The Economics of Enquiry

R.J.Anderson

    Manchester Polytechnic

and

W.W.Sharrock

    Manchester University

One of the many jibes that sociologists have often been taunted with is that they seem more willing to observe egalitarianism in principle than in practice. Whilst we might be inclined to agree with this allegation, we do so not because we feel (or rather not only because we feel) that some of our professional colleagues are just a little too concerned with pecking orders and hierarchies and where they stand in them. We do so from a suspicion that, in general, sociologists demand that quite a different order of treatment be accorded to their own activities than that which they are prepared to hand out to the people, organisations and events which they study. Sociological analysis is rarely (and even then only under very special circumstances) extended to our own work. Now, while this may be perfectly understandable, we find it just a little disconcerting. For us, the prospect of placing sociological activities on exactly the same footing as any other set of activities is an extremely attractive one. Indeed, it is one that we have been attempting to implement in a number of small projects. This paper will present some intial results from these studies. Before we get on to those, however, perhaps it might be just as well if we were to outline why it was that we came to take up this concern with the nature of sociological work in the first place.

Like everyone else here, we have an interest in studying the commonplace orderliness of ordinary activities. But, we found that no matter where in the literature we looked for guidelines on how to study this orderliness, the

research reports we read encouraged us to produce depictions
of social structures which were excessively business-like,
goal directed, purposeful, close-knit and coherent in
character. This view of social life we found difficult to
reconcile with the nature of the activities and events which
we encountered and observed when we were carrying out our
researches. Gradually we came to the conclusion that the
requirements which formal analysis imposes on research
practices are inimical to our kind of interest in commonplace
orderliness. To illustrate what we mean, let us give you
a couple of examples.

> (i) First, formal requirements demand that analysis,
> description and observation be, among other things,
> coherent, integrated, systematic, thorough, exhaustive,
> tidy and explicit. All too easily, these virtues can
> be transferred from the analysis itself to the phenomenon
> under study. Indeed, if the analysis wishes to sustain
> its aim of exhibiting the phenomenon's character, then
> this transference has to take place.

> (ii) Second, Although one might start out with an initial puzzle
> concerning the kinds of orderliness which participants
> may be able to detect in activities, organisations and
> settings, this can easily be supplanted by a concern
> with the extent and degree of orderliness which the
> analyst can find there. When this occurs, the analytic
> eneterprise becomes autonomous having detached itself
> from an interest in, or dependence on, participant-
> discoverable orderliness.

Once we were in a position to formulate our anxieties
and discontents in this way, we began to become interested
in how this work of transference and supplanting was carried
out in the conduct of sociological ..enquiries and analysis.
What exactly was the work of sociological research? Our
aim, then, became the study of sociological enquiry and
reporting as an ordinary,. everyday, practical matter.

One caveat is probably worth making at this point.
Although our work originated in an anxiety over the ways
that activities have been, and are being, portrayed in sociology,
our remarks should not be taken as being critical of the
organisation of research practices,either in general, or
in any particular cases that we might refer to. We do not
mean to deprecate, undervalue or ironicise these practices
any more than anyone else here would wish to undercut
the legitimacy of the practical activities which they study.
The fact that our comments may sound critical, and may even
be taken by some people as offering criticism of their work,
is in itself of some interest. It is a matter requiring
considerable thought why it is that describing courses of
action in a purely practical way often results in an
assymetry between what the analyst wants to say about those
activities and what the participants would have them amount
to. Why does a practical treatment of activities imply
a sceptical one? Is it necessary that it should? As we
say, we feel that this topic requires a great deal more
reflection than we could give it here. In the work which
we will report, we have tried to cope with this by adopting
a strategy which views activities as co-terminal with

their end-products. To paraphrase a remark of Gilbert Ryle's,
for us, participants only do what they do. And that is
not a tautology.

