UNIVERSITY OF SOUTHAMPTON

THE IDEA OF EMPIRICAL SOCIOLOGY

by

JAMES EMMANUEL VALENTINE

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UNIVERSITY OF SOUTHAMPTON

ABSTRACT

FACULTY OF SOCIAL SCIENCE

SOCIOLOGY

Master of Philosophy

by James Emmanuel Valentine

The subject of the discussion is the meaning of 'empirical' sociology, and what this means in practical terms. This is explored in terms of what we are entitled to call sociological 'data', and sociological observation. The discussion centres upon the idea of understanding, on the one hand, and positivistic approaches to social enquiry, on the other, and an indication is given of how these may be reconciled. A re-definition of what empirical sociology means is put forward, which is elaborated in detail in the conclusion.

The discussion is carried out with reference to examples of contemporary sociological practice, primarily in criminology and the sociology of deviance. The claims of phenomenology and ethnomethodology to challenge existing modes of empirical enquiry in sociology are critically assessed. Subculture theory and its relationship to more formal modes of enquiry is examined. The role of quantification and measurement is looked at with relation to various claims made about its suitability for criminology, and arguments about whether this confers a scientific status to the subject.

THE IDEA OF EMPIRICAL SOCIOLOGY

INTRODUCTION

The aim of this discussion is to illuminate certain issues in the philosophy of social science, with reference to case-studies drawn from the field of criminology and the sociology of deviance. The essential question being asked is whether we are entitled to see the study of society as a primarily empirical activity. I have come to the conclusion that we are. That is to say, sociology is something more than the purely philosophical activity outlined by Peter Winch in The Idea of a Social Science.

My strategy has not been to go over the Winch controversy in detail, which I think suffers from a basic ambiguity with reference to how far the protaganists (and in this I include Winch's critics as well as Winch himself) are talking about 'real-life' issues. This point is elaborated in the conclusion.

I have tried instead to look at some of the objections to the idea of empirical sociology by means of detailed studies of those practitioners or theoreticians who have attempted to provide alternatives, and others who have set out to defend it.

The key dimensions to the argument resolve around,

1) whether there are sociological 'facts', whether those 'facts' are independent of sociological theories, and whether they can test these theories,

2) what we mean by 'understanding', and whether understanding is compatible with other approaches to sociological enquiry, and

3) what we mean by sociological explanation, given our answers to 1) and 2).

It is very difficult to put forward a crisp exposition along these lines so instead I have opted to attempt a continuous argument from which these points should emerge.

The issues as a whole are obviously bound up inextricably with whether the sociology is seen as being similar to natural science.

It is this aspect which has led me to concentrate on the criminology, or sociology of deviance, field, as a sub-disciplinary area in which to analyse the issues in real-life terms. This is because there has been an intensity of research, on areas like juvenile delinquency, which is unparalleled, and which invites direct comparison with natural science on this basis.

At the same time, natural science itself is not a monolithic or homogeneous enterprise, and the possibility of an easily-manifested description of the philosophy of science does not exist. It has too often been assumed that it did. 'Philosophically reflexive sociology owed much less than one might have expected to the philosophy of science and the philosophy of the social sciences. Until recently the philosophy of science was largely identified in the minds of non-specialists with logical empiricism; again and again one read sentences like "the philosophy of science shows" or "the philosophy of science teaches" which should really have been rephrased, "logical empiricism shows" or "Braithwaite teaches" (for instance.)"[1]... the philosophy of the social sciences has largely defined itself as the the theodicy of positivism'[2].

This is now changing. There may in fact have been an over--reaction, so that non-positivistically oriented philosophies of the social sciences have laid too much emphasis upon a critique of logical empiricism without taking account of the fact that there are other alternatives which demand alteration. To quote another of Martins' comments, "Philosophers of science like Popper, whose relationship to logical empiricism has grown ever more distant, have often been interpreted in a positivistic, logical empiricist approach, at least in Britain'[3].

In this approach, therefore, I have recognized that the philosophy of science itself is a matter of contention. This involves taking note of a very wide variety of literature, and thus I have only been able to deal with a small number of the problems which are relevant to the field. For this reason I have adopted a critical approach, and looked at those problems which are most contentious. I have not attempted to approach anywhere near to comprehensiveness with regard to criminology, and have therefore again been very selective.

NOTES TO THE INTRODUCTION

1. HERMINIO MARTINS, 'Time and theory in Sociology', in Approaches to Sociology, Routledge, 1974, edited by John Rex, 285.

2. Ibid.

3. Ibid.

CHAPTER ONE

PHENOMENOLOGY

Observation

How do we observe social activity ? What do we mean by observation in the social sciences ? If we are to talk about 'empiric--al' sociology, then we obviously have to come to a clear understanding on this point.

There is one answer to this question which should be dismissed at the outset. Social 'activity' cannot be reduced to 'behaviour' or physical movement. We can only describe social activity by grasping the sense of what it means (putting aside for the moment what this state--ment adds up to). ' ... the identification of the events to be under--stood necessarily depends on understanding the rules which make them count as events of whatever kind it may be. Thus when we describe a set of actions as praying this is necessarily to employ religious criteria; when we describe an act as that of voting this is necessarily to employ political criteria'[1]. This entails something quite different from describing physical behaviour - talking into the air while kneeling, or putting pieces of paper into boxes.

It is mistaken to believe that describing social behaviour in this way makes it, as has been claimed by the behaviourists, any more scientific a way of going about social enquiry. It is not just that the methods of physics are unsuitable for the study of social activity, but that it is misconceived to suppose that physics or natural science in general is founded upon the sort of basis which the behaviourists imply is the case.

This basis is seen as a foundation of certain knowledge which is represented by 'pure' or 'raw' sense-data, uncontaminated by perceptions or preconceptions of any kind.

There is no sense in which this idea stands up to examination. Modern cognitive psychology and neurology have demonstrated the inex--tricable link between sensation and perception, to the point that it probably does not make sense to talk of pure 'sensation' even in a physiological sense. They have shown how perception modifies our environment by means of mechanisms like size constancy and shape constancy. In particular, they have shown that such modifications are normal adaptations, so that we now acknowledge that it is an essential part of our perceptual make-up that we see, for instance, parallel straight lines receding to a point at infinity.

Observations are in some way distinct from theories, but it is not possible to produce simple criteria for what constitutes 'raw' or 'sense--datum' observation on the basis of the way scientists produce theories, including those celebrated ones which are appealed to as exemplars of the scientific method. The 'observations' of natural science may include such diverse phenomena as the movement in a columnof mercury, the image at the end of a telescope, or a trace in a bubble-chamber.

The most striking demonstration of the prominence of perceptual mechanisms is afforded by the familiar ambiguous representations - duck/rabbit, lady/vase, etc., produced originally by the Gestalt school of psychologists.

This phenomenon has had wide attention drawn to it by philosophers of science - notably, Norwood Hanson, Errol Harris, Michael Polanyi and Thomas Kuhn, as well as by philosophers like Wittgenstein. The essential question is what it means; does it mean, as Kuhn states, that observations are actually shaped by scientific theories, such that no observation can overthrow a theory, or can we acknowledge that observation is selective and yet still say that observations are in some way independent of theories - Popper's position.

Understanding

What do we then mean by 'understanding' activity, the 'sense' or 'meaning' of activity ?

There is again one point of view on this subject which it should be made clear is being dropped. This is the idea, not derived from behaviourists specifically, but from philosophers like Popper and Nagel, that understanding has a role, but only in the formation of hypotheses, which are then tested by means of statistics etc. This view sees 'understanding' as being equivalent to 'empathy' - imaginatively reconstructing a situation by trying to place oneself in someone's shoes[2]. To hold this point of view is also necessarily to be rather sceptical about the whole idea since it raises the notion that, as Nagel for instance suggests[3], one must be mad oneself in order to understand a madman.

It can be shown fairly easily that this argument and others like it are misdirected. The most convincing demonstration of this is contained in Winch's discussion of Weber in The Idea of a Social Science. He looks at the phenomenon of understanding from the point of view of the Wittgensteinian concept of 'form of life'.

In this discussion I will not be concerned with exploring what this phrase means but I will be concerned with exploring 'ways of life' which I use in an everyday sense. That is to say, we can speak, according to the context, of a Western European 'way of life', the British 'way of life', or cricket as a 'way of life'.

The idea of understanding is, then closely bound up with that of ways of life. Our way of life determines what we are able to under--stand; someone from a certain way of life cannot always immediately grasp the significance of activity which is alien to him. People from France find it difficult to understand why cricket audiences applaud when the batsman returns to the pavillion. 'Understanding' in this sense means understanding something about the rules of cricket, and the Brit--ish way of life; it is not achieved simply by looking.

This is very similar to the sorts of points which Winch makes. Where this discussion goes off along different lines is that he leaves the concept of 'form of life' as something which is relatively unspecific and ambiguous. There are good reasons why this should be so, in the context of his general arguments, but what I am concerned with is making the idea of 'ways of life', if not more specific, more explicit. In particular I think it is important to bring out the difference between very general or pervasive ways of life (like the Western European 'way of life'.) and much more specific or limited ways of life (like deviant subcultures). It is this sort of distinction which is crucial to an understanding of the practice of sociology in its necessarily 'reflexive' sense - that is to say, an understanding which takes account of the fact that the sociologist, just as much as his subjects, is part of a way of life.

Phenomenology

The term 'phenomenological' sociology is often used as if it were synonymous with 'understanding' sociology[4]. I think this is much too vague. The working definition of 'phenomenological' sociology being used here is sociology which is explicitly derived from phenomenological philosophy - from authorities like Edmund Husserl, Maurice Merleau--Ponty, or Alfred Schutz.

This vagueness is a problem in that it makes a critique of phenomenological sociology very difficult, since I subscribe to the idea of understanding but reject the idea of phenomenological sociology. Maurice Roche[5], for instance, includes as contemporary examples of phenomenological sociology, work by Cicourel, Douglas, Garfinkel, Matza, Laing, Goffman, Lemert and Scheff.

I think Cicourel and Garfinkel's work is very closely related to phenomenology, in that they both propose methods of research which are opposed to conventional models, and which rely heavily upon a phenomeno--logical philosophical foundation. Ethnomethodology is nevertheless different from phenomenology and some examples of ethnomethodological research will be looked at separately.

Sociologists like Goffman, Lemert and Scheff have drawn attention to the way in which deviant behaviour arises through a process of social reaction, or interaction; labelling, institutionalization, stigma, etc. I think a great deal of this sort of research has illuminated our understanding, of deviance in particular, but I do not think it makes much sense to ascribe the term 'phenomenological' to it. Historically, it derives much more closely from symbolic interactionism, and philosophically it does not share the phenomenologists' preoccupation with presuppositionless observation. A number of phenomenologists themselves realise this - sociologists like Becker (who I think has made an important contribution to our understanding of deviance), have been criticised precisely because of this 'unphilosophical' outlook[6].

Laing's findings have something in common with this. He and his colleagues stress the influence of the social environment on mental illness, rather than simply looking at the individual. Philosophically, Laing's work is closely based on existentialism, and therefore, indirectly, phenomenology, but again he does not share the phenomenologists' methodological position.

Matza's concept of 'naturalism' is closely related to similar concepts like 'accuracy' in phenomenology. There are two sides to the idea of describing phenomena as they 'really are' with respect to phenomenological philosophy. There is one aspect, which is often expressed by the slogans 'fidelity to the phenomenon' or 'priority to the phenomenon' which I think is useful. The essence of sociological activity is not physical behaviour. Phenomenologists have been in the forefront of the critique of behaviourism and reductionism. This was so in the contribution of Maurice Merleau-Ponty, for instance, whose <u>The Phenomenology of Perception</u> demonstrates the validity of mechanisms like size and shape constancy, the 'observation' of objects which are not actually within our visual field, and in general a critique of the limitations engendered by the sense-datum criterion for observation. Spiegelberg in his account of the essentials of the phenomenological method[7] stresses the conscious opposition to reductionism and the insistance on evidence from the 'sense organs'. It has been stated that the case for phenomenological psychology is simply that 'consciousness' still can and should be studied[8].

Roche is then talking about some of the contemporary proponents of 'understanding' sociology. Elsewhere he equates phenomenology with a 'humanistic' approach - one that accepts that human beings cannot be studied in the same way as natural objects. If this were the beginning and end of what 'phenomenology' meant then this critique is irrelevant.

But 'phenomenology' obviously indicates something more than this. The other key element in phenomenological philosophy[9], which has also been stressed by all the phenonomenological sociologists, is the idea of phenomenological reduction. This process (also referred to as époché or suspension) denotes the putting aside of the presuppositions or preconceptions inherent in the 'natural attitude' or attitude of everyday life.

I think that it is this basic element in the programme which brings about difficulties in attempting to apply it to sociology. I agree with the concept of being 'true to the phenomena'; the point is that in sociology 'the phenomenon' is meaningful action. I contend that phenomenological reduction, albeit paradoxically, renders fidelity to the phenomenon impossible, since it does not recognize the way in which the observers' way of life necessarily enters into understanding. This is why phenomenologists, although successfully criticising behaviourism have not transcended it in the sociological context, and this has the even more paradoxical result that some of the things they say add up to something very similar to ideas of the behaviourists.

Jack Douglas' study of suicide has been chosen for detailed analysis because it has attracted a lot of attention, and so as to bring out the distinction between 'phenomenological' sociology in the sense of humanistic sociology, and actual phenomenological practice, before going on to examine some ideas in ethnomethodology, which represents the main way in which sociologists have attempted to apply the canons of phenomenology.

The Social Meanings of Suicide

Douglas' book raises a series of problems about the topic of suicide which relate to more general problems in the philosophy and practice of social science. Although I am in sympathy with its general aim of retaining fidelity to the phenomenon, I think that the way it sets out these problems is more misleading than illuminating, and is internally contradictory. Douglas does not succeed in elucidating the nature of suicide any more clearly than the accounts he criticizes, or than common sense suggests, and he does not indicate successfully how such an elucidation could take place. I have divided my discussion into a number of sections which I think represent the main theoretical concerns of the study, although the issues are actually put forward in a rather different fashion.

1) The definition and identification of the phenomenon in terms of intention

This issue is central to all the other problems Douglas raises, and illustrates the way in which a phenomenological approach can paradoxically result in a kind of residual sensationalism or belief that the availability or otherwise of sensations constitutes a problem.

Suicide is usually defined as the intentional taking of one's own life. I contend that this is the proper definition of suicide, and the best one available; it is the basis upon which we may identify suicides in order to describe and explain them. According to Douglas[10] this definition is inadequate. We need something more precise. It is taken to be axiomatic that such precision is necessary for the work to be properly scientific. The concept of intention is "vague"[11].

It is certainly the case that we are unable to see, hear or touch intentions. But this should not be a reason for excluding them from sociological enquiry. It is ironic that an approach which eschews positivism should put so much store on "precision". Attempting to eliminate certain objects from enquiry because they do not appear to be amenable to one's preconceived ideas about scientific practice represents exactly the scientistic attitude; that is to say, that of putt--ing the dictates of methodology before the requirements of discovery, or putting the cart before the horse. Douglas' reluctance to discuss intentions is analagous to, for instance, the behaviourists' reluctance to talk about minds.

Douglas has a great deal to say about social meanings, but the idea that meaningful behaviour means, primarily, intentional or motivated behaviour, is never acknowledged[12]. At the same time what is meant by "menaingful" is not made clear. It remains an unexplicated term whose meaning appears to be conceived to be impossibly profound; 'The concept of "meaning" is so fundamental to any consideration of human actions that it is exceedingly difficult to define it in terms of the factors. That is not to say that an attempt to define the concept is not worthwhile. It is simply to admit that the endeavor is far too much of a job in itself to make it part of such a work as this. The literature on the subject is immense and terribly involved...[13]

The identification of "meaning" with "intention" in Weber's work also goes unrecognised. Weber is seen as having concerned himself with the "inside story" or "meaning" of events as the actor himself sees them, that is to say, the actors' <u>own</u> account[14]. Douglas admits of Weber's "conviction" that the actions of men cannot be explained entirely in terms of what they think they intend. But the corollary of this - that explanations need to be put forward in terms of the actor's intentions, though not necessarily according to the actors' own account of what these intentions are - appears to be rejected, or at least is not raised any further. The proper approach to the Weberian principles is not made apparent by reference to post-Weberian sociologists. Both the Chicago criminologists and the "subculture" theorists are offered as examples of a correct interpretation[15]. Although the Chicago researchers made extensive <u>use</u> of the actors' own accounts, however, neither they nor the subcultural theorists always <u>accepted</u> them. (Burgess, for instance, made extensive use of the concept of rationalization; A K Cohen's concept of "reaction - formation" is based on the idea of unconscious drives).

The question of the actors' own accounts is especially significant with relation to the understanding of suicide, or the intentions that lie behind suicide. We do not have the opportunity to study suicides' own accounts, or only in the very selective form of notes or diaries. Douglas mentions the work of suicide prevention bureaux in this context. What is not made clear is that this is "action" research, and must necessarily be so. This does not mean that the research is useless; but rather that it does not have the same status as the continuous, "participant" research undertaken by the Chicago researchers with, for example, thieves and hoboes.

The study of suicide may be contrasted with these sorts of researches, therefore. It has particular methodological problems. I contend, however, that there is no difference in principle between understanding suicidal actions and any other sort of action. Actors' own accounts, although they are an important source, may need to be discounted. What is common to the understanding of all forms of actions is the <u>imputation</u> of intentions. "Imputation" must be the essence of the process of "understanding" social action, but it is treated with great caution by Douglas. Consider the following statements; 'Though no one has every empirically determined precisely what the social beliefs (or explanations) concerning suicide are, it seems probable from the literature on suicide that most people in the Western world have for centuries believed that an individual commits suicide because he is unhappy, especially because he is depressed or melancholy... There is, for example, the very old belief that loneliness or self-imposed isolation tends to make one depressed or melancholy and, therefore, much more likely to commit suicide[16].

I would not disagree with the content of what Douglas says but the way in which it is put illustrates the sort of blocks he is setting up for himself. The point being discussed is the nature of common-sense understandings of the reasons, or the factors behind the reasons, for suicide. The issue here is that of making imputations about these imputations; imputing statements about peoples' beliefs. No scientific purpose is served by being so tentative about such general or all-pervasive beliefs as this. After all, we are all qualified to make statements about such beliefs by virtue of being part of that general culture whose beliefs we are describing. No one has ever empirically determined what these beliefs are because an empirical investigation along these lines is inconceivable, let alone a "precise" empirical investigation; Likewise, what the literature on suicide suggests is quite irrelevant; it is misleading to imply that essentially intuitive ideas become available in this way.

2) The question of cultural homogeneity

Is it possible, however, to say that we are a part of a general culture for these sorts of purposes ? Common sense says that it is; I see no reason why this should not carry over into sociological enquiry. Douglas appears to reject this possibility, and this is probably the reason for his rejection of imputation. He calls it "the assumption of cultural homogeneity" and is concerned that this assumption is implicit in most existing studies of suicides, especially American ones[17]. He argues that this assumption is quite unwarranted; 'Sociologists dealing with deviant behaviour and other categories of social action have been argueing that there are fundamental differences in group behaviour patterns caused by the existence of sub-cultures that change the meaning of social acts'[18].

Subcultures certainly do change the meaning of social acts to the individuals involved, and in many fields of enquiry it is dangerous to assume that the culture is homogeneous. A K Cohen has argued, for instance, that theft in the delinquent subculture does not primarily represent an activity which is oriented to what we would normally conceive to be its natural end - accumulation of money or goods - but rather towards purely "negativistic, malicious and non-utilitarian" ends.

Suicide may mean different things to different groups of people. There is some evidence that suicide means different things in Japanese context from what it means here. But this does not mean that we should assume that it means different things without having contingent evidence that this is so.

It is unclear whether Douglas' assumption of cultural <u>hetero-geneity</u> - of subcultures - is supposed to be an independently ascertainable fact, (which is how delinquent subcultures came to be investigated) or whether it is true by definition; whether different social meanings mean different cultures. The difference is that the former is dependent on independent empirical investigation (Matza, for instance, has cast doubt on the existence of delinquent subcultures) whereas the latter is not; if we are unable to identify the "suicide subculture" then we must presume it to be hidden. In fact Douglas does not claim to have any such knowledge. '... a nation, especially the U.S., or even a large city must be considered to be a multiverse of sociocultural systems. Just determining what the significant boundaries of such systems are would be a problem obviously far beyohd the present knowledge and theoretical sophistication of sociologists[19]. It is implied in this context that discoveries about the 'social meanings' of suicide must await knowledge of what these 'significant boundaries' are. The difficulty with such an unspecific arguement as this - we do not know either what the categories of action are or where to find them - that it is so general as to be inapplicable. It makes any application seem worthless, as it appears to make the very concept of empirical discovery of social forms of action meaningless.

This argument by its nature cannot be proved wrong because of its internal consistency, but it is easy to put forward counter-argue--ments which suggest that this is an inappropriate way to set about the problem.

One such arguement would be that the act of suicide is an intrinsically individual phenomenon, and that it is found throughout society. This implies both that subcultural definitions of suicide are rare and that definitions of suicide are fairly general. This would suggest that intuitive or common-sense ideas about what these meanings are form an adequate basis for investigation - each individual is as well qualified to make statements about these matters as any other individual.

Thus, although common-sense imputation may be wrong, there is no particular reason to suggest that this is the case with reference to suicide. There is no reason to doubt, for instance, that suicide is intentionally killing oneself or that the reason why people kill themselves is because they are unhappy.

