

Taking Professor Gluckman Seriously: The Case of Participant Observation

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Part of the heritage of Professor Gluckman's teachings and writings may be seen in his insistence on, and advocacy of, detailed observation of social life as on-going processes. Whilst not an anti-theorist, he gave his students the strongest message that the elaborate collection of valid data represented the greatest necessary constraints upon the vagaries of loosely articulated theory.

The two studies here discussed were both participant observation field studies which, in the above tradition, attempted to take this recommendation of Gluckman's seriously.

The first study (Lee 1970) commenced with the object of formulating the culture of a 'left wing' in relation to the local Labour Party in which it existed. The second study (Anderson 1977) initially intended to describe aspects of the social world of professional actors. Because of our concern with the validity of observations drawn from our materials, and with the basis of the collection of those materials, *we* encountered methodological problems that we were unable to resolve whilst retaining the original object of our endeavours. Whilst we could have proceeded to fulfil our object, we would not have been able to do so in a way that satisfied our desire for rigour; we would not in our view have been able

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to claim warrant for our observations. Consequently, in our endeavour to take the spirit of Gluckman seriously, we have proceeded to analyse the nature of our 'data' in some detail, in order to make explicit our methods and problems. Our intention in so doing is to take our 'troubles' and turn them into a resource which, when focused upon our materials, enables us to extend the scope of our analysis.

Our intention is to suggest that the problems we encountered are irresolvable in a fashion that would meet the requirements of the claim for any distinctive scientific warrant. We further suggest that these problems are a product of preconceived ideas held by researchers about the nature of sociological materials. These ideas are *a priori* theoretical conceptions of society, and preclude a critical analysis of the nature of the phenomena under investigation. We are concerned therefore to analyse the nature of our materials in order to see how it is that their natural organisation frustrated the nature of our initial objectives, and defeated our attempts to incorporate them into the sociological models of our enquiry. It would thus be proper to suggest that our investigation teaches us about the nature of our data. It is not directed as an attempt to resolve our troubles to enable us to pursue our original purposes.

Our argument is that in our research we encountered what we experienced as persistent troubles. We were unable to escape or hide from these troubles; whatever contrivances of method we could or did display, the troubles remained. Our intention is to make a resource for these problems, to examine them for what they are rather than to hide them. In this way, we believe that instead of making the world fit our conceptions (contained in the methods), we should examine the nature of our data (the experienced world) to see how it does not fit, and, in so doing, how it is organised in such a way as to frustrate our previous methodology. Both studies were conducted by participant observation.

Study (i) involved the researcher attending all levels of local Labour Party activity, and directly observing and questioning its members as to the content of 'left-wing' culture. This involved relating the activities and beliefs of the 'left-wingers' to the activities and beliefs of other party members.

Study (ii), influenced by the researcher's eight years as a professional actor, involved depth interviewing and direct observation of the ways in which actors evaluated such matters as proper membership to the profession, competency, and the nature (initially the changing nature) of the craft.

Before drawing attention to the epistemological problems that we faced through the adoption of participant observation as our research method, it is worth noting the claims made by those who support this method as a basis for securing the scientific authenticity of their studies. Particularly notable are those arguments which suggest that it provides a method particularly appropriate for the study of real social-world phenomena. These latter arguments, of which

Professor Gluckman was a notable exponent, suggest that sociologists adopting participant observation do so in relation to their views, sometimes made explicit and sometimes not, of the nature of social phenomena, social order, and social organisation. By studying participant observation and sociologists' reasons for so choosing it as a research method, we are studying the conceptions and theories of society embodied in those choices.

Amongst the claims suggesting the superiority of participant observation for the study of social reality are the following:

1. That participant observation provides for the collection of data from genuine interactive settings, rather than artificial situations or members' constructs of artificial situations obtained via questions, interviews, etc.
2. That participant observation data is rich in detail, providing the researcher with infinite resources of descriptive detail.
3. That it embraces an appropriate model of man (naturalistic), who relates and constructs his actions to this culture-ensconced 'definition', or understanding of 'social reality'. This incorporates the advantages claimed in terms of an understanding of participants' use of natural language.
4. It is claimed (and seems obvious) that observation of persons over time enables the student to focus upon issues of process and social change, rather than his obtaining a statically reified, structural account.

It is clear to anyone who reads method books describing participant observation, or who engages in it, that a clear presentation of its detailed methodology is unavailable, and indeed would be nonsensical. Generally speaking, it consists of a pretty varied set of prescriptions and recipes for gaining entry, collecting data, and building theory. What one might expect such treatises to emphasise is the need to 'play it by ear', to observe one's data, and to develop pertinent problems according to the context of the field and the practical and epistemological problems that it produces. The richness of detail of experience, and the contingent nature of those experiences, mean that one must learn to see and understand the world according to its own, to be discovered, relevant dimensions. In this, the participant observer is expected to be truly participant in that his guide to procedures, and rules for dealing with such procedures, are emergent properties in very much the same way that they are for the novice who learns how to behave appropriately in law courts, political parties, at weddings, or whatever. It is of course for this reason that detailed recipes or methodologies for how to see the world are not available. The descriptions of participant observation seem to indicate trust in tried and commonsense ways of being in the world. Our troubles resulted from

our attempt to enlist such commonsense practices into the service of science; our new task therefore is to examine these commonsense practices.

We suggest that the core of our problem is that the participant observer is in fact irrevocably involved in enlisting the support, persuasions and arguments of his subjects in such a way as to make dubious any claim he might produce as to the independently or 'objectively' achieved status of the material. For this reason we are not presenting an argument that he should gather his materials more carefully, make certain validity or reliability checks, etc. Rather, what we have found from the analysis of our data is that the world the sociologist encounters is so organised as *necessarily to involve the researcher* in enlisting the support and persuasions of his subjects in the production of *any* version, or *any* description of *any* part, of the social organisation that he encounters. This unanalysed involvement of the participants in furnishing the results of the researcher's enquiries, because unexamined and perhaps unexaminable, renders the would-be scientist's analytic description into the status of polemic or scientifically unwarranted argument.