I

Previously the study of research activities has often
been envisaged as the examination of a supposed distinction
between a reconstructed logic and a logic-in-use. However,
we feel that this particular way of characterising matters
has been less than helpful, if not a hindrance. It has
usually be taken as ponting to, and placing emphasis on,
a distinction between how research is carried on "actually"
and how it is written up. The use of the distinction proposes
that there is a discrepancy between what went on and what
is said. Further, it seems to imply that what is necessary
is wholesale revelation of the "actuality behind the facade".
Utilising the distinction between logic-in-use and reconstructed
logic not only encourages a view that research is inevitably
shambolic, disorganised and out of control. It also suggests
that if researchers were to be honest they would have to
admit that they know that this is the case, and they would
have to own up to the mistakes they made, the important
matters which they disregarded and the problems they faced
and failed to solve. Once they were to do that, or so this
argument runs, then everyone would know what research was
really like.

We would like to demur from all this and take up the
issue in an entirely different way. We would like to treat

any reconstructed logic as providing an account, an instance, of a logic-in-use. That is to say, we would like to suggest that research reports do provide accounts of the work that went on; accounts that are rendered in methodologically respectable ways of speaking about it. We accept that there may very well be discrepancies between the activities carried out in the research and those that are reported, but there is no discrepancy between the kinds of reports that can be given. The research report, as a reconstruction of the activities of research, describes for the occasion of its presentation or production (that is, as a logic-in-use for formal presentation and analysis) exactly what the research came to and how it came to be like that. A purely practical description of these matters could not tell anything formally recognisable or interesting about the character or topic of the research.

We can elaborate on what we are trying to say here by taking as an illustrative example our own paper. We hope that what we have to say emerges as an unfolding, coherent set of points, thoughts and arguments which give rise to and follow from one another. We began with a candidate problem - our puzzlement and anxiety ✦ we shall offer some ideas of what solutions might be like; we might even come to some conclusions. But it will come as no surprise to any of you if you were to learn that our paper was not at all written like that. We wrote it under the pressure of some very practical constraints. We didn't begin it until after the arrival of an invitation we were no longer expecting; an earlier paper, which we had offered to give, had died on us and now seemed beyond resuscitation; because of personal

and teaching commitments we only had a limited number of
opportunities to meet. And yet, if one of us was going to
come to Konstanz, something had to be written. So, we met
one morning and decided roughly what we wanted to say and
went away individually to start on first drafts, parts of
drafts, lists of topics, connecting paragraphs and so on.
These bits and pieces were cannibalised into various versions.
Points were taken from one place and located in others. Often
we had points before we had anywhere in the argument to
make them. Almost the last thing we managed to find was a
way of begining, although, once we had that, we had ways of
writting the whole thing up. We were then able to slot
the parts into place in what we like to think is a unified
order with a step by step arrangement providing a measure of
coherence. For us to describe how the writing was really
done, what it amounted to in practice, would be of little
value to anyone, not even ourselves. All that such a
description could consist of would be schemas, notes, drafts
and outlines useless, perhaps even unintelligible, to anyone
other than ourselves. And yet, despite the discrepancy
between the shape or form of this paper and the way that
it was put together, there is no discrepancy between doing
the research and the writing up of the paper. That what
we said in the drafts and how we have said what we have said
here, are at variance does not mean that there is no continuity.
There have been many changes of mind, many changes of
direction and topic. Nonetheless, this version, this paper,
this report stands to all the other materials that we might
have used, as our way of saying here and now what we wanted to
say all along.

As our own example illustrates, the presentation of
any research report involves facing up to and coping with
a whole variety of contingencies and constraints. We would
like to suggest that any investigators, enquirers, analysts
or researchers (and that includes ourselves) when writing
up their reports, make use of a whole collection of routine
devices or practices in order to make the connections that
are necessary and to cement together a formal account. While
we can list now some of these devices we know that, long
as that list is, it is by no means exhaustive. The ones
that we have noticed we have called bodging, slanting,
judicious truthfulness, spurious unity, solution-before-puzzle
and generous rendition. In our view the giving of a formal
account of enquiries means using these devices to make whatever
the most of
was done, whatever is now available, so that research activities
and research findings can be made to sound as good as can
reasonably be expected.