3. Degrees of generality

Generalisation is bound to involve a degree of inaccuracy or abstraction of the real-life phenomenon, and this constitutes a genuine problem for phenomenology and sociology in general. That is to say, there is an inevitable tension between accuracy of description, and abstraction, typification or generalisation in more universal terms. I do not think that Douglas successfully resolves this problem, and neither do I think it is soluble within which what is normally considered to be the phenomen--ological approach. The account given is contradictory, and this is necessarily the case.

This contradictoriness or ambiguity is found in the presentation of the case-study material. It is proposed that attention should be concen--trated 'on the analysis of those patterns of actions and meanings which seem, from a general survey of the literature in the Western world on suicide, to be more frequent'. This implies that generalisation is possible. On the other hand, '... for doing research on suicidal actions it would seem best, once one has thoroughly reviewed the literature on the subject so that he has some vague overall picture of suicide in the Western world, to generally work from the specific descriptive material up to the patterns - i.e. the abstractions'. By admitting of the possibility of an overall picture but nevertheless calling it 'vague' (there is no reason why we should not have a <u>clear</u> overall picture) the possibility of generalisation is cast into doubt.

In fact Douglas abstracts, typifies and generalises from case studies in exactly the same way as do the non-phenomenological sociologists he criticises. For instance, the idea that an "appeal for help" is a common reason for suicide is doubted for various reasons: 'If there are such cases, ... they must be rare; few cases seem to this author to fit the pattern[21]. A "pattern" is an abstraction or generalisation. A number of discrete <u>types</u> of suicide are pointed out, for instance, those motivated by the desire for reunion with a deceased loved one[22] a need for atonement[23], or as a means of getting revenge[24]. These "types" are

a) of the kind which are commonly accepted to be types, or reasons, for suicide, in common--sense terms,

and b) they are exactly the same, in level of gener--ality and, with regard to some authorities on suicide, in their substance, as non-phenomenol-ogical typologies.

Douglas' typology is no closer to the phenomenon, or the individual concrete events, than any other typology.

I contend that the contradiction between detailed accounts of individual cases and more general abstractions of large numbers of cases can in fact be resolved simply by appreciating that they are both normally needed in order to provide a good picture of the phenomenon. Accounts of individual cases are what we usually call the "case-study". The aim of the case study is to add colour to the sketch of the phenomenon provided by more general analysis, to put flesh on the statistical skeleton, to elucidate the phenomenon in qualitative rather than quantitative terms, in short, to retain fidelity to the phenomenon. Numerous case-studies exist in all areas of sociology; they are represented in all sorts of forms. One of the best-known examples of a case-study for instance, is the "life--history" type produced by sociologists like Clifford Shaw. Douglas himself quotes lengthily from non-phenomenological research on suicide of the type which is composed mainly of long case-studies.

His own account only gives one case-study in any length, at second hand. It is worth noting that there is nothing distinctive about the way these case-studies are interpreted. Three "rules" are given for analy--zing these reports[25]. First, it is unwise to place too much importance on any single statement. 'We must look for basic, recurring patterns'. Secondly, the words must be seen in their context, above all in the context which the communicator put them. Thirdly, we must be wary of assuming that a word or phrase used by the communicator means the same for him as it does for us.

These rules are so obvious as to be truistic. There is nothing distinctively phenomenological about them., I shall look at various forms of case-study in more detail, particularly with respect to the problem of how they relate to more general types of explanation. The important point is that, although case-studies are essential for retaining fidelity to the phenomenon, the adoption of a "phenomenological" approach does nothing in itself to solve the question or problem of this relationship.

Douglas appears to be aware of the difficulties inherent in his attempt to avoid generality; '... my whole method of analyzing social meanings leads us to try and see the general in the particular and the particular in the general; and certainly one of the most fundamental ideas of the (Zirkel im Verstehen) method is that the particulars are frequently comprehensible only in terms of the general context in which they occur, so that one must have some idea of the general context in order to understand the particular ...'[26].

That is to say, collecting unrelated facts is pointless unless we have some sort of idea of how they are to be put together. However Douglas appears to be firmly inductivist, to the extent that he is unable to take the arguement a step forward, to say that facts need to be gathered within the framework of some sort of theory; that "theory" or at least pre--supposition, must exist before data-collection takes place. 'The immediate goal before us ... must clearly be that of providing .. careful, comparative descriptions of many forms of social action. Only then can we get on with the general task of constructing more abstract theories to explain social actions'.[27]

Any inductivist-inspired account is bound to attempt to conceal the source of its theoretical concepts, since it is inconceivable to undertake observation without some sort of idea of what one is looking for. These concepts often arise from the investigators' own experience, for instance. Douglas correctly points out[28] that such a concealment is implicit with regard to Durkheim's use of his own experience in a number of topics which he investigated, presumably because of his inductivist princip--les and his distrust of a priorism. He does not realise, however, in spite of his opposition to "positivism", that these principles are wrong. If Durkheim ignored them, he was at least able to transcend them, albeit at the cost of some contradictory statements about methodology. He engaged in free theoretical speculation in spite of his inductivist conception of science. Douglas' insistence on working from the "bottom upward"[29] does not recognise that it is this image of science, not Durkheim's actual practice of it, which is wrong. This is the irony of Douglas' critique of Durkheim, in particular, and his treatment of the question of generality, in general; the critical approach to prevailing images of science does not draw attention to and apparently completely accepts the conception of science as a primarily inductive activity. His The Social Meanings of Suicide and all his other writings are peppered with attacks on positivism. A definition of positivism is never offered but two prominent varieties of positivism - logical positivism and logical empiricism - both rely heavily on this conception.

4. The use of official statistics

Douglas' comments about the use of official statistics attract most attention because they are intended to condemn most contemporary practice in sociology, not merely in the study of suicide. Existing enquiries into suicide, from the pre-Durkheimian researches up to the present day, have drawn heavily on statistical resources. Douglas rejects this, and by the same token, analagous research in other fields. The question of the use of statistics in sociology is therefore crucial to the phenomenological perspective within which Douglas is working. His position appears to be - although it is not explicitly stated - that the requirement of fidelity to the phenomenon precludes the use of statistics in the way that this has been carried out by most sociolog--ists. I believe that this is quite wrong - the two are not incompatible so long as the statistics are used critically.

It is important to realise that there are two arguments about the use of official statistics in The Social Meanings of Suicide, and that these two arguments are incompatible. This is not apparent immediat--ely from the text, in which the discussion is put forward as a single continuous theme. This may have brought about a lack of appreciation of the full complexity of the issues involved. For instance, Atkinson[30] and Wolfgang[31] both treat Douglas' critique at face value, i.e. as if it has only one dimension. Gibbs[32] correctly notes the paradoxical quality of some of Douglas' statements about suicide statistics but does not, I think, bring out as clearly as he might the polarity of approaches which brings this quality about. The only commentator to have done this is Hindess[33].

A. The anti-statistical argument

Much of what Douglas says with relation to the question of the use of official statistics implies that they are meaningless. This aspect of the argument follows directly from his rejection of the idea of observing intentions. We cannot observe intentions directly. Imputation is unjustifiable because the actor and the observer are assumed to inhabit different cultures. Therefore it is impossible to identify an action as suicide (or anything else) without a lengthy process of investigation - suicide statistics are unattainable. How can one, Douglas asks rhetorically[34], construct a "real rate" of "intention "? 'It is a fundamental part of the argument throughout this work that there does not exist such a thing as "a real suicide rate". Suicides are not something of a set nature waiting to be correctly or incorrectly categorized by officials. The very nature of the "thing" is itself problematic so that "suicides" cannot correctly be said to exist (i.e. to be "things") until a categorization has been made. Moreover, since there exist great disagreements between interested parties in the categorizations of real-world cases, "suicides" can be said to exist and not to exist at the same time, though this might seem a rather incong--ruous way of putting it'[35].

The conventional approach to the analysis of suicide rates or any other set of statistics - looking for systematic errors or biases and attempting to correct for them - is rejected. Douglas notes[36] that many European students of suicide have attacked the proposition that suicide rates would not be affected by any errors in the official statistics. They concerned themselves with the problem of hidden suicides, the systematically different methods of collecting and tabulating data on suicide in different areas, the problems of registration of death, etc. 'Almost all of these analysts, however, missed the critical problem that underlies the problem of the validity of official suicide rates: the systematic variation in the <u>meanings</u> of suicide'. Because the "meanings" are different, one is not comparing like with like. If there is no "real rate" we cannot elucidate biases; the term "bias" or "error" is meaningless. (These terms are usually placed in inverted commas to emphasize, I think, that they do not correspond to possible measuring processes).

B. The critical approach to statistics

Douglas' anti-statistical argument makes sense given the assumption of the inaccessibility of mental states. If we do not accept Douglas' interpretation of this, then the anti-statistical argument is destroyed - we are left with what might be called the conventional or critical approach to statistics. The critical approach to statistics - the approach adopted by most of the suicide researchers who Douglas criticizes - does not necessarily accept that the statistics are right. They may be inaccurate, unreliable, or invalid. But they assume that the statistics are, to some extent, corrigible. They may become more accurate or more reliable; we may usually conceive of a valid version. It may sometimes be impossible to do this in practice and suicide may well be one of the cases in which this is so. Suicide researchers may have been too hasty in assuming that the rates of suicide are corrigible. But in fact there appear to be many ways in which they are.

The problem with suicide rates is that it is not always obvious what is or is not a suicide. There is bound to be a proportion of apparent suicides which are in fact accidents and a proportion of apparent accidents which are in fact suicides. In Douglas' terminology, "the social meanings are problematic". Common sense, and the conventional or "critical" approach, says that some suicides are <u>more problematic than others</u>. The individual who hangs himself and leaves a note is almost certainly a suicide. Death from barbiturates or an "inexplicable" car accident are much more problematic. Douglas does not accept this distinction, and correspondingly, he does not accept that the proportion of problematic cases can be calculated.

Douglas' comments on this matter are not consistent but they add up to the idea that all suicides are <u>equally</u> problematic. For instance, there is a discussion of a case of Russian roulette, whose problematic character is noted[37]. Whether Russian roulette really is or is not suicide would certainly seem to be the most profound problem, the sort of problem which could never have a clear-cut answer. Douglas does not mention that this is not a typical form of suicide; conversely, it is implied that the case illustrates the weakness of suicide statistics in general. The problematic nature of deaths from drowning, barbiturate overdose and falling from high buildings is also noted[38] but the question of the proportion of these cases in relation to the total is not raised. This is presumably because the concept of a total or real figure is rejected. 'Determining the "real causes" of a single death is, in the majority of cases, a great problem in itself. Frequently, even a team of psychiatric "experts" on suicide can only arrive at more or less rough categorizations of the causes of a single death after many days, or weeks, of intensive investigation'.[38] Marilyn Monroe's presumed suicide is offered as an example of this. There is no consideration of the typicality of this death or others of its type, or of the fact that many deaths can be adequately classified far more easily than this.

It is pointed out[39] that in fact there are relatively few ambiguous official categorizations of suicide - open verdicts etc. 'It might be suggested here that the reason for this relative lack of uncertainty concerning intention is that actions that can be interpreted as suicidal at all receive their specific meanings in some good part from the meanings of the situations and the substantial self of the individual involved; if these are of the types that can be interpreted as suicidal in nature, or conducive to suicide, then the actions of the individual will be seen as "adequately interpretable" as suicidal. (For example, if an elderly woman dies from an overdose of sleeping pills, it might well be thought that this was an accident. But if one knows that her son committed suicide a few hours earlier, that she was deeply attached to this son, and that she was "brooding" for several hours before her death, then it seems unquestionable to the observers that this was a case of suicide.)[40] Within this passage the question of whether officials and others are <u>right</u> in being relatively certain in many cases is lost. The way in which these comments are made somehow casts doubt on the validity of the inferences made, although in fact there is not the slightest reason to doubt them on the basis of existing evidence.

What form could a critical analysis take ? Douglas provides some good examples; apparently it was found in one American city for instance, that there was a consistently low number of suicides each year because the coroner in that city did not label a case suicide unless a note was found[41]. Presumably the proportion of coroners who worked in this way could be estimated, and the statistics corrected accordingly.

Douglas actually contradicts his anti-statistical argument by attempting to do something of this kind himself, although not quantit--atively and not from original sources. He seeks to demonstrate that consistent biases exist in the suicide rates. For instance, there is an underestimation of suicides in the suburbs as opposed to the city interiors[42], underestimation of women[43] and underestimation of rural suicides as opposed to urban ones[44]. The middle classes are also underestimated with relation to other classes[45]. I do not know whether these arguments are correct. They are supported by no independent empirical evidence, and would seem to be too prioristic (in a way that invites comparison with similar arguments of Durkheim's which Douglas criticises.) The issue in question is that this sort of argument is

a) identical to the sorts of arguments employed by all suicide res--earchers hitherto (although many of them have come to the conclusion that the statistics are nevertheless broadly right) and this would appear to nullify Douglas' critique of them along these lines in the first part of the book, and

b) it contradicts his argument that suicide statistics are meaningless or incorrigible. It is impossible to argue both that statistics are unattainable and that they are wrong.

Suicide statistics may be wrong, but they are not meaningless; no more meaningless than statistics of voting of church-going. There are, of course, special problems associated with them - the non--availability of self-reports. This immediately puts them in a more problematic class than, for instance, statistics of juvenile delinquency. (Although they are probably a good deal less problematic than many other forms of social activity - rape, for instance). However problematic the phenomenon, assessing it in statistical form does not necessarily obscure it or do injustice to its nature. The critical or conventional approach to statistics should certainly be a very guarded one, and should never take the statistics forgranted.

The critical approach sometimes leads to assessing certain statistics as invalid, but this is because of the nature of the phenom--enon or the particular way in which the statistics are gathered, not because social statistics in general are invalid in principle.

An aspect of the question of validity is that sometimes, those social statistics which are shown not to relate to the phenomenon may relate to something else; in the field of deviance, for instance, there has often been discussion of statistics which measure social reactions to deviance, rather than the deviant phenomenon itself - "crime waves" which are the result of increased vigilance on behalf of the police or public, for example. This is a product of a critical approach to stat--istics - not the anti-statistical approach of the kind advocated by Douglas. A conceivable example of this sort of phenomenon would be changes in suicide rates which were a function purely of changes in coroners' practices. In fact Douglas does not switch his study to an examination of these practices, even though, Gibbs[46] and Atkinson[47] note, it would seem to be a logical response to those sections of the study which appear to seek the making of the classification procedures a topic. There is probably some convergence between phenomenologists like Douglas and the symbolic interactionist school of deviance theorists, who have in certain cases claimed that social statistics measure reactions to deviance rather than deviance itself. With regard to the way in which social statistics are used, however, there is no connection, or at least as far as a connection is conceived of, I think it is based on a failure to distinguish the different issues involved.

5. Common sense

Much of what has been said about Douglas' study might appear to represent an appeal to common sense. It is nothing of the kind - -simply to accept the common-sense version at face-value is to abandon any attempt at scientific enquiry. What I have done is to emphasize the way in which a common culture often means a common imputation of (intentional) activity. When we designate a certain behaviour as constituting a certain kind of activity, we can never be certain that we are right. The most "obvious" suicides, for instance, can turn out to be something else. On the other hand, there is no need to assume that we are necessarily wrong; because we cannot always assume cultural homogeneity does not mean that we must always assume cultural heterog--eneity. In particular, there is no point in doing this when all the evidence points to homogeneity rather than heterogeneity, to accepting our intuitive identification rather than questioning it, as is the case with the definition of suicide as intention to die, for instance.

According to Douglas[48], 'All of sociology necessarily begins with the understanding of everyday life...', that is to say, common sense necessarily forms the basis of sociological investigations. (Although the statement is ambiguous; it could mean that sociology should make everyday life itself a topic of research. This is suggested by some of Douglas' arguments, by other phenomenologists and by ethnomethodolog--ists. One variety of this argument, which I think Douglas half-sub--scribes to, is Schutz', who emphasizes[49] that sociologists should first explore the nature of the everyday world and only after this go on to study social constructs. Apart from the central problem of induction and phenomenological description, I believe that this conception of the way areas are selected for study is contradicted by the pragmatic notion of "problems in hand" which leads, or has lead in actual fact, to a reverse order of analysis, in terms of social "problems", for instance. The distinction between constructs of the first and second degree is also unrealistic, since "theoretical" knowledge of the social is itself surely part of our everyday social world. The making of the everyday world a topic is on the whole something closer to ethnomethodology: I think there are profound problems associated with this project but it would not be appropriate to go into them here). In this context Douglas is saying, I think, that common sense represents the source of sociological investigation. I agree with this premise, but I disagree with the methodology derived from it, which displays characteristic mistakes of the phenomenological position.

Douglas attempts to break through or put to one side preconceptions by the use of "phenomenological suspension" or "époché".

What does the attempt to put aside common-sense understandings mean in practice ? Statements about everyday life are put in inverted commas, so that intention to die becomes "intention" to die, an unhappy individual becomes "unhappy", a brooding woman is said to be "brooding", and so on. That is, they are put indirectly, so as to give the observer, and ourselves, a feeling of detachment or standing above everyday events. This is sometimes elaborated; for instance, the statement which proposes that suicidal explanations are more likely if the subject has just lost a close relative has "it is usually believed that" tagged on to the beginning.

I think there is value in this technique when it is used in the right context, that is to say, when we have a different view of social reality to advance. There are many reasons why this may be the case. We may believe that the actors' own accounts of the situation are wrong, or that the established view of the situation on the part of other investigators or the general public is wrong. The technique may be used in order to point out aspects of a situation which we think is bizarre, or to heighten our sense of the ridiculous. But in this case, no different view is offered. There is no evidence to show either that the actors' conception of the phenomenon, or the established sociological view of what these conceptions are, is incorrect. In the face of this, the attempt at detachment serves no purpose; it does not elucidate the phenomenon or offer us any new information about it. Also, I think this practice is rather objectionable because unless we analyse the context closely, it gives us the impression that we are discovering something, or being told something, new about the phenomenon. It appears to be scientific, in other words, merely by virtue of the form in which it is put, and this hides the fact that nothing new has been added to the original conception.

Common sense is often wrong; but there is no need to doubt it when there is no apparent reason for doing so. In any case, we cannot possibly question all of our common sense assumptions, with regard to suicide or any other topic, all of the time; there is bound to be some acceptance of common ground for the process of communication even to start.

Douglas is partly persuaded, I think, by the very counter-intuitive or anti-commonsensical character of his argument that common sense can never be wholly transcended. 'One simply has to rely upon his own vast experience in his culture to understand the meanings of the phenomena he observes within a given realm of experience[50]'. But the solution he suggests[51] is a fallacious one. 'He can and should treat his own experiences as something to be investigated'. Presuppositionless knowledge is unavailable, therefore we should make our presuppositions explicit. This concept is elaborated in the essay 'Understanding Everyday Life' in terms of Natanson's notion of 'phenomenological insp--ection'. The absurdity of the concept has been pointed out by Hindess[52]: 'Unless the sociologist is accorded the capacity, denied to ordinary mortals, to describe objects or events without the intervention of background expectancies or tacit knowledge, then his accounts must be subject to precisely the same type of limitation as those of other observers... For every sociologist's account we require a second account of how his background expectancies affected his account. This second account requires a third, and so on... We are faced with an infinite regression at no stage of which it is possible to escape the determination of seen but unnoticed background expectancies'.

It is obviously good practice to state one's biases or pre--conceptions as far as one is aware of them, but this self-awareness can never be more than partial; such a statement is an aide to object--ivity but objectivity can never be ensured by it.

In practice, the attempt to bracket basic ways of thinking about everyday life can never be done consistently. This is demonstrated by a basic ambiguity in Douglas' argument; first, everyday understandings are put aside in such a way as to suspend belief in their validity, such as references to (in inverted commas) an "unhappiness theory" of suicidal behaviour, but elsewhere their validity is tacitly admitted, so that in Douglas' own explanations for suicide all the supposedly predisposing motives are in fact associated with unhappiness. This point will be elaborated in chapter two.

Conclusions

It could be argued that Douglas in this investigation goes directly against the canons of phenomenology. My argument is that the actual phenomenon of suicide - an intentional act - is lost. However, the reason why this paradoxical position is reached would seem to be a reflection of the phenomenological reduction. The natural attitude of everyday life must, according to phenomenology, be put aside so as to properly observe the phenomenon; the point being stressed here is that the common understanding must be drawn upon in order to identify it. The phenomenological stance is bound to question too much, because sociological observation necessarily draws on the common understanding. Identifying social activity is an interpretative process, and too great a suspension of preconception will eliminate the observer's capacity to interpretatively understand, or observe or identify. Thus the essence of the phenomenon does not become any clearer, but is rather obscured.

Thus, it would be wrong to jump to the conclusion that Douglas is a bad phenomenologist, or to assume that genuine phenomenological sociology would tackle things differently. The paradoxical results of Douglas' enquiries are a necessary result of the phenomenological approach, and hence the contradictoriness of his account is not simply that his project was badly executed; it suggests that phenomenological sociology is a contradiction in terms.

This argument must not be misunderstood. Sociology cannot proceed from an unquestioned and unquestionable basis of everyday life. It must necessarily proceed, as Schutz and others have pointed out, in a reflexive sense.

The common understanding, or "common-sense", which forms the basis of sociological enquiry is often wrong, and sociologists will sometimes be concerned to point this out. The point is that although these understandings can never be established, they can be accepted with a far greater degree of confidence than the phenomenologists suggest.