Our experience of fieldwork tells us that the participant observer is remorselessly engaged in a variety of lay and commonsense methods for sense-assembling the experiences he encounters into data for his research. To achieve such a transformation, that is, *to make sense of what he observes*, he must of necessity engage himself in the intersubjective world of his subjects. He must also do this in his production of any description or account of what he sees. In so doing he must learn and use the commonsense methods by which his subjects characterise their world, its objects, events and persons. This, of course, also means that his method for the production of description and comparisons from contingent circumstances includes, and is thoroughly characterised by, the unexplicated, unanalysed, lay commonsense knowledge and methods of sense-assembly that he has incorporated from his field.

By presenting his materials as a world of externally available facts the participant observer has made use of a variety of such undisclosed methods but has specifically refused to acknowledge such use. Instead, he wishes to disguise the contingent achievement of every one of his descriptions, and present those descriptions as if they were standardly available and objective. He does this in order to provide for the scientificity of his findings.

How members in social settings (including our participant observer) invoke their commonsense methods in order to describe those settings is crucial in determining the nature of the objects and items that make up the settings. Consequently, how an observer sense-assembles the world in which he participates is crucial to an understanding of the nature of that world. What this means is that for the observer/researcher the rules and methods of his sense-assembly, or

understanding of the objects he encounters, are criterial in determining what the 'data' really are, or can be taken to be. When we say, therefore, that observers have taken the nature of their data for granted rather than as open to analysis, we are saying that what are fashioned as data (objects of description, the results of categorisation procedures, etc.) have an indeterminate status.

MEMBERSHIPING

To illustrate our discussion of members' commonsense methods of producing and describing the world, we have chosen to discuss membershiping. Membershiping represents a way by which persons in social settings categorise objects of knowledge, incidents, events, and other members. It is therefore revealed in the accounts and descriptions that persons in the world furnish, and is thus constitutive of the objects of their orientation. It represents the results of their sense-assembly methods as they utilise conventional rules and procedures in practical purpose settings, in order to know their way around, and instruct others as to how to see the world correctly. It is the sense in which members of a culture deal with contingent events, and render them into categories such that 'this' may be found to be 'another case of', or 'similar to', or 'the same situation as', 'that'. Persons may be found within the same practical auspices to be co-members of 'this' or 'that' class, co-believers, antagonists, etc.

One obvious feature of what we have already suggested is that the sociologist, when encountering his 'data', is encountering the practice of, the results of, these commonsense membershiping activities. The realisation of this presented us with our first and abiding trouble contained in the question: how do our sociological descriptions and their membershiping procedures relate to the membershiping procedures involved among those who make up the field of study? This problem first became of practical concern when a detailed study of the ways in which both actors' and politicians' membership revealed that such categorisations and accounts were remorselessly done pragmatically and for practical purposes. The methods by which they counted membership appeared to shift and change according to contingent circumstances and occasioned relevances. Thus in both studies we were faced with the problem that membership, as achieved by participants, appeared equivocal when viewed in terms of the scientific and rigorous standards imposed by the practical requirements of sociologists insisting on the objective nature of the task involved in defining the field. Our problem was that in the face of *seeming* inconsistency, disagreement, and the occasioned relevance of membershiping categories, how could we achieve membershiping in a strictly warrantable manner? To our mind there would be arbitrariness in the selection of any set of firmly constituted criteria when such criteria would clearly involve us in

the disagreement, or disjunction with at least some members of the field for at least some of the time of our study. The choice that both of us faced when encountering the problem was whether to accept this disjuncture and the arbitrary nature of our own membership practices, or to make membership practices (theirs and ours) the topic of our enquiry in order to see what such practices achieved, and to see what place such practices had in the very achievement of the order that we had chosen to study. Our choice was obviously the latter. We made this choice in order to base our explanations squarely upon the ways of thinking adopted continuously and diversely by the subjects under study.

In one sense the researcher concerned to analyse attitudes and activities of actors found no practical difficulty in discovering whom to interview and how to categorise the characters so interviewed. Actors are actors, and so an appropriate procedure was to approach a theatre and a film location, and form a sample from those seen to be acting. That this pragmatic procedure is methodologically unsatisfactory was revealed to us by an examination of the variety of different and sometimes seemingly inconsistent ways that members of the field themselves counted their membership. The basic problem for researchers in these situations is: on whose authority do they classify persons as actors, or as left- or right-wing? For both of us the answer to such questions was an apparent necessity, given our methodological requirement of attributing characteristics (demographic, attitudinal, etc.) and thereby assembling what might be called a corpus of culture. The point was brought home to us by the realisation that, in both studies, membership persons into such cultures was subjectively problematic. We shall illustrate this from the fields of our research.

The category 'actor', and relatedly 'acting', can be seen as public categories. The assignment of members to these categories, therefore, is a matter of interest. 'Doing acting', even when 'doing professional acting', is not as unproblematic as it may seem when one considers just some of the diverse situations that professional actors may find themselves in under the guise of working in the business. They may be working on stage in a repertory theatre, in the 'West End', or on tour; they may be working in a television or radio studio, or on a television or film location. Actors could find themselves doing a 'voice over' for a cartoon, or working in a variety summer season, a pantomime, or an old-time music hall; they could be entertaining at a children's party. Actors could be involved in the major 'growth sector' of the acting business, 'theatre in education', where it would be difficult in some instances to distinguish what they were doing from many of the sorts of activities that teachers get up to. More significantly, however, and more likely, they may be doing none of these things: they may be signing on the dole, or washing dishes (a 'traditional' activity for out-of-work actors, presumably giving some scope for their apparent love of suffering for their art). They could be

involved in many other occupations. In what sense would these people be actors? For members who consider themselves to be actors, and who are often involved in such activities, they would certainly be actors. Indeed, there is a very strong sense in which members can categorise people who have not done any of these seemingly non-acting things, who 'haven't had it rough', as 'not really actors'.