In order to give you some clearer idea of how these
devices work, we will take each of them in turn and provide
some illustrations. We could have taken our examples from
any discipline - medicine, literary criticism or botany. -
but we felt it wiser to confine ourselves to fields with
which you will all be familiar. The cases we mention, then,
may prove a little contentious.

(i) bodging : involves taking some item, say a fragment
of data, a concept, a quotation and knowing that it
is not quite what it should be, or not quite what one
wants to say, or doesn't really fit in this style of
argument, but using it all the same for whatever job
has to be done pending a "more complete", "more exhaustive",

"more elaborate" treatment at some later stage. The
ways that sociological research statistics are cobbled
together provides the most easily recognisable (and
the most notorious)instances of bodging. Although
the samples are not random or consistently structured,
and the questionaires were not administered uniformly,
the computations and data arrays are all that can be had,
so they will have to do for now. Similarly, in the
absence of suitable data fragments, utterances may
have to be made up, lifted from elsewhere, re-used
in different places or transcriptions used that are
known to be unsatisfactory. By patching over the awkward
items, they can be made to work somehow.

(ii) <u>slanting</u> : what we are referring to here is the writing
up of some phenomena in terms of a theme to which,
on the surface at least, they do not seem_{in the least} amenable. It
does not appear to matter whether the topic be T.V
newscasts, the novels of Jane Austin or the love poetry
of John Donne, decision making in local government,
eating habits and forms of apparel in 16th century,
there appear to be some sociologists, who appear to be determined to play
'Button, Button, where is the capitalist class?'. In much
the same way Sacks' ability to find descriptions of
cars and teenagers relevant to a broad discussion of
psychiatry, revolution and cultural domination also
exemplifies slanting in the way that the materials
are worked around until they can be treated in terms
of the theme.

