

CONCEPTIONS OF ORDERLINESS AND THEIR IMPLEMENTATION

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Ethnomethodology (hereafter EM) and ethnography (EN) have one or two very important things in common which set them apart from the mainstream of sociological thought.

Both of them regard the work of sociological description as a serious and central task that is deserving of just as much care and attention as some sociologists would like us all to lavish in 'explanation'. It is difficult to obtain the recognition that the work of sociological description is complex, difficult, demanding and deeply problematical, fully in need of the same kind of serious and principled examination that is so readily accorded to the 'theoretical' problems over which sociologists love to agonise. It is much more difficult to obtain the further recognition that these problems are not those of 'mere description' but are the practical representation of those issues which are presented speculatively at the level of 'theory'. Within the tradition of ethnographic work, the most forceful attempt to establish the intimacy of 'theory' and 'description' was in the work of the Cognitive Anthropologists (or whichever of their many names you would prefer to use) who sought to identify the two, making the theory of a culture the same thing as its descriptive apparatus. The difficulty in obtaining the widespread recognition of these things, however, may be due to the fact that description necessarily involves one with the particular case. Wittgenstein complained of philosophy's contempt for the particular case, but it is just as well for him that he did not know about sociology.

Both EM and EN give a prominent place to the *in situ* inspection of the phenomena they aim to describe. Neither confines itself entirely to the observation of 'naturally occurring' courses of action, and both are prepared to employ contrivance to bring their phenomena to

view but both are sociologically unusual in that they are willing to regard the observation of naturally occurring activities as generative of materials that are just as good, if not actually preferable to, those which can be obtained only through the use of the most elaborate methodological artifice. This conjoint acceptance that the observation of naturally occurring activities as a suitable locus for inquiry indicates a further measure of agreement, that the topics of analytical interest are to be found within the daily lives of society's members, that *somehow* the naturally occurring activities of those members exhibits the life of the society, and that, therefore, an appreciation of what the society is like, can (at least partially) be obtained through inspection of its everyday conduct.

These commonalities are not, however, sufficient to establish bonds of close kinship between EM and EN, for there are differences which are even more important and which lead the two enterprises to be about different things, in search of different ends, doing their work in *fundamentally* different ways. Putting the differences in terms which are favourable to our own position, we can say that EM has thought more systematically and thoroughly about the problems of sociological description and has been prepared to face up to and press through the implications of those problems. We recognise, of course, that ethnographers wouldn't agree to such a relatively favourable assessment of EM though we will try to give some reasons for thinking that it is not altogether unreasonable. We shall mainly be trying to articulate the differences between EM and EN in order that these can more clearly be seen, rather than trying to argue for EM's advantage. Everybody in sociology now knows what EM is all about - or at least, they think they do. They have managed to contain it as another sociological paradigm as though it were a series of variant answers to the questions sociology

traditionally poses, thus directing attention away from the fact that EM was meant, from the outset, to drastically affect the *practice* of sociological inquiry. As far as we can see, most sociologists *do not know* what ethnomethodology's arguments would mean in terms of their effects on things that researchers do and they do not, therefore, have very much idea of what EM is all about. It is for this reason that EN provides a useful point of contrast, for it is a form of inquiring activity and is, then, the sort of thing that EM should transform.

In order to display *some* of the *critical* differences we will offer some reflections on Clifford Geertz' stylish essay on 'thick description', an essay in which he takes many of the positions that are seemingly regarded as the same type as those characteristic of EM - e.g. on the need for an 'interpretive approach', 'taking the actor's point of view', recognising the 'public' character of culture - and yet despite these proximates Geertz' solutions to his problems most decidedly do not satisfy us and they fail to do so because of what they mean in terms of researchers' practices. Despite the fact that Geertz' arguments are (and are presented as) being at odds with the whole array of anthropological doctrines about ethnography, there is no *essential* difference between them because those doctrines are all compatible with the same unreconstructed ethnographic practice.

There are many different ways in which the events in sociological thought over the past several years can be presented. One is to say that the idea of 'social facts' as 'constructions' has come to replace (at least to challenge) the idea of them as 'things'. The 'new' view is that facts exist only within a frame of reference, that, therefore, 'a fact' is always the product of some interpretation and that, therefore, the researcher is not observing things but interpreting meanings - hence the talk of 'interpretive' sociologies. It is amongst such that

Geertz includes his own approach - 'an interpretive theory of culture' - and we think that although bald and hardly so subtle, eloquent or elaborate, that the 'new' view is a fair summation of Geertz' views on thick description. The view of social facts as 'constructions' is perhaps to be welcomed but not if it is to be so inconsistently applied as the 'new' view tends to have been, such that skeptics (like us) would be inclined to summarise the 'new' view as: - there are no objective facts - except when they suit. The inconsistency which we find in the application of the 'constructionist' or 'interpretive' emphasis may be due to the fact that thoroughgoing implementation of it inevitably leads to serious changes in the conceptions of what sociologists are doing and can hope to do, changes which are rather more consequential than many seem to like. They do not want to change their estimates of the nature of the work nor the goals they can realise and hence they retain these, much the same as they have 'traditionally' been conceived, and try to reconcile them with their new conceptions of what kind of things cultures and social structures are. Thus Geertz retains both the aim - the description of culture - and the means - the practice of fieldwork - but seeks to revise, essentially, the notion of what kind of thing a culture is; it is 'essentially a semiotic one' (p. 5).