Even if a participant observer chose a set of activities and decided that anyone who did them was an actor, his problem would be far from solved. What if someone had done one of these activities ten years ago and had been employed as a teacher ever since; would he be an actor? Indeed, am I an actor? – a question one of our authors asked himself very seriously, particularly during the course of his research, and not only for concerns related to it. He had thought, until he returned to the field of actors, that he was 'a professional actor who was at the moment doing some postgraduate sociology'. For those working in the theatre, however, he found that he was just someone who was writing about the social interaction that takes place among actors: someone who needed to be shown 'how things are around here', even though they knew that he had been an actor. When he found himself talking about having been an actor and what it entailed, he experienced exclusion work done by those with whom he was conversing – even by those with whom he had worked not that many years previously. He suddenly discovered that, to them, he was now no longer an actor. Should this, however, appear strange? Surely we do not believe that the description 'actor' is one that, once given, lasts for all time, forms the orientations that one has to the world, and is the way that people in the world orient to one on virtually all occasions.

If we refer to the categories of people who may be working in front of the cameras in a television studio, such as 'actor', 'extra', 'walk-on', etc., we can see that not all those people would be classified as actors in many circumstances; but for some people in some circumstances they would be. In theatres one gets people such as assistant stage managers who may take small parts. These people sometimes turn out to be trainee actors, but they may not be.

We shall turn now to a consideration of the British Actor's Equity Association and its criteria for membership. This is meant to serve two purposes: firstly, it will demonstrate the practical difficulty of categorising members as actors or not, for our sociological purposes; and secondly, it will be used as an example, in the following discussion, of the use of such a device sometimes employed by sociologists. The device is that of taking an 'official' body as standing for the categorising work.

The British Actor's Equity Association operates a fairly rigid closed-shop system. Entry into the profession is regulated by a quota system for repertory theatres and some tours, which allows managements to employ a small number of non-members, in relation to the numbers of existing full members they employ.

These new recruits must immediately join Equity as associate members. This is the way that non-members manage to become members. The researcher had assumed, therefore, that if he was going to choose a definition of who the actors were, this might be as good a method as any. It turned out to be a most unsatisfactory method.

Equity now incorporates the Variety Artistes' Federation. Therefore club and cabaret artistes, whom the researcher would not normally have considered to be 'actors', are included in the membership figures. Perhaps the most interesting feature of this, for his notion of what an actor is, is the fact that these people sometimes do acting. Even this division and the information included about methods of entry into the profession is not that clear-cut. As many would-be actors find themselves unable to 'get their card' through the quota system, and as, once a member of Equity, one is free to work in almost any branch of the business,¹ club work often turns out, for these people, to be a relatively easy way of gaining membership. In order to join Equity as a variety artiste, an applicant has to show that he is an already working variety artiste (full-time or part-time). Equity, far from having a closed-shop policy on variety, has the opposite situation: it has difficulty in recruiting members in the field, and thereby maintaining any sort of influence in that direction. The criterion for demonstrating that one is a variety artiste to Equity is therefore somewhat lax: the provision of 'at least two recent contracts of engagement at a recognised venue'. People who employ variety artistes, of course, do not require Equity membership as a criterion of employment. Indeed, there is some evidence to suggest that some variety employers may try to avoid employing Equity members, as they have to be paid, even if they are no good. The qualification required for such employment may therefore be the ability to work cheaply. The difficulty in obtaining contracts may only be related to the fact that some potential employers try to avoid issuing such encumbrances to free enterprise whenever possible. A well tried way, therefore, for an aspiring actor to gain an Equity card is to get himself a couple of nights singing or telling jokes at the local working men's club (or some pubs are acceptable). He will then be eligible for Equity membership, from which time he can seek employment as an actor. The figures issued by Equity relating to methods of entry into the union would therefore be of no assistance. Another method of entry, that for obvious reasons would present difficulties to a researcher, would be straightforward corruption.

Extras form another large group within Equity the members of which would not normally consider themselves, or be considered by others, as actors. Extras within Equity are in a somewhat ambiguous position: they are regarded as members, full members after having served the initial probationary period, but working as an extra is not a way that one can gain entry into Equity. Most of the

members who work exclusively as extras were enrolled almost *en masse* about fifteen years ago when the union managed to establish a pre-entry closed shop for all performers in television. The people who were already on the books of the various television companies were accepted into membership. Most of them were people who were otherwise unemployed, such as housewives, or those whose employment allowed them occasional free time during the day, such as publicans. The income from extra work is not usually seen as a means of primary support. Most extras would consider that they were busy if they were called more than two days a fortnight, and usually much less than this. Latterly, some Equity members such as club artistes, who would otherwise be unemployed during the day, and wrestlers who have gained their cards by 'fighting' on television, have joined the ranks. A notable exception among Equity members doing extra work when otherwise unemployed are those who consider themselves to be actors. The researcher noted from his own experience as an actor that dish-washing would carry a higher status for an actor than extra work.

There are also those who consider themselves to be, and who would be considered by others, to be actors who are not in Equity. Fringe theatre, so called, has seen a great increase in recent years. It is an issue within the British Actors' Equity Association at the present time that this is not an acceptable way of gaining membership and therefore entry into the profession – much to the annoyance of those involved. This, however, is not as clear-cut as it appears, as it would seem that whilst those involved would wish to qualify for Equity membership as a result of their involvement (some of them are, of course, Equity members as a result of other involvement in the business), they would not be willing to accept the sorts of restrictions that Equity membership would normally involve, such as a ban on working with non-members, as a large part of the ethic of such theatre is community involvement and open access.

Whether one is, or is not, an actor can be a highly political business: political in the sense of what M. Pollner (1975) has called 'the politics of experience'. By this he means the claims that people make in the world – such as who is, or is not, this or that category – can be seen as part of the business of positing versions of how the world is. This is, for members, an activity of overwhelming significance.

Let us now discuss the sort of work that a sociologist can achieve by using an 'official' organisation such as the British Actors' Equity Association. Sociologists frequently employ this device, and for members on relevant occasions such a method may also be employed. We can see how an 'official' organisation can present an authoritative persuasion for such as sociologists faced with ambiguous criteria. Warrant for such a device might be inferred from such theoretical writings as Blumer's (1962), in which, for the author, 'the acting other' whose role participant observers should take does not have to be an individual: it can be an

organisation. This reifies organisations to the level of consciousness. Thus members' organisations which for members can be centres of power can for a sociologist become authoritative over the limits of his study; such a sociologist can be seen to accept the versions of the world proffered by such power hierarchies. In the legal sense, organisations 'lay down the law'. The actor or the sociologist who wants to find out who is 'in' and who is 'out' by using such a criterion must assume that people obey the law; he then counts those who have obeyed the law as 'in'.