Colgate : 30°?/
~~~~~ filters.
Asela. since panels.

(iii) <u>judicious truthfulness</u> : we have to be very careful here.
In research reports we think this is to be found in
quite literal and truthful descriptions of what has
been found out, what has been seen, which leave the
reader to make his own, perhaps illegitimate, inferences.
Ethnographers, for example, are prone to pluralise
activities, feeling free, on the basis of the people
they have seen and talked to, to say what street life,
police decision-making or the cognitive hierarchies
in West African society are like. But this is no
different from the citation of standard forms, usual
cases as instanced in particular examples of utterances,
without showing how and why these particular items
can reasonably, clearly, obviously stand as representatives
for all those that cannot now be cited. By judicious
truthfulness we mean the leaving out of the account
exactly how representative this type is; how often
it occurs in preference to some other form; exactly
what observing police discretion or Kapelle Zo consisted
in.

(iv) <u>spurious unity</u> : this points to a tendency to present
activities which have all been done for very different
reasons as if they were done with a single purpose in
mind. It is in this way that activities came to be
collected together as types of a unified form and
thus as alternative versions of each other. It may
very well turn out that the provision of a spurious
unity is characteristic of sociological analysis.
Certainly it seems to have reached its most advanced form

with the ecological anthropology of Marvin Harris.

In Conversation Analysis, its effects are to be seen
the reports   on closings, identifications, laughing,
formulations, repairs etc etc.

(iv) solution-before-puzzle :   once a collection of data,
activities, fragments·or whatever has been compiled,
they are related by finding the puzzle or problem to
which they can stand as the solution. Once the puzzle
has been designated then the collection can be presented
as if it had been collected with just this kind of a
solution to this puzzle in mind. By heaping together
instances of references to persons, terminal utterances,
location and type of excuse given in court, it becomes
possible to find the problem which they can then be
treated as having been designed to solve.   But, perhaps
the classic example of working up the solution before
the problem became available is Marx's definition of
the revolutionary nature of the proletariat. The puzzle
to which that is the solution (namely the inherent
contradictions in capitalism) could not be specified
until some twenty years after the solution's discovery.

(v) generous rendition : finally, this device consists in
the depiction in abstract or procedural terms things
that were done for happenstantial, biographical, practical
or,at best,only marginal-to-the-project reasons. Generous
rendition results in a retrospective orderliness in
research activities by which they are given a begining-to-
end structure with the rational for what comes where
being
is a formal one and not the procedural ordering in which

the research was done. For example, as a summary of
work up to the time of its publication, the _Simplest
Systematics_ seems to have turned the whole order around.
The work that was done first is presented last as
illustrations and exemplifications of the principles
which it pre-dated. The _Structure of Social Action_
provides a revision of an intellectual biography on
an even grander scale.

We want to emphasise that we are not suggesting that,
by employing these devices, the researchers in question
packaged up their research reports in order to deceive their
colleagues, their sponsoring institutions or the public
at large. (Although, of course, they might very well provide
a way of doing just that.) Rather, we want to suggest that
these devices provide ways of coping with some of results of
all the normal constraints that force researchers to budget
their time and efforts. They provide means by which what
was actually done, what can be made use of now, what is
available in the corpus of materials, can be made use, most
profitable and economically. Using them, researchers try
to make as much as is possible out of what is to hand; they
make what is of little or no value, of general interest; they
can show as inevitable what is yet to be provided for, argued
for and demonstrated. In short, these devices provide
routine ways of circumventing cumbersome and time-consuming
procedures while at the same time preparing defences against
criticism. They are, then, ways of licking materials into
shape.

At the begining we said that in order to pursue our
interest in the practice of research, we have undertaken
some small projects designed to illuminate different features
of research work.   In the rest of this paper we will look
at just two elements from these projects.   We have already
discussed some fairly broad devices that are available. In
the next section we would like to go on to discuss some of
the ways that devices such as these can be used to enhance
the orderliness of activities and thereby compensate for
the opportunity costs involved in making any enquiries.   In
the final section we will review some of the ways in which
we encountered these costs while we were trying to make
situated enquiries via the use of video film.

                              II

We have already indicated that analysis often consists
in the bringing out of an orderliness which activities have
but which can only be discerned here and there in any
particular set of materials being examined.   The corpus
of materials both exhibits orderliness and obscures it.
Hence analytic treatment is neccessary so that the underlying
and pervasive order can be made visible in each and every
part.   We think of this orogenic treatment of materials
as enhancing their orderliness.   In looking at the ways