If, however, we *do* take an interpretive approach and if we take such an approach consistently then we surely see that the practice of fieldwork is not some 'neutral' instrument for the assembly of 'facts' but is, itself, the work of interpretation, of making out, from amongst them, what events and organisation are - they are the ways in which the researcher actually identifies 'the culture' that he is inquiring into. The textbook ensemble of field techniques is not, then, a methodology which yields up materials which can then be interpreted by a 'theory of culture' but is, itself, the embodiment of a 'framework' for interpre-

tation, a set of ideas about what social structures/cultures are like, how they are to be investigated, how their properties reveal themselves/are revealed, and how they are organised to produce those properties such that they can be induced to reveal them in response to the investigator's manipulation.

A first fairly significant point of differentiation may here be noted in respect of attitudes toward the construction of methods manuals, something which is neither so peripheral nor gratuitous as it may now seem. The preparation of manuals of method is a standard feature of sociology and the specialist methodologists are quite prestigious members of the profession and the purpose of their being seems to be that of constructing a standardised set of procedures which can be learned and applied by researchers, this being regarded (it seems) as something which offers our best prospects in the way of emulating the objectivity of the successful (i.e. natural) sciences. However, as our colleague Dave Hatch points out, the natural sciences do not feature or recognise method as a specialist aspect of their organisation, nor do they feature standard methods in the sense that some sociologists seem to imagine sociology doing. The kinds of procedures of inquiry used vary with the matters inquired into and are related to ideas about the nature of those matters - high energy physics is not conducted in the same way that laboratory chemistry is, and many of the differences between them seem to have to do with the ideas that are current about the nature of micro-particles and chemical elements. There is one further peculiarity about sociological method - it is not only felt, seemingly, that standard methods can be designed but that they must be laid down *in advance* of inquiry, must stipulate the conditions of proper inquiry. They don't, therefore, cumulate what has been learned from productive inquiry, they dictate what kinds of things can be recognised

as findings. The extent to which you find this situation exceeding strange is perhaps a measure of your likely responsiveness to EM.

The point about sociological method, then, is not that it is dissociated from ideas about the nature of its phenomena but that it has some pretty firm and preconceived ideas which can impose themselves, willy nilly, on the researcher who uses them, with regard for the ideas he might *think* he is implementing. Thus, our concern at the exclusion of ethnographic work from explicit consideration.

Geertz begins, from our point of view, very promisingly. He notes that the idea of 'culture' has been the keystone of anthropological thought but that it is (to mix metaphors) something of a conceptual stew. He argues, forcibly, that the idea of culture needs connecting to ethnographical work, that understanding the latter is a key to the former. He says:

'Operationalism as a methodological dogma never made much sense so far as the social sciences are concerned, and except for a few rather too well-swept corners - Skinnerian behaviorism, intelligence testing, and so on - it is largely dead now. But it had, for all that, an important point to make, which, however, we may feel about trying to define charisma or alienation in terms of operations, retains a certain force: if you want to understand what a science is, you should look in the first instance not at its theories or its findings, and certainly not at what its apologists say about it; you should look at what the practitioners of it do.'

Geertz rightly reflects that strange doctrine about the meaning of terms, operationalism, but adopts instead a position which seems to us a (sensible) modest operationalism, i.e. to find out what something is, look to see how it is done, to find out what anthropology is, see what anthropologists do

'In anthropology, or anyway social anthropology, what the practitioners do is ethnography. And it is in understanding what ethnography is, or more exactly *what doing ethnography is*, that a start can be made

toward grasping what anthropological analysis amounts to as a form of knowledge.'

This kind of operationalism is introduced by Geertz in service of a pejorative contrast between what anthropologists say they do and what they *really* do, a contrast which may well be justified but which depends upon the making of a more basic connection - that the idea of culture is closely connected with the practice of ethnography, that ethnographic work is the things the notion of culture means to be talking about, it seeks to make sense of and to further organise ethnographic work. As a methodological doctrine, Geertz' modest operationalism offers the prospect of the kind of test that we think can profitably and regularly be applied in sociological argument: what do the various positions look like in terms of research practices and materials. When, that is, we talk of 'actor's points of views', 'cultures', 'social systems', 'authority relationships', 'nuclear families' and all the other abstractions we deal in, what are we walking of in terms of researcher observable doings on the part of members of society? When we talk of 'culture', what kind of ethnographer observed organisation of activities are we intending to refer to - what are the members of society doing that makes up/exhibits 'common culture'?