The interesting thing about this persuasion is that we might wish to see the purpose of such organisations as to be authoritative over some dimension of the world. The warrant for so being is, however, a practical and pragmatic warrant for people in the world, deciding such issues as what justice in the world is, as against the scientific warrant that a sociologist might wish to claim. In accepting the membership policy of an 'official' organisation the sociologist abandons his investigative role. Instead, the question of how his field works, or in this case what this field consists of, is decided by the organisation's membership practices. He allows this precisely because in his own everyday world he knows of organisations like trade unions and so forth, and he can see and understand what their purposes are as a part of his commonsense knowledge. He uses this device because he believes that, as organisations have this purpose for members in the world, he can 'tap' members' knowledge by 'tapping' organisations. This is, however, one thing he cannot do if he wishes to claim scientific warrant.

E. Bittner (1965) treats Weber's use of organisation, as contained in his concept of bureaucracy, in a similar way. Weber, Bittner argues, has accepted a members' commonsense version of organisations without investigating what might be entailed in such a version. As Bittner tells us:

When one lifts the mantle of protection from the unstated presupposition surrounding the terms of Weber's theory of bureaucracy one is confronted with facts of a particular sort. These facts are not sociological data, or even theoretically defensible hypotheses. Instead, one is confronted with a rich and ambiguous body of background information that normally competent members of society take for granted as commonly known. In its normal functioning this information furnishes the tacit foundation for all that is explicitly known, and provides the matrix for all deliberate considerations without being itself deliberately considered. While its content can be raised to the level of analysis, this typically does not occur. ... If, however, the theorist must be persuaded about the meaning of the terms in some prior and unexplicated way, there then exists collusion between him and those about whom he theorises. We call this unexplicated understanding collusive because it is a hidden resource, the use of which cannot be controlled adequately.

(Bittner 1965)

It is this that the conventional sociologist does when he confuses a warrant for power with a warrant for science.

We can, however, discover the operation of another members' device contained in the sentence that begins this section: 'The British Actors' Equity Association operates a fairly rigid closed-shop system, entry into the profession being regulated ...'. The device is the 'entry' device used by persons in the world as a membership device. If members want to look at who is 'in' and who is 'out', one of the ways they can do it is to look at what the entry methods are, and see who corresponds with those who have passed through those entry methods. This is a special case of a members' device with a much wider application. Many things in the world can be seen to exist because members can find a beginning for them. Atkinson et al. (1978), in discussing the recommencement of a meeting, point out that one of the methods by which members can find a meeting 'now in progress' is to find a beginning. Turner (1972) has shown that 'beginning' is something that participants to a therapy session can orient to as a way of finding 'we are now doing therapy', in a search for 'what's involved' in doing therapy.

The phenomenon works thus: as members we can see a category of people; in order to be in that category one must at some time have joined it. Therefore a way of finding who is to be considered a member is to fix the method of entry, and see who went through it. Some persons are said to belong to a category. Any person's evaluation of who belongs to that category, and who does not, must relate to the rules for being 'in' and 'out' of that category. Thus, if we can find out the 'rules' for such inclusion/exclusion, and see if our views of the world correspond to those 'rules', we have a geographer's method for mapping out a category for the practical purposes one might wish to use it for. These methods are quite satisfactory to do the world, but for the sociologist wishing to claim scientific warrant they must remain problematic.

The 'rules' for deciding such an entry can lead the sociologist to 'buy into' the moral issues involved for members. In other words, someone who had gained entry by 'corrupt' means might or might not be excluded from membership. For members, he might or might not be included as situational exigencies revealed themselves. However, the 'rules' for entry are revisable for members, and members' organisations. We referred earlier to the fact that about fifteen years ago a large number of extras were recruited into the organisation. By Equity's own criteria these people can be found to be professionals or not professionals as situations arise.

Similarly, in the field of politics, the researcher was concerned to look at the nature and composition of the party's 'left wing'. He very quickly realised that

who the 'left wing' were was a highly political issue in itself. When a member was referred to by the category 'left wing' there was an occasioned relevance to this description. Those doing the description might have been contrasting the described in relation to others in the party on a particular issue. They might, however, have been insulting him, praising him, or counting him as a supporter, or whatever: the important point is that, whatever they were doing, they were *doing something*. Categories do work within the context of their occasion. The nature and composition of the 'left wing' was 'continually' in dispute and 'continuously' being solved. It continually required demonstration by a 'commonsense method' proof which appeared to have a contingent and situationally based logic. One member of the party, when asked who the left-wingers were, replied, 'It's all those who stood against me on the fascist issue'. Membership is a thoroughly *ad hoc*-ed and occasioned phenomenon.

We are not saying that it is necessarily the case that membership is continuously in dispute for members of a cultural setting: only that it can be. However, what we are saying is that, whether the fieldworker encounters obvious dispute or not, he is still reliant upon members' commonsense membership practices in order to formulate his research categories. This reliance poses the serious problem of his incorporation of unanalysed features of the research object into the methodological, or analytical, machinery of formulating that research object. In other words, the potentially subjectively problematic membership that occurs in the field makes apparent the inability of the researcher to formulate his categories in a manner that allows him distinct scientific warrant.

The researcher's version, constructed as it is from an unanalysed borrowing of descriptive apparatus from members of the field, potentially stands in opposition to some members' versions of 'how it is', and yet can offer no good reason for its claimed superiority. Hence, if in order to solve the practical research-directed question of who is an actor one trades upon one's commonsense versions, for example 'he who holds an Equity card', how is one to deal with those who claim to be actors but hold no such card? Indeed, the fieldworker investigating the world of actors discovered that in some situations and on some occasions, this matter did come into dispute. However, in most engagements that he encountered, the issue as to who and what an actor was, was unproblematic. This is not to say that it was solved for all time, only that it was not an object of cognitive relevance to the doings of those he sought to study. What of course he did find was that this issue became a critical and often criterial matter in relation to the claims, and hence the potential disputes as to the legitimacy of claims, that members of the field sometimes made.

