SOCIOLOGICAL WORK:
Some Procedures Sociologists Use for Organising Phenomena*

R.J. Anderson and W.W. Sharrock

I

We will start by going over what we hope are some familiar matters. The first of these is the anaemic, if not quite terminal, condition of the sociology of knowledge. Despite the fact that we have had several decades of research and reflection, not all that much is known about the socially organised character of any one body of knowledge, not even of any of the natural sciences so often the target of sociological interest. What discussion there has been has concentrated on what might be termed the "cognitive" and "political" aspects of scientific and other forms of knowledge. How does a scientific revolution take place?[1] What is conceptual relativism?[2] What is the rational basis of magic in a western society?[3] How does the community of scientists and quasi-scientists identify and mark itself off?[4] These are the kinds of questions which the sociology of science and the sociology of knowledge have been taking up. And, if this is the case with the studies of science, how much more is it likely that the same disattention will be given to the practical organisation of sociological activities? But, although sociology has nothing like an elaborated and fully developed set of procedures for the investigation of bodies of knowledge such as science, necromancy and sociology, nonetheless it does have the embryo of such procedures in the concepts which form the leading ideas of the sociology of knowledge; namely, "ideology", "mode of discourse", "social production of knowledge" and "the social construction of reality". At the moment though, these terms have something of a sloganising character, being brandished as solutions to analytic problems rather than being seen as the vague characterisations of topics for investigation that they really are. Apart from these somewhat crude sensitising concepts, the sociology of knowledge possesses nothing which could be used as the basis for the detailed investigation of what working within a corpus of knowledge actually entails as a practical, routine and recognisable activity.[5] In this paper we hope to make a gesture towards what this might be for the body of knowledge encompassed within sociology. What we are suggesting is a transformation of the consideration of cognitive and political aspects of bodies of knowledge into an organisational framework. Where the sharing of a body of knowledge such as science or sociology has hitherto been thought of as a cognitive or political matter, we now propose to treat it as an organisational one by focussing on the socially organised character of the activities involved in 'doing' sociology. The main purpose of this paper is to provide a general sketch of what is involved in this transformation. We see this as indicating the possibility of founding the sociology of knowledge as an empirical and investigative discipline rather than the speculative and didactic one that it tends to be now. In other words, we are proposing a familiar course: that it is time to start all over again; time to turn the sociology of knowledge upside down and begin at the beginning once more.

The second of our familiar matters has to do with the distinction that Abraham Kaplan[6] once made between a "reconstructed logic" and a "logic-in-use". Kaplan used that distinction to point to the differences between the ways that we might talk about a set of scientific activities as the mundane, routine, day in day out things that scientists do, and the ways that those same activities are presented in the formal setting of a research report. In such reports, activities are reconstituted according to the requirements of some formal canon of scientific procedure. In this reconstituted form the haphazard, thoughtless, ramshackle and intuitive rule of thumb aspects of scientific discovery are processed out and, instead, the logical and formal connections between events and their effects, arguments and their conclusions are emphasised. Kaplan's distinction is an interesting if somewhat misleading one. It is interesting in that it directs attention to the formatted and constructed nature of research reporting and hence to the institutionalised nature of the activity of writing and reading such reports. It also makes strong play of the practicality of the "logic-in-use" as a means for solving day to day problems, of making findings and generating observations. The distinction is misleading, however, since it directs attention away from the practical aspects of giving research reports. The "reconstructed logic" of the research report is the practically oriented "logic-in-use" for the presenting of such reports. The formalisation of the "reconstructed logic" is just another "logic-in-use" which is displayed in the ways that the report is put together.

The implication of this view of Kaplan's distinction is the denial of the possibility of a fit between some depiction of an activity and the activity itself. We only have access to activities in the ways that we talk about them. Any account or description of some event or activity formulates that event or activity in the course of providing the account or description. It is only in the course of the telling of the story that it is possible to provide for what the story is about. Describing, accounting, telling are all accomplished in the course of the activity itself. None of this should be taken as indicating that we feel that there is no room for argument over alternative possible descriptions and their appropriateness. That would be nonsensical. But, any such disagreements arise only in the context of disputes concerning further conceptions about the relationship between orders of designation and the locale in which they are to be found. It is never a matter of deciding upon just one way of characterising the activities and events and applying that universally. It is always a matter of deciding for whom the designation is being given, and alongside what other descriptions it is to be placed. The denial of the logical possibility of a fit between the phenomenon and its description follows quite simply from the observation that phenomena are always encountered as phenomena-in-some-sort-of description or account, and never a phenomena *tout court*.