Geertz' modest operationalism seems, further, to have equally profitable use as a basic sociological policy, a procedure for looking at the activities it studies. If we want to know, sociologically speaking, what something is, then we should look to see how it is done. The law is (in some sense) what lawyers, judges, legislatures and other involved parties do, science is (in some sense) what scientists do, medicine is (in some sense) what doctors, paramedical personnel, patients and others do and so on. Indeed, in a way, that programme of modified operationalism might be seen as akin to what most sociologists see them-

selves as doing - looking at institutions, theories, ideas, etc., as organisations of actions. The argument amongst us perhaps comes when we begin to explicate the bracketed expression (in some sense), start to spell out what the programme of 'looking to see what people do' means in practical terms. Certainly, it is at that point that Geertz and ourselves take quite different directions; we are deeply disappointed when he says:

'This, it must immediately be said, is not a matter of methods. From one point of view, that of the textbook, doing ethnography is establishing rapport, selecting informants, transcribing texts, taking genealogies, mapping fields, keeping a diary, and so on. But it is not these things, techniques and received procedures, that define the enterprise. What defines it is the kind of intellectual effort it is: an elaborate venture in, to borrow a notion from Gilbert Ryle, "thick description".'

We should have made it clear, above, why for one reason we would have thought it was very much a matter of methods, that the materials accumulated by 'selecting informants, transcribing texts, taking genealogies, mapping fields, keeping a diary' and all the rest of the paraphernalia of fieldwork are what *effectively* 'the society' or 'the culture' will be. The issue, however, goes somewhat further, having to do with the notion of the role of 'culture'. Geertz points to its pivotal status in anthropology but does not ask in what capacity it occupies that status. It seems to us that the idea of culture occupies its pivotal position as a means: it is a notion which (is meant to) enable us to understand the orderliness of activity, the way a society works. In Geertz' treatment, however, the notion of culture appears to have achieved the status of an end: the aim of ethnographic enquiry is to describe the culture and the problem than is to say what kind of thing a culture is, in order to know what needs to be done to describe it *properly*. Geertz' line of argument thus precludes the very

question we like to raise: is the notion of a culture necessary to the understanding of the production of orderliness in social life?

This is a particularly crucial dividing point where, it seems to us, one can make options. Either one does treat 'cultures' as the object of inquiry for anthropology (in which case our remarks are largely an irrelevancy) or one regards the notion of 'culture' as a candidate solution to the problem of social order, one which, in its crudest form, such that it will serve as the lowest common denominator for all the varied usages of the term, solves the problem through "learning". *If* one adopts this latter view, *then* our argument has some further force. *If* one takes this view, then, a society is something which can make itself known through talking with some of its members, reading some of its writings, listing the relationships of some of its families and so on. The question is, what kind of an organisation is it that can make itself known thus? Clearly, from that point of view, a proper understanding of what ethnographers *do do* would be conducive not only to an understanding of ethnographic work, but of the nature of social organisation itself.

However, at this point we might seem to be countered by one of Geertz' lines of argument. His emphasis upon 'thick description' is meant to argue against precisely this view, to show just how much *further* interpretation is required to make sense of the materials that the fieldworking ethnographer trawls up in the field. Toward that end he quotes extensively from his fieldnotes, a long, informant-related tale, that shows 'quoted raw, a note in a bottle....how much goes into ethnographic description of even the most elemental sort' and that 'what we call our data are really our own constructions of other people's constructions of what they and their compatriots are up to' (p. 9). This leads to the view that the ethnographer's work is a matter of

pattern-finding (culture, operationally speaking, becomes the patterning template) and that it is hard work, patterns are not easily found, any more than was the code to Linear B:

'Doing ethnography is like trying to read (in the sense of "construct a reading of") a manuscript - foreign, faded, full of ellipses, suspicious emendations, and tendentious commentaries, but written not in conventionalized graphs of sound but in transient examples of shaped behaviour.'

(p. 10)

What now becomes the issue, from which other considerations follow, is the idea of orderliness and the ways in which this may be detected. Geertz' presents a view which does not seem, to us, at all unusual or idiosyncratic but seems rather to be a very standard notion of the relationship between sociological research and the order it portrays: social life is axiomatically held to be patterned, regular, orderly (this is, of course, the very first assumption of the most elementary sociology class) but *at the same time* the pattern, regularity, orderliness, is regarded as obscure, concealed within the very materials that make it up. The analogy with a jigsaw puzzle occurs to us: its pattern may be held to be present within its pieces, but not recognisable in them because it is fragmented, dispersed, disordered and will become visible when the whole thing has, painfully, slowly, piece by piece, been re-assembled. The jigsaw analogy serves to suggest another feature of the conception, that the pattern which is sought is an *overall* pattern, one into which *all* the pieces must be assembled to make up a unified pattern - the notion of system is never very far away (in sociological terms) and Geertz' account turns out to be a variant - albeit a *reluctant* variant - of the system's approach:

'actual systems must have a minimal degree of coherence else we would not call them systems; and, by observation, they normally have a great deal more. But there

is nothing so coherent as a paranoid's delusion or a swindler's story.'