In the field of politics, it was also found that announcements of criteria of left-wing membership were part of a never-ending process of 'ruling in' and 'ruling

out' proper persons in whom one could trust, or rely upon, and with whom one could involve oneself, whilst still sustaining one's own claims.

Let us assume for the purposes of our argument that we had satisfactorily solved the issue of category membership and decided who was not an actor, or who was and who was not a member of the left wing. As we have said, such solutions are ongoingly achieved, though they may be subject to revision by members of the field for their own practical purposes. This assumption would not solve the methodological problem of relating activities and attitudes of such persons to the category 'actor' or the category 'left wing'. That is, we would not be able to say with warrant that such activities or attitudes represented the behaviour or persons *as* 'actors' or *as* 'left-wingers'.

During our practical involvement with the persons we chose to study, we noticed (and it should hardly come as a surprise) that persons wear many hats. Over time, our 'actors' spoke to us as 'fathers', 'mothers', 'older citizens', 'elder statesmen', and spoke to us as 'any person' – ordinary members of the public. Politicians spoke as 'trade unionists', 'lay lawyers', and as 'practical reasonable men'. What this meant was that, over time, we were able to assemble a whole series of cultural attributes to those persons: attitudes, opinions, ideologies, etc. However, it was by no means clear to us, or to them in the course of their engagements, that they were here speaking *as* actors or *as* left-wing politicians. Indeed, a careful inspection of their talk sometimes made it clear to us that they were expressing themselves in relation to other relevant category membership which they either 'had', or were by virtue of their talk thereby claiming.

During the course of an interview with an actress of many years' standing the researcher asked a question that he meant to be about the relationship between the content of courses at drama schools and the practical business of doing acting. The respondent began her reply, 'I always tell young people entering the business that they should have a second skill to enable them to make a living when they are "resting" ...' The interviewer had put the question to a 'practical actor', but the content of the reply made it clear that the answer came from an 'old sage'.

When asked about the composition of the 'left wing', a respondent in the political field prefaced his reply by: 'If you want a scientific answer ...', at once formulating the category of reply, demonstrating the fine recipient design of the answer, formulating the relationship between questioner and answerer, and distancing the respondent from his answer. This reply, by no means an uncommon type of answer in general terms, explicitly recognises the contingent nature of members' commonsense membership practices.

In both the above examples the members concerned made relevant their occasioned production rules for the membership which occasioned their talk. The capacity to do this is important for members in order to lay stress on, and to

make claims to, experience and expertise. It enables members to make distinctions which might not otherwise be obvious. However, needless to say, members do not always make those rules available to discourse but may leave it to the good sense of a listener, or simply consider it of no matter: after all, they are not always talking as 'social scientists', nor are they always 'the model social science respondent'.

In order for a participant observer to make persons stand as representative of categories, he would require what Harvey Sacks (1972) has called a PN-adequate device: that is, a set of categories such that these categories could be applied to any given population, resulting in the unproblematic ascription of all population members to one of (and not more than one of) the set of categories. However, the categories which sociologists are required to use in the assembly of the data are obviously not PN-adequate. It is obviously the case that a man may be seen as a 'father', 'worker', 'immigrant', etc., as situational relevances unfold themselves. Hence a researcher has no warrant for attributing an 'actor's' opinions or characteristics to incumbency of the category 'actor'. Indeed, what we have already suggested is that characteristics taken to be representing the culture of 'old man', 'professional' or 'helpful respondent', etc. Of course, the problem is compounded when, in order to describe a culture, the researcher occasions a corpus of further categories which he bases upon his use of his original categorisations. The researcher might create his research object, for example the culture of the 'left wing', by using the inference that members of the category 'left wing' also have membership of the category 'city party member'. The researcher is thereby doing membership work, in this case assigning 'left wing' membership, on the membership work he has already done, in this case assigning 'city party' membership.

The investigation of our troubles has already brought to our notice a simple but nevertheless vital feature of the data that we observed: *for members, categories are revisable as different relevancies unfold themselves*. Thus it is critical for the maintenance and sustenance of an ordered world by members for them to be able to see that a person designated as a 'left-winger' on some occasions, may on other occasions be seen as acting in the capacity of, for example, 'elder statesman'. Furthermore, it was a vital feature of the political research to notice that the activity of 'ruling in' and 'ruling out' as different circumstances, and therefore conditions of relevance, unfolded, made the criteria on which 'left wing' membership itself was based on revisable issue. Hence it was possible for one member of the field to provide for the researcher a count of 'left wing' membership, and subsequently after the passage of one day, in the circumstances of a split vote, to declare that 'there isn't, and never really was, a left wing in the city'. To emphasise this is to emphasise that members' descriptions and ascriptions as to the nature of

the object are themselves doing social-interactive work as part of the contingent circumstances of their production. A strange feature of participant observation is that it is liable to find the respondent inconsistent in his use of his own cultural object. Analysis might more profitably turn to an investigation of the variety of different kinds of work that the respondent's uses of the category can accomplish.

Much attention has been given by methodologists of participant observation² to the effects, and to countering the effects, of the fieldworker's participation in the field of his enquiries. Strangely, however, little attention has been directed to an analysis of that participation itself. Presumably this is because sociological work has been directed towards ways of repairing or discounting the effects of such participation. As the data is constituted by and from the participant's experiences and interaction in the scene of his studies, this omission is somewhat surprising. A fine-grained analysis of the researcher's participation in his studies is necessarily an analysis of the sense-assembly procedures by which he experiences his field. The analysis of the methods by which he encounters and experiences that field is of necessity an analysis of the way in which the field is organised for members (including the sociologist).

To illustrate this point we might focus upon the issue of recipient design (Sacks, *passim*). It has been noticed that co-conversationalists in the course of talk (and, therefore, of course, social interaction) design their utterances and activities with fellow interactants in mind. Indeed, the very sense which is given to utterances and activities is necessarily analytically created by respondents' capacity to answer such questions as *who* is saying it, *how* he is saying it, *where* he is saying it and *why* he is saying it. Of course, this fact is known by speakers in that they use respondents' capacity to produce such analyses in the course of their talk activities. This is of the utmost significance to social interaction in that it allows members to incorporate features of the setting, and of the biography of its members, into the production of the on-going setting itself. What would the 'Monty Python' world be like where this was not the case, and where persons conversed without at least an assumed knowledge of each other, of their biographies, motives, intentions, and inclinations, etc.?

