# DISCOVERY: LOGIC AND SEQUENCE IN A SCIENTIFIC TEXT (1)

STEVE WOOLGAR

*Brunel University and McGill University*

The work of this paper is part of a more general exploration of the viability of an ethnomethodological approach to understanding science. I have argued elsewhere that ethnomethodologically inclined approaches to the study of science should provide useful insights into the nature of the activities which constitute scientific practice. In particular, an examination of some of the standard objections to ethnomethodology shows them to be misguided, in that they are based on an overly narrow conception of the range of phenomena with which ethnomethodology can deal and on an intractable definition of the legitimate substance of sociological inquiry (2). However, a full assessment of the value of ethnomethodology for understanding science can only be made in relation to the fruits of empirical inquiry (3). The argument of this paper is divided into two parts. In the first part, an analytic perspective informed by ethnomethodological thinking is located in relation to the debate between rationalist philosophy and the strong programme in the sociology of knowledge. This debate is taken as the basis for an examination and clarification of the position that a fruitful understanding of the nature of scientific inquiry must eschew many of the features of both philosophical and sociological perspectives on knowledge production. An attempt is also made to explicate the central assumption of the analytic perspective: that of isomorphism between presentational context and scientific concepts. The second part of this paper offers a tentative analysis of one aspect of the practical activity of scientific investigation: the accomplishment of a discovery as related in a scientific text.

## 1. Rationality and the Strong Programme in the Sociology of Scientific Knowledge

Although there is now widespread agreement that it is both possible and desirable to analyse the social processes of scientific investigation, this general formulation conceals a number of different positions as to the precise form which adequate analysis should take. Even Merton's early work could be said in one sense to have concerned social processes of scientific investigation, although current writers would probably argue that this is stretching the applicability of the phrase to bursting point. This is not what they had in mind at all! A more exact version of what is required depends upon one of the more celebrated claims of recent work in the sociology of science, that analysts should look more carefully at the cognitive content or substance of scientist's work (4). In other words, a thoroughgoing sociology of scientific knowledge must concern itself, for example, with the way in which social interests affect the construction and use of the minutiae of esoteric mathematical proofs, or with the way in which particular knowledge constructs constrain scientific action. However, much current controversy hinges on the extent to which precepts about the truth or falsity of the knowledge under study should enter into analysts' explanation. Parties to this controversy can be denoted as the 'rationalists' on the one hand, and the supporters of the 'strong programme' on the other (5). In order to contrast the philosophical and sociological perspectives of these parties with the kind of analysis I shall be recommending, it is necessary to give a brief exposition of the positions of 'rationalist' and 'strong programmer'.

The rationalists can be characterised as a body of thinkers strongly influenced by traditional philosophical commitments to epistemological realism. That is, they are committed to the view that there exists a world of material things which do not depend for their existence on the fact that some mind is aware of them. Translated into more sociologically familiar terms, this means that the truth or otherwise of the matter is independent of the presence of social actors. In Popper, we find knowledge products preserved in the social equivalent of a hermetically sealed domain: the third world (6). Knowledge thus constituted is independent of the knowing subject. Bhaskar suggests that the removal of mankind from the face of the earth will not affect the existence of physical laws. Without man, he suggests, there would continue

to be tides: gravity would continue to operate (7). None of the rationalists go so far as to argue that the presence of social actors is entirely irrelevant. After all, knowledge objects are undeniably used and manipulated by human agents. The rationalists are thus prepared to permit sociological investigations of science which confine themselves to relationships between knowledge handlers. Rationalists' eyebrows are raised when it is claimed that such social relationships determine the form and content of truth. And there are howls of anguish when it is further claimed that an understanding of the social determination of knowledge should proceed independently of the truth or falsity of the knowledge in question.

The perpetrators of this last outrage are, of course, the strong programmers. One of their central methodological imperatives is that analysts should not be swayed by the truth status of the knowledge under consideration: Barnes urges us to be naturalistic (by which I shall here take him to mean 'non-evaluative') (8); Bloor asks that we be impartial (9). This would be uncontroversial but for the insistence of the strong programme that sociologists concern themselves with the substance of scientific knowledge. Since the substance of scientific knowledge is traditionally the domain of philosophers of science, who can be characterised as attempting to expose the internal logic of scientific arguments, it is perhaps not surprising that the intrusion into this territory by non-philosophers causes consternation. If these non-philosophers wield a methodological armory which explicitly denies the relevance for their explanations of attempts to specify the rationality or truth status of knowledge, consternation is likely to be redoubled.

An exhaustive examination of the range of different positions adopted in this debate is well beyond the scope of the present paper. For present purposes, the relevant facet of the rationalist critique of the strong programme is the assertion that any attempt to conduct investigations into the nature of knowledge from a truth neutral standpoint is *absurd*. This absurdity appears to hinge on the fact that such a perspective starts by ruling out certain questions which are relevant from the point of view of the rationalist philosopher: rationalist philosophical study of knowledge requires *ab initio* some specification of truth content. In any case, it might be said by rationalist philosophy, it is simply *impossible* to maintain a truth neutral standpoint: assumptions about the veracity of objects in the real world will inevitably be smuggled into the analysis, if only because we adopt a realist standpoint in the course

of confrontation with the 'objects' of our world. The strong programmers would presumably reply that this does not invalidate attempts to push analysis as far as possible from a relativistic viewpoint; that the truth status of knowledge is not denied by the strong programme, it is merely suspended for research purposes. Yet there is one aspect of the rationalist argument which is not satisfied by this reply. As both analysts and members we are inevitably tied to a structure of discourse which includes the use of modalities, the achievement of objectivity, the assessment of what really is the case and so on. To the extent that we are constrained in our use of available language resources, we will inevitably reproduce the rhetoric of realism. To me, this suggests the importance of a venture which attempts critically to examine the nature of this rhetoric.

