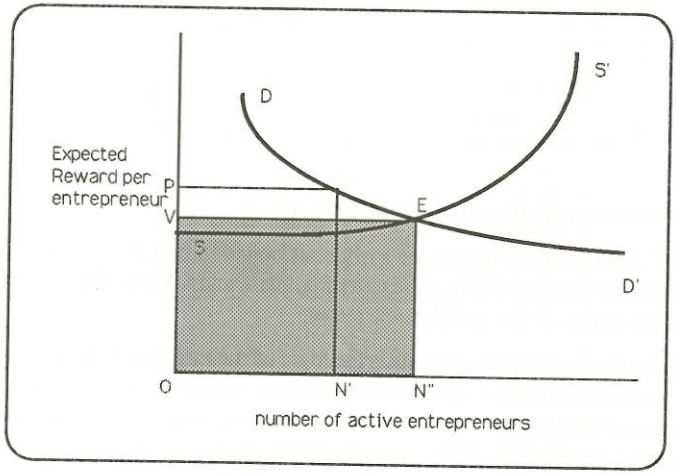

Figure 2.5 The market from entrepreneurial risk taking (from Casson 1982, p. 336.)

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).

3. Sociological reasoning

Introduction

We have now assembled all of the elements needed to assess one part of Etzioni's strategy. We have outlined the general features of **Cartesian Economics** and its application to entrepreneurial activities. In addition, we have seen how its motivational psychology, the rational actor theory of economic decision making, has itself been subjected to the Galilean tendency of indirect mathematization and formalisation. If, as we have seen some of its critics propose, much of contemporary economic reasoning and investigation leads only towards unrealistic theories, models and findings, is it necessarily the case that this tendency can be effectively countered by the incorporation of a broader framework, one which includes, significantly, the theories, findings and analyses of Sociology? That, in a nutshell, is what this chapter is about. It would be re-assuring to think that all we had to do was to run through a collection of appropriate findings and theories in Sociology andSocial Psychology to show, indubitably, how they fitted the bill. But, naturally, things are never as simple or as straightforward as that. The claim made about Economics, it will be remembered, was that it had adopted a distinctive style, indirect mathematisation, and a set of specifying assumptions, rational actory theory, which had led increasingly to a mismatch between economic depictions of economic activities and how such activities were viewed by individuals and were experienced in ordinary life. Philip Mirowski (1987b) has condensed the argument as follows.

> The neoclassical school of economics.....adopted all the trappings of the Cartesian world view - mathematical formalism, axiomatization, derogation of literary narrative, and mimesis of natural science

> terminology and attitudes - but had also endowed their mannequin of rational economic man with exclusively Cartesian powers and abilities: transparent individual self knowledge, mechanical algorithms of decision making, independence from all historical determination, and all social action ultimately explained by rational individual assent. (Mirowski 1987b, p. 70)

This mannequin, the economic actor in neoclassical theory, we called *Algy the Ready Reckoner*. What we now have to decide is whether *Morph the Plastic Man* who plays much of an analogous role in sociological thinking and Algy are really brothers under the skin. Are the principles on which their construction is based symmetrical? Are their modes of deployment reconcilable? If we decide they are not, then no amount of exhortation, no amount of critical exegis and comparison, will make the synthesis of Economics and Sociology feasible. For without symmetry there can be no hope of reconciliation. And without that, the combination of sociological and economic theories , findings and models is likely to be less than adhesive.

In order to get a reasonable grip on the issues to which we are pointing, it will probably be best if we look first at the nature of sociological accounts of economic activities. Our task here is not quite as complicated as it was with our analysis of economic reasoning. Sociology's theories and descriptions are, by and large, much less formalised, much less abstract, more clearly substantive than those in comparable branches of Economics. While it is possible to represent sociological ideas by means of sets of formal apparatuses and then to apply modes of mathematical transformations to them, it is not essential to the style of analysis that this be done.[1] This is both helpful for a sense of immediacy and misleading for full comprehension. That Sociology talks about entrepreneurs, buyers and sellers, market makers, owners of captital, and so forth **as if** they were the ordinary inhabitants of our social worlds, can lead us to overlook their theorised nature. For Sociology no less than for Economics, the actor in the market place is a theoretical object.