Of course, the participant observer also observes in a world of shared understandings: *his* questions, observations, indications, inclinations, attitudes and motives are discerned by respondents in the course of their continuing interaction with him. He can be found by interactants to have and to display a whole range of motivated interests in the activities in which he engages. As might be expected, during the course of prolonged participant observation these motivated interests may vary in a variety of ways which others need to discover in order that they may interact with the participant observer in diverse and contingent situations. We could of course assume that responses expressing attitudes, beliefs and

activities are organised towards him with the biographical repair 'sociological researcher' as a relevance criterion. Although such an assumption seems to be a regular artefact of sociological enquiry, it strongly contradicts all experiential evidence of the way in which interaction takes place, and hence of the way in which the social world is organised. Consequently the researcher adopting such an assumption has no basis for construing the responses and constructions of the world designed for him as offering a clear guide to the constraints instructing members' responses in the real-life situations of their inter-activities.

One consequence of the subject's compliance with the research could be his design of a social world which corresponds with his understandings of the desires of the research worker. This is not to say that he lies or mischievously constructs a sociological world, but that, for him, the presence of his construction of the research worker's problem provides an analytic focus of relevance, allowing him to *ad hoc* responses that solve a practical purpose problem. This is to say that respondents interact with researchers in terms of a rational motivated model of what is required of them, and use this to discover a possible sense of direction to the researcher's activities. The situated interaction of researcher and researched is a practical purpose situation in which the subjects' compliance is sought and often received according to the normal constraints of courtesy, helpfulness and good manners. Members of the field are thus seen to design their utterances, analyses, and explanations produced for the researcher, with a version of the researcher's requirements in mind.

One of our authors, after a considerable time in the field, decided to ask 'actors' what they thought an 'actor' was; none of the respondents was lost for an answer, though it became clear that the type of answer varied considerably, and varied in relation to respondents' knowledge of the researcher. Some of those without knowledge of his involvement and past status as a professional actor were clearly concerned to treat him as some form of novice, and hence responded with some guide to 'how things are around here'. It should also be noted that considerable variation was found to occur according to how the respondents saw themselves as being selected for the question.

Similarly, responses to questions such as 'who' or 'what' are the 'left wing' varied according to respondents' classification of the questioner, and according to their capacity to locate his biography in the scene of their activities. In the example previously cited, one such respondent prefaced his description of the 'left wing' with the following: 'If you want a scientific answer, it's all those who opposed me on the fascist issue'. Others later responded to the question not so much as a question but as a comment on the seemingly intractable dilemma that preoccupied much of their reflective concerns. Thus in later months of fieldwork the 'question' produced from the same respondent: 'Ah, yeh, who indeed?'

Possibly the respondent now (or at least on this occasion) considered the researcher to be aware of the impossibility of producing anything other than a situationally contingent solution to the problem.

The sociologist who attempts to solicit members' versions of the 'truth' with the intention of incorporating these versions into a scientific depiction of events is soliciting the collusion of his respondents in the research enterprise. In so far as he is seen to be soliciting 'truth', then he may be treated by respondents as an auxiliary in that he is a receptive ear to their version of how things are. In that he attempts to treat their responses independently of the organisation of their production, he treats the persuasive accounts that he figures they are presenting to him as a corpus of material which constitutes and shores up his explanatory model. In this way the researcher treats his respondents as his auxiliaries in the production of the research. He is thus happy to find in his treatment of the responses as data a verification of the authenticity of his enterprise.

Fieldworkers have experienced with some surprise the fact that persons 'in the field' also operate with theoretical models, and engage in what has been called 'lay sociological theorising'. Both the authors encountered elaborate theoretical constructions of the social world of their activities, and heard these expressed quite frequently. For example, political activists in the field were often heard to use a version of the Michels (1949) thesis: 'socialists "sell out" and become bureaucrats on achieving positions of responsibility in the organisation'. The discussions among members often paralleled the form of discussions that occurred between fieldworkers and respondents, as researchers solicited materials that acted as potential confirmation of their theoretical edifice. This is not, of course, to argue that members of the field co-opt explanations from social science, but rather that such social science theoretical models are succinct distillations of 'what everybody knows'. There are, however, some significant differences in the intended uses of the sociologist's and the member's sociological models. We have observed that the respondent is called upon to produce situationally relevant and, therefore *ad hoc* responses to sociologists' enquiries. The need to *ad hoc* was found to exist as a requirement of finding a practicable solution to the interactive problems that the sociologist posed. We take it, as a result of our observations, that the scene of practical activities called for the continuous usage of such *ad hoc* devices.

Thus, in the field of left-wing politics, the sociologist conducting research discovered numerous persons who subscribed to a version of what we have described above as the 'Michels thesis'. It was argued among the 'left wing' Labour Party members that as soon as a local Labour councillor accepted 'establishment' office, in this case the chairmanship of council committees, he had 'sold out'. This meant that they considered that he no longer espoused 'left wing' views

as these related to local council affairs over which he now had some responsibility. It was interesting to note that a version of the Michels thesis could also be demonstrated by councillors who held 'establishment' offices and yet continued to claim that they themselves were 'left-wing'. Indeed, such people seemed to operate with a 'but not me' clause which enables them to retain the thesis as true. The *ad hoc* production of such a clause could be seen as enabling them to review the activities of other suspect parties who occupied similar positions without simultaneously impugning their own integrity.

The above example of lay sociological theorising provides the practical actor with a resource enabling him to maintain a view of his own actions as consistent. It also represents important ways of attaining 'overall pictures': a persuasive framework of what's going on. However, the use of such a framework is directly tied to the capacity to '*ad hoc*' some version, or sense, of what is relevant 'here and now'. It enables the actor to engage in ordered and organised disputes as to whether 'this' item counts as one of 'these', or one of 'those'. It also applies as to whether 'this' or 'that' piece of theory is a relevant dimension at 'this' juncture in time. Such matters are solved not as a matter of course, and not idiosyncratically, but as part of the negotiated micro-political traffic of interpersonally organised social life. Thus we are saying that, whilst lay sociological theorising is a vital enterprise in the sustenance of members' social order, its viability and vitality hinges upon the socially engineered capacity to '*ad hoc*' theory into, and as part of, contingent situations.