that this enhancement is brought about we have noticed a
number of techniques,(colligation, incongruity procedures,
transformation of forms and format borrowing) which we will
discuss somewhat briefly.

(i) colligation

Quite recently, when we were involved in a discussion
of fieldwork procedures, and in particular participant
observation, with a number of colleagues, both anthropologists
and sociologists, we were amazed at the tendency to overlook
the fact that, no matter what kinds of problems could be
raised in theory and in practice about sociological descriptions,
there are some people, namely researchers, who are engaged
in finding working solutions to just those problems. Their solutions
are embodied in their field-notes, in the descriptions which
they give of the settings they are studying. In general,
these descriptions have a character we have termed "one
damn thing after another". First one thing is observed;
then somebody does something else; then something else again
happens; and so on and so on and so on. As a result, the
flow of events as displayed in the field notes has a haphazard
character. And yet, naturally enough, this contingency,
this one damn-thing-after-another character is missing
from the final report.

One of the ways that this haphazardness is processed out
of the report is through the dissociation of data or
materials from the ways in which they were assembled. The
materials form a corpus primarily because they _are_ what
the researcher has got. They are all his fieldnotes: they
comprise his body of data. That he possesses them gives
them their unity in the first place. Often they have been
collected in a variety of ways (following one person
around, staying put in the same setting, mixing with different
members of the same group, bugging your own phone, letting

the tape run) and depend on a variety of circumstances

(who you could make friends with, whose archives you could

get access to, who would lend you tapes and transcripts).

There is, therefore, a massively contingent aspect to them,

usually only recognised in the acknowledgements. The materials

consist of whatever happened while the researcher was there;

whatever could be transcribed; whatever was made available.

This contingency of assembly is disregarded in the analysis

in favour of a treatment of the corpus as expressing some

kind of putative formal organisation. Within a formal

analysis the data is broken down and reconstituted into

numerous different data runs. The specific historical

circumstances of assembly are dismissed as irrelevant since,

potentially at least, they may threaten the possibility of

the detection of the underlying formal organisation. 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

and connections can be made.

Colligation, then, may be thought of as involving three

distinct steps. First it is neccessary to acquire a sufficient

familiarity with the corpus to allow search procedures to

generate puzzles, obscurities and themes and to allow

decisions to be made as to what will be of interest to look

at and what it is safe to ignore. Second a more disciplined

inspection of the corpus must be instituted to select out

like instances of fragments and excerpts of data which can

be related to the theme. When this loosely related sub-corpus

has been compiled, finally, it can be recombined in analysis

by juxtaposing items in order to make observable matters
which were invisible when the fragments were taken in
isolation. In this way,relationships are both specified
and demonstrated.

(ii) incongruity procedures

Yet another longstanding observation about sociology
concerns the trouble that it has constructing logically
equivalent classes of phenomena with which to build its
formal theories. And yet, despite this, we sociologists
seem to be dab hands at constructing practical equivalence
classes since a great deal of our analysis depends on the
use of types and typing. But the ways we put together
types, however, is rarely the product of a complete survey
of the materials which are to be classified. Much more
often, the types are assembled early on in the process of
classification,with the inevitable result of classificatory
inertia. Where phenomena are turned up which do not fit
neatly within the classificatory schema, it becomes neccessary
to stretch the classes so that the anomalies can be brought
within them. What seems to happen is that the criteria
for demarcating category membership are taken as referring
only to 'base-cases', that is,those instances of the class
or category which can be readily and unambiguously recognised.
All other instances are then related to the base-case as,
perhaps, vestigial or differentiated forms; their features
having been displaced or disrupted through contextual
modification. In this way, these anomalous instances can
be treated as variations of the base case under particular

circumstances, the general type having been elongated to encompass the alternative warpings and sub-types.

Once the base-case has taken on this normative character, it can be used to generate observations about the incongruities between it and any particular instance. Accounts of the incongruities can be provided by the spelling out of some contextually located reasons for the production of the deviant case. Underlying the ways in which these observations can be made seems to be a set of principles which correspond rather closely to the set of maxims which Paul Grice once postulated as describing the logic of conversationalists. In summary, Grice suggested that conversationalists attempt to be as relevant, truthful, informative, brief and simple as is possible. In doing this, he argues, they produce the two characteristic features of conversation, namely that it is co-operative and economic. Rather than these maxims simply referring to the design features of utterance production, we are suggesting that they describe the rationalising procedures that analysts engage in to find all occurences to be co-operative and economic. If some instance deviates from the base-case, then it is more cumbersome and awkward to handle than it could have been. The analyst can then proceed to look for reasons why this more complex form is, in fact, minimally efficient for the particular occasion on which it is used. This can be done in many ways, by reviewing participant motivations, knowledge in common, or possible perlocutionary activities. Incongruity with the base-case, therefore, provides a very effective means for generating phenomena for observation and