Bearing this first caveat in mind, what does Sociology have to say about economic activities and economic actors? Obviously we do not have the space to survey the whole of what is sometimes called 'The Sociology of Economic Life'. We will concentrate, instead, on our own chosen area, namely studies of entrepreneurs. In addition, in order to help speed things along a little, we will look at just one or two instances of sociological analysis. We feel free to do this precisely because part of the argument we will develop is that the appearance of diversity and plurality of approach within Sociology is simply an illusion. While sociologists may differ radically over everything else - their conception of history, their view of human nature, their definition of their discipline, their methodology, when it comes to the sociologising, they are in almost complete accord. Or, at least, if one forgets what they say about their analyses and looks at what they do, how they argue sociologically, they are.

The overall approach has been one which looks to place the entrepeneur as a social role within a system of such roles. While, as we say, the precise details have been various, in all cases the concern has been with how performance of the entrepreneurial role contributes towards the stable reproduction and transformation of this system. Here, of course, the sociological analysis looks to be closely related to that offered within the approach known as "institutional economics" (Gunning 1986, Prendergast 1986). Both have a concern with the

temporal development of the system and the role which key actors might play in this process. The work of Schumpeter is central here since, for many, he straddles both disciplines. His analytic history of the development of Capitalism and the place which the entrepreneur holds within it, is taken to de definitive of one approach to the comparative sociology of economic systems.[2] The focal point of his analysis is, as we saw, the economic function performed by the entrepreneurial role in the transformation of Capitalism. As one element of a system of economic roles, the entrepreneur, by exploiting short-run opportunities, transforms markets and thus induces changes in the systems of production and distribution as a whole.

Leaving aside the moralistic implications which some have drawn from analyses such as those offered by Schumpeter, just how does the system of social roles reproduce and transform itself and just how does the entrepreneur contribute to this? In what has become a classic anthropoloical study which, alas, is all too little known outside that field, Frederik Barth described the role of entrepreneurial store-keepers in Northern Norway.[3] The store-keeper stood at a significant nodal point of social and economic relations within the local community. By bringing socially valued resources, what Barth calls ' prestations', into the local economy, the entrepreneur frees the circulation of value within the system. Economic and social goods and services can be exchanged. In the terminology of the modern neo-classicical economics of Casson (1982), the entrepreneur makes a market for the exchanges which are then transacted. In so doing, the entrepreneur introduces the mechanisms of the market place, price determination, the procedures of capital accounting, and so forth into what was only minimally a money economy. In consequence, this local economy becomes much more tightly connected to the wider market system. Valued goods flow in and out; credit systems come into play, novel developments in the division of labour are sponsered, and so on. The system of social relations is in orderly (but not necessarily conflict- free) transformation.

A similar theme of orderly transformation - what Schumpter called market 'adjustments', is to be found in Scase and Goffee's (1982) (Goffee and Scase 1985) account of entrepreneurship in the modern British economy. Here, utilising the more conventional terminology of class determinations, Scase and Goffee seek to identify how entrepreneurial styles are responsive both to the transformations taking place in the capitalist economy and to the relative position such entrepreneurs stand in with regard to the class system associated with that economy. How are individuals inducted into specific sets of entrepreneurial roles and how are these roles then legitimated?

In all three analyses, different though they are with regard to the specifics and details, it is the overdetermination of the entrepreneurial role which catches attention and is the feature which makes the accounts distinctively sociological. The entrepreneur is defined as performing a specific transformative role which is, in its turn, the outcome of an array of socio-cultural forces shaping the patterns of opportunities available and the range of activities by which such opportunities might be realised.