The sociological importance of this surfaces at two levels. The more general of these is the topic for analysis which it provides for the sociology of knowledge. Any body of knowledge can be seen as providing, for those who work within it, a range of methods by which the "problematic possibilities" of description can be resolved in the course of giving the descriptions. Such routine methods for the organisation of descriptions and accounts within any body of knowledge are precisely what we have referred to as its socially organised character. By extension, the second level at which "problematic possibilities" are to be found is within sociology as one particular body of knowledge. The problem within sociology is simply an instance of its wider currency. Within sociology, it can be thought of in something like the following manner. No one would disagree with the proposition that the object of sociological scrutiny is social life. What is in contention within the discipline, though, is what is to be allowed to count as sociological data on social life. Since

80

there appears to be much room for argument, it seems obvious that, in fact (rather than, say according to a particular methodological prescription), there is also a great deal of room for manoeuvre in deciding what is and what is not sociological data. An awful lot of methodological argument turns upon this "in fact". No doubt rival sociologies debate the authenticity of each other's data, but the very fact of the existence of competing claims means that there is, again as a matter of fact, a range of things that count at any one time as "data" for sociology. Hence, different aspects or features of social life will be taken as data for investigation by different kinds of sociology. This failure to display agreement over what is to be taken as data is not, then, simply an artefact of theoretical naivety. Rather, the fact that social life can generate problematic possibilities for sociology, in that different data will be taken as representing social life, is both endemic to, and an oriented-to feature of, sociological descriptions.

Let us elaborate on this a little. When faced with the possibility that (almost) anything might be taken as proper sociological data about some particular set of activities depending on precisely which methodological stances have been adopted, one way of deciding what should count in this or any instance might be to assume that only those things that are "obviously" and "immediately" recognisable as social phenomena will do. What this might mean, though, is the reduction of sociology to a parasite upon commonsense, or perhaps better, commonsense theories of social life. So, for example, if the most obvious thing about some activity in some setting was that it was "clearly" and "demonstrably" dominated by differential performance of tasks and differential control of resources then such sociological categories as "roles" and "distribution of power" would likely be invoked to provide "recognisable" descriptions. Such a move solves the problematic possibilities issue by assuming a stance of "naturalism" or "naive realism". It is a perfectly feasible thing to do and lots of what are otherwise difficult and puzzling matters can be swept away by it. Whether it is an adequate move, given certain motivations towards formality interpreted as consistent and rigorous description, is another matter — one that does not concern us just now. Whether such a "naturalism" is adopted or not, sociological work still consists of the transformation of observable elements of social life into sociological data by coding them or reading them in some way. Naturalism may be the most obvious form of coding to adopt; but it is still a form of coding. The centrality of the issue concerning "problematic possibilities" should now be in evidence. Any particular kind of organisation of sociological data is just that, an organisation — one possibility among many, even when it is the most obvious way in the world to look at things. It is at this point that we can draw out the phenomenon we are interested in discussing. Any such organisation could, at least in principle, be supplanted by some other. This gives those who wish to provide sociological descriptions of social life, a trouble, a problem to solve, a task to accomplish. They have to show how and why some aspects of social life and not others must and can be taken as criterial, significant, revealing, etcetera. That is to say, such sociologists have to display how they have solved the problematic possibilities problem and to ground the way that they have done it.

It will pay to be just a bit cautious at this point. One thing we are certainly not saying is that every piece of analysis or description has to have, right at the start, a thorough-going consideration of all the possibilities that sociology could provide. Nor are we suggesting that analysts face this problem in any conscious way at all. They certainly do not all go around wondering how they can escape from this methodological corner by justifying what they are doing. Indeed, most of them rarely suffer from any methodological *angst* at all. The problem does not arise because most sociologists are engaged in the eminently practical task of getting on

81

with their sociology and not reflecting upon it, and certainly not in the ways that we are. The parallel between this distinction and that between the natural and the sociological attitude on daily life is striking. The ordinary orderliness that social activities display is sociology's central puzzle. It solves its puzzle by construing all such activities as themselves solutions to the orderliness problem that members face. But, even while doing this sociology recognises that constituting social life in this way is to do so under special theoretical auspices. Members live out their lives under the very different auspices of commonsense practicalities. In exactly the same way, we are positing that sociologists can be treated as operating under the very practical constraint of having to solve the problematic possibilities problem as they go about the business of giving sociological descriptions. The descriptions that they give display, demonstrate, give an account of the solutions they have adopted. Finding such solutions is just what sociological work is about.

Another related aspect to the phenomenon of "problematic possibilities" is the cumulation of findings on data. Treating findings on data of a particular type as cumulative gives the analyst the task of locating any specific set of findings within the collection of findings-made-to-date-on-this-kind-of-data. What this is, in other words, is the achievement of the sequential location of findings and their analysis. One way to begin to look at this is by taking up, albeit a little differently, Cicourel's notion of "triangulation".[7] Via a list of previous studies and their various contributions, this study and its contribution can be located. Triangulation is both a way of "surveying the field" and locating this new set of findings within it. Such triangulation also provides a resource for the development of appropriate standards of evaluation. It is at this point that a first set of constraints on the free rein of "problematic possibilities" is encountered. Although almost any organisation to social life could be taken as sociological data, in fact not just any organisation will. The history of sociology provides an inventory of standard formats. It is in the ways that researchers make their organisations *recognisable* as one or other version of some standard format that the standardising that is the product of descriptive work, is achieved. In summary, then, sociological work consists in making some descriptions recognisable as sociology and that means standardising them. It is when we consider the problem of giving descriptions in such a light that our conception of "problematic possibilities" can be seen to be clearly different from the Weberian notion of the inexhaustible richness of the world.[8] The latter perspective begins by proposing that sociology must try to come to terms with the impossibility of ever providing a complete or exhaustive description of some activity, whereas the idea of "problematic possibilities" suggests that it is recognisably adequate descriptions which we try to give in sociology. In sociology we do not aim for completeness and necessarily fail. We aim for adequate recognisability. It is testimony to the social organisation of our descriptive skills that, most of the time, we succeed.