This dispute between rationalist and strong programmer hinges on the fact that while both are committed to understanding the nature of scientific knowledge, the former approach presupposes a style and form of adequate explanation which is qualitatively different from the latter. Put another way, the debate between the rationalists and the strong programme is a debate over the nature of adequate explanation. So long as explanation depends on assuming truth status, there is no room for the strong programme; so long as questions of truth status are ruled out of court by sociological elucidation of the strong programme, there is no place for the rationalist (10). It is, of course, possible to imagine that at some future date, this dispute might be said to be 'resolved': from the point of view of parties to the debate, it might be apparent that the argument of one or other side 'had prevailed'. As a practical matter, the issue would thus be reckoned to be closed. In principle, however, arguments of the kind outlined will always be available to potential protagonists. It is therefore important to distinguish between the 'resolvability' of the debate as a practice accomplishment and the essential 'irresolvability' of the debate in principle.

Having established a tension between competing parties to a controversy, the stage is set for the introduction of the author's resolution (11). However, this author's attempt to resolve the tension is not an attempt to resolve the argument between rationalist and strong programmer. In other words, my interest is not the practical task of establishing the ascendancy of one or other position. Instead, I shall try to alleviate tension by suggesting that resolution of the debate, or even alignment to one or other position is of little

consequence for a fruitful understanding of scientists' activity. The discussion requires refocussing so that arguments between strong programmers and rationalists are subject to critical appreciation. From this vantage point it is possible to see a congruence between this dispute and those which constitute scientific practice. The analyst's task is not to resolve such disputes, but rather to develop an appreciation of their form and currency.

The argument can be expressed differently as follows. Science is an essentially practical activity, in the course of which participants themselves engage in negotiating the character of the phenomenon with which they are dealing. That is, scientists can be seen to be routinely concerned with questions about what exactly the thing is that they have got, whether or not they *really* have got something, whether or not that thing would make its presence known under different circumstances, and so on. The activity of science thus comprises a practical concern with issues which we meet in slightly different form in the discussions of professional philosophers and sociologists. The similarity between the practical concerns of working scientists and proponents in the debate between rationality and the strong programme in the sociology of knowledge is particularly striking. As has been argued elsewhere at length the practice of everyday laboratory work comprises continual disputes, strategies and arguments at a local level (12). The occurrence of these actions is emphatically not limited to occasions of 'controversy' in the usual sense of the term. Instead, participants display a constant and routine concern to settle or deal with matters which, in the universe of discourse associated with professional academia would be characterised as being of profound epistemological significance. Laboratory practice thus involves participants' attempts, on the one hand, to establish the reality of truth of a phenomenon which they claim their actions to be about or related towards, and on the other hand, to specify a variety of sociological circumstances or factors which 'explain' the scientific work with which they are familiar. I see in these activities of laboratory scientists a microcosm of the debate between rationalists and the strong programmers described above: in an important sense, practising scientists variously and interchangeably take the position of rationalist or of strong programmer (13). The major difference, however, is that the adoption of one or other argument is not merely a matter of academic indulgence. Rather, the position taken appears to have immediate and significant practical consequences: "If the existence of *this* particular substance can be established

(as a practical matter), it is then possible to pursue *this* strategy for further work". Or, alternatively: "If we can demonstrate the dubious social circumstances in which *this* claim was made, it is possible to discount it for purposes of our next experiment".

The wide range of different forms of explanation in which scientists engage includes both the establishment of truth status of items of knowledge and the identification of extant social factors or circumstances. In the latter regard, their efforts to characterise the behaviour and knowledge claims, both of themselves and others, variously follow the form of structural, functional and hermeneutic explanations familiar to social scientists. This also means that participants are rabid imputers of social motives and interests. They find evidence of social interests at work in a diverse collection of instances of scientific practice. Moreover, it is clear that the work of imputing social interests is recogniseably a defeasible activity for participants. That is, they frequently pay attention to (and attempt to forestall) the ever-present possibility of explanations which propose an alternative set of motives, intentions, socio-political world views, attenuating circumstances or prevalent cosmologies as the primary influence on knowledge construction (14).

Thus far, I have suggested that the debate between rationalist and strong programmer is irresolvable except for practical purposes, that scientific practitioners engage in reasoning practices similar to those of parties to the debate, and that scientists are mindful of the essential inconcludeability of their reasoning practices. These observations together suggest the importance of examining the nature of the practical reasoning engaged in by scientists. Instead of contributing directly to the debate between rationalist and strong programmer, it may be more fruitful to investigate the practical management of either or both positions (or of the debate between these positions) as found in the activities of scientists (15). For sociologists, in particular, I am recommending that instead of adopting the perspective of the strong programme, we recognise that modes of reasoning essentially similar to those constituting the strong programme — including activities such as the attribution of motives or interests, the specification of social context, the search for causal influence and so on — both characterise and have significant consequences for scientific work. Thus, the practical reasoning activities of scientists provide an alternative focus for our attempts to further our understanding of the 'process of scientific investigation' (16).