(p. 17-18)

These conceptions fit, very well, with the standard conception of sociological method, which is *very generally* treated as being the primary characteristic/virtue of being 'systematic', where that characteristic is held to be contrastive with the 'unsystematic' and 'impressionistic' experience of the members of society. The methods are intended to bring the pattern, orderliness, regularity of social life to view but they are to bring to view patterns which *would not* be detected by the members of the society because they are only discoverable in the actions of large numbers of people, through the accumulation of vast quantities of materials, by observation over prolonged periods of time. It is a *presumption* of much research that the 'researchers' and the 'natives' versions will be discrepant, a presumption that will inevitably be justified if methods are *designed* to reveal patterns which are not/cannot be recognised by those who live within the society.

We recognise, of course, that there are those who *also* see this for the problem that it is, and who propose, as remedy, to 'take the point of view of the actor', as does Geertz himself:

'Nothing is more necessary to comprehending what anthropological interpretation is, and the degree to which it *is* interpretation, than an exact understanding of what it means - and what it does not mean - to say that our formulations of other people's symbol systems must be actor-oriented.'

(p. 14)

Agreed, and we think it means something other than Geertz thinks it does, and we will return to the topic below.

The first thing that arises from the conception of 'orderliness' which we find in Geertz and prevailing throughout sociology, is that this conception looks upon the activities of 'the world of everyday life' as

fundamentally disorderly, in the same way that the pieces of the jigsaw, scattered about, are disorderly - their patterned character cannot be recognised *until* they have been reconstructed. In accord with such a view, we find the sociological literature stuffed with complaints about the ways in which everyday activities are disorganised, about the disorganised character of practical reasoning and practical inquiries, about the ways in which the organisation of daily life creates obstacles and inhibitions to the proper execution of the requirements of research method, about the ways in which the appearances of daily life function to 'conceal' the real determinant structures which are the proper objects of sociological attention. Orderliness is conceived very much in terms of the imagined requirements of 'systematic' research method and the way activities have to be organised to fit the programme written out of those requirements, and those requirements are then applied to the affairs of daily life which, surely enough, don't comply with them. The two most vexed cases are, of course, those of 'rationality' and of 'ordinary language'; in the first case, models of 'rational action' are stipulated by the researcher and then used to appraise the extent to which everyday conduct is rational (hardly surprisingly, not a lot) whilst in the case of 'ordinary language' that has been derogated for its failure to satisfy the (supposed) conditions of scientific communication, e.g. determinate sense, precise meaning, etc. Everyday activities are perceived, then, in terms of their lacks, their failings, their shortcomings: they are not orderly, not organised, according to the standards of orderliness set up for scientific inquiry. Of course, the disorderliness is only 'superficial', there *is* an orderliness to be found, one which is brought to light through the application of sociology's techniques of inquiry.

To continue a little further with this line of argument, we can say that the sociological display of orderliness is frequently achieved

through showing that the requisite activities can be fitted into some 'overall pattern' of organisation which requires their occurrence *or* through showing that the activities fit a pattern the same as, or very similar to, that which is to be found in other activities. In either case, the description of the organisation activities in sociology exhibits a feature which is surprising, puzzling, and massively noticeable to ethnomethodologists which is, simply, that they make little, if any, mention of the activities being organised. The point is one which can be made by a rare exception; Peter Letkemann's study of crime as work. The literature on deviance, as you know, is vast and has expanded enormously in recent years (though the great boom is now over) and the literature of deviance studies has an immense amount devoted to crime and criminals but there is next-to-nothing about what criminals do when they commit crime (the main thrust of the deviance literature being toward finding 'good reasons' why people do commit crime, what sorts of people commit crime, etc., etc.). Letkemann's contribution was to ask criminals how they did their work, how they raided banks, blew safes, parked their cars, made their getaways and so on. We're not, then, saying that there aren't *any* studies which describe what people do when they do some particular activity, but we are claiming that the exceptions are pretty *rare* exceptions and remarkable as such. It is, to repeat, striking that sociological inquiries seldom, very seldom, address the activities which they take as topical - studies of scientists show little interest in the science they are doing, studies of doctors show little interest in the dispensation of medicines, etc. The point can be made by a complaint repeated and endorsed by Spradley in his book on the ethnographic interview, the complaint that one recurrently hears from beginning and working ethnographers, that the members of the society spend a lot of time going about their business rather than doing anything

'sociologically interesting': *viz.*

'I recall one beginning ethnographer, a physical education major, who against my advice, chose a member of the swim team as an informant. "I'm not a swimmer", he said. "I know hardly anything about the swim team". But soon he brought in his field notes with the common complaint: "I can't find anything in what my informant says. There are different kinds of strokes and things they do at swim meets, but not much else".' (p. 50).