explication. If some instance is deviant then it is deviant
for a reason. The reasons provide ways of elaborating
motives and organisation as well as ways of generating
puzzles and assessing their solution. Incongruity procedures
provide a mechanism, then, where what appear to be unnecessarily
uneconomic or cumbersome examples of some phenomenon can
be shown to have been very precisely designed for the
context in which they are to be found.

(iii) transformation of forms

In our discussion of colligation we suggested that
one of the ways of finding similarity between types of
activity is to extricate t em from their local setting and
proceed to compare them almost in a vacuum. Through this
extrication from the clusters of irrelevancies in which
they are to be found, the objects become purified and, hence,
more easily assembled in patterns, types and classes. By
this refining or transformation of form, it becomes possible
to treat individual items as alternative versions of each
other. Each is an instance of a particular member of the
set of types. So, one is an embryonic form, another a mutant
form, another a degenerate form, a compressed form, and
so on. The characterising features that define membership
of the type and designate the base-case are thereby
traceable out as vestigial, overdeveloped, compacted, projected
but not realised, etc etc. In this manner huge numbers
of examples can be organised and laid out in as economical
a manner as is possible. Analysis consists in a step
by step working through from one member of the type to
the next, as all the logical possibilities are played out.
Transformation of form, the purification of data of its

awkward and unique features is the basis on which colligation
and incongruity procedures can take place.

(iv) format borrowing

What we have been talking about in this section are
ways that items of data can be assembled, sorted and
organised. Analysis requires that although data be drawn
from many different sources and settings, it has to display
an orderly structure. This implies that the corpus must
undergo process of integration and segregation. Some items
must be interrelated; others must be kept strictly apart.
One kind of organisation that can be used to provide criteria for
segregation and integration simply by taking over the format
of the activities under study to structure research, analysis
and reports. Thus trials, intake procedures, consultations
and other similar organisationally defined and situated
courses of action can be depicted systematically by adopting
the stages or episodes of the course of action itself.
Reports of legal proceedings may be segmented into pre-trial,
trial and post-trial episodes, with each of these further
sub-divided into individual phases. The orderliness of
the activities can be displayed in the way that the sequence
of episodes is managed; how the trial is begun; how the
consultation is moved from first things to readily recognisable
medical topics, and so on. In the same way, the study of
conversation can be built around its begining to end structure,
thereby, aggregating many levels of observation and analysis.
Work can be directed to beginings, to first topics, to
topic shifts or closings. One can also deal with the preferent-
-ial organisation of identification in first turn, second

turn, the sequential location of particular token types
or activity types, the organisation of story-telling, first
stories, second stories, and so on and so on. Borrowing
the orga^nisation of the activity to provide a structure for
research has one very major and distinct advantage. It
enables a readily recognisable segmentation of the activity
for the production of independent and self-contained reports.
Using the techniques of incongruity procedures, colligation
and transformation of forms, these reports are able to
collect together myriads of examples from different situations
and ~~examples~~ settings and present them in virtuoso displays of aspects
of phases in an activities proceedings.

III

Otto Neurath it was, wasn't it?, who suggested that
the ~~practice~~ doing of science was a bit like building a ship,
plank by plank, while on the high seas. We rather like
this image but suspect that in sociology's case the craft
would have to be Noah's Ark, and the animals would all be
in residence. In this concluding section we want to take
a look at this waterborne carpentry to see what it entails.
As our thoughts about these things are the least worked
out of the ideas that we have presented so far, we will
offer them as first formulations only. Our theme has been
the ways in which practical matters are turned, in sociological
discussions, into methodological ones and hence how research
activities appear in our reports as over-structured, over-
procedural and over-orderly. What we want to address now

are those delicate matters of what to do first and what
to do next; where to get started; where to go and whom to
approach; those known-commonly-but-rarely-referred-to-in
public-and-*certainly not*-on-occasions-such-as-this matters about which
we go fishing for advice from our friends, colleagues and
mentors, and about which we lay down the law to our students.
~~At least~~ In the first part we will try to use our own