Scientists' practical reasoning comprises a wide range of activities amenable to investigation; the analyst can profitably turn to the use of evidence, interpretation of data, argument and so on. He can find these activities in the actions, conversations, seminar discussions, conference presentations, inscriptions, recordings and writings of scientific work. For purposes of illustration I have arbitrarily chosen to concentrate in this paper on the last of these activities – the writing of scientists. More specifically, I shall examine participants' practical management of the notion of 'discovery' by considering the character of discovery as made available in a scientific text. Thus, the particular aspect of scientific practice studied here is the work of accounting through textual organisation. Before proceeding with the analysis, however, it is necessary to review the basis for assuming the availability of 'discovery' in a text.

## Isomorphism

Collins' study of replication in physics makes a persuasive case for the need to look at scientists' practical actions in fine detail (17). Collins argues that in negotiating what counts as an adequate (or competent) replication of an experiment, participants in the field of gravity waves were deciding upon the list of culturally relevant variables. In other words, the substance of negotiation concerned which factors should or should not be taken as affecting the detection of the phenomenon under consideration. Collins points out that in so delineating the relevance of candidate variables, participants were engaged in specifying the nature of the phenomenon. That is, there is an essential congruence between negotiating what counts as a good experiment and negotiating the character of the phenomenon. For present purposes the argument can be expressed rather differently: both the existence and character of a particular phenomenon can be established through the negotiation of interested parties; the out-there-ness of a phenomenon is accomplished in establishing its properties (18).

Collins argues that in order to recognise the phenomenon as the upshot of social construction, it is important to monitor contemporaneously the process of negotiation. For sociologists, this is most obvious in terms of controversy, in the aftermath of which, the social contingency of fact construction is said to be difficult to retrieve. It is at this point that facts appear to take on a life

of their own, passing into a realm of existence beyond the immediate research of human agency and managing to disassociate themselves from all traces of their production (19). In another sense, however, the work of establishing the facticity of a phenomenon does not end with the settling of controversy. On each and every occasion that participants refer to a fact they do so in such a way that the facticity of the phenomenon is re-established. In referring to a thing, work is done to sustain its fact-like nature, or to say it differently, to pass off the thing as being a fact. It is precisely this work, this continual reaccomplishment of a phenomenon as a fact, which makes the task of the sociologist so difficult. For it is this work, this continual re-expression of facticity, which hinders sociological reconstruction.

My point is that is possible to extend Collins' argument. For Collins, the negotiation of what counts as a good experiment is the same as negotiating the actual character of a phenomenon. The present suggestion is that the practical expression of, or reference to, a phenomenon both recreates and establishes anew the existence of the phenomenon. In describing a phenomenon, participants simultaneously render its out-there-ness (20).

The issue of congruence between practical expression and the nature of a phenomenon arises in an article by Dorothy Smith (21), in which she examines the text of an interview purporting to describe how a girl came to be defined by her friends as mentally ill. The central assumption of Smith's analysis is that there is an essential congruence between the notion of mental illness and the way the text is organised so that this notion is made available to the reader:

> The method of analysis assumes that the structure of the conceptual scheme 'mental illness' which the reader uses in recognising 'mental illness' is isomorphic with that organising the text and hence is discoverable 'in' it (22).

The assumption of isomorphism has certain components which I take to be implied, although not explicitly suggested as necessary, in Smith's usage. The assumption seems to suggest that there is no sense in which we can claim that the phenomenon of mental illness has an existence independence of the means of its expression (23). This has two immediate implications. Firstly, it makes nonsensical any attempt to arbitrate on the actual existence or otherwise of phenomena. This is a question which any attempt to understand practical reasoning simply has to bracket (24). Secondly, the only way to

recover the character of a phenomenon is to examine the work which is carried out by participants in effecting or bringing off its existence. This position has several parallels in the work of both linguists and ethnomethodologists. For example, in his classic work of the 1930's, Bloomfield argues that it is wrong to infer the presence of 'concepts' on the basis of scientific speech utterances. He argues that the 'inner goings on' of scientists are simply unverifiable and hence unavailable to us (25). In addition, terms such as 'ideas' are merely misnomers for linguistic events:

the term 'idea' is simply a traditional obscure synonym for 'speech form' (26).

More recently, Garfinkel has pointed out the essential interdependence between the recognisably achieved objectivity of accounts and the socially organised occasions of their use (27). A reformulation of this position appears in McHugh and, significantly, is quoted by Collins as an expression of the "epistemological stance" which underlies his argument (28).

We must accept that there are no adequate grounds for establishing criteria of truth except the grounds that are employed to grant or concede it – truth is conceivable only as a socially organised upshot of contingent courses of linguistic, conceptual, and social courses of behaviour (29).

It thus appears that the assumption of isomorphism between textual organisation and the achieved existence of a concept (in Smith's case 'mental illness') is no more than a particular case of well known epistemological position. Although it is usually thought to be in the nature of an epistemological standpoint that it is undemonstrable (30) it is worth considering certain observations which lend support (if not substantiation in any strong sense) to this assumption. Let me relate two examples of observation from a recent ethnography of laboratory practice (31).

In the course of interaction with members of the laboratory, it was suggested by the observer that members' work could be best understood as an obsession with literary inscription; that practising scientists devoted the majority of their activity towards accumulating and juxtaposing a series of figures, traces and diagrams in a manner not entirely dissimilar to the traditionally understood mode of literary production. The response of laboratory members was that the observer had made the usual mistake of sociologists of science in failing to recognise the true significance of the scientific papers;

that these were merely a means of communicating, or effecting the transfer of facts reported in them. When the observer asked for elucidation of the nature of these facts, he found that the scientists provided elucidation in the same terms as those contained in the papers. That is, the scientists employed the same kinds of persuasive device, organisation of rhetoric and so on as they used in the papers. The scientists were unable to account for the existence of phenomenon which they argued the papers to be about, except by using the same forms of expression deployed in the papers. Needless to say, the observer remained unconvinced that it was possible to demonstrate the existence of facts except through the devices of literary organisation which scientists claimed were simply a means of talking *about* those facts. Facts were not available except by virtue of their literary organisation.