What emerges from all this discussion is the work that goes into establishing what we want to call the "contextual shapeliness" of some descriptive organisation of data. Such description both accounts for the organisation that the data is held to conform to as well as demonstrates that organisation. What is being referred to by a notion of "the contextual shapeliness" of a particular description is the symmetry between "this account" and "others already provided", the similarity between the data studies "here" and "elsewhere", the illumination that this work throws on central problems in the discipline, commonsense problems of life, fieldwork experience, etc., etc., etc. It is this managing of the "contextual shapeliness" of an account which is to be the topic of this paper. In the next section we want to discuss first a set of procedures by which this symmetry might be achieved. We will then go on to discuss ways that such "contextual shapeliness" can be secured by reference to

82

the orderliness that is "discovered in" or "imputed to" the data in analysis. This second set of procedures we shall talk of as *order enhancing* procedures. Our conclusions will be that it is in the demonstration of the hidden orderliness which analysis has discovered as *the orderliness which data possesses* (i.e. the provision of a viable descriptive organisation) that the troubles talked of as inherent in the "problematic possibilities" provided by data are solved and contextual shapeliness provided. The problem of finding a descriptive organisation is, then, both a first and a central one for sociological work. But it is not a problem that is solved at the beginning and then dismissed. Good sociological descriptions work up and work out solutions in the course of their production. In fact, the descriptions are the solutions.

What we would like to suggest, then, is that one way of taking up a sociological interest in sociological descriptions is to treat them as constructions. When this is done some of the procedures which are used to organise the production of such descriptions can be brought into focus. As such, the treatment of sociological descriptions as designed and organised for the occasion of their use provides an indication of how the sociology of knowledge might be refurbished. Such a sociology of knowledge would take as its starting point the investigation of how it is that the traditional location of a piece of work as, say, post Freudian Psychoanalysis, or Marxist historiography, is made available and recognisable in the work itself by use of the procedures that, as we have suggested, shape the context of the materials presented and enhance their orderly properties. It is to the examination of these two clusters of methodic practices that we would now like to turn.

II

What we have been saying so far ought to have something of the ring of truism about it. We are saying no more than everybody knows, everybody, that is, who has ever had to write a research report, conduct an investigation or justify their time engaged in fieldwork. We all know that writing a report involves facing up to a whole gamut of contingencies; there is too much data or there is not enough; too much time was spent on some things, not enough on others; there are no good publicly defensible reasons why some things were done and others were not, since whim and personal preferences tend not to be "good investigative reasons". That these and many more "practicalities" have to be diagnosed and overcome is what we all know. The researcher's task is to overcome them *somehow*, to some extent, and perhaps but not always, to his own and his colleagues' satisfaction. Our suggestion is that sociological work involves the use of sets of procedures to facilitate the discovery of working, and workable, solutions to this problem. In a very strong sense it might be said that the procedures provide sociological methods for working out, and working up, precisely what it is that any set of data or collection of fieldwork materials might be said to amount to. What we will offer now is a general introduction and not a technical exegis of particular examples. It is hoped in later papers to provide detailed exemplifications of these methods in specific instances. What follows here is merely a sketch of what a proper consideration might be composed of.

(a) *Bricolage*

This is the first of our procedures and borrows heavily on the way that this term has been used by Lévi Strauss.[9] In its original use, the term was a metaphoric representation of the methods by which social relationships are constituted in myths. The bricoleur is a jobbing builder-cum-handyman whose speciality is to be

83

able to make do with whatever is to hand for whatever task has to be completed. What bricolage points to is the virtuosity and ingenuity with which myth makers use their own social and ecological environment to put together standardly recognisable creation myths. We would like to extend the application of the term to the methods by which sociological investigators pick upon a fragment of material — say, a quotation or a citation or a striking response, and despite the fact that one cannot be quite sure that the report is a fair one, or the fact that the quotation is not really about the topic in hand, or that the citation is to a work that has not yet been read, or that the example is not quite clear enough, or forceful enough, nonetheless, its use is sanctioned by the need to get something done, to put something there; and 'this' will do for now; 'this' will do pending further research, a more thorough analysis or future developments in the theory. Generations of students have now come to see social statistics as something of a shanty discipline; that measures of "poverty" reflect bureaucratic convenience and measures of "inflation" political expedience. We also know that coroners decide on mode of death by a variety of *ad hoc* means[10] and, more amusingly, that if you compare the statistics for the 'same' phenomenon collected by two different agencies they are liable to differ remarkably![11] But, despite all this, investigators are left with the fact that these statistics are the only ones which are available and can be used, and so these are the ones that will have to be used with all the reservations, qualifications, emendations and explications they may feel they have to make. Although everybody knows that they are not what one would like, they are the ones that will have to do. It is this force of circumstances aspect to research reporting that grounds the bricolage that we have been referring to. Researchers display all manner of ingenuity and virtuosity in patching over the limitations and failings of their data, cobbling together their arguments and pointing up the virtues of their way of doing things as against the inadequacies of others'. Robert Nozick has captured the essence of this bricolage beautifully in the following description which he gives of his experience of doing philosophy.