In Scase and Goffee, the depiction of overdetermination takes the form of a typology of modes of entrepreneurial involvement. Each member of the typology plays out a distinctive role as a response to the patterns of forces within the social and economic spheres. Because, in this analysis, the key to understanding the pattern of entrepreneurship is taken to be the way in which the institution of entrepreneurship makes certain roles possible for actors,

thereby serving to legitimate both entrepreneurship, its correlates and the capitalist system which contains them, the thematic underlying Scase and Gofee's typology is what Weber (1978) might have termed rational-legal organisation. The groups are 'the self employed', 'the small employers', 'the owner controllers', and 'the owner directors'. These are collected as a gradation of size of organisation. Membership of one or other type is held to be determined by socio-cultural factors such as motivation, family and personal history, familial resources, and market opportunities, all of which are themselves class related. These factors account for the varying trajectories of entrepreneurial experience which the typology summarises. Thus membership of a type can be conceived as a sociological career in a certain socio-economic role. The relative stability of the entrepreneurial function, the riskiness of certain market places as opposed to others, the willingness to adopt a strategy of conservative innovation, and the general marginalisation of entrepreneurship within the British middle classes, are all seen as shaping the experience of entrepreneurs in the economy. The existence of such differential experience is, in its turn, used to account for the functionality of the entrepreneurial role within the system of class relations, since the possibility of success as an entrepreneur legitimates the acquisition of private property, the market system of distribution of goods, the existing channels of upward mobility, and the value systems associated with them. In sum, the entrepreneur instantiates the process of the reproduction of capitalist social relations and the strains inherent in it.

From the overly condensed and admittedly simplified account which we have given, we might think that not too much stands in the way of Etzioni's project. All that looks to be required is the development of a sufficiently flexible framework within which the economic and sociological accounts of activities in the capitalist economy can be interrelated. One possible contender, and this is the favourite of many of the contemporary critics of "positivist economics", is the behavioural functionalism which takes one form in Schumpeter and a very different one in "institutional economics", and which appears to be congruent with the sociological descriptions just summarised.

This appearance is, we think, misleading. It results from not following through carefully or systematically enough, the pre-suppositions of sociological reasoning. It is, therefore, a failure to engage with the groundings of sociological thinking, and especially what is involved in constituting social activities for sociological analysis. Obviously this is a deep matter. In other places we have attempted in various ways to draw out what we think is involved (Anderson et al., 1985). One thing which certainly can be said is that the conventional contra-distinctions between economic theorising and Sociology are not sufficient to express what is at issue here. Claiming that Sociology is holistic while Economics is individualistic, gets part of what is involved but in an unfocussed way. So does the suggestion that sociological accounts feature an **oversocialised** social actor while economic ones are about an **asocial** calculator. Indeed, we have been using that theme ourselves thus far. What we have to do now is try to sharpen the focus considerably to see why sociology prefers the holistic, oversocialised approach to that implied within asocial individualism.

Theorising in Sociology and Economics

One fairly obvious way way to begin this task is by asking what Algy and Morph are designed to do. What are these two conceptual constructions for? What is their deployment supposed

to achieve? At the basic level, the answer is, or should be obvious. What Morph and Algy do is make co-ordination of activities possible within the theory. In Economics, populating the theory with Algies enables exchange to take place. The market place is the system of transactions among this population. In Sociology, what invoking Morph provides is the possibility of social action; that is, the co-ordination of meaningful social behaviour. In this sense, then, Morph and Algy are co-ordinating devices and little more. In a justly famous passage, Alfred Schutz (1962) describes the place which these devices are allocated in social science theorising.