On several occasions during the same investigation participants would refer to past episodes of great scientific achievement. Frequently, they would tell the story using the format 'one day so and so had an idea'. However, if the story was subjected to detailed scrutiny, it was possible to reveal a plethora of social circumstances, chance meetings, guesswork and strategical manouevres which made up the situation referred to by tellers of the story. It thus appeared that 'having an idea' was used as a means of shorthand summary in participants' retrospective accounting. In other words, participants' organisation of social circumstances, etc., in their story-telling provided for the gloss: 'an idea occurred'.

These examples suggest that it is not possible to retrieve the character of phenomena (specifically, this includes 'ideas', 'concepts' and 'facts') independently of their presentational context (32). As a corollary, if we wish to understand what for participants counts as a phenomenon, we are obliged to investigate the organisation of presentational context which makes a phenomenon available to them.

## 2. Analysis

### About the Data

In my analysis, part of the text of Professor Anthony Hewish's nobel lecture address is taken as data and reproduced below (33). Hewish and Sir Martin Ryle were jointly awarded the Nobel Prize for Physics at the end of 1974.

The Nobel citation named Ryle for services to the development of radio astronomy and Hewish specifically for the discovery of pulsars (34).

## DATA

−5    PULSARS AND HIGH DENSITY PHYSICS
−4    Nobel Lecture, December 12, 1974

−3    by

−2    Antony Hewish
−1    University of Cambridge, Cavendish Laboratory, Cambridge, England

## 0  Discovery of Pulsars

1     The trail which ultimately led to the first pulsar began in 1948 when I joined
2     Ryle's small research team and became interested in the general problem of the
3     propogation of radiation through irregular transparent media. We are all familiar
4     with the twinkling of visible stars and my task was to understand why radio stars
5     also twinkled. I was fortunate to have been taught by Ratcliffe, who first showed
6     me the power of Fourier techniques in dealing with such diffraction phenomena.
7     By a modest extension of existing theory I was able to show that our radio stars
8     twinkled because of plasma clouds in the ionosphere at heights around 300 km, and
9     I was also able to measure the speed of ionospheric winds in this region. (1)
10        My fascination in using extra-terrestial radio sources for studying the
11    intervening plasma next brought me to the solar corona. From observation of the
12    angular scattering of radiation passing through the corona, using simple radio
13    interferometers, I was eventually able to trace the solar atmosphere out to one
14    half the radius of the Earth's orbit. (2)
15        In my notebook for 1954 there is a comment that, if radio sources were of
16    small enough angular size, they would illuminate the solar atmosphere with suffi-
17    cient coherence to produce interference patterns at the Earth which would be
18    detectable as a very rapid fluctuation of intensity. Unfortunately the information
19    then available showed that the few sources known were more than one hundred
20    times too large to produce this effect, and I did not pursue the idea. This was sad
21    because the phenomenon was discovered by chance, about eight years later, by
22    Margaret Clarke long after I had forgotten all about my comment. She was involved
23    with a survey of radio sources at Cambridge and noticed that three particular
24    sources showed variations of intensity.

My general objective is to discern what for participants counts as a discovery (35). I shall employ the assumption of isomorphism between textual organisation and the notion of discovery, and examine those features of textual organisation which provide for the reading of the lecture: "a discovery

took place". It is important to note that it is this retrospective reading, as opposed to the contemporaneous reading: "a discovery is happening", which is the focus of concern in the data at hand. To understand more fully the way in which participants' accounting procedures provide for the occurrence of discovery as it happens, we require a different kind of data (36). The present text can be though of as a "celebration" of discovery rather than a claim to discovery. In the present text events in the past are rendered consistent with the idea of a discovery having happened.

Although a number of devices have been recognised as playing a major part in textual organisation (37), the organisation of textual presentations involving discovery makes use of a special combination of these devices as well as certain devices hitherto not recognised. As a way of setting up the analysis, I can do no better than adopt Smith's formulation of the analytical problem. In the following paragraph, I repeat her presentation verbatim, except where my substantive concern with "discovery" (rather than "mental illness") has necessitated some changes of wording (38).

Science can be thought of as one agency of social control with institutionalised procedures for assembling, processing and testing information about the observation of phenomenon so that it can be matched against the paradigms which provide working criteria of class membership. In analysing the text below, my quest is for those features of textual organisation which provide for rules or procedures for representing phenomenon as belonging to the class of "discovery". The account purports to be and can be recognised as a report of a discovery. Recognising in events the "fact" that a discovery has occurred involves complex conceptual work:

It involves assembling observations from actual moments and situations dispersed in time, organising them or finding that they can be organised, in accordance with the instructions which the concept provides. A simple, immediate and convincing recognition of a fact at this conceptual level implies that much of the work of providing events with the appropriate conceptual order has already been done. All that the reader/hearer has to do is to (find) in those events, or rather that account of events, the model which enables her to classify them (39).

An account which is immediately convincing is one which forces the classification of events as a discovery and makes any other difficult. And if it does, then the events (or the account of them) must already display the order which gives them the shape of the fact to the reader/hearer. In analysing this

account as an account of a discovery I am, I argue, recovering the structure of the conceptual model which is made use of in recognising that that is what it is.