experiences of video-taping as a means of illustration.

We see this topic as ~~the~~ ways that we make, and make
something of, enquiries. We begin, much as we did earlier,
from the position that no matter what the practical or
ethical problems that can be raised about the "observer's
role", some people, ie researchers, have to live with those
problems by finding working solutions to them. The data
which they collect and subsequently analyse stands as evidence
of the solutions which they find, the ways that they cope
with and overcome the problems of data assembly. Although
we are mainly concerned here with active field-work, we
do not think that *using* data archives or banks, computerised
data retrieval or whatever, necessarily absolves one from
involvement in these matters. Utilising such data sources is
just another working solution, one which entails making
some sacrifices for advantages that are deemed preferable,
incurring costs that are felt to be worth the price. The
rules of thumb that ~~xxxxxxx~~ guide research strategies and
practices seem to us to be summarisable in a double question.
'If I do that, will it make any difference? Is it worth
doing then?' *Answer to* This double question provides, for researchers,
ways of fulfilling standard pieces of advice while at the

same time assembling their data. The pieces of advice that
are given, might be put as: do what you can; surrender the
initiative; stay on the periphery; don't get involved.
For us, research work consists in trying to follow these
maxims when all the practical exigencies militate against
them. Let us take them one by one to show what we mean.

(i)   do what you can : it isn't all that remarkable that
choices of topic and method display a constrained opportunism
that is altogether missing from the ways that they are
usually talked about. Research reports generally present
such choices as essentially theoretically and methodologically
determined. Theoretical developments have been made,
particular criticisms proposed, findings built up which
can be extended, or re-worked, or tied into congruent findings
in adjacent disciplines. In our reports, theory and method-
-ology are reconstituted as heuristics laying out potentially
interesting areas for research. This way of talking about
research choices purifies them of the biographical and
circumstantial nature that ours always seem to have. Like
lots of other people;we chose to video-tape just to see
what could be done with it; we chose a museum because there
was one close and they allowed us in; we had a friend in
a health education project who gave us access to amenable
doctors and clinics. Although we could talk about the
project in theoretically justifiable terms, it was not
begun in that way. Given the practical and pragmatic
considerations that we didn't want to do anything else,
we had some money so we had to spend it, one of us had
a sabbatical and hence some time to spare, then all in all

this seemed the best thing to do.

(ii) <u>surrender the intiative</u> :   ethnographers often make

the point, nearly always plaintively, that they are powerless

to begin until they have been alloted a place in the society.

Begining to find out who's who, starting on the kinship

chart, or discovering what's done around here, is, then, not

an active procedure but a passive one.   Researchers seem

to want to enter the scene by a strategy of minimal

initiative.   The net result of this casts the researcher-

subject relation in terms that are more properly described

as a guest-host one.   As a guests, the researcher has all the

usual obligations of not intruding too much, fitting in

with the hosts wishes and timetables, reacting to the hosts, and

doing as they do.   Such a research strategy consists in

tagging along behind, and finding out how the setting or

organisation makes itself available and is utilised, comprises

looking at whatever they happen to do under whatever

circumstances and in whatever settings you happen to find them.

The materials assembled on the setting's organisation

consist of just those things that occured.   In a very strong

sense this strategy constrains the data that can be made

available. And Analysis can only respond to whatever features

the corpus happens to display.   By surrendering the intiative,

choices of collection strategy (eg whether to follow people

around as they wander past cases or disappear from sight

to re-appear a little later on, or whether to sit in one

place and so have lots of instances of 'the same' activity)

are determined by the feasibility of capturing what they

do.   What can be brought out in analysis is determined

by what could be preserved in the data. This is not to

say that there ought in principle to be ways of filling

it out, of providing more. It is to say that, what there is is all

that is ever going to be given.

(iii) <u>stay on the periphery</u> : closely connected to surrendering

the intiative is the advice to stay on the periphery.

Observers are told to make sure they can find a place where

they can see what is going on without intruding. Two sets

of considerations arise from this. First of all, finding

what's going on depends on the ways that the organisation

makes itself available for public use. So the doctor's

surgery looked ,at first, as if it was custom-made for video.

Sitting in the corner we had the doctor and mother together

in camera most of the time. Unfortunately whenever they

stood up to examine a child it meant they turned their backs

to the camera, so we could not see what was going on. A

situation made much worse by the requirements of holding struggling

infants still. The museum presented its own kinds of

difficulties. As it is an environment constructed for the

public to roam about in, tracking through a course of action,

the trajectory of a visit from begining to end, meant most