> One form of philosophical activity feels like pushing and shoving things to fit into some fixed perimeter of specified shape. All those things are lying out there, and they must be fitted in. You push and shove the material into the rigid area getting it into the boundary on one side, and it bulges out the other. You run around and press in the protruding bulge, producing yet another in another place. So you push and shove and clip off corners from the things so they'll fit in and you press in until everything sits unstably more or less in there: what doesn't gets heaved *far* away so that it won't be noticed. (Of course it's not all *that* crude. There's coaxing and cajoling. And the body English.) *Quickly* you find an angle from which it looks like an exact fit and take a snapshot; at a fast shutter speed before something else bulges out too noticeably. Then, back to the dark room to touch up the rents, rips and tears in the fabric of the perimeter. All that remains is to publish the photograph as a representation of exactly how things are, and to note how nothing fits properly into any other shape (Nozick 1964:xiii).

(b) *Thematising Phenomena*

If bricolage describes the manner by which research reports are compiled, then, thematising their phenomena is one of the objectives of such activity. What we mean by this is the way that vast arrays of instances of what appear to be very different things culled from many different settings and locations are lined up with each other by relating them to a theme to which on the surface at least, they bear little or no relationship. The provision of the theme enables the collection of the instances

84

together and justifies their co-classification. Talcott Parsons excited much debate when he insisted on treating such widely divergent phenomena as money, power, influence and commitments as essentially the same insofar as they could all, from a point of view concerned exclusively with social system interchange, be considered to be media facilitating such interchange. However, the thematiser *par excellence* in the social sciences must be Freud. Who else has aligned such a range of scattered phenomena as dreams and hallucinations, obsessions and phobias, jokes, religions and superstitious beliefs, parapraxis and numerical associations to the single theme of the expression, repression and sublimation of sexual identity? As he says in *The Psychopathology of Everyday Life*, we

> shall not be able to form a correct picture of the strange psychical work which brings about the occurrence of both parapraxis and dream images until we have learnt that psychoneurotic symptoms, and especially the psychical formation of hysteria and obsessional neurosis, repeat in their mechanism all the essential features of (condensations and compromise-formations) (Freud 1975:343).

(c) *Imposing a Unity of Purpose*

Closely related to the thematisation of disparate instances is a procedure by which individual cases are examined as versions of one another. That is to say, they are treated as if they were all produced for exactly the same purpose, but with their distinguishing features being accounted for by the contextual circumstances of the occasion, locale or personnel involved with their occurrence. One leading example of the presumption of a unity of purpose has been the 'intellectualist' account that Skorupski[12] and others have argued for the similarity of purpose engendering western scientific practices and non-western magical ones. Of course, the whole edifice of functionalist description is premised upon the viability of the imposition of unity of purpose, since, without it, cross cultural comparisons would be rendered impossible. What the imposition of a unity of purpose provides is a device by which selective attention can be given to certain aspects of phenomena, thereby bringing up their general and shared features and playing down those which make them individual instances. In this respect no-one has used the technique of imposing a unity of purpose to greater effect that Erving Goffman. In all of his many papers and books are to be found lists, catalogues, typologies and collections of widely dispersed and apparently unrelated pheonomena and activities that are co-classed with one another simply by the attribution of a common interactional motivation. This reaches its apotheosis in his first report, *The Presentation of Self in Everyday Life*, where he defines his interest in social activities in the following way:

> Regardless of the particular objective which the individual has in mind and his motive for having this objective, it will be in his interests to control the conduct of others ... (1971:15)

A little later this is extended to:

> ... I assume that when an individual appears before others he will have many motives for trying to control the impression they receive of the situation (1971:26).

And, on the basis of this, what goes on in hotels and family parlours, operating theatres, on stage, school classrooms, indeed anywhere, can be taken as being to all intents and purposes 'the same' in that it displays the techniques by which co-participants orient to and use "the arts of impression management".

(d) *Ad Hoc Generalisation*

Routinely researchers face what might be thought of as the practical (as opposed to philosophical) problem of induction. Somehow they have to make it feasible, plausible, acceptable that the arguments, descriptions and accounts that

85

they offer for the data which they have collected and cited can be generalised beyond 'just this collection' to 'all phenomena of this type'. One way to do this is to operationalise standard statistical techniques for the calculation of the representativeness of the data as a sample of the population in question. Such techniques provide one way in which the globalisation of research findings can be substantiated. In ethnography another strategy entirely is used. This involves the pluralising of activities on the basis of the researcher's familiarity with the setting which he is describing. Because they have spent months, weeks or days 'in the field', we take it on trust that researchers can recognise a normal scene from an abnormal one and that what they are describing is what normally goes on 'on the street',[13] or when a Zande consults his oracle[14] or on the back wards of a mental institution.[15] The generalisation is *ad hoc* in that the strength of the regularity of the behaviour described, the preference for this type of activity over another, the ways in which the researcher knows that this is what it is claimed to be, are all left out of the account itself. We are never told what observing oracle consultation, the buying of a 'fix' or a total institution actually amounted to. We ought to point out here that we would not want to be seen as pouring doubt on the veracity of ethnographic field reports. Field researchers do describe the things they see and hear, and anyway, since we have no principled means of checking on them, we have to take what they say on trust. This is not a matter of professional competence or of the relative inadequacy of the discipline. *Ad hoc* generalisation is at the very centre of ethnographic field research as an essential part of its process. It allows the researcher to go from what he knows and has seen, what he can say about people he met and those that acted as his informants, to the way of life in general, and the conceptual frameworks enshrined in the culture. It is the presentation and elucidation of generalities from particulars such as these that ethnography is about.[16]