## Preliminary Instructions

Certain features of textual organisation can be understood as initially providing the reader with instructions for making sense of the text with which he is faced. In the Hewish text, we can find three examples of such preliminary instructions.

### (a) The Setting

Gusfield argues that the setting of an article, for example, in a 'serious' journal, establishes a claim for the text of the article to be taken seriously; it should be read as authoritative fact rather than fiction (40). In other words, readers are asked to orient to a range of vetting procedures which they could imagine having taken place before the story was allowed to appear in its current setting. Tacitly, the judgement of (presumably expert and hence reliable) others has preceded the reader's own assessment of the authority of the text. Before even beginning to read the text, the reader is faced with the choice of concurring with this expert evaluation or dismissing its authority out of hand. In the present example, the designation of 'Nobel Lecture' (1.—4) (41) and the institutional affiliation of author (1.—1) constitute a setting which provides for the authority of the text (42).

### (b) Headings

Headings, typically in the form of titles or sub-titles establish the category of attributes and actions which follow. More precisely, a heading acts as a reader's guide to understanding the sense and the relevance of the terms which are set out in the subsequent text. The instructions made available by the heading are: interpret all that is said below in relation to meanings appropriate to the substance of the heading (43). In addition, headings can do the work of a summary, especially in the absence of any formal abstract. This means that a heading can act as a representational device, indicating that

there is a part which is separable from the whole, that the text is actually
*about* something which can be distinguished by the reader from the elaborate
morass of language in which it is located. In other words the reader is told
that the significance of the text can be separated from the wider body of the
text. The heading 'Discovery of Pulsars' thus tells the reader what to look for
in the subsequent text and how to interpret the relevance of the words which
make up that text. As a fairly obvious example, 'stars' (l. 4) is to be read as
part of a collection which we might call 'Scientific investigation of astro-
physical phenomena' and which includes 'laboratory' (l. −1), 'discovery' (l. 0),
'pulsars' (l. 0, l. 1), 'research' (l. 2) and so on (44). This eliminates other
possible understandings of the relevance of stars, simply because the reader
is unable to find members of another collection which might, for example,
include 'fame', 'fortune', 'show-biz', 'television' and 'Hollywood'.

#### (c) *Textual Opening*

The importance of textual openings is now well known. In the scientific report
examined by Gusfield it was noticed that the first paragraph established a
'tension' between two points of view (45). Immediately at the beginning
of the paper the 'denouement' is given, thus providing the reader with the
author's solution to (or resolution of) the established tension. In Smith's
terms, an assertion advanced in the opening sentences of a text provides
instructions for the reader to understand further parts of the text as being
about or supporting that assertion. Like headings, openings provide at least
one member of a collection which the reader can use to make sense of the
rest of the discussion. Unlike headings, however, openings do stronger work
in that they establish or assert the 'conclusion' to the argument of the text. It
is not just that openings provide a convenient system of categorisation for
making sensible reading. They additionally instruct the reader to relate the
contents of the text to a system of categorisation which is presented as the
answer or solution to the phenomenon or events described in the text. In
Smith's data, "I was actually the last of her close friends who was opening
willing to admit that she was becoming mentally ill" acts as an instruction
that all the things subsequently related in the text be interpreted in terms of
the categorical scheme 'mental illness'. Naively, we might expect a series of
facts to be presented, on the basis of which the reader can decide what to

make of them. In fact, the solution asserted at the outset encourages the
reader to relate all subsequently described events to this solution (46).

In the present data, we have "The trail which ultimately led to the first
pulsar began in 1948 when . . . " (l. 1). I suggest that this does some of the
work described by Smith (and by Gusfield) and additionally provides for
reader's work of particular importance for the notion of discovery. Firstly,
the assertion of the solution or conclusion ("the first pulsar") encourages
readers to understand all subsequent description in terms of its relevance
for this solution. To paraphrase Smith again, the problems presented by
the account is not to find an answer to the question "what do all the events
described here indicate?", but to find that this collection of items is a proper
puzzle to the solution "the discovery of pulsars" (47). The 'reasonableness' of
the solution follows from the reader making sense of described events in
terms of the solution. Secondly, this in turn suggests that the construct "the
first pulsar", in explaining a collection of otherwise inexplicable events, has a
character rather different from any of the events. It is not just another event;
it is something which explains these events. Hence, the solution is an entity of
a different order from the events and in this sense is independent of them.
Thirdly, the independence of this entity is reinforced by the fact that some
motion is required in order to get at it. We would suppose that an entity of
our own creation might be fairly readily at hand at the time when it was
first noticed as existing. But "the first pulsar" is to be understood as having
had a pre-existence, a quality of *out-there-ness* which required that it be
*approached*. I shall focus first on features of the text which reinforce the
independence or out-there-ness of the phenomenon. Subsequently I shall
examine how the particular plausible means of approaching this phenomenon
suggested to the reader is that of following a trail or path.