That the members of a swimming club spend their time swimming and talking about swimming seems to the observer an utter irrelevance, distracting them from the *real* work of social life (i.e. doing those things which sociology recognises as proper activities, e.g. maintaining identity, creating social boundaries, enforcing a narrative order, exercising social control, whatever - there is a very, very long list of 'things to do' in this sense.

Quite why this tendency is so marked can be the subject of lengthy investigation but *one* reason for it is, perhaps, the desire for generalisation which takes the operational form of an attempt to represent any activity as a version of activities-in-general, to see its activities organised *in the same way* as are those of some other pursuit to which it has no other relationship or resemblance. The hallmark of a sociological achievement is (for many) the production of a surprising view of a topic, making it look quite different than it 'normally' appears and one way to do this is to make it look like some other activity to which it has no other readily recognisable connection, to treat this activity as a version of some other. A recently popular strategy for this has been the use of 'economic' models, attempting to show that all activities can be seen to have an exchange principle as their basis, even those activities which are not 'normally' regarded as having any economic attributes, e.g. friendship, love. Another popular approach in the same view has been to view all activities after the fashion of military-cum-diplomatic dealings,

as 'strategic' in character, processes of bargaining and negotiation. And that approach, of course, is closely associated with the ubiquitous practice of making 'the game' a model for social life, for non-game activities.

The sociologist, then, is engaged in idealising away the "identifying" characteristics of activities, in order that he may detect-and-display their formal structures and whatever justification may be produced for this practice, it remains, for us, a most odd one, which deprives activities of the very characteristics that make them the doings that they are. It is not a matter of attempting to 'recapture the taste of soup', of complaining about the use of idealisation, but rather a question of the *kind* of idealisation that is involved, an idealisation that extracts out precisely the actor's *practical* involvement with the relevant activities and with the production of their orderliness. Whatever similarities one might find between diverse activities, however many common 'social processes', one might locate within them, from a *practical point of view* they are not the same things, they are crucially different. Whatever formal similarities one can find between, say, chemistry teaching and chicken farming, and however much one may analogise the treatment of students with the handling of battery hens, for those who do chicken-farming the remorseless fact is that these *are* chickens, *and not* students. The idealisation is done, then, with an eye to the production of formal similarities by the sociologist rather than with the emphasis upon the location of those features of the activity which are 'essential' to its production *as that activity*. We put 'essential' in brackets because we do not want to suggest that there is some "essence of chicken farming" but because we do want to retain the emphasis that chicken farming *is* chicken farming, *not* "social control" or "a hierarchical authority system".

Taken together then, the preceding points suggest that 'the world of everyday life' is the recipient of a 'raw deal' from sociological analysis, looked upon as disorderly and disorganised, stripped of the very concerns that give its activities their recognisable character, viewed from a standpoint of idealisation which (typically) emphasises the purpose of cognition, suspending the (seemingly) central everyday concern with pragmatic accomplishment, with doing things. *Of course*, the world of daily life is very, very badly organised for the purposes of anyone who would conduct a pure enquiry of the Cartesian sort, but that is not (presumably) what the members of society are about. It is, however, equally axiomatic for us that the world of everyday life is orderly *in its own terms*, an assumption attested to by the fact that it does not disintegrate into utter confusion, is a scene within which the members of society are able to find their way and achieve many of their ends without excessive difficulty. The world of everyday life is recognised (even by sociologists) to have those attributes which are the classic features of 'structure' or 'organisation' *viz.*, stability, predictability, persistence, objectivity etc. The conclusion is, then, not that the world of everyday life is disorderly but that the approaches of sociology are poorly designed for identifying its orderly arrangements. They *don't* do for that, and there are good reasons for supposing they *won't* do for that.

The fact that occasional practitioners, like Letkemann cited above, can address the topic of an activity's characteristic orderliness, its own organisation as that activity, does not mean that we can thereby simply adopt the approach taken by such rare researchers, and treat these concerns as additional to those already pursued in ethnographic work because the aim is to place investigation into the orderliness of everyday life at the centre of the enterprise, as its main (even exclusive)

business.

Of course, one of the arguments which develops about ethnomethodology at this point is over the relationship between 'the orderliness of daily life' and the orderliness of 'social structure' or 'culture', that overall pattern which sociologists postulate as the organising framework for the world of daily life. The argument, as you will know, gets cast in terms of 'subjective meanings' versus 'objective social structures' and in terms of the relationship between 'macrosociological organisation' and 'microstructures of social interaction'. The first relationship has, of course, to do with both knowledge and its object, and action and its constraining circumstances and thus with the question can actors know what they are doing, can their ideas effectively guide their conduct? The latter issue, of 'macro' and 'micro' sociology becomes a problem of inference, well stated by Geertz:

'The methodological problem which the microscopic nature of ethnography presents is both real and critical. But it is not to be resolved by regarding a remote locality as the world in a teacup or as the sociological equivalent of a cloud chamber. It is to be resolved - or, anyway, decently kept at bay - by realizing that social actions are comments on more than themselves: that where an interpretation comes from does not determine where it can be impelled to go. Small facts speak to large issues winks to epistemology, or sheep raids to revolution, because they are made to.