(e) *Solution-before-puzzle*

Once a set of data has been compiled, the research notebooks filled, the questionnaire coded and the print-out produced, the results, the findings, the phenomena have to be made understandable, sensible, recognisably sociological in some way. This is done by specifying that the relationships between the items exhibit that they are all solutions to the same puzzle. The puzzle has to be found for which they can stand as the solution. Once the puzzle has been delineated it is possible to treat the findings, the data, as if they had been collected with just this puzzle in mind. Collections of types of familial organisation, types of politeness exchange and ritual,[17] the writings of seventeenth century French dramatists[18] can each be shown to display or evidence that they are solutions to some particular problem, be it of the 'fit' between family life and economic organisation, the solidaristic function of remedial interchanges or the ideological hegemony of Jansenism. But, if we accept Timothy McCarthy's[19] account, perhaps the classic case of working up a solution before the puzzle was available will turn out to be Marx's "discovery" of the revolutionary role of the proletariat in modern capitalism. This solution was formulated in his *Critique of Hegel* and elsewhere several years before the nature of the puzzle had been analysed as the inherent and exacerbating contradictions in the operation of the relations of capitalist production. Indeed, it would not be all that unfair to represent Marx's life work as the search for the problem to which his rejection of Hegel had provided the solution.

(f) *Construed Orderliness*

The purpose of a "reconstructed logic" was to remove from any account the circumstantial and contingent aspects of its happening. Such a "reconstructed

86

logic" involves the use of what we like to think of as a construal of orderliness. Whatever was done for biographical, intuitive or idiosyncratic reasons is rationalised in abstracted or procedural terms. Such a rationalisation gives an account a sequentially organised and proceduralised structure with the elements contained in it following on from one another in a regular and formatted manner. If we might take Talcott Parsons as our example once more, *The Structure of Social Action* proceduralises and structures Parsons' intellectual history up to the point of its publication by reconstituting the intellectual resources assembled as the result of luck, chance and misfortune as he moved from Amherst to London to Heidelberg, back to Amherst and finally to Harvard, as indicating a theoretical convergence in social science as evidenced by Pareto, Marshall, Durkheim and Weber. These were simply the people he knew about, these were the courses he had taught and the themes he had debated. They were all that he had available to include in his synthesis and it is only the effectiveness of the case that he makes for the synthesis which makes the construed convergence such a forceful one. The elements of *The Structure of Social Action* were gathered together as the adjuncts of Parsons' biography and are presented as displaying an inherent, theoretical orderliness which justifies their collection and which provides the theme of the book.

We will finish this section by repeating what we said at the outset. The observation of the use of devices and procedures such as the ones we have outlined is not to be taken as portending a critique of their usage. Such devices and procedures provide ways in which the routine, natural and essential constraints on budget and time as well as human resources are to be coped with. They provide viable means by which what was done and can be recovered, what is available here and now, can be put to the most effective use. They provide ways of fitting things in as well as leaving things out, of making as much as is possible from the resources to hand, for closing off arguments that cannot be answered and defending the use of data that just happened to have been collected. In short, they provide routine, practical and effective ways of circumventing lengthy, cumbersome and probably useless procedures while at the same time throwing up defences against criticism and attack. By their means the shapeliness of the account is accomplished.

III

We have already indicated that analysis often consists in the bringing out of an orderliness to activities which cannot be discerned except here and there in a set of materials under examination. The corpus seems both to exhibit the orderliness and obscure it. It is for that reason that analytic treatment is necessary in order to make the underlying and pervasive orderliness visible in each and every part. We think of this way of handling materials as enhancing their orderliness. In this section we would like to set out some of the ways that such enhancement can be accomplished.

(a) Colligation

Quite recently we were engaged in a discussion of participant observation with a group of colleagues all of whom were anthropologists or sociologists. We were very surprised to find that, despite their fieldwork experiences and undoubted expertise, they tended to overlook the fact that no matter what objections and problems could be raised both in theory and in practice to the provision of sociological descriptions, researchers are engaged in finding working solutions to just those problems. For fieldwork to be possible, principled weaknesses, discrepancies and the like have to coped with *somehow* as researchers go along. The solutions that are worked out are embodied in the fieldnotes that are collected and the descriptions which are given of the settings that are studied. In general, such

87

descriptions have what we like to think of as a 'one damn thing after another' character. First one thing is observed, then somebody does something else. Then something else happens; and so on and so on and so on. As a result, the flow of events in the fieldnotes is somewat haphazard. And yet this haphazardness, this contingent, one damn thing after another character disappears in the final report.