*Externalising Devices* (48)

As externalising device provides for the reading that the phenomenon de-
scribed has an existence only by virtue of actions beyond the realm of human
agency. In this case, externalising devices provide for the reading that pulsars
are neither the product nor the artful creation of scientists; scientists came
upon these objects rather than creating them. The dilemma is that the author
should be trusted as teller of the tale, but at the same time should not be seen

as intruding upon the object. Gusfield suggests that the identification of the role and institutional affiliation of the author establishes the reader's trust in the author (49). In terms of my earlier comments, the setting can provide for this trust. The scene of the action is then externalised by the use of the quasi-passive voice:

I was able to show (l. 7)
I was also able to measure (l. 9)
My fascination . . . brought me to the solar corona (l. 10–11)
I was eventually able to trace (l. 13)
In my notebook . . . there is a comment (l. 15)
patterns . . . which would be detectable (l. 17–18)
the information then available showed (l. 18–19)

This kind of formulation is extremely common in the text. It provides for (and reinforces) the reading that events occurred independently of the author's involvement which was relatively incidental to the steady accumulation of knowledge. The sustained usage of this device gives rise to an almost coy picture of the scientists as 'fortunate bystander' or 'lucky witness'. To appreciate the importance of this device it is instructive to substitute alternative expressions wherever the quasi-passive is used. Whereas in other circumstances it might be quite appropriate, for example, to use 'I showed' instead of 'I was able to show', the sustained use of the active voice makes the present account read quite differently. More exactly, the active voice provides for the reading: "These objects (pulsars) were artfully constructed by the teller of the tale". At once, this conflicts with the expectancies and interpretations set up by the initial textual opening. What manner of objectivity could be claimed for the research process such that the claimed *inevitability* of the outcome results from the merely *idiosyncratic* manoeuvres of the researcher?

Of related importance is the notion of community. Membership of a community is invoked in the sense that human agents comprise a residual category separated from the realm of objects. To restate the author's dilemma, he has to display himself as a member of the community, with no privileged epistemological access to the realm of objects, and yet he has to show himself as a reporter of the nature of objects in that external realm. The quasi-passive emphasises that the world of external objects has impinged upon him and that all that happens in the course of path following is the result of external

circumstances rather than of his own volition. The invocation of community membership can be read as a demonstration that the "author has no special vantage point or viewpoint compared with the audience" (50). This means that any other member would arrive at the same conclusion, given the same circumstances. The notion of community membership is commonly invoked by the use of the royal 'we' (51). In this data, 'we' appears infrequently (but consider "We are all familiar with . . . . " (l. 3—4). Nonetheless, there is frequent citation of 'things we all know' which provides for a reading of the author as member of a community whose action results from the community's knowledge of the external world; his action is again thoroughly non-whimsical. In lines 1—3, for example, the author's first introduction to a communally defined problem is related. The problem can be read as belonging to a community (52) by identifying "research team" and "general problem" as members of the same collection (53). The coincidence of the author's joining the research team and his becoming interested in a problem can be interpreted as a 'natural' occurrence given the available tie between 'research team' and 'problems'. In lines 5—6, for example, the introduction to communal knowledge is brought about by a teacher. Had anyone else similarly been provided with this access to technique, they too would have been able to make the consequent steps (54).

The externality of the events described in the text is in part provided for, then, by the use of passive formulations and by the invocation of community membership. At the same time the text is clearly about the author's individual part in the events leading up to the discovery. The solution to this particular aspect of the author's dilemma is the portrayal of the *coincidence* of the inevitability of unfolding events with things which happened to the author. In part, of course, the passive formulation achieves this: it is only by virtue of the combination of circumstances essentially beyond his control that he was permitted to pull off one or other particular event. For example, in line 13, "I was eventually able to trace" can be read as happening as a result of events allowing him this possibility. Concidence is more obviously provided for right from the start of the text, in lines 1—2: "The trial which ultimately led to the the first pulsar began in 1948 when I joined Ryle's small research team . . . " I have already made a case for the way in which the objectivity (out-there-ness) of the phenomenon is provided for. In the following section, I examine the objectivity assigned to the trail which leads to the phenomenon. Given

this objectivity, any event obviously involving human agency (such as joining a research team) is to be read as a coincidental rather than constitutive occurrence.

## Pathing Devices

One of Kuhn's most significant, but often neglected points is that scientists' descriptions of scientific episodes are a source of considerable obfuscation to sociological and historical understanding of science (55). The source of such obfuscation is not simply that scientists are poor historians, nor just that they tend to confuse scientific and historical accuracy. More fundamentally, the marked tendency towards' etiological structure found in scientific discourse reflects the fact that scientists routinely engage in establishing relationships between current work (or knowledge) and past events. One important consequence of this is scientists' insistence on portraying work as the latest in a long line of cumulative development (56). It is this picture of development with which Kuhn takes issue. According to Kuhn, sociological and historical evidence can be adduced to provide a picture of cyclical development, which contrasts with the linear-cumulative model found (especially) in scientific textbooks. Of course, much sociological and historical capital can be made of the fact that uncritical reliance on linear-cumulative descriptions can lead to an 'erroneous' understanding of the way science 'actually' develops. More importantly for present purposes, this reliance neglects consideration of a central feature of scientific investigation: the way in which participants render events as stemming, or following on from past events.

I shall refer to one aspect of the work done by scientists in accomplishing connections between events as the use of pathing devices. While providing for a reading of the out-there-ness of pulsars, it is also necessary to relate how this independent entity was 'captured', how its existence was brought within the realm of scientific knowledge. The pathing device portrays the capture in terms of a path followed by the capturers. In Smith's data we find "My recognition that there might be something wrong was very gradual" (57). "That something might be wrong" is the independent solution which readers are asked to use as a way of ordering the facts described. In addition, the acquisition of this solution is set up as a *process*. The reader is encouraged to orient to textual events as part of a process of gradual realisation. Significantly,

this asks us to suspend our assessment of the facts until they have all been told. It is therefore inappropriate to query the 'oddness' of individual items of behaviour described in the text. Instead, we are told to look for the combined and *cumulative* effect of all the events (aspects of behaviour), and for the adequacy of the achieved categorisation of this cumulative effect.