humping the camera and recorder around behind groups, trying

to keep up with them as they dissolved and reformed, selecting

which individuals to follow as they disappeared behind

cases and into alcoves, only to rejoin the group further on.

These problems of trapping the course of action in its

course had all to be reconciled with, requirements such

the quality of light, the possibility of picking up sound,

the ability to leave the machinery to run without the kids

wrecking it. In the museum example, all this coincided
in the fact that we have no data at all on the two most
popular exhibits, in fact the two places where most people
went to immediately, namely the collection of mummies and
that of snakes, alligators and other reptiles. The light
simply wasn't good enough. So, what Manchester Museum
means for most people, is we give there missing from our data.
Staying on the periphery requires that the researcher find
somewhere to film, select someone to follow, stay close to them,
but not too close, in short find a solution and pay the
price of all the budgetary constraints on effort and impact.

(iv) <u>don't get involved</u> : not getting involved is, of
course an extension of the work of surrendering the intiative
and staying on the periphery. Being on the scene, but not
in it, requires the researcher to cope with the possibility
of his own incorporation into the action. In the museum
and in the clinics this incorporation took two forms. Adults
treated us with careful, almost studious disattention. The
camera was the one thing they would not look at, would not
talk about. Children, of course swarmed all over us, demanding
to be on the film and giving us headaches about the safety
of the machinery and whether they would get electrocuted.
Not getting involved meant having to cope with the fact
that our data seemed to consist of people taking too little
or too much interest in us. Our engagement in the scene,
and hence our need to deal with that, is best exemplified
in the case of the doctor's surgery. Because consultations
might be lengthy, it was necessary to have someone beside
the recorder to change tapes etc. This, coupled with

the usual medical ethics, meant that the parent's permission

had to be obtained before the [consultation] could start. Explaining

who we were and what we were doing there became the

doctor's way of getting the consultation started. By a

neat inversion, we became a resource for him to do his

medical activities.

Space.

Given that research work involves the mundane considerations

of the kind that we have been sketching and given that

these disappear from our research reports, being replaced

by the formalisations we discussed earlier, how do we generate

the ideas we have about data that make them amenable to

formal analysis? Essentially, we see this work as a public

and co-operative one although it is more usually presented

as if it were the result of some Cartesian exercise in

pure enquiry. We would like to end by pointing to a couple

of the devices that we [think are] used to rake over the bits and pieces,

run through the field-notes and fragments to find something

in the data. We have given these two the provisional titles

of 'pooling and stirring' and 'brainstorming'.

Inevitably, we have already discussed facets of pooling

and stirring when we were talking about the activity of

enhancing orderliness. We see pooling as the bringing together

lists of similar examples, counter-examples and variations,

finding similar cases in other reports, getting [and giving] access to

data banks and archives, working papers and drafts. Stirring

is the process of sorting through, banking up, [comparing,] running and

re-running until something [analysable], some features emerge. The activity

of pooling and stirring clearly depend on [considerations as to] who is approachable,

who is willing to give help, advice and directives; who
will allow their work to be cited, the~~i~~ data used to support
alternative analyses, which, in turn, depends on the availability
of methods of comparison, common procedures and protocols.

We have borrowed the term brainstorming from the
application of decision theory to the study of organisations.
We use it to refer to attendance at seminars and the reading
of papers at gatherings such as this; sessions over beer
or doffee in houses, offices or pubs; huddles around
print-outs, transcripts or T.V. monitors. All of these
consist in the use of others to trigger ideas, in a mulling
over, rooting through, turning upside down and starting
again, in a pillaging of other people's minds as a way of
getting started. Brainstorming is carried out under the
need to get something out of the data, to work something
out in the time available, to find something to say in
a paper, ~~some way of~~ meet~~ing~~ a deadline, or keep~~ing~~ the
faculty promotions-board happy.

We have gone on ~~far too long~~, and we have time now for
only a hurried conclusion, a very brief reprise of all the
stones that we've turned over in this paper. If we were
to be asked to say in so many words what we are after, we
think that we would ~~say~~ that our interest is in the politics
of research, in the ways that the institutionalisation of
research reporting has played down the importance of the
practical, the necessity to deal with the contingencies
of ~~of~~ who controls what, who will give you access to what,
who is in authority with favours to hand out, who has to
be satisfied and what you have to do to satisfy them

It seems to us that any sociological account of the
orderliness that the work of sociological enquiries ~~which~~ produces which
failed to pay attention to these matters would rob ~~them~~ it.
of the circumstantial, pragmatic, competitive, personalised,
worked-out-as-it-goes-along, in sum practical, orderliness
that research seem ~~to us~~ to have. Or, at least ~~it~~ ours does
when ever we try doing it.