The reason that this is so is that ways of life like the Western European or North American way of life which form the background to Douglas' investigation are pervasive. There is an infinite degree of variation within this way of life. When we are able to denote some specific aspect of cultural variation we might be concerned with a subculture, for instance. But we have to accept that there is a vast range of what we understand in an everyday sense which is more or less ubiquitous.

I would include in this category things like birth, marriage, death, suicide. These are not raw data or irreducible facts. They are given to us by our way of life. The point being made is that they are so fundamental to our way of life that it does not make sense to question them, at least not in a routine way.

The phenomenologists take exception to Durkheim's injunction to treat social "facts" as "things". The injunction should not be treated too literally, since it easily leads to a lack of appreciation of internal states, and a generally over-concrete view of social reality, as Douglas has pointed out along with many other commentators on Durkheim. Nevertheless, if the injuction is re-interpreted; 'treat the entities of social reality as they appear to us as real entities' then I think it is valuable.

The contrast with behaviourism is again useful. Douglas quotes at length[53] from Sorokin's attack on those like Lundberg who have criticised the use of terms like "meaning", "consciousness", "mind", "thought", on the basis of their supposed vagueness, or tries to reduce them to other terms like "verbal sign","verbal symbol", etc., and I agree with this attack. And yet Douglas is very reluctant[54] to use the term "thing" himself, in a way that is analagous to his objection to the term "intention". In fact, "thing" is a very apt term for expressing the everyday experience of the objects of social reality.

If we accept social phenomena for what they are then we should accept their existence, in ontological terms. Douglas' reluctance to do this makes it difficult for him to accept the reality of social phenomena. The phenomenological view of social phenomena is, effectively, idealistic, however much that might appear to contradict the stress on removing preconceptions and describing the phenomenon itself[55].

NOTES TO CHAPTER ONE

1. ALAN RYAN, The Philosophy of the Social Sciences, London, Macmillan, 1973, 143.
2. This misconception rather limits the ground of C I Dessaur's Foundations of Theory Formation in Criminology, The Hague, : Mouton, 1971, which covers a similar area to that being covered here. He states (p. 50) that the value of Verstehen is what it has to offer in the discovery phase of scientific activity, but feels that it would be useless for the validation of scientific statements.
3. ERNEST NAGEL, The Structure of Science, London : Routledge, 1961, 483.
4. This is strongly implied in, for instance, Alan Dawe's 'The Two Sociologies', B.J.S. 21, (1970) 207 - 218. One of the reasons for this confusion may have been the influence of Schutz's writings on contemporary ideas about phenomenological sociology. Schutz attempted to combine a verstehen approach with phenomenology, and in doing so gives the impression that the two are conceptually linked. Schutz's ideas are thus of course quite distinct from Husserl's phenomenology.
5. MAURICE ROCHE, Phenomenology, Language and the Social Sciences, London : Routledge, 1973, 321-6.

6. For instance David Walsh in 'Varieties of Positivism' in Paul Filmer et al., New Directions in Sociological Theory, London : Collier-Macmillan, 1972, who states; '... labelling theory continues to be firmly anchored in the depiction of the real world that underlies appearances even though it shifts its concerns away from positivistic categories of behaviour to meaningful categories of action. Thus, the examination of everyday deviant activity ... remains wedded to an account of what the social world is really like. It differs from positivism in conceiving of that world and the socially deviant acts within it as having a shifting precarious and negotiated character. However, it still treats of that world as "real" and "out there". Socially deviant acts, then, are part of that world and are available for examination as such... It is at this point that such theory parts company with sociological phenom--enology, which would deny the existence of the social world independently of the social meanings that its members use to account for it and, hence, constitute it'. (p. 49) Melvin Pollner has criticised labelling theory along similar lines. In my view [redacted] precisely the strength of labelling theory is that it realises that social activity is a meaningful and negot--iated process while at the same time accepting that it does make sense to treat the social world as a structured reality.

7. HERBERT SPIEGELBERG, The Phenomenological Movement, The Hague: Martinus Nijhoff, 1965, chapter 14.

8. P B MACLEOD, Phenomenology: a challenge to experimental psychology' Behaviourism and Phenomenology, edited by T W Wann, Chicago U.P., 72.

9. This has been disputed by Herbert Spiegelberg (op. cit., 691) who argues that it has never been common ground for those who are aligned with the phenomenological movement. He argues that the concept facilitates genuine intuiting, analyzing and describing of the given; 'it frees us from our usual preoccup--ation with "solid reality", which makes us brush aside what is "merely in our imagination" or "by convention only" as unworthy of our attention. This does not mean that the suspension of our existential beliefs is indispensable for

9. an unbiased stock-taking of our phenomena. What is all-important in phenomenology is that we consider all the data, real or unreal or undoubtful, as having equal rights, and investigate them without fear or favour'. This emphasises the phenomenological injunction of being true to the phenomenon, but it begs questions about social understanding.

10. JACK D DOUGLAS, The Social Meanings of Suicide, Princeton U.P., 1970, 167-9.

11. Ibid., 169, footnote.

12. 'Intention' has double significance here. All social action can be identified in terms of intention, or action oriented towards a certain end. However intentions are intrinsic to the act of suicide in a way they are not to other actions; that is to say, although most activities can be accidental, habitual, automatic, or traditional, which in various ways is indicative of a looser means/end relationship, if suicide is not intentional then it is not suicide. The example of suicide therefore high-lights problems of observing internal states generally, as well as intentions specifically; it unambiguously poses questions about understanding since the physical activity of death in itself tells us nothing.

    'Intention' is being used in this context in its everyday sense. It has nothing to do with 'intentionality', a term used by the phenomenologists. The term intentionality approximates to another more accessible phenomenological term, 'pre-reflective experience' so I have used this expression when appropriate to avoid confusion.

13. DOUGLAS, op. cit., 245.

14. Ibid., 236.

15. Ibid., footnote.

16. Ibid., 217.

17. Ibid., 155.

18. Ibid.

19. Ibid., 230, footnote 76.

20. Ibid., 284.

21. Ibid., 310.

22. Ibid., 300.

23. Ibid., 302.

24. Ibid., 310.

25. Ibid., 296-7.

26. Ibid., 242.

27. Ibid., 340.

28. JACK D DOUGLAS, 'Understanding Everyday Life', Understanding Everyday Life, edited by Jack D Douglas, London : Routledge, 1971, 5. Douglas calls Durkheim's use of his own experience 'the assumption of sociological omniscience'.

29. DOUGLAS, The Social Meanings of Suicide, 256.

30. MAXWELL ATKINSON, Review of Douglas, op. cit., Sociological Review, 1968, 405-6.

31. MARVIN E WOLFGANG, Review of Douglas, op. cit., Science, February 1969, 921.

32. JACK GIBBS, Review of Douglas, op. cit., A.J.S., 1968, 204.

33. Barry Hindess, The Use of Official Statistics in Sociology, London: Macmillan, 1973.

34. DOUGLAS, op. cit., 231.

35. Ibid., 196, footnote.

36. Ibid., 178.

37. Ibid., 187.

38. Ibid., 184-5.

39. Ibid., 230.

40. Ibid., 274.

41. Ibid., footnote.

42. Veterans Administration, Washington 23, D.C., Department of Medecine and Surgery, Medical Bulletin, March 1st., 1961.3.,

42. Quoted in Edwin S Shniedeman and Norman L Farberow, Clues to Suicide, New York: McGraw-Hill, 1957, quoted in Douglas, op. cit., 185.

43. DOUGLAS, op. cit., 215.

44. Ibid.

45. Ibid., 224.

46. Ibid., 211-12.

47. GIBBS, op. cit., 204.

48. MAXWELL ATKINSON, 'Societal Reactions to Suicide: Role of Coroners' Definitions, Images of Deviance, edited by Stanley Cohen, Penguin, 1971, 186.

49. DOUGLAS, 'Understanding Everyday Life', op. cit., 3.

50. Schutz, op. cit., 12.

51. DOUGLAS, Social Meanings of Suicide, 270.

52. Ibid., Natanson's version of the meaning of a "presuppositionless" philosophy in Husserl's sense is that, 'Presuppositions are rendered explicit through phenomenological inspection and so neutralized to whatever extent neutralization is possible in rational operations'. Maurice Natanson, Literature, Philosophy and the Social Sciences, Martinus Nijhoff, The Hague, 1962, 13-14. It is difficult to work out exactly where Douglas stands since he is aware of the infinite regress objection - 'It is important to note, however, that no matter how far back one goes in reducing (or bracketing) one's phenomenological "reductions", there inevitably comes a point at which one either accepts total solipsism and the impossibility of "knowing" anything of grounds his thought in some presupposed (common--sensical) experience'. ('Understanding Everyday Life', 22). Douglas says in the same context that Natanson has "clarified the whole issue", but surely this criticism makes the idea a non-starter.

53. HINDESS, op. cit., 11-12.

54. P A SOROKIN, Social and cultural dynamics, IV, 12, footnote 10, quoted in Douglas, op. cit., 243-4.

55. DOUGLAS, op. cit., 273, footnote 4; 'The word "thing is used here because it is a term normally used in our culture to refer to "motives" etc. Sociologists, psychologists, etc., generally use such terms as "variables", "factors", etc.; it simply seems preferable when talking about everyday meanings to use everyday ways of talking about it. (Just what the term "thing" means in general is clearly one of the most difficult questions in the English language).'

56. The idea of a contradiction between idealism and phenomenological suspension is derived from J N FINDLAY, 'Phenomenology and the Meaning of Realism', Phenomenology and Philosophical Understanding, edited by Edo Pivcevic, Cambridge U.P., 1975, 157. Ingarden, in On the Motives Which Led Husserl to Transcendental Idealism, Martinus Nijhoff: The Hague, 1975, suggests that Husserl moved to an idealist position from an originally realist standpoint, against the feelings of some of his colleagues.

CHAPTER TWO

ETHNOMETHODOLOGY

Fundamental problems associated with the consideration of the idea of empirical sociology are raised by the attempt to apply phenomenological principles to it. But a more significant set of issues are raised by the field of ethnomethodology. These need to be given at least equal prominence to those of phenomenology because it is this area which has produced most examples of substantive enquiry, whereas in phenomenological sociology most of the work has been programmatic.

Even stating the issue in this way begs important questions. Ethnomethodology, unlike phenomenology, is not a _methodology_, but rather the study of a particular area - "ethnomethods" - and is therefore not comparable in the sense I am implying. (Though it will be argued that in fact its practitioners often treat it _as_ a methodology). Furthermore, the essential question remains, of how we actually define "empirical" enquiry.

Ethnomethodology is essentially phenomenology as re-interpreted by Harold Garfinkel in sociological and experimental terms. There are a number of other influences, like aspects of linguistic philosophy and linguistics. It would be misleading to try to define too precisely in the first instance what this adds up to. As Filmer[1] points out, Garfinkel himself provides at least four lengthy definitions.

Understanding Everyday Life

There is one concern which is central - the examination of every--day life. A key idea of phenomenology is that the outlook of everyday life (which includes science) or "natural attitude" must be transcended by the "philosophical attitude" which is able to reflect on normally unexamined background assumptions or tacit knowledge enshrined in the natural attitude. The way the ethnomethodologists, for instance Zimmerman and Pollner[2], express this idea is that ethnomethodology makes everyday life the topic, not simply the resource, of its investigations.

'The operations that one would have to perform in order to multiply the senseless features of perceived environments; to produce and sustain bewilderment, consternation and confusion; to produce the socially structured affects of anxiety, shame, guilt and indignation; and to produce disorganized interaction should tell us something about how the structures of everyday activities are ordinarily and routinely produced and maintained.

A word of reservation. Despite their procedural emphasis, my studies are not properly speaking, experimenta;. They are demonstrations, designed, in Herbert Spiegelberg's phase, as "aids to a sluggish imagination". I have found that they produce reflections through which the strangeness of an obstinately familiar world can be detected[3]'.

This question, as well as illustrating a characteristic way in which Harold Garfinkel conceives the study of everyday life, also indicates an important ambiguity in his attitude towards the studies themselves.

Elsewhere he states that under no circumstances should his results be treated as 'findings'[4].

Let us take as an example Garfinkel and McHugh's wellknown 'experiment' in which students were instructed to act as boarders in their own homes. What the 'experiment' shows us is something about the significance of rules and conventions in everyday behaviour. When some--one acts in an unexpected way people react to him in a bewildered, indignant etc., way because he has broken a rule which is no less real by being normally unnoticed and unexplicated.

What the experiment could be said to amount to is a highly effective demonstration, in empirical terms, of the claim (made for instance by Winch) that all activity is rule-governed.

However by putting this sort of thing into the empirical category Garfinkel rather confuses the issue. Perhaps he is not necessarily mistaken in his strategy, but if he is really not in the business of providing 'findings' then the idea of an ethnomethodological programme, for instance, is meaningless. Having demonstrated an essentially conc--eptual point, it does not make sense to demonstrate it again and again.

Understanding everyday life - the'obvious'

The question of whether or not we are in the business of providing 'findings' or 'news' is of the essence of the concept of understanding everyday life. Studies, the results of which cannot, on the face of it, by any means add up to 'news' are seen as basic to the ethnomethodolog--ical project by Zimmerman and Pollner. They state we should study 'the obvious'[5].

To explore this idea - of studying essentially familiar social phenomena - it is worth initially illustrating the issue by providing an extract from a piece of ethnomethodological work which I think conforms to this criterion - 'Notes on the Art of Walking' by Ryave and Schenkein.

'The ways in which doing walking is an ongoing members' accomplishment can be initially appreciated by considering what we shall refer to as the 'navigational problem'. We take it that our data captures utterly routine occasions of walking under circumstances of heavy walking traffic, and that there occur in our data no instance of bumping into one another is no freak happening. The avoidance of collision is a basic index to the accomplished character of walking; that participants to the setting 'manage' not to collide with one another or with some other (e.g. physical natural boundary is to be viewed as the product of concerted work on the part of those co-participants'[6].

'The task of determining the procedures by which members decide who is walking with whom in some setting was indicated by the observation that we were able, when relevant to do so, in various degrees of agreement, to come to determinations about the matter. It became apparent that what was essentially involved was some membership ability to distinguish between 'alone' and 'together'. If these are members, the primary concern becomes discovering the procedures for applying these categories by member observers.

Our task then becomes one of demonstrating some attributes of what might be called 'togetherings' and 'alonings'; that is, we now could focus upon combinations of setting and activities of which a constituent feature is 'togetherness' and 'aloneness'; the procedures and observations of walking - alone and walking - together then, have and other settings where the dimension revealed by categories 'alone and 'together' is prominent'[7].

Initially it should be made clear that there is nothing wrong with the injunction 'understanding everyday life' is itself - in fact it could be said to be essential to what we generally understand as scie--ntific enquiry. We cannot take everyday phenomena forgranted, whether natural phenomena or social phenomena.

However this does not legitimate the study of the socially 'obvious' in the sense implied here.

What Ryave and Schenkein are actually <u>doing</u> is quite clear. 'The task of professional sociologists in the terms of ethnomethodology ... becomes a matter of <u>not</u> taking forgranted what is typically taken - for - granted at the level of everyday actions. They must make the accomplishment of adequacy in meaning (of sense) in everyday, commonsen--ional explanations, itself a topic of sociological enquiry. This seems, to Garfinkel, "unavoidably to treat the rational properties of practical activities as 'anthropologically strange'"[8].

Seeing the everyday world as 'anthropologically strange' is probably more characteristic of phenomenological sociology and eth--nomethodology than anything else. It is why ethnomethodological texts normally contain a very high proportion of inverted commas.

As was emphasised in chapter one, the common understanding is not unquestionable. But we need to have some reason for such an examination, or some standpoint from which to make it. We need to disapprove of it, or find it bizarre or ridiculous, or to find it in some way exceptional. We do not normally think about the institution of money, for instance, but it is quite legimate if we are unhappy about it to draw attention to the way that in order to travel on public transport we are made to hand 'pieces of metal' to the conductor and get handed a 'piece of paper' in return, in order to hold the institution up to ridicule.

We may draw attention to aspects of everyday life - ways of walking, talking, greeting each other etc., which when looked at from an 'outsider's' point of view, seem in some way strange. A vast range of artistic activity of all kinds can be seen as doing something of this kind - depicting everyday life from a particular standpoint, or bringing out some aspect of it for special attention. In fact the examination of everyday life amounts to something which we normally see as being much closer to artistic activity. The most dramatic example is probably something like surrealism.

But there is always still some sort of standpoint, some implicit comparison at the very least. We cannot have an ethnography of everyday life, in the sense suggested by Mary Douglas, who links anthropology and ethnomethodology to some extent along these dimensions[9]. I do not think one can even have an ethnography of primitive societies, since it assumes an impossible pre-theoretical standpoint. But at least the ethnography of primitive or different societies makes sense in that it is concerned with activity that is recognizably different. It is of interest from our point of view that people believe trees have souls, as it is interesting for people from different cultures that people in our culture talk into the air while kneeling, especially on Sundays.

One of the important elements of the ethnomethodological movement, again expressed explicitly in Zimmerman and Pollners' paper, is the interaction of lay and sociological understanding in society. The sociologist investigates the social world from a back-ground of normally unexamined prereflective knowledge about the social world, and lay social understandings are themselves partly composed of social scientific or semi-social scientific understandings - ideas about the relationship of broken homes to troublesome behaviour, for instance. Part of the sociological enterprise is concerned with examining these understandings and in this sense making them a topic of enquiry.

This is a situation in which it is possible to have an opinion about an understanding, to doubt it (although when these sorts of understandings become part of a way of life it is sometimes very difficult to see them as things which <u>may</u> be doubted). But this is not the case with the sort of enquiry which is suggested by the idea of the study of the essentially unexceptional features of everyday life.

It has been suggested that the examination of lay understandings - demistifying, debunking, etc., is fundamental to ethnomethodology. David Walsh, for instance, stresses the distinction between social problems (activities which members of a society find problematic) and sociological problems (activities which sociologists find problematic, regardless of whether or not they are socially problematic).[10]

Sociologists should not confine themselves to the study of social problems (although it would be wrong to see what Walsh is suggesting as central to ethnomethodology) but the point is that the sorts of activities which constitute sociological problems can only be those sorts of activities which sociologists find in some way exceptional. This is not the case with something like walking. What is relevant here is the sociologists' membership of the way of life he is investigating - he is as much a part of it as any of its other members. The more the activity being investigated is part of his way of life the less he can genuinely find it exceptional or problematic.

Social "facts" are not like natural objects, in that to only a limited extent do they exist independently of the observer. There is in no sense a finite range of social phenomena which we can systematically describe. In order to be aware of a social phenomenon at all we have to be able to see some significance in it.

What the sociologist who examines normally unreflective common-sense ideas about the family, class, etc., is doing is to put, quite legimately, activities which may not immediately be seen, as socially significant under sociological scrutiny. When he attempts to examine activities which, as far as he can see, are completely insig--nificant, then what he is doing is to fail to recognise that he, like all the other members of society, is a member of a way of life.

The sorts of things he describes are of no genuine interest to himself or any other member of society (although they could be interest--ing to someone from a different society).

It should be reiterated that presenting the everyday world as anthropologically strange has rationale if it is done in a graphic or entertaining way. This is the only way we can actually be made to think about the familiar. I said that the dramatist needs a standpoint; if he is effective enough, then this standpoint need not be very explicit. 'Ethnomethodology' of this kind would make sense, once it was realised that its practitioners were not in the business of actually giving us sociological information.

The idea of a distinction between giving information and painting a picture or attempting to change our world view in this context is taken from Frank Cioffi's paper 'Information, Contemplation and Social Life'[11]. Cioffi discusses various sociologists like Goffman and Riesman who provide us with different perspectives on everyday life essentially by supplying metaphors or analogies with which to typify aspects of society. I do not think that this is intended as an attack although it may be to some extent launched against ways of looking at these sorts of studies which are misconceived.

Neither I am seeking to say that there is anything intrinsic- -ally wrong with these studies. What I wish to point out is that to see them are being concerned primarily with giving us information is wrong. Sociologists like Goffman are in a sense part of the forerunners of ethnomethodology. What Goffman does in The Presentation of Self in Everyday Life is successfully to apply the metaphor of life - as - drama. Successfully, that is to say, not necessarily that one agrees with his picture. My own view is one of being rather sceptical about it since life can obviously be looked at equally effectively in any number of different ways. By successfully I mean that he clearly conveys his message; what he is getting at is made quite clear.

What those types of analysis which are generally called 'ethnomethodological' studies of everyday life do is to emphasise the information - giving mode of communication and to abandon the pictorial or metaphoric. Thus rather than Goffman's extensive use of figures of speech and rather literary stype we have, in the case of Garfinkel and Cicourel at least, very colourless, rather mechanical prose. There is an obvious reason for this from the ethnomethodological point of view (though from my point of view a bad one); the use of figures of speech clearly demands that aspects of the readers' under- -standings are taken forgranted, and therefore that they form part of a common way of life. This highlights the category-problem. Goffman's writings may be misleading in that they conceal what is essentially an attempt to provide a world-view under the guise of giving information but at least we have the option. The ethnomethodologists of everyday life do not allow us this kind of possibility, since they normally avoid as much as possible any definite viewpoint, or letting that view come through. This tends to persuade us further that they are in the business of giving us information.

However, exploring 'everyday life' does not necessarily mean exploring the 'obvious'. The essential distinction is one between looking at familiar activities in an unfamiliar light and looking at unfamiliar elements in familiar activities. The former adds up to making the everyday world 'anthropologically strange'. The latter is quite different - it refers to pointing out elements of everyday life of which we were not previously aware. Sacks and Schegloff, for instance, have examined[12] the way people 'close' telephone conversations - the sets of 'strategies' they use to bring the conversation to an end. What this sort of analysis, which is widely carried out by social psychologists, does is to tell us something new about something which is familiar to us, rather than simply to distance ourselves from the familiar. Sacks and Scheglofffs' project, for instance, must be contrasted with the 'examination' of talking qua talking; 'informing' us that conversations normally require at least two people, for instance.