In a similar way, line 1 of the pulsar data provides for a reading of "coming upon the first pulsar" as a process. The process is to be understood as cumulative in that it involves following a path ("trail"). This in turn provides instructions to the reader that all that follows should be understood as leading up to the discovery. We are asked to suspend critical evaluation of items mentioned in the text and to understand that the first section be read not for itself but as 'providing the background' or 'acting as a lead-in'. This means, for example, that if the account were being narrated, it would appear inappropriate to interrupt at, say, line 11 to raise a question about the 'solar corona'. The narrator could well rebuff a reader/hearer's interruption by explaining that the piece about the solar corona would become relevant later, that he was 'just giving the background' at this stage and that the main point of the story is the discovery of pulsars. Such an interruption is unlikely to occur, I suggest, precisely because readers are encouraged to notice line 1 as providing the instructions: "The first fifty or sixty lines are simply intended as introduction, as sketching out the background to the story, the real point of all this is yet to come, so that queries about items in this section are irrelevant or uncalled for" (58).

The pathing device in line 1 acts as a strong constraint on the way the reader interprets the text: the apprehension of the phenomenon as a discovery is to be understood by way of an account of the approach to, or capture of, the 'first' pulsar. At the same time, the initial statement about the path-like nature through which textual events must be understood, requires and finds reinforcement as the story unfolds. The instructions made available by the initial pathing device are restated throughout the text. This is done by re-presenting the events, not as 'any old collection' of events, but as a properly and sequentially connected series of events. Whereas the pathing device provides instructions for finding a link between the story's beginning and its (already asserted) conclusion, other devices establish for the reader that this link comprises an adequately connected sequence of events.

*Sequencing Devices*

The importance of sequential organisation in texts is recognised by Smith as follows:

> The ordering of events in the narrative constructs the objectivity of the fact, the items which might serve to suggest the opposite are not only relegated to the background, they are also not constructed in the same way. They are merely as it were lying about. A careful search may identify them, but the work of bringing them into order must all be done by the reader/hearer (59).

Sequential organisation thus acts as a 'cutting out' process, whereby other potential paths and other potentially relevant events are backgrounded. The reader is encouraged to concur with the relevance of described events for the sequence in which they are located. Irrelevant events, in particular false leads, red herrings and blind alleys, are either excluded or are not tied to subsequent events (60).

 Textual items can be read as connected or tied together if they can be found to belong to a common category or collection of items. I suggested earlier that this was the way that headings provided for the relevance of textual items. If we now extend this mechanism to activities and events, we can see how connections are provided for. For example, the connection between joining a research team (1. 2) and having a task of understanding something (1. 4) is made 'obvious' and hence incontrovertible by the location of a collection which includes both the former event and the latter activity. The strength of the tie between 'the task of understanding' and 'joining a research team' is reaffirmed not simply because 'everyone knows' that the task of understanding goes on in research teams, but also because the 'work of research' simultaneously provides the interpretative scheme whereby the particular sense of the activity of 'understanding' is established. Similarly, the reader can find ties between the 'task' of understanding and later activities such as being "able to show" (1. 7), being "able to measure" (1. 9), "studying" (1. 10), and being "able to trace" (1. 13). All these descriptions of action can be assigned a sensible meaning (that is, they can be understood) if that meaning concurs with the meanings of other members of the same collection: the sense of showing, measuring and so on is determined in terms of the nature of the task of understanding. At the same time, the nature of the task

is elaborated and refined by the meanings given to these activities. The sense of individual activities and events thus depends on their location in a common collection of relevancies; their common location for purposes of assigning sense thus goes hand in hand with their connectedness.

Although membership ties provide for the interconnectedness of events and activities, and although these connections are suggestive of order (for example, we take the sense of joining a research team that precedes assuming a task as part of that team) it is possible to specify certain features of the text which establish a sequential relevance for these connections.

Events and activities can be read as sequentially ordered by virtue of their textual juxtaposition. Events can thus be read as being related in time, even though no specific reference to date or time is made. For example, "My fascination in using extraterrestial radio sources for studying the intervening plasma next brought me to the solar corona" (1. 10–11). Events previously related (for example, the measurement of ionospheric wind speeds – line 9) become events which happened before the author's first mention of another event (coming to the solar corona). In some cases, activities are specified as having occurred 'at a certain time', where the nominated time acts as one member of a collection of previous and subsequent time points. Events can be read as having an order by understanding the sense of their associated accompanying time points in relation to other members of the same collection. Consider, for example, the following three phrases:

In my notebook for 1954 there is . . . (1. 15)
the information then available . . . (1. 18–19)
about eight years later . . . (1. 21)

Fairly obviously, we can use the 'then' as a time referrent to establish a sense of simultaneity about the availability of information noted in lines 18–19 and the making of a comment in 1954. This simultaneity also justifies the presence of lines 18–20: the contents are taken to be relevant to the current stage of the path being described. In addition the temporal tie between the two sentences provides for the second as an elaboration of the first. In this particular case, moreover, the elaboration also acts as a justification for not following a particular course of action on the basis of the 1954 notebook comment.