One of the ways that it is processed out is by the systematic disassociation of materials from the ways in which they are assembled. The corpus of data and materials is a corpus simply because it is what the researcher has. They are *his* fieldnotes; they are *his* data run. It is the fact that he possesses them that gives them their unity in the first place. Often they have been collected in a variety of ways — using different researchers or different informants, or the same informant in different settings or on different occasions. Hence they are a response to a whole set of local circumstances — whom you could make friends with, who gave you access to important people, data banks etcetera, etcetera. The collection, as a collection, has a massively contingent aspect to it, one that is often only recognised in the acknowledgments. The data consists of whatever happened to be going on while the researcher was there, whatever the locals were prepared to talk about or answer questions about. This contingency in the assembly process is disregarded in analysis by treating the corpus as expressing some form of putatively logical organisation. This is achieved by breaking up the data and reconstituting it in different clusters or data runs. The specific historical circumstances within which it was assembled are dismissed as irrelevant since, potentially at least, they threaten the possibility of detection of the underlying orderliness. The whole corpus is subjected to a process of kaleidoscopic colligation until, having sorted through in a variety of different ways, some features stand out, some similarities are striking and some connections can be made.

Colligation, then, may be thought of as entailing three distinct steps. First it is necessary to acquire a sufficient familiarity with the corpus to allow search procedures to generate obscurities, puzzles and themes and to allow decisions to be made to determine what is safe to ignore and what must be looked at. Second there is a more disciplined inspection of the corpus to enable the selection of like instances of data, fragments, observations which can be related to the theme which has been decided upon. Once this loosely related compilation has been put together, it can be combined and re-combined according to various different analytic principles which, by juxtaposing items, make observable matters which were invisible when the fragments were considered in isolation. This colligational method enables relationships to be discerned, specified and demonstrated.

(b) *Incongruity Procedures*

One of the reasons why those who would want sociology to develop formal theories tend to despair of ever achieving their aim is the apparent impossibility of constructing standardised equivalence classes out of which formal theories could be built. But, although sociologists may not be much good at the formal connection of such classes, they are dab hands at working out practical ones. Most of the research that is done seems to be organised around the exposition of the researcher's personal and idiosyncratic typologies. Very rarely are such types derived from a review of the whole collection of data which is to be classified. Much more often the types are decided upon early on in the process of classification and exported from one exercise in classification to others. This tends to result in classificatory inertia and when cases turn up which do not fit neatly, the whole schema has to be stretched to accommodate them. This gives rise to the familiar analytic ploy of "deviant case analysis".

88

What appears to happen is something like the following. The criteria demarcating category or type membership are taken as referring only to "base cases", that is to say to those items which can be unambiguously recognised as proper members of the type. All other instances are then related to the base case as displaying local features which make them underdeveloped or overdeveloped, less or highly differentiated, vestigial or embryonic. In this way all kinds of anomalous cases can be brought within the ambit of the classification by treating them as variations of the base case under particular circumstances, the general typology having been elongated to accommodate such warpings and sub-types. Examples of such incongruity procedures in the compilation of types abound in sociology. One has only to think of the way that the concept of a "capitalist chain"[20] has been used to effect a connection between industrialised, industrialising and non-industrial economies; or again, of the way that "alienation" as a characteristic of a particular relationship towards productive processes has been extended beyond the manual working classes to encompass the professional middle classes as well;[21] or, even more clearly, how differential attachment to sets of culturally defined goals and the means for obtaining them has been used to produce a typology of criminal and other deviant activities.[22]

Once the base case has taken on what might be thought of as a normative character, it can be used to generate observations about the incongruities between it and any particular sub-type or instance. Underlying this process seems to be a set of rationalising principles which correspond rather closely to the logical stipulations Paul Grice[23] once worked out for conversation. Grice summarised his logic for conversation in two master maxims, "Be co-operative" and "Be economic". In providing accounts to be read, researchers write them so that they are organised around the simplest possible set of principles and so that the set of principles is recognisable as being drawn from some institutionalised body of knowledge. Where Grice proposes that speakers design their utterances to be understood and that this is a presumption of their hearers, we are suggesting that readers and writers do the same. The two maxims provide a rationalising logic by which the elaboration and overburdening of a complex classificatory system is prevented. Deviant, anomalous and awkward cases are brought within a single, unified classification by the demonstration of how their particular features are generated by local conditions. Incongruity with base cases provides a very effective means of gathering large numbers of phenomena and generating observations about them. If some item is odd, it is odd for a reason which will testify to the power of the overall classification. Incongruity procedures provide ways, then, by which what appear to be unnecessarily cumbersome or awkward phenomena can be brought within the scope of the generalised treatment.

(c) Transformation of Forms

In our discussion of colligation we suggested that one way of finding similarities between activities is to extricate them from their local settings and compare them almost in a vacuum. This extrication of the item from the clusters of irrelevancies in which it was first observed purifies the phenomenon and hence makes it more easily assembled within a pattern or collection of types. This purification, or transformation of form, enables the treatment of individual items as alternative versions of each other, a strategy which we have already remarked upon. The incongruity and colligational procedures just discussed allow the tracing out in the transformed item of the characteristic and definitional features of the type. In this manner large numbers of individual cases can be accommodated within one master scheme. Such a process can be seen at work in evolutionary typologies such as those of Marvin Harris[24] and Wallerstein,[25] where the different

89

elements are treated as locally produced variations on each other. It can also be seen in more limited schemes such as Goffman's classification of frames[26] and Gail Jefferson's types of "troubles talk".[27] In all of these cases, analysis consists in moving through the typology from one member of the classification to the next, playing out all of the possibilities and permutations. It is on the basis of transformation of forms that the essential practices of colligation and incongruity procedures rest.