Once we are in the business of examining the un-obvious then we are closer to a realistic approach and further from ethnomethod--ology. Once we start to adopt the attitude that certain elements of an activity interest us, then we tacitly admit that there are other elements which are 'obvious', 'uninteresting' etc., which I think is inevitable but which goes against the philosophical imperative which these ethnomethodologists are so concerned to bring out.

Nevertheless, to the extent that these sorts of studies - in the general area of microsociology, conversational analysis, the study of non-verbal behaviour etc. are findings, I think they constitute a substantive contribution.

What is not accepted is that ethnomethodology amounts in any way to a critique of non-ethnomethodological sociology. Here again there is a fundamental ambiguity in Garfinkel's position, which perhaps relates to the ambiguity as between the empirical and the philosophical approach.

According to Garfinkel: 'Ethnomethodological studies are not directed to formulating or arguing correctives; .. Although they are directed to the preparation of manuals on sociological methods, these are in no way supplements to "standard" procedures, but are distinct from them. They do not formulate a remedy for practical actions... Nor are they in search of humanistic arguments, nor do they engage in or encourage permissive discussions of theory'[13].

Elsewhere he states that ethnomethodological studies abstain from all judgements of adequacy value, importance, necessity, practicality, success or consequentiality - a procedure which is referred to as 'ethnomethodological indifference'[14].

Giddens notes that this attitude upon which some ethnomethod--ologists, including Garfinkel himself, insist, is rarely maintained with the same nonchalance that it would seem simple to preserve if there really were the logical gulf that was claimed to exist between ethnomethodology and sociology[15].

The idea of ethnomethodology as a critique is expressed most strongly by the authors of New Directions in Sociology. Paul Filmer states frankly that this might contradict Garfinkel's voiced caveat along the lines quoted, but 'Nevertheless, proceeding in terms of D H Lawrence's dictum for literacy critics, 'Don't trust the writer, trust the tale', I want to examine what Garfinkel has written and how it might be used, rather than what he says he has written and how it ought not to be used[16].

Ethnomethodology as a critique - the sociology of organisations

The concept of ethnomethodology which those commentators have in mind, is I think, primarily that aspect of it which is suggested by the concept of 'methodologies' in everyday life.

A recent statement by Garfinkel on the origins of the term 'ethnomethodology' is significant in this context. He states that the term arose through an observation that people were carrying out research into ethnobotany, ethnopharmacology, ethnomusicology, etc. Ethnobotany, for instance, refers to the way in which ordinary members of a society envisage or classify the plant kingdom. This led to the idea of ethnomethodology, the ways in which people come to decisions, construct "theories" in everyday life.

Much of this work is, in a general sense, about organisations and bureaucracies. It is concerned with the way officials classify people or fit them into categories - the way members of suicide prevention bureaux, for instance, develop more or less explicit theories or typologies of the suicides they come across, or the way teachers classify their pupils and develop 'theories' about their performance. To a large degree classification procedures constitute a routine or relatively automatic process, and in this sense the ethnomethologists are concerned to bring these procedures out into the open or make them explicit. This is the way in which this concern is bound up with the general concern with the way the everyday world is built up.

I do not think that there is anything illegitimate about the study of 'ethnomethods' in this sense, but I think that the way it has been presented as a critique is completely unfounded. In studying bureaucracies and organisations this mode of ethnomethodology is necessarily involved with the question of how officials produce statistics. What I wish to re-emphasize in this context is that the ethnomethodologists do not succeed in demonstrating the sense of a critique of the way non-ethnomethodological sociologists have generally treated these statistics - that is to say, not necessarily using or accepting them, but being able to use expressions like 'bias' or 'inaccuracy'.

In only a very limited sense are these studies 'findings'. They do not actually tell us a great deal about suicide prevention bureaux, about classrooms, or hospitals. It could be argued - Garfinkel would presumably argue - that they are not supposed to do this. If this is the case then it represents an extremely complicated way of demonstrating a conceptual point; the misconception must lie in the way in which the enterprise is worked out.

I contend that Aaron Cicourel, for instance, does not actually 'tell' us anything about the social organisation of juvenile justice[18]. even though it is sometimes seen as providing a major contribution to our understanding of this area. Such information that is given tends not to bear specifically upon the phenomenon - it could usually be said about a whole range of other organisational practices; it is 'information' about bureaucracies and organisations <u>in general</u>.

Categorization

The main point that Cicourel demonstrates in his study of the organisation of juvenile justice is that the officials involved - police and probation offices - classify or categorize the juveniles in all sorts of different ways, and that at least some of the time they perform this categorization in an ad hoc or rough-and-ready way. Most of Cicourel's work on education adds up to the same thing - teachers classify pupils into 'good' and 'bad', 'failures' and 'successes', etc.

What needs to be emphasized is that this is the end as well as the beginning of what comes out of Cicourel's work. He does not show how the officials of various kinds are biassed i.e. he does not specify criteria by which officials place individuals in one category than another, and he does not show what effects, if any, this has on the individuals being classified.

Various areas of possible bias are hinted at in the study of juvenile justice. One of the areas of great significance with relation to the question of juvenile delinquency is whether officials are biassed with regard to the social class origins of juveniles - whether greater discretion is exercised in relation to middle-class offenders. It is reported that the chief of police of a wealthy Chicago suburb revealed from his unofficial files that adolescents in his community engaged in delinquent acts on a par with those of the 'worst' of Chicago[19]. Cicourel states that, with regard to his own study, 'My observations suggest police and probation perspectives follow community typifications in organizing the city into areas where they expect to receive the most difficulty from deviant or "difficult" elements to areas where little trouble is expected and where more care should be taken in dealing with the populace because of socio--economic and political influence. The partition of the city into areas of more or less anticipated crime provides both police and probation officers with additional typifications about what to expect when patrolling or making calls in the areas'[20].

However Cicourel does not actually elucidate how, if at all, these preconceptions are translated into actual bias, i.e., how far juvenile offences from middle-class areas are treated with more discretion that those from working-class areas.

In particular, the factor of city corruption is not explored. Cicourel demonstrates that the organisation of juvenile justice, and the bureaucracy in general, was very corrupt in one of the cities he examined, which would suggest that the children of favoured or politically important people would be treated with much more discretion if involved with offences. However this is not demonstrated: the question of varying degrees of corruption on the administration of individual cases is not explored. Another area of bias touched on is the officials' use of the sociological/common sense concept of the broken or "difficult" home. Cicourel correctly points out that 'factors' like these in juvenile delinquency have been devised in an ad hoc way[21]. However he does not show in what proportion of cases the probation officers involved, applied them in an ad hoc fashion. (Which is implied by the statement; 'the ways in which such factors are identified and are invoked by the practitioner to justify his actions')[22]. He does not show, for instance, if self-fulfilling prophecies were brought about; if the officers' preconceptions about "difficult" homes caused them to treat the juveniles in a different way and to produce problems which might not have been there, which I would think is the interesting question.

All this resolves around the question of whether the proportions of erroneous or ad hoc classifications can be calculated, or whether, like Douglas, he rejects the reality of social statistics entirely. Hindess has pointed out[23] that although Cicoural frequently draws attention to the practice of classification by 'fiat' (the practice of arbitrarily putting people into predetermined categories which is obviously associated to a greater or lesser extent with all bureau--cracies) he draws no distinction between classification by fiat in a majority of cases in which case the statistics are useless, and classification by fiat in a minority of cases, in which case it can be ignored or allowed for.

Cicourel has replied to this charge in the 1976 edition of his book. He states that 'contrary to remarks by Hindess, my research on juvenile justice can be used to improve crime statistics and to estimate and to control possible sources of error. But such improvements would not eliminate the problems inherent in using official statistics, nor would such improved statistics take the place of controlled field research on crime and delinquency. My concern with the limitations of descriptive statistical accounts and the observer's reports are designed to call attention to routine and normal sources of error or misclassification that are inherent in data-reduction procedures. Hence claims to knowledge must acknowledge and include efforts to understand the collection and organization of research materials'[24]. He also disassociates himself from Douglas' ideas about the "non-reality" of social statistics[25].

It should be said that although this is clear it is contradicted by other statements which imply that social statistics are incorrigible[26]. Furthermore, it makes the point of the accounts of organisational practices obscure. The phrase 'draw to attention' is significant in this context; the question is whether having these practices drawn to one's attention goes far enough or whether in itself it is necessary. What Cicourel's study, and analagous studies of organisational "ethnomethods", tend to add up to is effectively a demonstration that, in bureaucracies, officials classify and categorise people, sometimes in an _ad hoc_ way. The question is how far this adds up to a "finding"; my own view is that it is a necessary truth which is intrinsic to all bureaucracies. Another way of putting this is that it calls into question the significance of these sorts of studies with respect to any specific organisational area. Cicourel, although he provides a great deal of descriptive material, does not tell us much about the social organisation of _juvenile justice_; much of what he says could be applied equally to a whole range of organisations.

Much the same can be said, for instance, about Cicourel and others' work on education[27]. Most of this is devoted to analysing the ways in which teachers classify their pupils, often in an arbitrary or ad hoc way. Relatively little attention is paid to exactly how ad hoc the classification procedures are with respect to one another, or how far and with what impact this affects the pupils. The question of interest would surely be how far the teachers actually create successes and failures by the pupils' becoming aware of their definitions and categorisations - a question examined, for inst--ance, by J W B Douglas. What Cicourel and his colleagues do is to avoid asking these sorts of questions in favour simply of pointing out that categorization takes place. All this does is to abandon the sociologically interesting questions for ones which are obvious to most people anyway in a direct sense from their involvement with these sorts of institutions.

Much the same can be said of the work of Garfinkel, Sudnow, Bittner and others on suicide-prevention bureaux, record-keeping in hospitals etc. For instance, Garfinkel's comments on the Los Angeles Suicide Prevention Bureau note how the members of the organization classified suicides 'realistically' 'practically' or 'reasonably' according to their common-sense models of 'rational' reasons for suicide. This is presented as 'finding', which one cannot dispute, but which, in a very similar way to Douglas, implies that there is something illegitimate about the members' use of their common sense in this context. They do not simply have (common--sense) reasons for deciding one way or another - they can also have good reasons or bad reasons, and hence can construct good classificat--ions or bad classifications about which we are entitled to comment.

Ethnomethodology and the 'documentary method'

One of the most explicit ways in which Garfinkel proposes a critique of non-ethnomethodological sociology is with regard to the 'documentary method'. What this refers to is the way in which sociologists interpret or impute things (for instance, motives) on the basis of finite evidence.

'There are innumerable situations of sociological inquiry in which the investigator - whether he be a professional sociologist or a person undertaking an inquiry about social activities in the interests of managing his practical everyday affairs - can assign witnessed actual appearances to the status of an event of conduct only by imputing biography and prospects to the appearances. This he does by embedding the appearances in his presupposed knowledge of social structures. Thus it frequently happens that in order for the investigator to decide what he is now looking at he must wait for future developments, only to find that these gestures in turn are informed by their history and futute. By waiting to see what will have happened he learns what it was that he previously saw. Either that, or he takes imputed history and prospects forgranted. Motivated actions, for example, have exactly these troublesome properties'[28].

The argument here is similar to that used in respect to Douglas' about imputation and about imputing intentions specifically; it places into a problematic category something which, I believe, it does not make sense so to do.

The argument, developed in chapter three of Garfinkel's Studies in Ethnomethodology is put forward in such a way that it appears to be backed up by an experiment. It should be made clear that this is not in fact the case.

The experiment, carried out by Garfinkel and McHugh, consisted of the running of what the subjects believed was a counselling session with trainee counsellors, under the condition that the counsellors only gave the answers either 'yes' or 'no' in response to the subjects' questions. The order of the yesses and noes was programmed in advance on a random basis. Thus it bore no relation--ship to the subjects' questions. However the subjects interpreted the responses, as meaningful replies to their questions.

The experiment is in fact a striking demonstration of the way in which people can project meaning into an activity, on the basis of hardly any evidence. It shows the power of peoples' interpreting, or in this case over-interpreting, facilities.

But it does not in any sense prove the general point about imputation. People can be shown to impute motives and meanings on the basis of very bad evidence but in normal situations they do so on the basis of much better evidence. This does not mean that they are necessarily right in the conclusions they come to, but the decisions do not have the arbitrariness which Garfinkel, general--ising from the experiment, implies is the case.

This means that the documentation of everyday activity, both for members and sociologists, does not set up the sorts of problems suggested. This relates to the necessity of realising that, in most situations and contexts, we are concerned with shared or common ways of life, with common understandings which do not require questioning.

Garfinkel's ideas about the 'problems' of documentation have formed an important part of Cicourel's arguments, which have been in the forefront of the ethnomethodology - as - methodology movement.

He draws a strict distinction between his own style of presentation of descriptive accounts, and the "impressionistic data" of conventional research. The essence of Cicourel's objection to this is, I think, expressed in these sentences: 'The analysis is a constructed superstructure that represents the researchers' attempt to characterize a network of social relationships with a vocabulary presumed to be a clear-cut depiction of the organizations' activities and purposes. The verification of what is observed or inferred is recommended without reference to the interpretation of the actor or the researcher of each others' environment of objects according to their separate and joint common sense and scientific rules of procedure. The reader is invited to indulge in the adequacy of the rendered interpretations without explicit recourse to the steps that were followed in deciding the relevance of the materials quoted or observed by both actor and observer'[29]. The points which Cicourel objects to, to paraphrase his account, is that we cannot assume a common vocabulary as between the actor, observer, or reader; we cannot assume any identity between what the observer finds interesting and what the reader finds interesting.

My objection to this is that this objection is itself miscon--ceived; it does not recognise that the observer and the reader are inextricably part of a way of life which is for the most part common to both of them, that this way of life is largely expressed in terms of language, and that it is unnecessary and impossible for the observer to transcend it or make explicit the ways in which it impinges upon the environment he is describing.

Thus 'impressionistic data' are legitimate - we are entitled to ascribe a degree of validity to the observers' descriptions, and we are not forced to assume that there are insurmountable problems of comprehension which stand between the observer and ourselves. This is precisely because we know something about his way of life, and actually he is aware of this.

What Cicourel actually does is to present extremely lengthy, verbatim transcripts of the material - interviews between young people and policemen or probation officers, without indicating which parts are relevant.

A great deal of material is included which has no apparent relevance to the problems being investigated, or to put it another way, it is not clear what the central problems are. Certain issues do seem to emerge from the reports; for instance, the significance, in terms of the officials' future decisions regarding the juveniles, on the basis of whether they demonstrated a 'defiant' attitude while being questioned, or whether or not they were known to come from a broken home. But these themes are essentially implicit --they are in no sense conclusions.

In this sense ethnomethodology (and in this respect Cicourel's work is not untypical) is again balanced between making conceptual and empirical claims. It is unable to produce definite empirical findings, since calling something a "finding" assumes that it is possible to differentiate in terms of relevance or familiarity.

Cicourel's proposed solution - to make the observer's "encoding and decoding" procedures part of the research enterprise - is not feasible.

Cicourel has now claimed[30] that infinite regress problems never in fact materialize in his account of juvenile justice. He is probably correct in saying this, though I think it is significant that in this context a number of caveats are expressed. 'My daily activities with police, probation, school and court officials, as well as with juveniles and their families, required that I describe details that also produced selectively organized and remembered discourses and texts. This meant asking the reader's indulgence when judging my observations and assumptions about rude or impolite conduct by juveniles or policemen, aggressive or passive behaviour, laughter or tears, and the like, when trying to convince the reader that my remarks should be honored'[31]. 'Throughout the book, I assume that the researcher and reader are familiar with my use of American English. I force the reader to rely on his or her imagination or memory to understand my remarks that a juvenile's tone of voice is often the basis for a police officer or probation officer or judge calling the juvenile's behaviour "in defiance of authority" or an indication of "a bad attitude.."[32]. 'Throughout my study of juvenile justice practices I was sometimes unable to obtain the kinds of details that seemed essential for the methodological strategy I followed... I was often unable to obtain verbatim tape recordings of discourse that would reveal pauses, interruptions, extra-linguistic particles like uh, oh, ah, and the like that can provide us with a glimpse of the contingent character of police-juvenile or police-probation encounters'[33].

The point is that these sorts of caveats are unnecessary. They demonstrate that the only difference between conventional and ethnomethodological research in this sense is that ethnomethodologists make explicit what in conventional research remains implicit. Thus the ethnomethodologists' charge that conventional sociology is essentially naive in the way in which it approaches its objects of inquiry, could still be said to be correct, but the point is that it is a 'naivity' which is justified; ethnomethodology raises a new set of problems without indicating a way out.

What Cicourel actually does is to cut off the descriptive material at an unnecessarily remote level of relevance. He acknowledges that it is impossible to represent the infinite variety of ways of describing any social situation without being able to decide upon what is obviously relevant.

Language and the use of concepts

A few remarks about the ethnomethodologists' concept of language are in order here. Cicourel's comment to the effect that he assures the readers' familiarity with the use of American English is fundamental to the set of problems which ethnomethodology raises. It is a valid point that the very choice of language constitutes a massive assumption about the cultural audience being addressed, and hence about shared meanings and understandings. It may seem a trite point, but the use of the English language for an account in itself subsumes the decision not to present the account in, say, Chinese. One is presupposing a shared way of life and, in the sociological context, a shared knowledge of the concepts of sociology.

The ethnomethodological concern with language, insofar as it is expressed as a critique of non-ethnomethodological sociology, is partly to do with the use of these concepts - 'role', 'culture', 'socialization' etc. It sees their use essentially as something problematic.

I think there is a valid aspect to this concern if it is directed at the idea of a conceptual language, in a programmatic sense. Phillipson, (following C Wright Mills) notes the problems which this entailed in the field of deviance: 'Concepts drawn from biology carry ... horizons of meaning which liken society to a biological organism; an interesting example here would be the term 'social pathology' which was used for many years as a basic orienting concept in the area of social problems; its horizons of meanings, drawn from its origins in medicine, have very particular implications for the kinds of action programmes to be developed in relation to social problems'[34].

The moral is that to state sociological interests within too definite a framework - 'social pathology', 'role theory', 'the problem of order' etc., begs questions about its relevance and effectively creates a paradigm of explanation, and even possibly reacts upon its subject-matter; if deviants are treated as being 'pathological', for instance, they can come to act in a 'pathological way'.

However it is fair to say that the concepts of ethnomethodology - indexality, glossing, formulation etc. represents a prime example of this tendency. More significantly, the argument about the scope of concepts tends to be misconceived in a way which reveals contra--dictions in the way concepts are seen.

Cicourel[35] has devoted a lot of attention to an examination of common sociological concepts like 'status' and 'role'. The gist of his argument, if I understand it correctly, is that these concepts are extremely vague - sociologists have never worked out in detail what exactly counts as 'status' behaviour or 'role behaviour.

The question is, whether they ought to pay any more attention to it. I think the arguments against doing this are at least as good as the arguments for. A neutral observation language in sociology is unattainable both in principle and in practice. It is unattainable in principle because the criteria for identifying social activities are themselves the property of ways of life; it is part of my general argument that this is insurmountable and in itself not as problematic as the phenomenologists and ethnomethodoligists suppose. Another way of looking at the problem of neutral observation languages in sociology is by looking at specific examples of ambiguous or loaded expressions, like democracy, freedom etc. - it is quite obvious that substitutes very quickly themselves come to take on these sorts of ambiguous connotations; for instance 'scientific' labels for forms of mental illness which are often as least as perjorative as the 'unscientific' names that preceded them.

Terms like 'role', 'status' etc. are highly ambiguous but this ambiguity represents their heuristic value. The essence of the ethnomethodologists' criticism of everyday use of terms like this is that they <u>are</u> used in an everyday, unreflective way, rather than themselves made a topic of enquiry. I think this makes sense when some specific agreement appears about the use of such terms but otherwise is confusing and has the sorts of paradigmatic consequences which the ethnomethodologists would presumably wish to avoid[36].

NOTES TO CHAPTER TWO

1. PAUL FILMER, 'On Harold Garfinkel's Ethnomethodology', Paul Filmer et al., New Directions in Sociological Theory, London: Collier-Macmillan, 1972, 206.

2. DON ZIMMERMAN MELVIN POLLNER, 'The Everyday World as a Phenomenon', Understanding Everyday Life edited by Jack Douglas, London: Routledge, 1971.

3. HAROLD GARFINKEL, Studies in Ethnomethodology, Englewood Cliffs, N.J.,: Prentice-Hall, 1967, 37-8.

4. Ibid., 65.

5. ZIMMERMAN AND POLLNER, op. cit.

6. A LINCOLN RYAVE AND JAMES N SCHENKEIN, 'Notes on the Art of Walking', Roy Turner, ed., Ethnomethodology, Penguin, 1974, 266-7.

7. Ibid., 268-9.

8. FILMER, op. cit., 216, quoting Garfinkel's Studies, 9. I have used Filmers' words here rather than quoting the context of Garfinkel's comment in full because I reckon Filmer expresses the gist of Garfinkel's point accurately; Garfinkel's actual phraseology at this point is rather obscure.