A way of viewing the basis of our argument is to observe that both fieldworker and the persons in his field are unremittingly committed to membership in order to understand, and constitute, the scene of their daily endeavours. We could refer to these sense-assembly processes as involving man as a membership machine. It is membership that is at the heart of the characteristics of individuals or groups, of scenes, activities, and events. The sociologist, in so far as he learns from his field experiences, attempts to learn and characterise the way in which his co-participants achieve and re-achieve this as an on-going process. It might at first appear as though the participants achieve such sense-assemblies according to a determinative set of rules, as a computer processes data, according to finite instructions. If this were the case, then it might seem possible to specify such finite rules in a manner that would provide for a formal description or specification of the culture observed. The thrust of our argument suggests that such a model is gravely misleading, as the 'rules' which constitute a culture are necessarily contingent and circumstantially *ad hoc*-ed by those who produce the culture.

In understanding and producing practical actions, members are not concerned to follow determinate sets of rules. Their involvement in a developing and

unfolding world requires the capacity to re-learn, and re-constitute, 'events', 'activities' and 'lessons' in a world that is in some senses always changing. This fact, of course, continually confronts the sociologist with the problem of validity. The sociologist appears to require unchanging and consistent definitions and conceptual apparatuses. *He* appears to require a determinate set of rules that allow him to categorise issues and events in a manner that is standardised. It is possible that some features of members' processes may be described formally and unequivocally. However, these features relate to the processes by which members routinely create order out of chaos. The methods members achieve in such productions cannot involve the use of the kind of closed scientific definitions that can be properly standardised such that they may be incorporated into a scientific model. This is because such definitions, because of their prescriptive nature, necessarily preclose the issue of how order is maintained and sustained.

This point is further illustrated by an examination of some features of the way in which members and sociologists produce case methodology for their respective practical purposes. Of course, our argument will be that the sociological method of case production and utilisation is itself a commonsense method which, in borrowing from the common sense of its subjects, achieves no distinctly scientific warrant. We will also attempt to show that the method of case reasoning produced by some members of the field has distinctive characteristics which enable it to achieve a temporal ordering of their affairs. These characteristics enable members to link a relevant past to a determinative present, so as to provide for a prospective sense of the future (Schütz 1972). We shall be concerned, then, with the critical problem of how members constitute past, present and future happenings and activities as 'events', and how they learn about the present from the past, and thereby learn 'truth lessons', see activities as 'typical', and take stances accordingly.

TAKING CASES

Given a sociologist's need to produce a case to instantiate or substantiate his theory, we might ask how he recognises an event in the world as a case? Cases do not exist as 'cases' in the world, independently of the ways in which they are displayed and used as cases. The consequences of this for a sociological researcher is to raise the question of how he might claim validity and warrant for his production of 'events' in the world as 'cases'. The researcher could, of course, construct via his own imagination and common sense this or that 'event' before him as a 'case'. He could not, however, do this in a way that would satisfy the sophisticated requirements of a sociological methodology in terms of such issues as validity. Of course, the researcher does not have to rely completely on his

imagination. Members of the field are particularly adept at signifying to sociologists *their* ways of seeing the world. Thus quotations in field notes abound with remarks such as 'it's another case of', 'it's the same old thing', etc. Our argument is that the fieldworker adopts one of, or a combination of, the following strategies.

The researcher hears members' stories/examples, etc., and adopts these as 'cases', although he may redescribe them. More sophisticatedly, the fieldworker extracts a set of 'rules' from one or more such 'cases', and uses these rules to discover other 'cases'. The researcher may then refer back to his field members to 'check out' that he has successfully used the cultural apparatus. This reversal is an attempt to satisfy one of the validity requirements of this method. For a case to be used as a part of the participant observer's methodological apparatus, one requirement is that it is seen as constructed in relation to how it is an experienced object of significance in the members' cultural world. This requirement follows on from the generally accepted rationale for participant observation, that units of analysis should be based on the subjective world of participants.

We shall discuss one such 'case' from our research. As previously referred to, a person who had been styled as a 'left wing' leader became, largely as a result of 'left wing' support, a member of the council's establishment: in other words, the chairman of the council's housing committee. His 'first act' (the act so seen as his first) on becoming chairman was to vote for an increase in council house rents. This act was seen by 'left wing' members as something of a betrayal. The members discussed the matter with the participant observer in terms of 'It's the same old story', 'It's another case of the way our leaders sell out when they gain administrative power', etc. The event became much talked of and was seen by members and observer alike as an event of some significance. The significance of the event itself became a topic of discussion among the 'left'.

We shall now attempt to analyse this case for how it was constructed by members as a 'case'. Firstly, it was necessary to see an action by the man concerned in terms of his motivations for that act. In other words, in this instance it was necessary to see the vote by the now chairman as an event of significance to him, expressing some sort of motivation categorisable in terms of a political stance. Secondly, it was necessary to see the 'event' as an example of something in general: 'leaders forget their old allegiances'. This generality is supplied by a lay sociological theory: 'leaders forget their old allegiances and "sell out"'. Thirdly, it was necessary for the members to make the candidate 'case' fit the theory.

Fitting work is multifarious, but certain features seem to dominate. Argument among members of the field ensued as to how this act was a consistent feature of the man's general motivation. This involved the re-writing of his biography in line with the theory: 'he was never really one of us', etc. This provided adequate

grounds for *his* 'type' (seen as a specific instance of a general 'type') to perform an act of *this* 'type' (seen as a specific act of a general 'type'). One of the features of this commonsense reasoning was that members were then able to ignore the previous characterisation of the late leader, seeing it as only a 'mere appearance' or a 'front'.

Other possible contra 'cases' (that is, other instances of actions that might lead to alternative conclusions) were now systematically ignored. Also ignored were other possible features of the man's character. It is as if this act, now a 'case', becomes decontextualised and frozen in time. The systematic ignoring of his previous characteristics and of other features of his personality, as demonstrated by other actions, does not imply conspiratorial behaviour. These features are ignored because they are not relevant features of the practical purpose activity in which members are currently engaged.