(d) *Format Borrowing*

What we have been trying to get at in this paper is the elucidation of the ways that items of data are assembled, sorted and organised. We have suggested that analysis requires that although data is drawn from many different sources and settings, it has to display a unified and orderly structure. This implies that the whole corpus has to be subjected to processes of integration and segregation. Some items have to be interrelated; others have to be kept strictly separate. One kind of organisation that can be used to provide the structure that is required is simply to take over the format of the activity in question to organise the presentation of research in the final report. Political, social and economic changes can be reconstructed as historical narratives.[28] Trials,[29] consultations,[30] in-take interviews [31] can be depicted in terms of the episodes within the course of action itself. Accounts of the economic structures of some locality can follow through the stages in the production processes themselves.[32] The orderliness that the activities are described as possessing is derived from the sequence of episodes. Recent studies[33] of class-room interaction have used the temporal phasing of teaching activities to delineate the performative activities of getting and keeping control, focussing attention, imposing categorical schema and so forth as they are carried out during the teaching itself. Similarly, conversation analysis is predicated on the beginning to end format of conversations to generate topics such as greetings, 'preliminaries to preliminaries', first and second stories, closings and so on.[34]

The borrowing of the organisation of an activity to provide a structure for research has one major advantage. It enables the readily recognisable segmentation of the activity to be utilised for the phasing of the report itself. Using the techniques of colligation, incongruity procedures and transformation of form, such phases organise vast amounts of data in what the researcher hopes is a virtuoso display of the social organisation of the activities under investigation.

What we have been trying to do in this paper is give an initial indication of how sociological descriptions might be seen as displaying the methodical use of some institutionalised analytic procedures. Such methodic use accomplishes the shapeliness and the orderliness that the materials and data have within the report. Associated with this suggestion has been the proposal that such practices are part and parcel of the work that the researcher engages in to resolve the 'problematic possibilities' provided by data. Any set of sociological findings, any research report, any theoretical schema or any exposition of the literature can be investigated for the ways that it has resolved this matter and the procedures which it has used to assemble and display its structure. Since such procedures have as their aim the recognisability of their contents as *bona fide* sociology, or ethnography, or conversation analysis, or whatever, they can be treated as having been designed with their readers in mind and to make use of the resources which their readers will bring them. As such, these procedures can be treated as ensuring the collaborative accomplishment of sociology as a continuing and developing body of knowledge and practice. It is for this reason that we feel that the pursuit of investigations of the kind we have outlined is more likely to lay the foundations of an empirical

sociology of knowledge than many of those which currently seem to bear that title. Where such an approach differs is that it does not see itself as revelatory, debunking or relativising. Epistemological points are not being made, only organisational ones. In wishing to look at the organisational characteristics of studies in sociology, we do not wish to make any more or any less of them than those who engage in them would. Talking about sociological work in practical terms is not to deny its theoretical, cognitive or methodological basis. It is merely one way of looking at such work and does not entail any judgement upon its value. To treat sociological work as embodying practical reasoning is merely to propose that it displays organisational processes. It treats such sociological work as outcomes, as products, and nothing more. Finally, such an approach is essentially a reflexive one since the procedures we have outlined can also be discerned in our own account. If they couldn't, then our account would have failed as a sociological description of sociological practices.

NOTES

1. Recent discussions of Kuhn's original (1970) idea are Kuhn (1977), Laudan (1977) and Holton (1973).
2. See the discussion in Hesse (1974) and (1980) and the review of the topic in Pettit and MacDonald (1981).
3. Favret-Saada (1980).
4. E.g. Collins (ed.) (1981).
5. We would have to exempt the pioneering work of Michael Lynch (1977) and Garfinkel, Lynch and Livingston (1981) from this.
6. Kaplan (1973).
7. Cicourel (1964).
8. Despite its advanced age, Parsons (1968) remains the best exposition of Weber's methodology.
9. Lévi Strauss (1972).
10. Cf. Garfinkel (1967).
11. Cf. the Editorial "The Reliability of Suicide Statistics", *Suicide*, vol. 10, no. 2, 1980.
12. Skorupski (1976) is both a fair guide to the field and a reasonable defence of the intellectualist case.
13. Agar (1973).
14. Evans Pritchard (1951).
15. Goffman (1968).
16. For a different approach to this same topic cf. Sharrock & Anderson (1981).
17. Goffman (1972).
18. Goldman (1964).
19. McCarthy (1978).
20. Frank (1967).
21. Westergaard & Resler (1975) provide a general discussion of this.
22. Merton (1957).
23. Grice (1975).
24. Harris (1978).
25. Wallerstein (1974).
26. Goffman (1975).
27. Jefferson (1980).
28. The classic examples are the socio-historical accounts of revolution and other similar phenomena. Cf. Skocpol (1979) and Moore (1969).
29. Cf. Atkinson & Drew (1979).
30. Strong (1979).
31. Garfinkel (1967), Ch. 7.
32. E.g. the discussion of the bazaar economy in Geertz (1979).
33. Hammersley (1976), McDermott *et al.* (1978) and Sinclair & Coulthard (1974).
34. Schegloff (1980), Jefferson (1978) and Sacks & Schegloff (1973).