9. MARY DOUGLAS, ed., Rules and Meanings, Penguin, 1973, Introduction.

10. DAVID WALSH, 'Varieties of Positivism', Filmer et al, op. cit., 43.

11. FRANK CIOFFI, 'Information, Contemplation and Social Life, The Proper Study - Royal Institute of Philosophy Lectures IV, Macmillan, 1971.

12. HARVEY SACKS AND EMMANUEL SCHEGLOFF, 'Opening up Closings', Turner, ed., op. cit.

13. GARFINKEL, op. cit., viii.

14. HAROLD GARFINKEL AND HARVEY SACKS, 'On Formal Structures of Practical Actions', John C McKinney and Edward A Tiryakion eds., Theoretical Sociology, Appleton-Century-Crofts, New York, 1970, 345.

15. ANTONY GIDDENS, New Rules of Sociological Method, London: Hutchinson, 1976, 39.

16. FILMER, op. cit., 205.

17. HAROLD GARFINKEL, 'The Origins of the Term Ethnomethodology', Turner, op. cit.

18. AARON CICOUREL, The Social Organisation of Juvenile Justice Heinemann, 1976.

19. Ibid., 31.

20. Ibid., 67.

21. Ibid., 100.

22. Ibid.

23. BARRY HINDESS, The Use of Official Statistics in Sociology London: Macmillan 1973, 18.

24. CICOUREL, op. cit., XIX.

25. Ibid., XVIII.

26. Expressed, for instance, in the following passage; 'I am saying that private and public agencies that produce structural data and social scientific surveys employ improvised or ad hoc procedures for obtaining, labelling, categorising, and presenting their information in tabular form; such procedures produce data containing not merely technical errors that can be estimated, for which allowances can be made when making inferences about findings, but the improvised procedures are integral features of arriving at and interpreting the end product and cannot be dismissed or "corrected" by estimates of error. The improvised or ad hoc procedures are necessary features of making sense out of the events or objects under consideration'. (112).

27. AARON V CICOUREL AND JOHN I KITSUSE, The Educational Decision - - Makers, Bobbs-Merrill, 1963, and Aaron V Cicourel et alia Language Use and School Performance, Academic Press, 1974.

28. GARFINKEL, op. cit., 77.

29. CICOUREL, The Social Organisation of Juvenile Justice, op. cit.

30. Ibid., XIX.

31. Ibid., XII.

32. Ibid.

33. Ibid., XV.

34. FILMER, op. cit., 115.

35. AARON CICOUREL, Cognitive Sociology, Penguin, 1973, 11-42.

36. This discussion to some extent draws on Popper's discussion of observation languages. However, it should be made clear that Popper's extreme nominalism is not subscribed to, in the sense of his rejection of even basic conceptual clarification, and his tendency to refuse to understand arguments which are presented in terms of unfamiliar concepts.

CHAPTER THREE

UNDERSTANDING A SUBCULTURE

The central point of my critique of phenomenology and ethnomethodology is that these approaches do not give the correct status to our normally unreflective understanding of everyday life. They do not realise the extent to which, for the most part, it necessarily remains a resource, rather than the topic, of enquiry. The phenomen--ologists' and ethnomethodologists' mistake is, I think, ultimately traced to an undervaluation of the degree of homogeneity in society - not homogeneity in an absolute sense, but with regard to a vast area of everyday activity and understanding. The 'way of life' which forms the background to the investigation of a phenomenon like suicide is extremely pervasive.

Nevertheless, ways of life obviously are differentiated. We often do have grounds on which to suppose that we are looking at an aspect of cultural variation. This has been recognized in the soci--ology of deviance, in which a major school of thought has been devoted to the analysis of 'deviant subcultures' - cultures which are within the general culture but which possess a modified set of rules, beliefs, or values which run counter to society's rules in general.

Understanding activity in the subcultural context therefore must mean an understanding which takes account of the meaning of the activity from the subcultural frame of reference.

The only way in which such an understanding can be achieved in the first place is by making oneself familiar with the subculture, by 'participating' in it. 'Participant observation' is sometimes seen as a rather scandalous procedure by criminologists[1]. It appears to be a woolly exercise in comparison with the examination, for instance, of criminal statistics. It has also been linked[2] with a rather bohemian or sentimental 'underdog' approach, and with a tendency towards salaciousness or voyeurism.

In relation to the substantive focus of some of the practit--ioners' work, there is probably more than a grain of truth in the latter charge[3]. But in fact participant observation is nothing more or less than the necessary outcome of the procedure of understanding in the subcultural context, and thus, in my view, a necessary part of empirical discovery in general.

'Participant observation' is, of course, in a sense a contrad--iction in terms. One cannot participate - genuinely participate - in a group while at the same time observing it. But one can, in the sense which is intuitively apparent to all of us, associate with a group to such an extent necessary to observe its activities in the members' terms, and this does not mean one necessarily joins in those activities fully. One can associate with thieves without necessarily thieving oneself. For this reason it is wrong to over-stress the moral and practical problems involved in observing criminals.

'Participation' effectively means, making oneself familiar with the members' everyday lives. Many of the practitioners of participant observation have drawn an explicit comparison in this context with the Malinowskian revolution in anthropology; they see the criminol--ogists' role as 'going native' in an analogous way.

It does not mean that one has to play a part. To act a part successfully means that one runs the risk of being found out; to act it badly means that one looks ridiculous. This point is brought out very effectively by Whyte - he was made to feel rather foolish by the gang members by his clumsy attempt to become 'one of the boys' by swearing etc.[4].

One of the most significant ways in which participant observation has been criticised for a supposed lack of rigour resolves around the problem of representativeness. Deviant sub--cultures are, to a greater or lesser extent, by definition, hidden. It is impossible to get very far simply on the basis of official records, and this in any case may well produce a picture which we have every reason to believe is unrepresentative. Individuals with criminal records, for instance are those who have been unfortunate or careless enough to get caught; they are not, as all criminolog--ists point out, necessarily representative of criminals as a whole.

The characteristic way in which Whyte, Becker and others have attacked the problem is to exploit an initial introduction to the member of the subculture, by gradually building up a picture on the basis of his introduction to other members, who will then introduce him to others, and so on. As Howard Becker[5] points out, such a sample cannot be random, since no one knows the nature of the universe from which it would have to be drawn. On the other hand, the invest--igator can have a good idea whether it is representative - he can cross-check, for instance, with other sources. A more significant point is that random samples are themselves normally contentious. We can take a perfectly random sample of criminals in a known universe, for instance prison, but the essential question remains the relation--ship between the prison population and the population of criminals as a whole.

Subcultures and the status of members' beliefs

It is stated that we need to examine subcultures from their 'own frame of reference'. The question is, how far in doing this we need actually to accept the menbers' own beliefs.

This question is essential to the general issue of the relation--ship of understanding to empirical discovery in general. A possibility that is raised is of a purely relativistic solution. Subcultures, after all, constitute to their members, in a sense, different realities. How far can things which form part of a way of life be said to be wrong ? Something close to a relitavistic conception is implied in a suggestion made by Stanley Cohen;

'Sociologists are increasingly becoming traders in definitions; they hawk their versions of reality around to whoever will buy them'[6]. This implies that what the student of subcultures is really doing is advocating its members' beliefs.

This view may be contrasted with another view, which is also implicit in statements sociologists of deviance have made about sub--cultures, and to which I subscribe myself. This view sees the obser--vers' accounts as being able to corroborate or falsify general ideas about the subculture, notwithstanding the members' own beliefs. It sees the observers' view of the subculture - its nature or aspects of it, or how it develops, continues, etc - as providing a corrective to the prevailing view of the subculture; prevailing either in the soc--iological context, or in lay terms, for instance in terms of the "official" view, the "common-sense" view or the mass-media view. It should be emphasized that these two conceptions are quite distinct from one another, although they are not usually distinguished in this way.

Understanding must take account of what an activity means to its members. If we do not take account of this then we risk literally misunderstanding what is going on. Drinking and drug-taking are more or less universal phenomena but can mean quite different things in different cultural or subcultural contexts. We can only understand the phenomenon properly by observing what it means in these contexts. For instance, if we take our conception of marijuana use from the countries in which it has traditionally been widely used, we find that, among other things, it has been employed by people doing hard physical work to relieve fatigue, or by violent killers. The essential message of sociologists like Becker is that marijuana-smoking in the West does not fulfil this function, being taken for mainly social reasons; this is why the early opponents of marijuana-smoking were quite wrong in supposing that the drug would turn young Americans into violent maniacs. Becker's empirical discovery in this sense was not a discovery 'about' marijuana; it was a discovery about marijuana subcultures.

But understanding in members' terms does not mean we have to accept everything the members say. The distinction is an important one. It is usually accepted that we need to understand things in 'meaningful' terms, but exactly how this is to be done is often left rather vague. For instance, Harré and Secord state that 'the things that people say about themselves and other people should be taken seriously as reports of data relevant to phenomena that really exist and which are relevant to the explanation of behaviour'[7]. The essential point that they are making is an anti-behaviourist one, with which I would agree, that these ideas constitute 'real', observable data. However 'taken seriously' must not be interpreted as 'accept' or even 'be inclined to accept'.

This is where I think the confusion lies about the observation of subcultures, and specifically about participant observation - the idea that the observer ends up as a hawker-around-of-definitions or 'takes sides'[8]. In fact there is nothing to stop the participant observer rejecting most of what the members of the subculture say - he certainly need not take it all at face-value.

For instance, much of what Lewis Yablonsky says about the juvenile gang milieu of the New York in the 1950's[9] suggests that the gangs could indeed only really be understood, that is to say, internally, by rejecting a great deal of what the gang-members had to say about their own behaviour.

The two most significant aspects of the gangs which were relevant in this context were their size and the nature of their organisation. The picture painted by the mass media at the time was of very large groups of individuals, numbering hundreds or thousands, in some cases held together by formal alliances, and highly organized into hierarchies with well-defined leaders, a clear structure of command, etc.

The picture which Yablonsky suggested was radically different. He indicated that much of this account was grossly exaggerated. There were various clues as to how this exaggeration might have taken place. One of the gang leaders with whom Yablonsky became acquainted made available his diary, which gave a graphic account of ten or so 'divisions' of at least two hundred members each[10]. The members of the gang whom Yablonsky actually observed numbered about twenty-five regular members, and never more than thirty-five were ever observed together[11].

Yablonsky arranged a number of conferences between rival gang leaders in order to attempt to bring about peace treaties, and these also suggested that the idea of great size and organization was more fantasy than reality. In the course of a single conversation the gang-leaders would come out with a series of figures of incredible proportions, which varied as the conversation was in progress. 'Each supported the others' gang size and seemed to get satisfaction and security from hearing all about the powerful alliance which had apparently been further cemented by the conference[12].

Another way in which the gangs'size and organization was invest--igated was by despatching two of the gang members as 'researchers' to interview the gang population. They only managed to record fifty--one members of their own gang (although when asked to estimate its total size many of those interviewed gave figures in the hundreds or thousands).

As well as overestimating the size of the gangs, the newspapers tended to give an erroneous impression of the nature of their organization. The conception put forward was of a Mafia-like organization, with well-defined leaders and a distinct authority structure.

What Yablonsky found was that there were a number of 'fantasy leaders' or individuals who were reputed to be part of the senior hierarchy of the gang but did not actually participate in it. It was found on a number of occasions that reputed leaders never appeared at important gang meetings. Most of the gang members whom Yablonsky observed were young or 'junior' members. All the gangs supposedly had senior 'divisions', but these groups never turned up.

Finally, the picture of gang warfare which emerged was one of spontaneity rather than organized activity. Perceived threats from neighbouring gangs were exaggerated out of all proportion. The gang 'forms around violence in a spontaneous fashion, moving into action - often on the spur of an evenings' boredom - instead of kicks'[13]. Gang violence was not carefully thought out in advance. In the actual gang-warfare situation, according to Yablonsky, most of the youths had little or no idea of why they were there or what they were expected to do, except assault someone[14].

What all this suggests is that exaggeration was intrinsic to the nature of the gang. A great deal of gang activity was, in actuality, fantasy activity. This does not mean that the actual events that took place were any less violent or bloody, but it does mean that the members' own accounts should not be taken too seriously. Exaggeration was part of the gang-members way of life.

We ought not to think there is anything particularly strange about self-deception of this sort. It does not mean that (as Yablonsky quite unjustifiably asserts in his theoretical writing about the gangs) that the individuals are crazy. In just the same way we all accept that it is part of the anglers' way of life to magnify the size of the ones that 'got away'[15]. It has often been suggested that in certain groups the expression of incidence verbally is of the essence of the activity, whereas its manifestation in terms of physical violence is exceptional, so that football supporters often ritually yell abuse at each other, for instance, without any fights actually taking place.

<u>Subcultures and the idea of empirical discovery</u>

The cultural variations in the meanings which actors attach to various kinds of behaviour do not therefore necessarily constitute a problem for empirical discovery in sociology. Findings about subcultures are just as real as findings about the wider culture. The participant observer is not reduced to being a sort of cultural messenger, bringing the word about the subculture to the culture at large; observation does not mean that he necessarily agrees with everything the members say. Findings about subcultures do not constitute 'soft' data, which need supplementing by statistics.

The latter point is probably the most significant way in which some criminologists have been doubtful about the idea of participant observation, and of subcultures. Theorists like Albert K Cohen proposed a 'delinquent subculture', situated among young working-class males, association with which is the essential reason for an individual youth's turning to juvenile delinquency. One argument which can be put against Cohen and the other 'subculture' theorists is that no explanation is offered as to why one individual rather than another becomes delinquent.

The way Cohen counters this argument is to state that the sorts of questions he is asking - what is the nature of a subculture, or how does it come into being - are at least as important, and that the question of why one individual rather than another becomes delinquent is relatively unimportant.

I do not think this line of argument is in fact backed up very well in the sense that Cohen states it, but nevertheless I think it is legitimate, albeit for reasons which he perhaps did not fully acknowledge. Subcultural activity breaks the rules of the wider society, and in this sense the activity is exceptional with regard to society as a whole. On the other hand within the subculture the activity is normal. If we accept that activity needs to be understood within its cultural context, then there is no reason why we should look for abnormality behind activities which are culturally proscribed any more than behind those which are generally accepted. Repeated theft, for instance, has often been explained by abnormalities of the psyche or of family upbringing which bring about irreversible behaviour change, whereas no one has attempted to explain belting at horse-races or going bankrupt along these lines.

Indeed, if the activity is regarded as normal then it would usually be regarded as mistaken to investigate its etiology. We would not normally countenance the idea of causal investigations into why people go fishing, or why they smoke or chew gum. We can isolate predisposing factors - this is the main argument of the next chapter. But we would not expect a conclusive answer to become manifest at the individual level.

Thus the tendency to regard subcultural explanation as something which needs to be supplemented by statistics is, I believe, simply a reflection of the tendency to see explanation in terms of immediate causation as the ideal form of explanation - an attitude which has no justification.

Finally it should be made clear that subcultural understanding has no conceptual relationship to phenomenology or ethnomethodology, although it is sometimes assumed that it does. As I stated in chapter one, the work of some sociologists of deviance is sometimes loosely called 'phenomenological'. Schutz's notion of 'multiple realities' has probably played some part in this confusion - the (legitimate) idea that however unreal aspects of a culture are to us (e.g., belief in witchcraft) this does not stop them being real to the participants. I think this is associated to some degree with the relativistic conception of subcultures. David Walsh states that 'phenomenologists have devised techniques such as participant observation'[16]. This is of course factually incorrect, but it also reveals the extent of the misconceptions surrounding the relationship between understanding and the phenomenological method.

There is simply no justification for this view. Walsh contrasts the method of participant observation in this context with 'positivism'. It is true for the most part that those criminologists who have ignored the influence of subcultures have done so because of mistaken ideas about imitating natural science but to denote one which does recognize subcultures as 'phenomenological' simply reduces the term to a catch-all expression for any approach which does not fall into these sorts of positivistic mistakes. The phenomenologists' misconceptions about participant observation are I think ultimately founded upon the same sorts of doubts expressed by criminologists like Donald Cressey - a failure to acknow--ledge the essentially logical nature of the process of understanding.

NOTES TO CHAPTER THREE

1. It has been attacked, for instance, in every edition of Edwin H Sutherland and Donald R Cressey's Principles of Criminology, Philadelphia: Lippincott, 1960, 69-70.

2. In ALVIN GOULDNER'S 'The Sociologist as Partisan:' Sociology and the Welfare State, American Sociologist 3, (May 1968), 103-16.

3. For instance in LAUD HUMPHRIES Tearoom Trade, London: Duckworth, 1970, an account of the behaviour of homo--sexuals in public lavatories, the sociologist-as-voyeur concept is taken to extreme lengths.

4. WILLIAM FOOTE WHYTE, Street-Corner Society, Chicago U.P., 1955, 304.

5. HOWARD BECKER, Outsiders, Free Press, 1973, 46.

6. STANLEY COHEN, ed., Images of Deviance, Penguin, 1971, 24. It is not suggested that Cohen consistently represents this point of view - other things that he has said would fit into the opposite category. I have picked out this particular statement because I think it is representative of a wider view which is implicit in much of the work of those who have investigated deviant subcultures.

7. ROMANO HARRÉ and P F SECORD, The Explanation of Social Behaviour, Oxford: Blackwell, 1972, 7.

8. HOWARD BECKER, 'Whose Side Are We On? ' Social Problems, 14, (3), 1967, 245.

9. LEWIS YABLONSKY, The Violent Gang, Macmillan, 1962.

10. Ibid., 73.

11. Ibid., 107.

12. Ibid., 49.

13. Ibid., 7.

14. Ibid., 157-8.

15. This example was suggested by Graham Young.

16. DAVID WALSH, Sociology and the Social World, Paul Filmer et alia, New Directions in Sociological Theory, Collier--Macmillan, 1972, 31.

CHAPTER FOUR

CAUSATION AND QUANTIFICATION

Quantification is that type of enquiry which is furthest removed from the ideals of phenomenological sociology. By definition it does not preserve the totality of social activity; it concerns the manipulation of abstracted elements of it. What I shall discuss here is an area of criminology which is characterized by the extensive use of quantitative methods. I shall be very critical of it, but at the same time I intend to show that the problem does not lie with quantification in itself; it is not necessarily an inadequate way of studying social activity.

Causation and Determinism

The best way of tackling the question of whether quantitative, or any other sort of explanations, amounts to causal explanation, is to look at it in the context of the idea of determinism. One of the simplest arguments for the idea of social science is that human behaviour exhibits regularities. This is quite true but it does not follow that the behaviour of humans can be analysed 'causally' in the sense that we are able to analyse the behaviour of inanimate objects. Human behaviour is not determined. People often have compelling and overwhelming reasons for acting or not acting in a certain way, but they are reasons, and they amount to a choice. It often does not look like choice. To modify slightly an example given by Ryan[1], let us examine the hypothetical example of a Martian examining the behaviour of traffic at a set of traffic lights. If he looked at the humans from a cybernetic point of view he would naturally come to the conclusion that the operation of the lights caused the drivers to stop or start. The reason why the behaviour appears to be determined is that it is so regular - people very rarely jump traffic lights, for very good reasons, but that does not make it mechanically determined, anymore than the shortage of a commodity determines that people raise its market price. (And the corollary of this is the argument about verstehen: the Martian cannot observe the humans' activity in a behaviouristic sense, but can only explain it adequately by understanding it in terms of human culture; in this case in terms of what different coloured lights mean to the members of the culture.)

This point has been made convincingly by philosophers like Alistair MacIntyre[2]. It has also been made by sociologists like David Matza[3]. He made the simple but pertinent observation that many people stop being criminal as well as becoming criminal; that being a member of what Albert Cohen called the 'delinquent subculture', for instance, does not mean that one is necessarily on the criminal path for the rest of one's life.

Early criminologists like Lombroso were completely deterministic in their outlook, in that they saw criminal behaviour as being entirely determined at birth. This outlook has now largely disappeared, apart from one or two notable exceptions like Hans Eysenck[4], who see crime and delinquency as the result of the combination of certain personality variables which are assumed to be inherited.

None of the contemporary criminologists are explicitly deterministic in this sense. Their analyses are expressed in the form of probabilities, not in the sense of calculi of probability, but of working within a set of acknowledged unknown variables. They are not normally couched in terms of genetics (though the XYY chromosome argument is an exception to this) but rather early family experiences.

However, they tend to be deterministic in the sense that explanation is assumed as being equivalent to prediction. "It is a familiar saying, that knowledge of a subject has never reached a truly scientific stage until it can be made a basis for reasonable prediction. Astronomy is a science; and we deduce the hour of an eclipse from our knowledge of the heavenly bodies. Physics is a science, and we deduce the volume of a gas from our knowledge of the effects of temperature"[5]. Burt claims that 'criminal psychology' will soon attain this sort of plane. This statement suggests two misconceptions; one, that astronomy can be regarded as typical of all natural sciences, and two, that astronomy can be regarded as analogous with the social sciences. Burt's statement is very significant since delinquency <u>prediction</u> has come to be the characteristic mode of statistical research in criminology.

Whether explanation and prediction are or are not equivalent in the natural sciences, they are obviously not in the social sciences.

That is to say, giving an adequate account of why people act in a certain way does not entitle us to predict that they will act in this way in the future. We may have a good idea of the course of action they are likely to follow, but to assume that this will necessarily be the case is to adopt an over-mechanistic view of man. Furthermore, the more effective our predictions are, the more likely it is we will either make them come true or prevent them from coming true, depending on the circumstances of the prediction.

The best known and most elaborate application of delinquency prediction was the 'Cambridge-Somerville' experiment. A group of boys who had all been predicted as high-risk delinquents were divided into matched pairs, forming two groups; a treatment group who were given intensive, unsolicited, social-work treatment over a prolonged period, and a control group who were given no such treat--ment. When the number of delinquencies of the treatment group were compared with those of the control group it was found that those who had been treated were consistently more delinquent.