> How does the social scientist proceed? He observes certain facts and events within social reality which refer to human action and he constructs typical behavior or course-of -action patterns from what he has observed. Thereupon he co-ordinates to these typical course-of-action patterns models of an ideal actor or actors, whom he imagines being gifted with consciousness. Yet it is a consciousness restricted so as to contain nothing but the lements relevant to the performing of the course-of-action patterns observed. He thus ascribes to this fictitious consciousness a set of typical notions, purposes, goals, which are assumed to be invariant in the specious consciousness of the imaginary actor-model. Among these homun-culi with which the social scientist populates his model of the social world of everyday life, sets of motives, goals, roles -in general, systems of relevances- are distributed in such a way as the scientific problems under scrutiny require. (Schutz 1962b, pp. 63-4)

Social Science models and their homunculi are, then, designed to be unrealistic in the sense that there is no attempt to ensure a direct symmetry between theoretical descriptions and those offered within the realm under scrutiny. The point is, to labour it once more, that Algy and Morph are unrealistic in entirely different ways and express entirely different relevances. We can see what this might imply is we look at a relatively uncomplicated (and unambitious) summary of what is involved for sociology.

> At the point where individual and society intersect stands homo sociologicus, man as a bearer of socially predetermined roles. To the sociologist the individual is his social roles, but these roles for their part are the vexatious fact of society. In solving its problems, sociology necessarily takes social roles as its elements of analysis; its subject matter is the structure of social roles. (Dahrendorf 1968, pp. 6-7)

What Homo Economicus achieves is the possibility of an economic order simply in virtue of the fact that Algy is calculative through and through. Economic order, as it is theorised, is predicated upon the possibility of calculation and its universal applicability. In that sense, what for the sociologist is termed 'problem of meaning' is solved *ex cathedra*. There is no problem of meaning in the market place. Everthing is and can be couched in the terms of rational calculation. Morph, on the other hand, is social through and through. But what this implies is that the problem of meaning is the co-ordinating task which he is constructed to solve. Systems of meaning, systems of interpretation, are assumed to vary with social

organisation. No single universal rationality can be induced. This being the case, the sociologist populates his model of the social with Morphs as a solution to the problem of aligning interpretations. What things mean for the social actor, what the social actor is constituted as, varies from situation to situation and context to context. Morph's task is to read off and co-ordinate appropriate interpretations from any given social context. This is no problem for Algy since, in the theory, only a single mode of interpretation is possible. Where Algy is asocial, then, this is radical and stipulative asociality. Where Morph is interpretive social actor, this too is radical and stipulative. Algy can orient to the market place as a collectivity of calculating devices just like him. Morph, on the other hand, begins from the pre-supposition that other actors have reasons, goals, motives, relevances - in brief rationalities - which may or may not match his. Co-ordinating activity is, for Morph, a matter of lining up definitions of the situation and ensuring meaning structures match.

The techniques by which Morph is constructed are, as we have said, many and varied. It is not our intention now to lay them all out and evaluate them. Suffice it to say that in every case, the problem of problem of aligning meaning is taken to one one of social organisation. That is , it is presumed that resources are available to the actor within the social setting which provide for its interpretation. Definitions of who people are and what they are doing which actors can call upon, are socially distributed. One conception of the social organisation interpretive social action of is, of course, that contained within the constellation of concepts associated with role theory to which Dahrendorf refered. Here the social availability of conceptions of roles, norms, values, identities and so forth is provided through a theory of socialisation. Fully socialised individuals are defined as sharing meaning structures. The possession of shared meaning structures just is what being a social actor means. In a different vein, the social organisation which as we saw earlier, Scase and Goffee draw upon is that associated with concepts of class and class position, ideology and legitimation, class interests and class power.[4] Both accounts could be used to show (and it is this which makes Morph a **plastic** man) that forms of rationality, forms of social behaviour, are not universal but context dependent. What any actor, (for example, a potential entrepreneur) might feel it rational, necessary, possible, desireable to do will depend upon how he or she views both his or her situation and that of others. And this will depend upon the social availability and indeed valuation of contending versions of what things mean and what it is felt should be done about them.