BIBLIOGRAPHY

Agar, M.
    1974   *Ripping and Running*, Seminar Press.

Atkinson, J.M. and P. Drew
    1979   *Order in Court*, Macmillan.

Cicourel, A.
    1964   *Method and Measurement*, Free Press.

Collins, H.M. (ed.)
    1981   "Knowledge and Controversy", *Social Studies in Science*, 11:1.

Evans Pritchard, E.E.
    1951   *Witchcraft, Oracles and Magic Among the Azande*, O.U.P.

Favret-Saada, J.
    1980   *Deadly Words*, C.U.P.

Frank, A.G.
    1967   *Capitalism and Underdevelopment in Latin America*, Monthly Review Press.

Freud, S.
    1975   *The Psychopathology of Everyday Life*, Penguin.

Garfinkel, H.
    1967   *Studies in Ethnomethodology*, Prentice Hall.

Garfinkel, H., M. Lynch and E. Livingston
    1981   "The Work of a Discovering Science Construed with Materials from the Optically
        Discovered Pulsar", *Philosophy of the Social Sciences*, 11:131-158.

Geertz, C.
    1974   "The Economic Structure of the Bazaar", in C. Geertz, H. Geertz and L. Rosen, *Meaning and
        Order in Moroccan Society*, C.U.P.

Goffman, E.
    1968   *Asylums*, Penguin.
    1971   *Presentation of Self in Everyday Life*, Penguin.
    1972   *Relations in Public*, Penguin.
    1975   *Frame Analysis*, Penguin.

Goldman, L.
    1964   *The Hidden God*, Routledge and Kegan Paul.

Grice, H.P.
    1975   "Logic and Conversation", in P.J. Cole and J. Morgan (eds.): *Syntax and Semantics*, vol. 3,
        Academic Press, 41-58.

Hammersley, M.
    1976   "The Mobilisation of Pupils' Attention", in *The Social Process of Education*, Routledge and
        Kegan Paul.

Harris, M.
    1978   *Cannibals and Kings*, Collins.

Hesse, M.
    1974   *The Structure of Scientific Inference*, Macmillan.
    1980   *Revolution and Reconstruction in the Philosophy of Science*, Harvester.

Holton, G.
    1973   *Thematic Origins of Scientific Thought*, Harvard University Press.

Jefferson, G.
    1978   "Sequential Aspects of Story Telling in Conversation", in J. Schenkein (ed.): *Studies in the
        Organisation of Conversational Interaction*, Academic Press, 219-248.
    1980   "On Trouble Premonitory Response to Inquiry", *Sociological Inquiry*, 50:153-185.

Kaplan, A.
    1973   *The Conduct of Inquiry*, Chandler.

Kuhn, T.
    1970   "The Structure of Scientific Revolutions", *International Encyclopaedia of Unified Science*,
        vol. 2, no. 2.
    1977   *The Essential Tension*, University of Chicago Press.

Laudan, L.
    1977   *Progress and its Problem*, Routledge and Kegan Paul.

Lévi-Strauss, C.
    1972   *The Savage Mind*, Weidenfeld & Nicholson.

Lynch, M.
    1977   Art and Artefact in Laboratory Science, unpub. Ph.D. dissertation, University of California,
        Los Angeles.

McDermott, R.P. et al.
    1978   "Criteria for Ethnographically Adequate Descriptions", *Semiotica*, 24:245-275.

McCarthy, T.
  1978 *Marx and Proletariat*, Greenwood.
Merton, R.
  1957 "Social Structure and Anomie", in his *Social Theory and Social Structure*, revised and
        enlarged edition, Free Press.
Moore, B. Jnr.,
  1969 *The Social Origins of Dictatorship and Democracy*, Penguin.
Nozick, R.
  1974 *Anarchy, State and Utopia*, Blackwell.
Parsons, T.
  1968 *The Structure of Social Action*, Free Press.
Pettit, P. and G. MacDonald
  1981 *Semantics and Social Science*, Routledge and Kegan Paul.
Sacks, H. and E. Schegloff
  1973 "Opening up Closings", *Semiotica*, 8:289-327.
Schegloff, E.
  1980 "Preliminaries to Preliminaries", *Sociological Inquiry*, 50:104-152.
Sharrock, W.W. and R.J. Anderson
  1981 "The Demise of the Native", *Human Studies*, 5:119-135.
Sinclair, J. and R. Coulthard
  1974 *Towards an Analysis of Discourse*, O.U.P.
Skocpol, T.
  1979 *States and Revolutions*, C.U.P.
Skorupski, J.
  1976 *Symbol and Theory*, C.U.P.
Strong, P.
  1979 *The Ceremonial Order of the Clinic*, Routledge and Kegan Paul.
Wallerstein, I.
  1974 *The Modern World System*, Academic Press.
Westergaard, J. and H. Resler
  1975 *Class in Capitalist Society*, Penguin.