The proponents of this study were dedicated to a certain conception of social work, which they believed to be highly effective. In this sense they were anti-deterministic. What I find remarkable, however, is that some sort of reactive effects were not hypothesized. Many reasons were given for the failure of the experiment, including an inconsistent approach on behalf of the social workers and interr--uption brought about by the War. The possibility that the social workers actually might engender delinquent activity in their subjects was not conceived of, and this was reflected in the planning of the experiment; for instance, rather than keeping the overall design of the project secret, every assistance was given to those subjects and members of the local community who wished to know what was going on.

Another aspect of the relationship of a deterministic and non-reactionist approach is the practice of basing studies on captive rather than free subjects. The best example of this is the work of the Gluecks, whose sample of delinquents was based entirely on boys in reformatories. This makes sense if it assumed that there is an actual difference between delinquents and non-delinquents, and that the majority of delinquents get caught. (This is itself the impression produced by the Gluecks' technique). On the other hand, it makes nonsense of the results if it is accepted that the inhabitants represent a partial or biased sample of delinquents in general, or that the institution itself changes the behaviour and the attitudes of its inhabitants. That there was a deterministic assumption in the Gluecks' project is shown by the fact that it was assumed that attributes of delinquency and non-delinquency whose discovery was proposed by the project were assumed to be representative of delinquents and non-delinquents in general.

Cause and Correlation

Hirschi and Selvin[6] cite three principle requirements that an empirical investigator must meet in order to be able to say that A causes B:

1. A and B are statistically associated ('association').
2. A is causally prior to B ('causal order').
3. The association between A and B does not disappear when the effects of other variables causally prior to both of the original variables are removed ('lack of spuriousness').

The correlation coefficient is the most widely-used statistical technique in criminology, but what it means is controversial. The controversy has usually been put forward in terms of different positions with regard to the question of the relationship between correlation and causation. Given that a relationship has been established according to these criteria, are we entitled to say that the relation--ship is causal ? Hyman[7] states that causation is not established until one or more intervening variables link the dependent and independent variables. This stipulation links causation in social science with the idea of mechanical causation in the natural world.

Whether this criticism is or is not necessary for causal explanation in a situation in which it is appropriate to talk of mechanical causes, this is not the case in sociology; in this context it is inappropriate to use a mechanical concept for conscious behaviour.

The best way of conceptualising what Hirschi and Selvin call 'causes' is probably as predispositious. What the Chicago criminol--ogists and their successors did, for instance, was to emphasize the way in which growing up in a certain kind of area predisposes one to act in a certain sort of way. This does not imply causal determination, or certainly that every circumstance of growing up in the area will produce the same result.

What the Chicago theorists to some extent realized, however, was that the factors that predisposed people towards criminality needed to be seen within the cultural context in which they operated. Whether a factor was operative depended on how the subject 'defined the situation'. I do not think this is best expressed in terms of an 'intervening variable'. Neither does it tell us very much to try to produce a theory of criminal behaviour along the lines of Sutherland's which states that it is the result of an excess of favourable definitions over unfavourable definitions. What it does mean is that the factor must make some sense within the subjects' own frame of reference; it must be comprehensible to him, even though he might not agree with our explanation. When we have reason to believe we are looking at a culture which is different to our own, then this explanation is contingent upon our first obtaining some understanding of that culture by learning about it or 'participating' in it. I think that the idea of cultural heterogeneity has, although relevant to criminology, been rather over-emphasized. The question is rather, do do the factors which are advanced in explanations of crime make sense to us ? This issue will be elaborated with regard to the question of the relationship between factors and indices.

A practical illustration of the way in which the concept of 'predisposition' is more realistic than 'cause' is afforded by the question of the relationship between social and individual dispositions. The way the Gluecks saw the 'delinquency area' studies was that, "This kind of approach ... although of much aid in studying the phenomenon in the mass, is of relatively little help in exploring the mechanisms of causation"[8]. "Anti-social aspects of culture are only potential or possible causes of delinquency"[9]. The concepts of mechanical cause and potential cause beg the question. If we accept we are looking for predispositions, not causes, then it is evident that growing up in a certain kind of area represents just as significant a predisposition as does the individual factors upon which the Gluecks concentrate, even if that area affects only a minority of the inhabitants in a certain fashion. The Gluecks' own 'causal' factors do not always affect the majority of the population, and in no case are they characteristic of it, but this does not prevent them (correctly) emphasizing the relationships which they discover. It is no more true to say that coming from an inadequate home, or being badly supervised, are 'causal' factors than it is for growing up in a certain kind of area.

In fact neither the Gluecks nor most of the other statistically--oriented criminologists have seen the aim of discovery in other than 'causal' terms. This has produced the unwarranted notion that the results of these studies represent in themselves an understanding of the phenomenon, whereas in fact an elucidation of pre-disposing factors can only be the basis for such an understanding.

It is worth giving some consideration to the other two 'criteria of causality' which Hirschi and Selvin set out. 'Causal order' can be expressed more clearly as 'sequence in time'. It should be noted that the most influential work in statistical criminology has been carried out by way of the cross-section or comparative study, which obviously makes it much more difficult to analyse time-sequences than the longitudinal study. This is another aspect of the bias towards the investigation of causes rather than processes. What the Gluecks and other criminologists do is to make retrospective judgements about time-sequences, to the extent that they even claim that the investig--ation of time-sequence is one of the most important aspects of their study. They propose to demonstrate convincingly, for instance, that criminal associations follow, rather than precede, delinquency. The way in which they feel able to construct these sorts of arguments reveals the emphasis on inborn characteristics as a determinant of behaviour.

An example of a prospective longitudinal study is that carried out by Donald West. A weakness of this study is that it takes over, as far as possible, categories of analysis used by the non-prospective researchers. Although doing this would seem to make sense for comparative purposes, it does not necessarily make the results any more conclusive.

Hirschi and Selvin make an important point about the need for a lack of spuriousness. The Gluecks present their data in dozens of two-variable tables, in which delinquents are compared with non--delinquents. Many of these tables show strong relations between the independent variable and delinquency. On some occasions, it is evident that an independent variable shown in one table may be strongly related to an independent variable shown in another table. The question thus arises, are these variables independent causes of delinquency, or is the relation of one of them to delinquency a spurious result of its relation to the other ? This question has more force when the relation between the two variables is logically necessary rather than empirical[10]. They analyse two of the Gluecks' two--variable tables, that between size of families and delinquency and rank of boy and delinquency. The Gluecks found that delinquents were more likely to come from large families, than non-delinquents, and were more likely to be middle children (as opposed to only children, first-born or youngest children) than non-delinquents. They propose the latter finding to be a significant refutation of early theories that emphasized the problems of only children, first-born and youngest children[11]. They do not consider that the larger the family, the greater must be the proportion of middle children. Since, in the Gluecks' data, delinquents do come from larger families than non--delinquents, a larger proportion of delinquents are li kly to be middle children, whether or not birth order is causally related to delinquency. Thus, the Gluecks' findings are prefectly compatible with the assumption that family size is a cause of delinquency while birth order is not; Hirschi and Selvin demonstrate that this is actually the case[12].

Multi-causal explanation

The best way of investigating the question of cause and meaning in relation to statistical criminology is to look at the concept of 'multi-causal' or 'multiple-factor' explanation, since this is the way in which most of the research has been expressed.

According to Burt, "One striking fact leaps out in bold relief" from the mass of correlations and tables which he set out in "The Young Delinquent":- " ... the fact of multiple determination. Crime is assignable to no single universal source, nor yet to two or three; it springs from a wide variety, and usually from a multiplicity, of alternative and converging influences. So violent a reaction, as may easily be conceived, is almost everywhere the outcome of a con--currence of subversive factors: it needs many coats of pitch to paint a thing thoroughly black. The nature of these factors, and of their varying combinations, differs greatly from one individual to another: and juvenile offenders, as is amply clear, are far from constituting a homogeneous class"[13].

Burt lists 170 distinct conditions for delinquency. These may be reduced to the following, in order of importance:-[14]

1. defective discipline
2. specific instincts
3. general emotional instability
4. morbid emotional conditions
5. a family history of vice or crime
6. intellectual disabilities, such as backwardness or dullness
7. detrimental interests, such as a passion for adventure, for the cimema, or for some particular person, together with the lack of any uplifting pursuits
8. developmental conditions, such as adolescence, or precocity in growth
9. a family history of intellectual weakness
10. defective family relationships - the absence of a father, the presence of a step-mother
11. influences operating outside the home - as bad street companions, and lack or excess of facilities for amusement
12. a family history of temperamental disorder - of insanity or the like

13. a family history of physical weakness

14. poverty and its concomitants

15. physical infirmity or weakness in the child himself.

One obvious mistake in Burt's concept of multiple causation is an extension of what A K Cohen called the 'evil-causes-evil' fallacy. The various factors, like a 'vicious' family and 'morbid' emotional conditions, are presumed to add up in such a way as to produce a progressively blacker character. This is a naive assumption for which no empirical justification is given. The other weakness of the concept is that it is so general; The fifteen sub-headings include so much that it is difficult to see what they leave out. This is expressed especially in two ways:-

(a) virtual 'et cetera' clauses, such as a family history of 'temperamental disorder - of insanity and the like',

and (b) contradictions such as 'lack or excess' of facilities for amusement.

A good example of the latter which Burt discusses at length are the penalities of being small in stature and the way in which this may be conducive to delinquency; he goes on to discuss the problem of being over-large in exactly the same way.

With regard to Burt's theory, Leslie Wilkins' charge of unfalsifiability is undoubtedly correct. "If it is claimed that the (multiple factor) theory applies to all factors which are operationally found to be related to criminal tendencies as they become known, it is apparent that the theory lacks the major and essential feature of any scientific theory - it is framed in such a way that it is impossible to find any test whereby it could be proved wrong. The theory of 'multiple causation' is then, no theory. At best it could be considered an anti-theory which proposes that no theory can be formed regarding crime"[15].

Nigel Walker has argued, in response to Wilkins' charge, that multiple factor theories need not be unfalsifiable so long as:-[16]

(a) the list of factors is a finite one (which would exclude 'et cetera' clauses)

and (b) each of the factors is itself capable of being falsified.

He argues that another aspect of the question of unfalsifiability is that, given a category of crime which is defined in very general terms, certain types will need to be explained one way and others in a different way, in the sense that, for instance, we have different explanations for different diseases as Merton pointed out, would find the idea of a single explanation for disease unacceptable[17].

Burt realised this when he noted that criminals were a far from homogeneous class. I do not think Walker goes far enough along this line of criticism, however, for, although he attacks the idea of a general explanation of 'crime', he does not draw the inference that the subject which has most concerned the multi-causal theorists - juvenile delinquency - falls into this class. Under the term 'delinquents' or children who 'fail', are included the whole range of criminal offences, from quite serious to petty offences, and a range of other actions which do not come into the criminal law, like truanting, or being out of control of the parents. The factors which lead to a child's playing truant may be quite different from those which lead to his thieving, or vandalising property.

The most suitable inference to draw from this is that we should, rather than list the different factors that make children 'fail', specify what sort of activity we are talking about. There is very little point in attempting to enumerate the factors behind an activity which we know is not homogeneous. (This does not mean the Durkheimian idea of causality is accepted. Durkheim has been justifiably criticised by many commentators for his concept of cause and effect -

that if there appear to be several causes of suicide, this is because there are several types. This idea is completely rejected; obviously there can be several different factors which predispose an individual to act in a certain way. The question is what we mean by acting in 'a certain way'. In most causes we can unambiguously state what an activity amounts to. My argument is that we all know what suicide means, or what theft means, by virtue of our membership in a common way of life. We cannot state in the same way what 'crime' or'juvenile delinquency' mean.)

Another way in which the 'multiple factor' concept is expressed is in terms of 'eclecticism'. According to Walker, "eclecticism means ... involving whatever body of theory seems to offer the most plausible explanation of a particular sub-group of crime".[18] The Gluecks state that the reason for their adoption of a 'many-sided' or 'eclectic' approach was that 'examination of existing researches in juvenile delinquency disclosed a tendency to grossly over-emphasize a particular science or explanation. Proponents of various theories of causation still too often insist that the truth is to be found only in their own specifal fields of study. Like the fabled blind men examining the elephant, each builds the entire subject in the image of the piece of reality which he happens to have touched"[19].

The idea that the phenomenon can best be tackled from a number of different aspects or by using different methods has already been acknowledged. The eclectic criminologists have also often emphasized that aspect which supports a multi-disciplinary viewpoint. For the Gluecks, it should be pointed out that this allows for the inclusion of psychology, physiology, medicine, etc., but explicitly excludes sociology. In general there is a limit to how far different paradigms of social science can actually be compatible with one another; in this context the conflict is represented by the Gluecks' method--ological individualism.

The most significant failure of the 'multiple-factor' approach rests on the failure to distinguish causal explanation of certain factors and the meanings which individuals place upon those factors.

According to Hirschi and Selvin: "The appropriate inference about whether some factor is a cause of delinquency depends on the relation between the factor and delinquency (and possibly other factors prior to both of these). All that can be determined about meanings, motives or reactions that follow from the factor and precede delinquency can only strengthen the conclusion that the factor is a cause of delinquency, not weaken it"[20].

This represents a confusion between 'factors' and 'indices' - a confusion which is often found in the multi-causal research. Albert K Cohen has drawn attention to a lack of distinction between factors and variables[21]. I think the term 'variable' may be more clearly articulated by 'index'. An index is an indicator or sign of a factor we wish to measure. The scale upon which the index is drawn approximates to variations in the factor. If we consider this index to be a good one, then we are entitled to emphasize the connection between the factor and the phenomenon which we are investigating. It is wrong to see 'meanings, motives and reactions' as secondary since factors need to be expressed in these sorts of terms. I think that when Hirschi and Selvin use the term 'factor' they are for the most part talking about indices, and the relationship between an index and a phenomenon like delinquency is by no means as clear-cut as they suggest.

This confusion gives a misleading impression of the significance of much of what the 'multi-causal' theorists say. In the summary to this section on 'the boy in the family' in "Unravelling Juvenile Delinquency", the Gluecks state: "With reference to the more dynamic pattern of parent-child relationship, the delinquents, as a group, were to a greater extent the victims, not only of less stable households, but of broken homes. To a far greater extent than the non-delinquents they had substitute parents, that is foster- or step-parents, or lived with relatives"[22].

Three things are being discussed here; stable households, broken homes and substitute parents. Stable households are originally introduced as a significant factor: "The stability of the home is perhaps the most important single factor to be explored from the point of view of wholesome family life, since insecurities, confusions about standards of conduct, and problems of identification of the growing child with his parents are all involved"[23]. They go on to say: "A normally stable household may be interpreted as a home in which pref--erably both parents, but at least one, are in unbroken family relationship with the child, except for brief and expectable absences"[24].

This redefinition makes it unclear whether an 'unbroken family relationship' is being offered as an index or not. It is implied that the two might be identical, but this is unwarranted on the basis of our usual understanding of the terms used; we could measure 'stabil--ity' by all sorts of indices, like frequency of using house, frequency of employment, or frequency of family quarrels. Assuming that 'un--broken family relationship' is an index, it is a rather partial one, as it emphasizes only one of the many possible dimensions of stability.

The association of broken homes with delinquency is convincingly demonstrated (assuming that we ignore the exaggeration in the figures produced by comparing convicted delinquents with boys who were selected for their lack of delinquency). Given this, it is erroneous to state that the delinquents were victims of less stable house--holds, if we assume that stability of households may be measured adequately by the incidence of a broken home.

If we look at the third item listed in the summary - 'substitute parents' - this is analytically linked to 'broken homes'. We would expect children from broken homes, all other things being equal, to have substitute parents, purely by virtue of their homes being broken. It is therefore misleading to list it as if it were a factor which is significant in itself, since it effectively simply restates what the broken home' variable indicates. The relationship could therefore well be spurious, and may even have an opposite effect to the one the Gluecks suggest, if we compare, within the population of boys from broken homes, boys with or without subst--itute parents.

The only conclusion which definitely comes out of this is the relationship between broken homes and delinquency. The 'broken home' variable is not clearly prescribed as an index; it is rather as if it came first, and the conceptualisation in terms of stability was secondary.

Hirschi and Selvin make the valid point that if we set out to measure something then it is contradictory to abandon a successful measurement for something conjectural. They give the example of the relationship of home ownership (the index), anomie (a conjectural motivational factor), and delinquency. "If anomie can explain away the relation between one of its indicators and delinquency, it can explain away the relations between all of its indicators and delinquency. No matter how closely a given indicator measures anomie, the indicator is not anomie, and thus not a cause of delinquency. The difficulty with these explanations is not that they may be false, but that they are non-falsifiable"[25].

It is true that any relationship can be explained away in this fashion. On the other hand it is still not legitimate to accept that a variable like home ownership or broken homes can explain delinquency adequately, if by adequately we mean in terms which are meaningful to the subject as well as to the sociologist. What we should be seeking is the measurement of factors which are expressable in these terms. Given that direct measurement is usually impossible it is legitimate to construct an index, and, if we consider this index to be a good one, then we can say that a relationship between the index and delinquency represents a relationship between the motivating factor and delinquency. If, however, the index is introd--uced as a detached variable without reference to motivating states, then what that variable means is by no means as obvious as Hirschi and Selvin suggest. Coming from a non-owned home, or a broken home, or having a working mother, can mean quite different things in different milieus or situations.

This does not mean that Douglas' anti-statistical argument about 'social meanings' is accepted. What is being stressed is that the social environment may change in such a way as to give us a good reason for abandoning an idea that was previously adequate. If we look at the phenomenon of a broken home, for instance, this may have been a good index of family instability at a time when, for instance, divorce and hence remarriage were difficult, and social mores, strengthened by contemporary sociological and psychological theories, laid great emphasis on its evils. On the other hand, this is not necessarily the case. Oscar Lewis, in "La Vida", gives a graphic account of a society in which an unbroken home is the exception rather than the rule, but the acceptance of couples living together combined with a powerful extended family network means that a reasonably stable 'family' remains.

The same sort of argument may hold true for having a working mother. This may be a good index of inadequate supervision when the mother's going out to work means that the child is left completely to its own devices, but it is not likely to be a good index when for instance, the child is taken care of by relatives or is able to attend a day nursery.

The confusion between factors and indices is one reason why the results of the ongoing research programme, of which the Gluecks intended their work to be a part, are so inconclusive. Donald West, for instance, in a recent study[26] found that there was, rather than a relationship between working mothers and delinquency, some indication of a relationship between delinquency and mothers <u>not</u> working. This result is informative and significant, in that the issue of whether mothers ought to go out to work has become a question everyday interest. It does not mean that the Gluecks' results were necessarily wrong for the period and location in which they were carried out. If we assume that in both cases the factor which we are attempting to measure is adequacy of supervision, then there is no reason to suppose that the relationship between inadequate supervision and delinquency has changed, since the family is just as significant a determinant of socialisation now as it was then. What has changed is the way we measure inadequate supervision.

It would be wrong to take this argument too far, since the existence of the idea of a relationship between working mothers and specific social problems like delinquency in itself justifies enquiring as to whether it is right or not. However, it is important to keep in mind that the reason why we have come to talk about motiviation in this fashion is because, and only because of the influence of research of the type being looked at.

There is another powerful dynamic agent in the confusion between indices and factors which has taken place. The indices which have been used have usually been introduced in a more or less ad hoc fashion, in such a way as not to be clearly related to motivational concepts. In the analysis stage, the investigation is limited to manipulating these variables, but there is relatively little attempt to interpret what they mean. Those variables whose relationship with delinquency is judged to have the greatest significance then form the basis of future studies. The statistical criminologists have usually subscribed to the inductivist concept that repeated demonstrations of a relationship confirm or verify it, and it is this 'confirmation' which gives the variables a spurious concreteness and militates still further against any examination of their significance.

One reason why the distinction between factors and indices has been under-emphasized is because of the statistical criminologists' predilection for 'delinquency prediction' studies. One of the problems with this concept is that more than one rationale exists for it. I am referring in particular to prediction as a device for prognosis or treatment:- a technique for social workers, magistrates and others to use in order to deal with delinquents scientifically. Great stress has been laid upon this idea. Whether it is a proper way of going about correctional treatment is not the point at issue. The point at issue is that it tends not to be distinguished from the other point of view, which sees it as equivalent to explanation. This militates against the question of understanding being raised, since it is possible to justify the inclusion of a variable for purely predictive purposes without having any idea of what it means.

All the statistical criminologists, the Gluecks included, pay some attention to the meaning of the variables which they employ, albeit far too little attention. In this sense it can be said that 'prediction', interpreted as 'explanation', has some relationship to understanding. However, the need for understanding is not expressed consistently because of the 'treatment' rationale.

This means that the Gluecks include among the variables they measure, for instance, the relationship between church attendance and delinquency. 'Church attendance' does not make sense either as an index or a factor. It could not be said to be a factor in preventing (or inciting) delinquency - there is no reason why one should not both attend church and go out thieving regularly. Hirschi and Selvin raise the idea, and imply, that church attendance may be an index of moral values; "Most religions emphasize proper behaviour and those who go to church should, therefore, behave better"[27]. This idea cannot stand up to examination. Church attendance is a notoriously unreliable index of moral values; in any case to mean anything it would have to be cross-related with both denomination and ethnic group, since, for instance, some groups will go to church more regularly than others.