(p. 23)

We could not ask for a more concise exhibition of our contention that the problem of ethnographic work is conceived as a matter of microscopic/macroscopic relationship and, thus, of inference of 'an overall pattern' from its data nor of an earlier argument that the activities of daily life are to be looked at in terms of *extrinsic* interests, unrelated to those which generate their characterising orderliness.

At the beginning, we made some strictures against concerning

ethnomethodology as a variant of stock positions in sociology and we can only say that the attempt to place it on the polarities of 'subjectives/objectives' or 'macro/micro' is to try to force it into dichotomies it does not conceive and does not really fit.

To enlarge upon this point, let us return to it below *via* a round-about route, and let us talk, first, about 'the actor's point of view'. All the good guys in the subject are now agreed that it is essential to good sociological work that we take account of the actor's point of view and that we build that (somehow) into our studies. The crux of the matter is *always* in the 'somehow': what in real research terms does that mean? For ethnomethodology it means, first, that we now have a task to do, *describing the actor's point of view*. We do not, at the beginning know what such a thing is and it is a problem to say how we would describe *one*. What the import of incorporating the actor's point of view is, then, is something that remains to be seen, and 'the actor's point of view' remains, as yet, to be described, though we are not entirely without ideas about the way to get on with the work of describing it.

The idea of 'the actor's point of view' often seems to equate with describing what is visible, what the actor can 'see', how things 'look' to him, which might seem acceptable enough. However, for many, the question is, how does the actor see the orderliness that the sociologist has found, how does he perceive the 'overall pattern' within which the sociologist wishes to locate him? The relationship has been incarnated in the 'emic' and 'etic' relationship, between the organisation that is 'really there' and the organisation the native can detect, and its problematical character has been well evidenced in the attempt to relate 'objective' class structures to 'subjective' class images. People are, the argument seems to me, included in the one overall class or stratification system which has certain real characteristics and real consequences.

How do the people in the society see that system, what kind of an image do they have of the 'overall' class structure and of their place in it? Research often shows that people don't have an 'image of society' in any sense comparable with that the sociologist has devised, they 'see themselves' *vis-à-vis* the matters the sociologist seeks to address, in terms of 'local' comparisons with neighbours, friends, relatives, co-workers and so on - it is necessary to do quite an extensive amount of reconstruction to treat these 'local' comparisons as derivatives from some perception of an 'overall' pattern, which 'informs' the actor's outlook. Thus, the actor's viewpoint becomes a complement of the sociologist's, treated as a set of perceptions (or misperceptions) of the matters the sociologist has decided are there for him to perceive. However, the idea of the actor's viewpoint has been introduced, in the first place in many instances, as a corrective to the tendency of sociologists to 'impose' upon the members of society. In its worst instances, the tendency to 'impose' is simply that of deciding what circumstances people must respond to and what they must do in a pretty arbitrary fashion that rests upon nothing but the sociologist's conviction that these *are* the real circumstances of action and that these *are* the things people can do. The introduction of the actor's standpoint, in its mildest form allows that what the world is like and what people do has (something, somehow) to do with what they 'think' the world is like and what they 'think' they are doing. Just what consequences this admission has depends, of course, upon the 'somehow' again - how do people see the world, what do they think they are doing? And how are we to establish these things: if we have methodological problems of objectivity, adequate description and the like in sociology (if these are *genuine* problems) then they apply to the determination of the actor's standpoint, as to everything else - the introduction of that provides new problems, not solutions.

What do the members of society see? EM proposes that they see the world of everyday life - that is what the member's vantage point reveals. They see a world that is furnished with the routine fitments of daily life, objects, other persons, activities, organisations, states, histories, prospects, etc. They see, in the favoured example of writers, whatever can be seen from the window before which they sit - in my case, I see my office, its fittings, other people's offices across the block, I hear the voices of colleagues down the hall, etc. The project of describing 'the actor's point of view' then, is closely related to the work of describing 'the world of daily life' itself. Another feature of the way the actor's point of view is conventionally described is that it is in terms of its content, in terms of what is seen, e.g. the class structure is seen as a hierarchical arrangement. However, that kind of thing won't do for a depiction of the world of daily life since I don't have some 'overall pictures' of daily life. I am present in and see this particular here and now and in a moment I shall leave the room and see something else, leave the buildings and see the street outside. The conventional treatment seeks to treat the actor's viewpoint as a snapshot of the overall pattern, whereas the world of everyday life seems to require something more akin to a filmic treatment, though the succession of things seen, though following in sequence aren't going to 'add up' to some overall picture. The actor's point of view, then, is better thought of as a process of analysis rather than as some image or picture. Actors have a continuing capacity to make sense of their setting, to determine what they are seeing *now*, what is happening *now* as the flow of events and activities passes before their eyes, through their ears, etc. Deciding how this or that matter 'looks' to the member of society is, then, only a step toward what would seem like the more sensible way of examining the actor's point of view, i.e. identifying the procedures/methods used to assign/recognise