The central contrast then between economic and sociological reasoning turns upon the context relativity of rationality. Economics depends, necessarily, on a universally generalisable form of rationalisation, namely calculability. This is not a consequence of economic investigations but *a priori* for them. Sociologists seek to relate forms of rationalisation to social context, and thus relativise them. It is this which makes any attempt at a simple synthesis between Economics and Sociology essentially SELF DEFEATING. The conceptions of what it is to act in an economic situation are radically divergent, and as such the analyses bult upon them are pulling in different directions. The failure to see this and to provide for an entirely distinctive conception of action, a new homunculus designed to solve the problems here adumbrated, is expressed in Etzioni's version of the constancy hypothesis. If Sociology and Economics use the same terms they must be talking about the same things. As we have just seen, they most evidently need not.

What we have called into play, yet again, is the constrast between calculation and social organisation. We have suggested that the synthesis of these two is not possible. This does not mean that nothing can be done. Rather, it implies the necessity of re-considering our investigative strategies and perhaps equally importantly our points of departure. Economics may be unrealistic in the sense we have described, but simply bolting someSociology onto it is not going to help. What then can we do? The implication behind the criticism of a lack of realism concerns a lack of understanding about how economic structures in daily life really operate. Our suggestion would be to begin somewhat differently. Instead of segregating calculability and social organisation and then seeking to integrate them, why not look at the social organisation of calculation? This does not mean imposing a sociological conception of activities upon an economic one, thereby demoting the importance of calculation. Nor does it mean subjugating the social to calculative rationality. Rather we can look to see where, how far, and in what ways, economic actors when faced with actual choices manage the segregation of social and calculative rationalities. How do they rank the grounds of choice? How do they relate the various criteria? Under what circumstances do practical economic decision makers trade-off calculative rationalty against managerial, organisational, personal and other social forms? In that sense, what we are proposing to is look at is the social organisation of calculability. And, given the prominence which entrepreneurs are suppposed to give to profits, markets and opportunities, where better to start that with calculation is an entrepreneurial firm?

In the next two Parts, we will introduce a number of exploratory studies. The are all concerned with this theme. We will look first in a more general way at the problems of managing the separation of the calculative and the social. The studies in Part Two, then, take up the generic place of calculation and its relationship to the problems of institutionalising the entrepreneurial role and as the thematic for project innovation and market deterimination. In the ThirdPart, we turn to more technical consideration of social organisation of calculability and the means by which actual economic actors reproduce the systems of calculability through which economic activities are related. We will look at calculability as a division of labour (Chapter 9), as a system of practical action (Chapter 7), and as an oranisational feature of negotiation (Chapter 8). In each case, our objective is to show possibilities not to refute positions. We hope to indicate what order of materials are available for analysis if one begins from the social organisation of calculability and how they might most usefully be treated. Our ambition is, so to speak, to open the door on novel kinds of analyses not to delimit the field or circumscribe proprieties. If we are at all successsful in this, what we will have achieved is the encouragement of both sociologists and economists to re-think the character of the arguments between them and to give at least house-room to the possibility that empirical investigations of mutual interest need not await theoretical unification. We do not say we have solved the problems which inhere in the disputation between Sociology and Economics. What we do say is that we are offering are different but equally interesting ones: problems that have their roots in empirical materials and not in the specifying assumptions of theoretical positions.

NOTES

[1] In fact, much of what is called mathematical sociology appears to proceed through a

kind of re-description of sociological ideas in formulaic ways. See for instance A. Stinchcombe. (1968), and J. S. Coleman (1964). A more recent instance from a somewhat different field is the use of graph theory in the analysis of technical and scientific change. See Callon et al. (1986).

[2] Arguments for incorporation raised specifically from studies of entrepreneurs can be found in A. Kalleberg (1986).

[3] See also Barth (1967) and Paine (1971).

[4] One of the problems for the class determination account is to provide a theory of the acquisition of sociality which does not turn into a sub-variant of socialisation theory. In this respect, the social psychology (broadly construed) of class relations remains a somewhat under-developed field. Forays by those such as Foucault seem to have a surprisingly Durkheimian cast. C.f. M. Foucault. (1972).

PART TWO
THE SOCIAL AND THE CALCULATIVE