What the Gluecks are able to do is simply to ignore its significance: "... no intensive exploration was made of the reasons for the lesser adherence to religious duties on the part of the delinquents. Our concern was only to ascertain the facts in regard to the regularity of attendance"[28]. This makes sense in the context of constructing prediction tables, but it is not made sufficiently clear that in the context of the project as a whole it contributes not one whit to our understanding. (Conversely, if the aim of the research is simply to construct a prediction table, then there is no need at all to take account of what the variables mean).

Measurement as a Means to an End, or an End in Itself

The essential mistake of the confusion of indices with factors is that what was originally introduced as a means of investigation merges into representing an end of discovery. This is a particular case of a more general disparity of views about the position of quantitative techniques in criminology which may best be expressed in this way.

The work of the Gluecks is especially significant with regard to this problem. Their enterprise as a whole can best be appraised by looking at it in terms of two objectives. One of these consists of a number of substantive claims about the nature of delinquency. The other is a methodological claim which is expressed in terms of a determined defence of 'multiple-causation' and all that this approach entails. Their own approach is proposed as a standard for criminology research, and alternative approaches (notably that of Edwin Sutherland) are dismissed as being useless or misleading. Although they claim to be eclectic, eclecticism is only permissible within the bounds of their own concept of method.

The most obvious result of this is that no account is taken of primarily descriptive studies of deviance; for instance, the nature of various criminal acts, or the process of learning criminality, or adapting to conviction. These areas of interest are linked with methods like participating which place primary importance upon phenomenological accuracy. It is a mistake to ignore this sort of work, even if we are primarily interested, as the statistical criminologists are, in explanation, since accounts of criminal activities usually in themselves suggest explanations. They form part of the theoretical framework within which investigation takes place.

The Gluecks also propose their work as an end of discovery in that it is not even acknowledged that it is possible to learn something about what actually motivates the delinquents by analyzing the figures. It is not simply that the Gluecks propose their work as the pre-analysis data which others may put into meaningful form; it is proposed in itself as the end of research.

Quantifiable data is not seen as an aid to understanding; the aim of the research is to produce quantifiable data. The only way in which the research may be followed up is by producing better data, but it is illegitimate to attempt to discover what this data means. This militates against us ever coming to an understanding of the phenomenon.

The following example is typical of the sort of reasoning which leads to this position being reached. In this context the issue is the use of prediction tables and their various criticisms of them, in particular the criticism that they assume determinism, or in the Gluecks words: "Underlying predictive techniques, it is said, is the assumption of a deterministic, if not fatalistic, sequence of cause and effect in human affairs; and this supposedly does violence to the nobler conception of human action as a result of the exercise of a "free will".[29]

This is their rejoinder: "Regarding the assumption behind predictive tables of an absence of freedom of will in human affairs, since it is impossible to measure the degree of free will which distinguishes recidivists from non-recidivists or potential delinquents from actual delinquents, this factor cannot practically be used in a prediction table"[30]. This is tantamount to saying, since free will is not measurable, we can ignore it. The realistic investig--ation of the way delinquents et cetera actually behave is subsumed to what can be manipulated quantitatively, even though this excludes that dimension of activity which commonsense suggests is an intrinsic element in it, and of which neither the Gluecks nor anyone else have shown otherwise.

This is very similar to the sorts of reductionist positions set out in Chapter One. I contend that quantitative analysis is an important and valid technique in sociological investigation, so long as it is accepted that it is a technique. If we are led into a consideration of unquantifiable factors then they should not be excluded. Conceptual analysis is an equally important aspect of investigation; it is when criminologists have attempted to disregard conceptualisation that the charge of meaninglessness is justified. This is the sense in which we should retain fidelity with the phenomenon, or give it priority. The point I am putting forward is that there is no contradiction between such a 'phenomenoligical' approach and the use of statistics. This point has tended to be over--looked in existing critiques of statistical criminology, and quantitative sociology in general.

This is expressed most clearly in A V Cicourel's work[31]. Cicourel is aware of the danger of an over-emphasis on quantification on the basis of its appearing to accord with the canons of scientific explanation: "Viewing variables as quantitative because available data are expressed in numerical form or because it is considered more 'scientific' does not provide a solution to the problems of measure--ment ..."[32] I disagree when he continues "... but avoids them by fiat. Measurement by fiat is not a substitute for examining and re-examining the structure of our theories so that our observations, descriptions and measures of the properties of social objects and events have a literal correspondence with what we believe to be the structure of social reality"[33].

It should be reiterated that the idea of the elimination of pre-conceptions by self-examination suffers from the criticism of infinite regress raised in Chapter One. Furthermore, the concepts of 'measurement by fiat' and literal measurement' reveal a mistaken conception of what measurement in sociology represents. I contend that the concept of 'literal measurement' is close to being self--contradictory since the process of measurement does not concern itself with 'literal' reality, if by this we mean realistic description. If we use the measurement of heat as an example (Douglas, whose concept of measurement is similar to that of Cicourel, discusses this example in detail, following Halbwachs) the measurement in degrees Celsius, Centigrade or Fahrenheit does not represent the way heat manifests itself. It is not actually produced in bundles marked Centigrade or anything else. These scales are arbitrary - they simply define a unit of heat in terms of variables or elements which are familiear to us. They were introduced by 'fiat' or convention.

This is not to say that Cicourel's concept is meaningless. He gives examples of, for instance, sociology researchers who, given anomalous or problematic answers to questions posed in terms of certain scales of measurements make an arbitrary decision to place the answer in one category or another. In this sense they allow themselves to be constrained by the dictates of their measuring--instrument, rather than designing the measuring-instrument to fit the situation. Given that this process should be avoided (although it is to a large extent unavoidable and, as Hindess[34] points out, the proportions of arbitrary classifications can be calculated), it is nevertheless true that in a broader sense <u>all</u> measurement is 'measurement by fiat'. All measurement is a process of imposing ordered relationships on reality for various purposes and according to various criteria. It is illogical to attack those who use social statistics on this basis.

The attack would make sense if it were directed specifically against those sociologists who treated their measurements as if they literally corresponded to social reality. As regards statistical criminology, this would be a very relevant point. The solution to this is to recognise that the measurement <u>is</u> by fiat, and make use of measurements as resources rather than findings accordingly; not to attempt to elucidate hitherto undiscovered 'literal' measurements.

<u>Quantification and Conservatism</u>

One of the major criticisms which has been levelled against quantitative sociology is that it tends to have a conservative bias. I think that there is a good deal of truth in the idea of this sort of relationship, but there is nothing about it which is intrinsic to the use of quantification. To suppose that it is, is to misunderstand the epistemological issues involved.

My general position with regard to the relationship of discovery and change is based on the idea that it does not make sense to say that sociologists can ever be disinterested with regard to their objects of discovery, in the sense in which natural scientists may be with regard to the physical world. This distinction is diff--erent in kind from those arising out of problems of objectivity of a purely epistemological nature. It arises because the objects of social investigation - social activities, problems and institutions - all derive their meaning exclusively from social definition, and these definitions necessarily play some part in the sociologist's personal outlook.

Furthermore, sociology does not leave its objects of investigation as it finds them. Neither does natural science, in principle, but this process is given particular significance because of sociology's being concerned with conscious or self-determining beings. The sort of changes which may be brought about are of the order which change the nature of the sort of questions which it makes sense to ask. The relationship between discovery and change is usually very indirect and accompanied by long time-lags but this does not make it any less significant. Some examples of this relationship have already been given.

Therefore sociologists need to have some idea of what it is that they are likely to bring about. Some acknowledgement of their own position is necessary, not simply as an end to objectivity, but because without it they will not have a clear idea of what they are doing with regard to the context of discovery, as well as the context of change.

Interest and Commitment

The fact that the sociologist has an interest in the object of his investigation does not mean that he shall be guided solely by a prior commitment to what it is he wants to change, criticise or reform. It makes sense for him to enter investigation in a spirit of enquiry and an over-committed approach will obscure this.

This is a pertinent issue in criminology, which has closely been associated with movements like prison reform, penal reform and community action. This is not to suggest that these are not worth--while activities, but the extent to which they have dictated the course of research has in many cases obscured the phenomena under investigation, without assisting the reform programme either.

I have devoted some attention to the way in which, in delinquency prediction studies, for instance, many questions are begged about the nature of crime and the appropriate ways of going about its elucidation. The irony of this approach is that its practitioners have felt it necessary to couch their investigations in this way at all, since they have been half-hearted about actually pressing for judges and magistrates to use the tables in the administration of justice . Successful penal reform can in any case never be brought about purely through research, since it requires for the most part attention to wider political issues, and therefore political techniques of bringing it into operation.

The same difficulties have attended the tieing of neighbour--hood investigations to community action projects. The Chicago Area Project, for instance, was carried out on the basis of a belief that community action could bring about a lowering of the crime rate, and this militated to some extent against the investigation of aspects of the community which did not accord with this belief, like the existence of relatively stable subcultures. Yablonsky's investigations in New York also suffered from the over-influence of his social-work commitment. Similarly, successful community action requires going beyond purely scientific considerations. The New York project, Mobilisation for Youth, was closely founded upon differential-opportunity theory and set out to organize the poor and thus change the opportunity structure. However, it foundered because of political attacks[35], which again suggests that it is the political issues themselves which are of primary importance.

The 'Multi-causal' Approach and Conservatism

The relationship of the dominant 'multi-causal' approach in criminology with a conservative outlook has been stated clearly by C Wright Mills: "If there is any one line of orientation historically implicit in American social science, surely it is the bias toward scattered studies, toward factual surveys and the accompanying dogma of plurality of causes. These are essential features of liberal practicability as a style of social study. For if everything is caused by innumerable 'factors', then we had best be very careful in any practical actions we undertake. We must deal with many details, and so it is advisable to proceed to reform this little piece and see what happens, before we reform that little piece too. And surely we had better not be dogmatic and set forth too large a plan of action. We must enter the all-interacting flux with a tolerant awareness that we may well not know, and perhaps will never know, all the multiple causes at work. As social scientists of milieux, we must become aware of many little causes; to act intelligently as practical men, we must be piecemeal reformers, one here and there"[36].

Mills is correct in pointing out that multi-causal approaches have not brought about social change. But the reason that this is the case is probably rather different. For example, Donald West claims to have discovered five main factors behind juvenile delinquency - low family income, large family size, parental criminality, low intelligence and poor parental behaviour[37]. He goes on to put forward a preventive programme based on, among other things, a transfer of resources from the better-off to the poor, family planning, and extensive instruction in child guidance[38].

This is much closer to what Wilkins has called the 'mass--attack' approach. It is not in itself a piecemeal strategy, but it is liable to be self-defeating. "The assumption underlying the mass--attack is that the whole of a programme is greater than the sum of its individual parts. But may not the reverse be equally probable ? ... Some actions may be operating to cancel out, or even render negative, the results of the other actions. The whole may be <u>less</u> than the sum of the parts, and certainly less than the sum of some of the parts". More of a good thing is not necessarily better[39].

Furthermore, social policy does not, or should not, operate in a vacuum. It is unrealistic politically to expect to be able to implement policies as far-reaching as the ones which West suggests with regard to a specific problem like delinquency. It only makes sense to adopt these sorts of policies in the context of an overall conception of the sort of changes we wish to bring about in society in general. This is because piecemeal policies may, apart from being internally contradictory, contradict other policies; they cannot all be carried out independently of one another. So, for instance, policies which affect the distribution of income are necessarily associated with income and expenditure policies; family planning programmes with health and education policies; child guidance with overall educational policy, etc.

Thus the reason why 'multi-causal' approaches do not engender change may not be that they are inherently conservative, but that they are self-defeating.

In spite of all this it should be emphasized that there is nothing intrinsically conservative about the use of a quantitative element in sociology, or nothing about critical analysis which demands that quantitative technique be abandoned.

The 'New Criminology' Argument

That there is both something inherently positivistic about the use of quantification, and that this is necessarily conservatively oriented, is strongly implied by the authors of "The New Criminology" : "The physical sciences had sought to discover 'law-like generalities' via measurement and quantification of phenomena. Positivist crimin--ology proceeded along similar lines, seeking to develop accurate and calculable units of crime and deviance as a preliminary to generalisation"[40].

The argument is compelling in the way they put it, in that the main target of their critique is Eysenck. I am in complete agreement with this critique, but it raises again the point that behaviourism is not the only way in which it is possible to recognise that human activity may be quantified. The key to Eysenck's mistaken approach to criminology is his refusal to examine concepts about crime, in favour of the injunction to 'stick to the facts'. From this stems his methodological individualism and his deterministic bias, as well as his ability to dodge normative issues.

However, it would be wrong to argue from this that quantification necessarily entails these mistakes. Quantification need not be the rationale or end of discovery in order to be used.

The authors of "The New Criminology" develop their argument thus: "The problem they (the positivist criminologists) faced was that of distinguishing crime and deviancy from normal behaviour on a quantifiable basis, and the immediate and obvious resort was to criminal statistics, furnishing as they did some details of both the quantity and the types of crime committed"[41].

They point to the obvious difficulty that criminal statistics are inaccurate - in some cases 'crimes known to the police' only form a tiny proportion of criminal acts committed - and may sometimes not even reflect changes in the degree of criminality at all, but rather a change in the deployment of police resources, the willingness of the public to report particular offences, etc.[42].

They propose that the response to this problem in 'positive' criminology met with two solutions; 'liberal' and 'radical' positivism. What they call the liberal approach to statistics is identical with what I called the 'critical' approach, i.e. it admits the inadequacy of official statistics whilst suggesting that certain revisions can be made in order that the statistics can be used in analysis. What I disagree with is their claim that this approach necessarily represents an attempt to 'arrive at a moral yardstick on which to build a positive science ultimately concerned with the diminution of unwanted behaviours'; or that there 'is a consensus about moral values'. There is a wide degree of consensus about what constitutes criminal activity. In other words, people are able to reach a wide degree of agreement about what constitutes theft, murder and the like. (This is the basis of the anti-phenomenological argument). But this does not mean that everyone accepts the morality upon which the legal classification is based. Neither does it mean that the criminologists accepts that morality. It may or not be true of Wilkins or or Sellin and Wolfgang, who are taken as being typical 'liberal positivists'. I do not agree with Sellin and Wolfgang's idea of setting up an index of delinquency, or a kind of average of what people consider to be deviant, since this only tends to minimize the genuine and significant differences in the nature of these perceptions. But there is no reason to suppose that in general the acceptance of a particular legal classification necessarily means that it is accepted as a norm. For instance, criminologists sometimes draw attention to the statistics of convictions under laws which they specifically believe to be wrong. Neither does not mean that any social consensus about the validity of the law is assumed. The statistics of criminal motoring offences are no less interesting for the fact that they are known widely not be regarded as crimes.

The authors go on to criticise what they call the 'radical positivists', who attempt more or less to back completely away from the official statistics in favour of self-reports, etc. They rightly point out that this leads to various problems; for instance, it makes certain kinds of criminality appear virtually ubiquitous, and they criticise attempts to formulate normative definitions in terms of system 'imperatives'.

However, this leaves their own position with regard to the use of statistics unclear. The authors comment on the problem of 'multiple realities':- "the idea that different members of society - marijuana-smokers, Jehovah's Witnesses, motoring offenders and professional criminals for instance, describe the world from different perspectives"[43]. This means that there is no set of completely objective facts". It is correct to say, therefore, that: "the social enquirer cannot dispense with the recognition that he faces a choice in the selection of his basic concepts, and that in exercising this choice he is, to some degree, supporting or subverting the system in power"[44].

This does not mean, however, that there do not exist 'realities' which transcend these subcultural boundaries. The authors themselves tacitly acknowledge these sorts of 'facts' in that they take the sub--culture theories seriously - a set of theories which are all based upon the apparent statistical fact that juvenile delinquents are primarily male and working-class. In fact elsewhere they explicitly disclaim a phenomenological orientation. They do not explicitly adopt the view that real rates of social activities do not exist.

The idea of a relationship between a quantitative approach and a conservative outlook is therefore introduced without it being made clear what the alternative to quantification is. My own view is that there is nothing inevitable about such a relationship. The arguments in "The New Criminology" are quite persuasive because the way in which criminologists have commonly used statistics - oriented around the 'multi-causal' idea - does not lend itself to a critical approach to the society in which the crimes take place. That this need not be so, however, follows from the fact that 'multi-causality' is not necessarily entailed by the use of statistics..

There are special problems associated with the idea of a critical <u>criminology</u>. I think that this may be the reason some of the inconsistencies in the argument arise. Taylor et alia, in the course of their attempt to penetrate and criticise existing categories about what is problematic or what constitutes criminality, are led to an examination of structural factors. Those factors are to an extent examined by criminologists like Quinney and Turk, who have noted a relationship between the operation of certain <u>laws</u> and different <u>interests</u> in society. Their work is a natural extension of this sort of analysis, in that they attempt to ground it in, not simply interest groups in the sense of isolated entities, rather the prevailing class structure. In attempting to construct a specifically Marxist criminology they are faced with the problem that, according to Marx himself or any conventional interpretation of his work, criminals and criminality had absolutely no political significance. This has led the authors of "The New Criminology" to some rather contradictory arguments, claiming in certain contexts the authenticity of apparently non-political behaviour like football hooliganism or vandalism - an anarchist, rather than a Marxist argument - while at the same time rejecting anarchism in other contexts.

It would not be relevant to deal with these problems in detail. What I wish to bring out is the idea of an incompatibility between an empirical approach and critical activity. The idea is particularly current in criminology but it has much wider currency. At the same time, I would suggest, it is based on the same sort of confusion between an unwarrantedly scientific approach and one which is empirical, but not positivistic.

As in the field of criminology, there have often been good reasons for associating the issues in this way. This is exemplified, for instance, by the traditional political sociologist's reliance on the study of voting behaviour. It has been pointed out by many critics of this field that the reliance on voting figures as the ideal form of 'factual' information, upon which to build political theories, has not in itself led to any theorietical development. Voting figures tell us about voting behaviour; they tell us a certain anount about political attitudes and beliefs, but only in a very limited and circumscribed way. The approach which puts a premium on voting studies effectively abandons the phenomenon - power or politics - and gives priority to the method, which is seen as being, above all, identified with quantification and the use of statistics. Furthermore, this approach has proved incapable of criticising the existing conceptions of the norms of political activity, because it limits itself so rigidly to modes of investigation which pre-supposes those norms' continued existence.

Nevertheless, there is no reason to suppose that there is anything intrinsically scientistic about the use of quantitative methods in the analysis of political behaviour - the data need not be voting figures, for instance. Neither is there any reason to suppose that this approach should be uncritical. It is an obvious example, but a pertinent one, that much of Marx and Engels' criticism of existing political economy was based on detailed analyses of social and political statistics.

There are also good arguments for supposing that only an empirical, (as opposed to empiricist) approach - in the sense that only an approach which recognizes that the social world constitutes a structured reality, as opposed to the sort of intangible reality proposed by the phenomenological sociologists - is capable of being critical in orientation.

The inability of the phenomenological sociologists to come to grips with this paradox leads to some curious ideas. David Walsh, for instance, criticises the 'British Empirical Tradition' for placing emphasis on the analysis of the incidence and the causes of poverty in Britain as being 'unselfconscious, naive and sterile' in that it confuses social with sociological problems and enshrines a commonsense view of sociology as a problem-solving discipline. He adds that 'this is not to deny that poverty may be a phenomenon of interest to sociologists'[45].

This represents the sense in which the phenomenological orientation has a conservative bias. It is true that 'social problems' (apparent problems in society) are often confused with 'sociological problems' (investigating the nature of social phenomena regardless of whether they are apparently problematic) and it is probably true that poverty studies have to some extent done this. But Walsh's criticism itself confuses the uncritical conflation of social and sociological problems with an unwarranted bringing-together.

The criticism of sterility can very easily be turned on itself, to phenomenological investigations of everyday life which are presumably favoured by Walsh, which have doubtful sociological interest and which are socially insignificant.

NOTES TO CHAPTER FOUR

1. ALAN RYAN, The Philosophy of the Social Sciences, Macmillan, 1970, 140. The example is taken from H L A Hart's The Concept of Law.

2. ALISTAIR MacINTYRE, 'A Mistake About Causuality in the Social Sciences', P Laslett and W G Runciman, eds. Philosophy, Politics and Society (2nd series) Blackwell, 1962.

3. DAVID MATZA, Delinquency and Drift, New York: Wiley, 1964.

4. According to Eysenck, in the 1970 edition of his Crime and Personality, 'Essentially what the deterministic psychologist says is that human conduct is always determined by specifiable causes. What the opponent maintains amounts to saying that behaviour is, at least to some extent, random, that it is not caused in any sense by motive, prior teaching, or in any way' (P. 185) This shows Eysenck's confusion between mechanical explanation and choice. We can explain conduct in terms of certain factors without assuming that these factors 'cause' conduct.

The nub of Eysenck's argument about determinism, and Eysenck's brand of behaviourism in general, is I think expressed thus: 'To some degree, the human being is indeed a machine; it is the task of science to find out the precise extent to which this is true. Whether human beings are nothing but machines is a philosophical question, not admitting of an answer at the moment, and not relevant to our problem. The established facts are our only safe guide in coming to a decision on the important question of how to treat our criminals' (P. 192).

I reckon that this argument is far too circular for comfort - it simply legitimises bad philosophical reasoning. By making 'sticking to the facts' the basis of his approach, Eysenck assumes that which he intends to demonstrate. It echoes similar statements made by the Gluecks (see below p.25).

5. CYRIL BURT, The Young Delinquent, London University Press, 1952, 609.

6. TRAVIS HIRSCHI and HANAN C SELVIN, Delinquency Research: An Appraisal of Analytic Methods, New York: Free Press, 1967, 38.