the sense of things *and to organise further activities.*

The actor's point of view is further, one which is ego-centric: this is *my* world and the matters which take place in it do not pass before my eyes as matters of theoretical possibility, but as matters of real interest and fateful consequence which I seek to bring about or avert. The actor's analytical work is *practical*, directed not toward the purpose of knowing, for knowing's sake, but toward the acquisition of knowledge for the sake of achieving ends, realising projects, where matters of knowledge are inextricable from questions about the relevance and effectiveness of it to the things I want to do. The actor's analytical work is not only practical, it is "in course", it is done as part of the activities that it organises, even as those activities are done, the work of acquiring the relevant knowledge organised in amongst the other doings constitutive of the activity.

The 'actor's point of view', then, as looked at from EM is the business of working out 'what is happening now?' and 'what to do next?' as the two central questions.

How does this bear upon the problem of macro/micro relationships from which the preceding remarks were premised as a digression? In this way, the world of daily life is *not*, viewed 'from the actor's point of view' a micro-world which presents us with the problem of inference about some molar, overall organisation: these local circumstances are looked upon *as part* of life-in-this-society and the members of society can see ways in which "this particular" shows what "this society" is like - residents peering out from their window and seeing a car parked in the street opposite, can 'see' that this is a stolen car and that their neighbourhood is going downhill, people reading the newspaper can see a story about hairstyles or fashion and find that the society is subject to moral decay, people hearing about a strike at the factory round the corner

can appraise the progress of the class struggle. My circumstances here and now are *continuous* with the world outside, separated from that by (to borrow a term from Schutz) an 'horizon' and these activities going on in my room are *part* of the university, this society, even the human condition, not evidence for them. I *do not* live in a world of face-to-face encounters from which I can "infer" organisations, institutions and societies, I live in a world which is populated by, amongst other things, organisations, institutions and societies and I can in my daily life draw all kinds of conclusions about what those things are like.

J.L. Austin argued that we can, if we see crumbs, a breadboard, a bread knife, butter, a bread bin, say that we can see 'the signs of bread' but, face to face with the loaf it doesn't make much sense to say we now see *all* the signs of bread, better to say we see the loaf. In the same way, it doesn't do to say we see evidence for the organisation of society when we are in the midst of it, living its life. To repeat, the society, viewed 'from within' is visible as *its* daily life, not as a series of microscopic, even microcosmic signs of its structure.

On this reasoning, the orderliness of the society, then, is *highly* visible to those who live within it - they can, without special effort, strenuous enquiry, prolonged delay, see what it is like - how does this place work? is the first question, not the last. The argument is stronger, the orderliness *has to be* readily observable, because the availability of that orderliness is requisite to its further production. EM's idea is that the orderliness of activities is produced 'from within' those activities, that it arises from the way those activities organise themselves and that, therefore, the orderliness of those activities has therefore to be detectable 'from within' itself. For us to connect our activities with others in the orderly, the *recognisably characterisingly* orderly way that makes up those activities, we need to be able to know

'what we are supposed to be doing', 'where we are up to now' where answers to those questions provide materials for the solution to the crucial next, 'and what should I do now?' That this occasion is a history class tells me what *kinds* of people to look for, e.g. teachers and pupils, it tells me ways to locate the relevant parties, e.g. that they often segregate themselves physically and the teacher sits (or stands) "in front", that there may be age differences, teachers are adults, pupils often children, and it tells me, too, how to locate the organisation of the talk going on - see the teacher talking to them, the pupils, collect up their actions in that kind of pair organisation and see the organisation of the talking not as being structured in terms of 'Smith, then Brown, then Smith, then Jones, then Smith, then Collier' but to see it as teacher, then pupil, then teacher again, and so on. That its a *history* class tells me to hear the talk as having to do with history, to see, e.g. that they are talking about 'King Henry and his wives' has to do with the lesson's topic and to find, e.g. that 'they have reached the Tudor period' thus seeing from the talk the history of their relationship, how they have come to this particular phase, what they will have talked about before, what they will go on to talk about etc.

The point at which, we think, EM departs most comprehensively from EN can be summarised in a capsule phrase, 'practical sociological reasoning, lay and professional'. This particular phrase is the one on which most sociologists choke since it attacks what is their basic article of faith, that there is a qualitative difference between Professional Sociologists on the one hand and Members of the Society on the other, that one set of persons do 'sociological' reasoning and the others do not. If, however, by 'sociological reasoning' is meant the attempt to determine how a social scene is organised, what features of

its activities are regularities, what kinds of persons frequent the scene, what actions and dispositions are typical of those different kinds, what rights and obligations they have *vis-à-vis* each other and the management of the setting, what they are now doing, why they are now doing it and why they are doing it that way, then the lay members of society have just as chronic and pervasive an interest in those matters as do professional sociologists.