The lessons members were able to draw from this 'case' were: (a) 'don't trust leaders' (general), and (b) 'don't trust him' (particular). These lessons *are*, however, the practical purpose motivation for members' procedures. They act as advice and practical directions to members, newcomers and participant observers; they also provide the basis for future claims regarding membership. In addition to this they act as a proof of the lay sociological theory that was used to discover the act as a 'case'. Thus the theory is used to discover what proves it; such is the necessary circularity inherent in such commonsense procedures.

The 'truth', what 'really' happened, or what the 'real' motivation was, is produced by members as a phenomenon subject to possible claims and disagreements of interpretation among members. This is not to say that they have any difficulty in deciding such issues as truth, but that for them logical rigour is not necessary in relation to their practical purposes. It is this fact that renders the 'case' unsuitable for the sociologist's scientific purposes.

For members, some version of the above is an overwhelming feature of their commonsense reasoning. It is the argument of this paper that not only do the sociologist's methods incorporate the results of such members' activities, but, even when his activities can be seen as different from members', the sociologist is inextricably involved in such commonsense reasoning procedures as a result of his own membership of the culture.

The sociologist identifies from the field some phenomena and depicts them as 'events'. From one such 'event' he is able to see a 'case' produced. The relevance of the 'case's' production is supplied by his theoretical model; by his theoretical model as aided or constructed according to the persuasions of his co-members; or by his adoption of members' lay sociological theory.

Every event in the world is a unique contingency. It is therefore necessary for the researcher to perform some heavy interpretive work in order to make his

'case' fit; that is to say, to see his case as a *general case*. In order to do this, the researcher must supply his actors with the appropriate motivations, according to which version of the theory he has used. The sociologist uses background circumstances as resources to confirm the appropriateness and the fit of his case. Part of the background circumstances he uses includes members' characterisations of events in general, and of 'this event' in particular.

The fieldworker is thus able to use his 'case' as illustrating a general model or hypothesis. He is also able to confirm his model or hypothesis by the use of the 'case'. It is, however, a logical and methodological requirement that the model or theory to be proved should be independent from that which is used to illustrate or prove it. The method does not meet the requirements of methodological adequacy because the 'case' is not independent of the theory: the theory was used to select the case. Further cases will be selected by the same procedure and these will therefore not only fail as independent of the theory but will not be independent of each other. Furthermore, the 'case' that is used to illustrate the theory is also the 'case' that is used to prove it: one cannot simultaneously illustrate and prove by the same device.

The method fails because the participant observer, like any other member with a practical purpose commitment to seeing the world in accordance with theories, has of necessity used folk models and folk theories in order to constitute the data as data: that is, in order to constitute an event as a 'case' in the first place. The case cannot therefore be accepted as scientifically independent of either (a) folk, or lay, sociological theory, or (b) sociological pseudo-independent theory.

What the researcher in fact achieves is, like a member, a practical purpose, *ad hoc*, produced fittedness, and thereby an attempted persuasion that we should see his world 'this' way. There is no independent scientific basis for the persuasion other than the researcher's own practical purposes. The sociologist either agrees or disagrees with members of his field; he does this by accepting their beliefs and entering their disputes. Are we too to see the 'left wing' leader as having 'sold out'? If so, here is one member of the field we disagree with: he didn't think that he had 'sold out'.

NOTES

1. Equity maintains a separate register for 'stunt artistes' on which not all members are entitled to inclusion.
2. See, for example: (i) Schatzman and Strauss 1973; (ii) Bruyn 1966; (iii) Filstead 1970; (iv) Junker 1960; (v) McCall and Simmons 1969; (vi) Hammond 1964.

REFERENCES

- Anderson, Ian G. 1977. The availability of a corpus of knowledge: Describing the social world of actors. Unpublished manuscript.
- Atkinson, Mick A., Edward C. Cuff, and John R. E. Lee. 1978. The recommencement of a meeting as a members' accomplishment. In *Studies in the Organization of Conversational Interaction*, ed. J. Schenkein, 133–153. New York: Academic Press.
- Bittner, Egon. 1965. The concept of organization. *Social Research* 32(3): 239–255.
- Blumer, Herbert. 1962. Society as symbolic interaction. In *Human Behavior and Social Process: An Interactionist Approach*, ed. Arnold M. Rose, 179–192. London: Routledge.
- Bruyn, Severyn Ten Haut. 1966. *The Human Perspective: The Methodology of Participant Observation*. Englewood Cliffs, NJ: Prentice-Hall.
- Filstead, William J. 1970. *Qualitative Methodology: Firsthand Involvement with the Social World*. Chicago: Markham.
- Hammond, Philip. 1964. *Sociologists at Work*. New York: Basic Books.
- Junker, Buford H. 1960. *Field Work: An Introduction to the Social Sciences*. Chicago: University of Chicago Press.
- Lee, John R. E. 1970. Conflict and cooperation in a local political party. Unpublished manuscript.
- McCall, George J., and Jerry L. Simmons. 1969. *Issues in Participant Observation*. Reading, MA: Addison-Wesley.
- Michels, Robert. 1949. *Political Parties*. New York: Free Press.
- Pollner, Melvin. 1975. 'The very coinage of your brain': The anatomy of reality disjunctures. *Philosophy of the Social Sciences* 5(3): 411–430.
- Sacks, Harvey. 1968–1972. Miscellaneous unpublished lectures. University of California, Irvine.
- Sacks, Harvey. 1972. An initial investigation into the usability of conversational data for doing sociology. In *Studies in Social Interaction*, ed. David Sudnow, 31–74. London: MacMillan.
- Schatzman, Leonard and Anselm L. Strauss. 1973. *Field Research: Strategies for a Natural Sociology*. Englewood Cliffs, NJ: Prentice-Hall.
- Schütz, Alfred. 1972. *Phenomenology of the Social World*. London: Heinemann.
- Turner, Roy. 1972. Some formal properties of therapy talk. In *Studies in Social Interaction*, ed. David Sudnow, 367–396. London: MacMillan.