7. HERBERT H HYMAN, Survey Design and Analysis, New York, Free Press, 1955.

8. SHELDON AND ELEANOR GLUECK, Unravelling Juvenile Delinquency, Harvard University Press, 1957, 5.

9. SHELDON GLUECK 'Ten Years of 'Unravelling Juvenile Delinquency': an Examination of Criticisms', Sheldon and Eleanor Glueck, Ventures in Criminology: Selected Recent Papers, London: Tavistock, 293.

10. HIRSCHI AND SELVIN, op. cit., 78.

11. GLUECKS, Unravelling Juvenile Delinquency, op. cit., 120.

12. HIRSCHI AND SELVIN, op. cit., 80.

13. BURT, op. cit., 590-1.

14. Ibid., 607.

15. LESLIE WILKINS, Social Policy, Action and Research, London: Tavistock, 1967, 36-7.

16. NIGEL WALKER, Behaviour and Misbehaviour, Oxford: Blackwell, 1977, 138.

17. Ibid., 127.

18. Ibid., 132.

19. GLUECKS, 'Ten Years of Unravelling Juvenile Delinquency', op. cit., 280-1.

20. HIRSCHI AND SELVIN, opt. cit.

21. ALBERT K COHEN, 'Multiple Factor Approaches', Marvin E Wolfgang et al., The Sociology of Crime and Delinquency, New York: Wiley, 1962, 123.

22. GLUECKS, Unravelling Juvenile Delinquency, op. cit., 133.

23. Ibid., 120.

24. Ibid., 121.

25. HIRSCHI AND SELVIN, op. cit., 132.

26. DONALD WEST, Present Conduct and Future Delinquency, International Universities Press, 1969, 66.

27. HIRSCHI AND SELVIN, op. cit., 52.

28. GLUECKS, Unravelling Juvenile Delinquency, op. cit., 167.

29. SHELDON GLUECK, 'Theory and Fact in Criminology', Ventures in Criminology, op. cit., 244.

30. Ibid., 91.

31. Particularly in his Method and Measurement in Sociology, New York: Free Press, 1964.

32. Ibid., 33.

33. Ibid.

34. BARRY HINDESS, The Use of Official Statistics in Sociology, Macmillan, 1973, 18.

35. RICHARD QUINNEY, The Social Reality of Crime, Boston: Little, Brown and Co., 1970, 201-2.

36. C WRIGHT MILLS, The Sociological Imagination, New York: Oxford U.P., 1959, 85-6.

37. DONALD WEST, Who Becomes Delinquent, London, Heinemann, 1973, 190.

38. Ibid., 201.

39. WILKINS, op. cit., 127.

40. IAN TAYLOR, PAUL WALTON AND JOCK YOUNG, The New Criminology, London: Routledge, 1973, 11.

41. Ibid.

42. Ibid.

43. Ibid., 27.

44. Ibid.

45. DAVID WALSH 'Varieties of Positivism', Paul Filmer et alia, New Directions in Sociological Theory, Collier-Macmillan, 1972, 43.

CONCLUSIONS

'Empirical' Sociology

The point of the critique which has been presented of the attempt to apply phenomenological principles to sociology is that sociology is based on certain types of observing, identifying denoting and understanding which are so basic to ways of life that it does not make sense to doubt them. The Phenomenologists legitimately argue that the everyday outlook must itself become a topic of enquiry, but the elevation of this to a systematic programme is impossible; neither does it make sense as a critique.

It is contended that the totality of these more fundamental understandings constitutes what might be called the 'empirical basis' of sociology. The word 'basis' in this context does not imply something that is absolutely fixed, uniform, or unchangeable, but something which is basic relative to sociological theories.

The empirical basis is itself 'theoretical' in that it consists of understandings about the world which are applied in a universal sense. This led Schutz to draw a distinction between first-order 'theories', or understandings, and 'second-order' 'theories about theories', in other words, sociological theories. I think it is valid to illustrate the issue in these terms, but if we do we have to remember that 'first-order' theories are 'theoretical' in a quite different way from sociological theories, in that it is not normally conceivable that we may give them up.

This 'basis' is therefore the basis <u>against</u> which or <u>upon</u> which sociological theories are developed. We cannot, as the ethnomethodologists suggest, <u>discover</u> the basis by empirical means.

It is given to us by our culture and is therefore part of our way of looking at the world.

This position has something in common with that of Wittgenstein, who stated: 'My having two hands is, in normal circumstances, as certain as anything that I could produce in evidence for it'[2]. Wittgenstein's argument in On Certainty is as I understand it concerned largely with demolishing the idea of sense-data foundations for knowledge. He does this by elaborating the concept of certainty. My own point of view is that the concept of certainty itself indicates the possibility of a different idea of foundations for knowledge.

This 'basis' of fundamental understandings is for the most part independent of sociological theories, and we are therefore entitled to say that it constitutes evidence for or against sociol--ogical theories. In this sense it makes sense to talk about empirical discovery in sociology; for practical purposes 'empirical' sociology adds up to something very close to empirical discovery in natural science, notwithstanding the central role of understanding. In particular we are entitled to talk about sociological explanation, albeit not 'causal' explanation.

One aspect of this 'facticity' is that quantification is possible. The argument against the use of statistics is, essentially, that it does not make sense to use social variables unless we know what they mean. I have brought out the way in which the failure to realise this has in fact vitiated a great deal of quantitative criminology - the meaning of variables like going to church, or the extent to which mothers go out to work, for instance, vary in space and in time and the measurement in itself tells us nothing. To explain social activity necessarily entails that it is explained in terms of factors which are expressed in meaningful terms. However, this does mean that social statistics per se are meaningless. We are entitled to assume their meanings by virtue of our own experience, under most circumstances. The fact that the meaning of social statistics are problematic does not make them any less real. We are entitled to talk about 'real rates'.

The fact that official statistics are produced by bureaucratic organisations does not necessarily render them meaningless in the way Cicourel and others imply. The corollary of the 'reality' of social statistics is that we can talk about 'mistakes' in classification and therefore 'bias' and 'inaccuracy'. These are legitimate expressions.

An important point is that there is no contradiction between this view of social statistics, and acknowledging that all sociological knowledge is founded upon understanding. It is not that statistics are intended to supplement the process of understanding - in fact this concept is unrealistic since it ignores the way in which social statistics themselves are founded upon understanding. The point is that there is a large area of understanding which is so familiar to us as not to constitute a <u>deliberate</u> or <u>conscious</u> attempt to penetrate another way of life - insofar as we are dealing within ways of life which are familiar to us, this understanding is more or less <u>automatic.</u>

Therefore it might not look, on the face of it, if 'the method of understanding' is being used. A distinction is often drawn between the method of understanding on the one hand, and the method of statistics, on the other. Weber is seen as being somewhere in the middle. Statistical sociology and the method of understanding are seen as alternative perspectives. My own position is that this conception is based on a false conception of the role of under--standing in sociology, as well as of the nature of social statistics.

When we talk about the 'method' of understanding we are referring to a way of going about sociological enquiry, but the fact that it is a 'method' in this sense should not obscure that in fact this is the way in which knowledge about the social world, is, relatively automatically, acquired. The 'method' of understanding really adds up to acknowledging or recognizing the function that understanding necessarily plays.

Once the idea of this dimension or distinction is abandoned, then it becomes inappropriate to label the statistical method 'positivist'. Although in actual fact the emphasis on statistics on the part of, for instance, Durkheim, and a number of the statistically-oriented criminologists, developed out of a desire to emulate the natural sciences, there is nothing in the use of statistics themselves which is necessarily associated with such a position - rather the reverse, since the acceptance of the idea of understanding, which I argue is fundamental, necessitates the acceptance of a radical difference between the methods of sociology and the natural sciences. The fact that statistics have often been used in a misconceived way (for instance with relation to the assumption of an identity between explanation and prediction, and the idea of 'multiple causality') should not blind us to this.

The corollary of this view, which sees social activity-as-it--is-normally-understood, as a real or substantial phenomenon, is that sociological theories are realistic expressions of that activity, or more than simply explanatory devices or instruments.

Sociological theories operate at a different level of certainty; it makes sense to be prepared to doubt them. Theories about suicide or about crime are theories we can test against evidence which we do not, and are not capable of, doubting in the same way - evidence about what it means to steal, for instance. This is not to suggest that this is not achieved by way of understanding - the concept of theft presupposes an idea of property - but that it is part of a very pervasive way of life, and therefore part of the common understanding.

The idea of testing does not imply that falsification is ever conclusive, but it is an important idea nonetheless. This does not mean that sociology has to restrict itself to the production of falsifiable theories. Much of what is normally considered to be part of the subject matter of sociology, like conceptual clarification, is unfalsifiable, and this is not to say that these arguments cannot be resolved; only that it is misconceived to suppose that they can be resolved empirically. This relates to the case of ideas which present themselves in empirical terms without being amenable to falsification. They are not necessarily wrong, but the realisation of the concept of a distinction between falsifiability and unfalsifiability is the only way in which it can be demonstrated that such a theory has a compelling quality which arises from something more than its substance.

Paradigms undoubtedly exist in natural science but with respect to sociology we should take a radical[3] approach to Kuhn's ideas. That is to say, paradigms have to be avoided in sociology, not constructed. This is because of the way sociological theories themselves enter into the common understanding. Thus any attempt to create an overall paradigm in sociology contradicts any attempt to criticise it. Explanatory models presuppose views of society, and the more hegemonic the model the more restricted our view of society will turn out to be. We cannot admit of both paradigms and the idea of sociological reflexivity.

Understanding Subcultures

The concept of an 'empirical basis' is not intended to suggest something which exists outside our consciousness.

This 'basis' is achieved actively, by a process of understanding Social activity is, to use an expression of the ethnomethodologists, 'negotiated'. This represents the distinction between the nature of empirical knowledge in sociology and the natural sciences. In the latter, data is not received passively (as has been shown by Hanson) and may even be culturally mediated (as has been suggested by Richard Gregory), but the perceiving activity does not require us to understand it in the way we understand the model of social activity. That is, in observing the social world we perceive something about the culture or the social environment of the object.

We need to understand something about that environment in order to understand what is taking place. Unless we are able to do this all we observe is an apparently meaningless or aimless set of movements.

If culture were homogeneous, then these perceptions would be uniform. Everyone would understand social activity in exactly the same way, and these understandings would therefore constitute a set of objective knowledge about the world.

Obviously culture is not homogeneous in reality. This raises the problem that empirical knowledge in sociology cannot be objective, if there is no universally agreed-upon way of identifying what counts as evidence. Viewed from the problem of the explanation of social behaviour, the problem arises that we cannot produce explanations that transcend cultural boundaries. We cannot, that is to say, explain activity in one culture by the standards of another - the evidence we produce would be purely relative to the culture we are observing.

The extent to which this is actually a problem is a function of the extent to which or the way in which society is actually differentiated. My conclusion, from looking at the use of participant observation in the sociology of deviance - that is, work that presupposes some degree of cultural differentiation - is that common aspects of culture remain which serve to make the relativistic picture one which need not be borne out.

Members' own accounts and conceptions enter into our understanding of the subculture, but we need not necessarily accept them at face-value. The members' world may constitute a reality which is somehow different from the world of the outsider, but we are still able to judge as to the correctness or falsity of members' conceptions. It is possible to show, for instance, that they consistently exaggerate. Opposing conceptions of reality are essentially translatable. This does not mean that they can necessarily be resolved, but that common criteria exist upon which to make these sorts of judgements (in this case, common criteria for denoting 'exaggeration').

It is worth extending this discussion to encompass the question of how we understand primitive societies, which represent an obvious analogy with deviant subcultures with regard to epistemological questions, and in.particular to examine Peter Winch's ideas in this context.

Winch's paper 'Understanding a Primitive Society'[4] was a comment on aspects of Evans-Pritchards' view of Azande society and on some of Alistair MacIntyre's criticisms of Evans Pritchard and himself.

Winch acknowledges the influence on the views expressed in this paper of some remarks Wittgenstein made about Frazer's <u>The Golden Bough</u>[5]. The essence of Wittgenstein's view is that Frazer was ethnocentric, in that he saw primitive magic and ritual as a kind of primitive science, without realizing the symbolism intrinsic to it. Winch criticises MacIntyre along similar lines - MacIntyre tends to see in Zande magic only a misguided technique for producing crops. 'But a Zande's crops are not just potential objects of consumption: the life he lives, his relations with his fellows, his chances for acting decently or doing evil, may all spring from his relation to his crops'[6]. He goes on to make the point that blindness to this point highlights the pervasiveness of alienation in our own society.

I think this argument is valid in its own terms, but it begs the question of how far Zande culture is actually a 'way of life' in the sense of being homogeneous. Winch's arguments themselves tend to subsume, within the paradigm case of 'primitive society' an attitude which in its own way is as equally ethnocentric as Frazer's - that is to say, one which sees 'primitive' societies as undimensional entities, not subject to conflict or change.

As Winch himself points out, 'language games' are not isolated - 'Language games are played by men who have lives to live - lives involving a wide variety of different interests, which have all kinds of different bearings on each other'[7]. This does not only mean that Western culture has something to learn from primitive societies, it also indicates, as MacIntyre[8] points out, that people in 'primitive' societies are themselves confronted by alternative belief systems.

In actual fact people in the southern Sudan, as in practically every other part of the world are, faced, for good or ill, with Western values, and the Western concept of science. This does not mean that the cultural features of the indigenous society are any less meaningful to its members, but they are more likely to be treated in the same way religion is in our society i.e. to some extent it is evaluated according to scientific precepts.

This is why although Winch's criticisms make sense when applied to Frazer's and to a lesser extent, Evans-Pritchard's views about primitive societies, they do not illuminate the real-life issues involved in understanding primitive societies or understanding society in general, in the way that he implies.

Ways of life are just as divergent and heterogeneous as they ever were, but the essence of ways of life, as Gellner points out[9], is that they are numerous, diverse, overlapping, and undergo change. What I have tried to bring out is that phenomenological and ethnomethodological sociology (and some of the practitioners have drawn explicit comparisons with Winch's ideas in this respect) have, in stressing the heterogeneity of ways of life, under-rated the common features, and have been therefore drawn into making obscure that which is clear.

Critical Sociology

Some attention has already been given to the idea that quantification in sociology entails an uncritical outlook, with reference to the 'multi-causal' concept, the idea of interest and commitment, etc. But there is an important and more fundamental argument associated with the Frankfurt school of sociologists which sees such an outlook as being entailed by the very idea of the existence of theories in sociology which are in some way independent of 'factual' elements. '... the object of sociology, society, is already structured ... Knowledge derived from an uncritical acceptance of empirical facts becomes a pure reproduction of the existing relations of society'[10].

In the examination of Douglas' study of suicide the concept of culturally-founded understandings or interpretations was equated with 'common-sense'. It is worth exploring further what we mean by common sense in this context. When Wittgenstein drew attention to Moore's notion of his two hands constituting proof of their existence he was talking about a category of understanding that is inconceivable to suspend under any circumstances. The class of activities which Douglas raises are not quite in this category. I am arguing that it is unrealistic under normal circumstances to doubt that suicide is related to unhappiness, or that the individual who hangs himself and then leaves a note is intending to do away with himself. It is unrealistic to assume, for instance, that when we see someone digging the garden that he is burying a dead body. But we can doubt these sorts of activities; there is nothing inconceivable about doubting them, in the sense that it is inconceivable to doubt the reality of our hands.

For instance, the hanging of an apparent suicide may have been contrived by a murderer, and the note forged. People digging in their gardens are not necessarily 'gardening'. A way of expressing the distinction is that while Wittgenstein was commenting on things which are intrinsic or of the essence of a 'form of life' Douglas is talking about aspects of ways of life. The criticism of Douglas is that his non-recognition of the pervasiveness of certain aspects of ways of life leads to a lack of recognition of the distinction between activities which are comprehensible in this, 'common-sense', way, and those kinds of interpretation which, by the standards of our form of life, must be unlikely - in the examples here, wildly unlikely.

So in referring to 'common sense' we are talking about something which can be shown to be wrong. One of the ways in which common sense is constituted, for instance, is by the internal--isation of social and political theories. What starts out as the idea of an individual or a limited group of people may, by becoming sufficiently influential, become the property of the culture at large. It becomes part of what Berger and Luckmann call the 'social stock of knowledge'[11].

It may be part of our everyday way of interpreting the world, for instance, to see juvenile gangs as organized groups, or to assume the validity of the escalation hypothesis with regard to drugtakers (One aspect of this, of course is that it is not actually conceived of as a 'hypothesis'). There are many other examples. One which comes to mind is the maternal separation or deprivation hypothesis - the idea originally inspired by Bowlby which places great emphasis on emotional development in the first five or so years and supposes incalculable harm as being concomitant with separation from the mother.

To examine these sorts of areas is therefore to engage in critical activity, in the sense not simply of criticising sociological theories, but of criticising existing social arrangements. The 'facts' which constitute the common understanding are questioned, and this involves questioning society.

However, rejecting the 'uncritical acceptance' of empirical 'facts' in society does not entail that we have to engage in a routine questioning or a programme of trying to 'discover' what lies behind the common understanding. As I stressed in chapter two, we can only criticise the common understanding from some point of reference. In the sense of the critique of ideology, this may often be traced back to social and political interests.

Thus although I am defending the idea of the independence of sociological theories from social 'facts' - a Popperian position - I do not think this entails an uncritical approach.

There are two main ways in which I differ from the Popperian position with respect to this. The first is Popper's rejection of the sociology of knowledge in its entirety. The problem with Popper's view in this context is that he tends to assume that the sociology of knowledge assumes a privileged out-look on society. This can easily be countered by the 'tu quoque' objection - that the sociology of knowledge is itself subject to the sociology of knowledge. The mistake is in arguing from this that the sociology of knowledge is itself illegitimate, since it does not necessarily entail a dogmatic view of social reality.

The other sense in which the Popperian view is rejected is with respect to the distinction between facts and values. It is not simply that the Popperian view of the value-neutral social scientist is unrealistic; values are themselves intrinsic to the observation of social phenomena. This does not mean that sociology is reduced to ideological warfare - we are not talking about values as things which are purely personal to the observer. Identifying everyday activity necessarily takes place in terms of the members' valuations. It is in this sense in particular that I have attempted to bring out the incongruity of Douglas' placing terms like 'unhappiness', 'brooding' etc., 'in inverted commas' as it were; it does not make sense for us to doubt the reality of these values, whether we actually agree with them or not.

Some Further Implications

It has to be said that there is no conclusive way of deciding what the 'empirical basis' of sociology actually is - what belongs to it and what does not. This is because the criterion for such a decision is itself based on our own life-experience.

My argument is that to identify, for instance, someone as voting, praying, thieving, gardening, etc., is so basic to our way of life that it does not make sense to treat it as a problematic phenomenon, but there is no justification for this view outside our own experience of the part that these activities play in our way of life.

Therefore the argument is apparently arbitrary, and ultimately circular. The Only counter to a charge of circularity is that if someone objects to such an identification - the activity of 'gardening', for instance - and asks us what justification we have for labelling it in this way, we can point out that he cannot have independent grounds for doing this, and that if he insists on objecting, that the objection is purely in a spirit of philosophical enquiry. That is to say, we know that he knows that we understand 'gardening' in much the same way as he does.

Ultimately all that we can say is that we have every reason for treating everyday social phenomena in this way, even if we do not have any justification for doing so.

In stating this the phraseology of Popper's discussion of the 'basis problem' is being used deliberately. The interesting thing about Popper's argument in this context is that it is conducted purely with reference to natural science. I do not think that Popper grasps properly the problems of observing social activity because of his lack of appreciation of the concept of understanding. But I think that in the sense of different levels of uncertainty what he has to say is apposite for what we find in relation to observing social action. It is impossible for the scientist to doubt the operation, for instance, of basic measuring instruments in the sense in which it is conceivable for him to doubt scientific theories. This is itself partly a function of the social environment in which he finds himself, in the restricted sense of the scientific community. I think this problem ultimately comes down to a psy--chological question to do with one's ability to doubt things.

NOTES TO CONCLUSION

1. ALFRED SCHUTZ, 'Concept and Theory Formation in the Social Sciences', Sociological Theory and Philosophical Analysis, edited by Dorothy Emmet and Alistair MacIntyre, Macmillan, 1970, 12.

2. LUDWIG WITTGENSTEIN, On Certainty, Blackwell, 1969, 255.

3. The distinction between a 'radical' and 'non-radical' approach to the possible application of Kuhn's ideas to sociology is introduced by John Urry in 'T.S. Kuhn as a Sociologist of Knowledge', B.J.S., 24, (1973).

4. PETER WINCH, 'Understanding a Primitive Society', Rationality, edited by Brian Wilson, Blackwell, 1974.

5. Wittgenstein's remarks on Frazer's Golden Bough, translated by Anthony Manser, Southampton University: xeroxed.

6. WINCH 'Understanding a Primitive Society', op. cit., 106.

7. Ibid., 105.

8. ALISTAIR MACINTYRE, 'The Idea of A Social Science', Wilson, op. cit., 129.

9. ERNEST GELLNER, 'The New Idealism', Cause and Meaning in the Social Sciences, edited by Ernest Gellner, London: Routledge, 1973, 56.

10. DAVID FRISBY, 'The Popper-Adorno Controversy' Philosophy of the Social Sciences, 2 (1972), 112-13.

11. PETER BERGER AND THOMAS LUCKMANN, The Social Construction of Reality, London: Allen Lane, 1967, 56.