EM, then, seeks to see both *the researcher and the native* on the same footing, to treat them *both* as inquirers, *both* as engaged in the work of finding out about social structures.

EM is often accused of supposing that the members of society *know* how the society works, thus precluding the possibility that these members can be mistaken or ignorant. It is difficult to imagine a more gross distortion than this, for EM seeks to emphasise *above all else* the possibility that members will find that, after all, they were not doing what they thought/imagined they were doing - this is called in the jargon the 'prospective-retrospective' sense of determination and it is recognised as a feature/hazard of the world of daily life that it will turn out in the future that appearances were deceiving, realities other than was supposed, the outcomes of action different than those projected, the organisation of things quite otherwise than they were understood to be. *But* EM does not suppose that sociology is *necessary* or *exclusively responsible* for pointing this out. What EM does suppose, is that members of society are capable of *finding out* about the society 'from within' and that their lives are much occupied with the work of inquiry - they *never stop* finding out, there is not some point at which they know all about the society and have no further questions to put to it, they are *endlessly* busy investigating its features. The questions 'how does this place work?' 'who runs this town?' '*cui bono?*' 'whose responsible for what?'

'what's the division of labour?' 'what are the rules?' 'what is the table of organisation?' 'where does the real power lie?' 'do realities fit formal chains of command?' 'what kinds of cliques are there, who belongs?' 'how to tell which sides people are on?' 'how to estimate the strength of a faction?' 'how to predict which way things will come out?' 'what kind of changes are coming up?' are not on the lips of sociological researchers alone but they are regularly *asked and answered* by the members of organisations and settings themselves and the business of asking and answering *is part of the* setting within which it is done. These questions, are not, however, within such settings, isolated from an indeterminately - perhaps infinitely long - list of other questions that get asked - what is he doing now?, what time is it?, when did I say I'd be home?, how long will this meeting last?, can I afford to go now?, should I call at the library on the way home?, will anyone notice that I leave, if they do, will they care? etc., etc., *ad infinitum*.

The researcher and the native, from the point of EM are on *exactly* the same footing, both are seeking from within the society to find out how 'it works', what 'its organisation' is like: the fact that the native answers a question does not mean that it cannot be asked again - I *thought* I knew how this place works, but after thirty years here I see that I don't. Indeed, some people try to make a philosophy of life out of a principle of increasing ignorance - the longer I live, the less I know. So, the native cannot be *assumed* an expert in a setting but must be looked upon as a co-inquirer, dealing with the same problem of finding out about social structures.

Those who want to insist upon the difference of native and researcher, then, can attempt to maintain it via a stylistic difference, by to the *ways* in which the respective parties make their inquiries. To take this line, however, is to arrive in the familiar position of

criticising the phenomenon, of arguing with things which are incommensurable. The member's inquiries are *practical* ones, their adequacy being decided *for all practical purposes*, not in terms of incorrigibility or even accordance with concerns of proper scientific inquiry. These inquiries have to meet the tests like - telling me what I want to know, doing so whilst letting me carry on with what I want to do, letting me find out without showing others I wanted to know, providing knowledge that will have been worth the effort of finding out (amongst others): they are, that is, adequate if they will do. A simple example: I pause outside a colleague's door, hear voices within, conclude 'He's holding a class'. This inquiry does not of course preclude the possibility that something else entirely might be going on - a radio is playing, a lunch break being taken - but it is reasonable to assume that, given the time and the place, the activities going on are the sort that regularly go on there, and to decide to come back later rather than to interrupt now. The topic of *inquiry*, for EM, then are the methods by which members of the society (lay or professional) *find out* about their social setting.

There are, then, in our estimate some significant differences in the things that EM and EN are doing and that they are all about, but the important differences are those that should be made to practice. How does what EM do differ from what EN does? In brief, the following considerations seem salient:

- (a) EN seeks to treat the problems of the researcher as being of a different order to those forced by the members of the society, e.g. satisfying canons of inquiry, meeting conditions of objectivity, etc., whereas EM seeks to treat the researcher's problem as being of the same order as those of 'the natives', *viz* finding a way about a social structure.

- (b) EN seeks to develop a specialised methodology whereas EM's researchers are required to use and rely upon their 'common sense' resources in seeing social structures - their question is not, how *can* we claim facticity for our observations but how *do* we?
- (c) EN tends to emphasise the need for large amounts of information, for prolonged contact, full documentation, warranted claims to intimacy with the natives etc., but EM tends to minimise the materials needed, to examine small fragments of data, to spend short times assembling them, to make it a matter of query whether one needs to know more than one now knows to see the orderliness of actions.
- (d) EM tends in consequence to emphasise just *how much* can be found from a small amount of data rather than to emphasise the limited informativeness of any fragment.
- (e) EN treats its work in, to follow, Stoddart, 'inquestive' fashion, seeking to decide what the research came to, EM seeks to look at research 'in course' to see how it comes to whatever conclusion it reaches, how the researcher's activities comprise the course of inquiry and how, over its course, it reveals the character of the social structure.