In a similar way, events can be read as having occurred in a sequence if they can be seen to be tied to explicit dates. In the text we can find "1948" (l. 1), "1954" (l. 15), "1965" (l. 42 and 49), "July 1967" (l. 59), "August 1967" (l. 71) and so on. The appearance of these dates in order throughout the text provides a chronological backcloth, upon which related occurrences are displayed. More importantly, events and descriptions are tied into this chronological framework so as to furnish a history involving minimal gap or overlap. For example, the dates in line 4 and line 1 establish the scope of the historical scenario: little will concern the reader that occurred before 1948 or after 1974. Line 1 also establishes the relevance in following lines of things occurring since 1948. By line 15 ("1954") six years worth of "background to the discovery" (l. 0 and l. 1) have been covered. Significantly, the coverage of these six years appears thoroughly adequate: at no point does the reader feel that an unjustified leap has been made by the author, that insufficient information has been provided to fill out what went on in (say) 1950, or that too much has been said about these years such that a reference to 1954 constitutes 'backtracking'. This effect is in part due to many of the devices already noted. Just as the heading, textual opening and pathing devices ask a reader to assess the significance of items (such as 'solar corona') in terms of 'what is to come later', so too are the contents of lines 1–15 to be understood as things which happened between 1948–1954 which are relevant to the (pre-established) purposes of this story. The strength of this effect entails a subtle inversion: lines 1–15 become "all that needs to be said about what happened between 1948–1954" and the completeness of the account is established as 'good enough'. The story then proceeds by the tied relevance of events in 1954 to those of the preceding six year period: for example, pre-1954 work associated with the solar atmosphere (l. 13) has a relevance in terms of the 1954 notebook comment about the solar atmosphere (l. 16).

Sequencing devices thus provide for the connection of described events and activities. The connectedness of these events and activities is to be looked for by the reader in the context of a pathing device which asserts the existence of a route to be followed. Thus, in the connectedness of events and activities the reader can find a cumulative relevance: the events and activities are not merely connected sequentially, they are constitutive of the route being followed.

## Logic

Logic can be generally thought to be concerned with the rules of valid inference. As one popular dictionary definition puts it, those who try to following the rules of logic aim to distinguish

inferences whose premises really entail their conclusions . . . from those whose premises do not. (61)

The issue arising here is that of the adequacy of connection between one state $X_1$ (described in the above definition as a premise) and another state $Y_1$ (referred to as a conclusion). If $X_1$ 'really' entails $Y_1$, if a connection can be seen to hold between $X_1$ and $Y_1$, if $Y_1$ can be seen to result from $X_1$ rather than from any other $X_2$, and if it is $Y_1$ rather than any other $Y_2$ that results from $X_1$, then the relationship between $X_1$ and $Y_1$ can be said to be logical (62).

Of course, there is no explicit claim in the text that the discovery process was 'logical'. That is, there are no explicit attempts to justify connections between successive stages of the discovery process using the kind of schematic representation just employed. This is perhaps not surprising in that we recognise a Nobel lecture as the occasion for celebration-of-having-made-a-discovery rather than for attempting-to-establish-a-claim-to-a-discovery. If anything, the legitimacy of an already established claim enjoys reinforcement rather than renewed scrutiny on the occasion of this lecture address. Yet I suggest that the text still stands as a report of a logical inquiry in the sense that participants can be seen to have proceeded in a correct fashion.

This reading is made possible by virtue of all the features of textual organisation outlined above. The reader is asked to concur with the authority of presentation, to interpret textual events and actions both as being about some external phenomenon and as giving rise to this phenomenon. In addition, the externality of this phenomenon is to be approached by way of a process of path following, in which the relevance of individual items is seen not for themselves, but as steps on the way towards the phenomenon. The intrusion of the author in this process is minimised by various externalising devices. The sequential nature of the procedure is further provided for by a variety of features which demonstrate connectedness. In terms of the above schematic representation, $Y_1$ can be understood to be entailed by $X_1$ if $Y_1$ is

a subsequent state (rather than just any other state), if $Y_1$ is a next mentioned event or activity by which the continuing sense of a developing story is to be retained, if the nextness of $Y_1$ can be read as another step towards the already asserted (and hence pre-existing) conclusion to the story, and if the particular sense of $Y_1$ is to be found by reference to a collection of events and activities of which $X_1$ is a member.

The total effect of a whole series of $X_1 - Y_1$ connections is that of 'cutting out' alternative ways of reading the text. It is not impossible to find other events $(X_2, Y_2)$ in the text. But their sense and connectedness is not easily established: they are not woven into the text in the same way. Thus the possibility of $X_2 - Y_1$ (or $X_1 - Y_2$) relationships is neither explicitly entertained nor provided for by their organisational presentation in the text. In this manner, alternative ways of *reading* are cut-out or background. But such is the congruence between the reader's means of understanding the path-like nature of the report and the means of understanding the path-like nature of the discovery process, that alternative ways of *proceeding* in the investigation are also cut-out or backgrounded. Thus it is hard to find alternative ways of 'coming to the problem', 'setting up the crucial experiment', 'seeing the link between one observation and another', and so on. The reading most compellingly provided for is that of an investigation which displays care and orderliness in its execution: participants have proceeded in a manner that has allowed them to infer conclusions about the existence and character of a phenomenon on the basis of observational premises.

## 3. Conclusion

In the first part of this paper I described a debate over the ways of studying knowledge between rationalist philosophers and 'strong programmers'. Although this debate is in no way exhaustive of the variety of positions adopted in philosophical and sociological perspectives on knowledge production, its description provided the basis for suggesting that alignment to such perspectives is (at least) an inappropriate strategy for the fruitful understanding of the process of scientific investigation. I argued that the form and currency of the activities which characterise this debate are to be found as a crucial constituent of scientific investigation. In other words, the work of science involves scientists in continual monitoring activity, in attempts to

specify truth status on the one hand and in searches for the presence of the social on the other. This point is perhaps particularly salient for adherents to the "strong programme" in the sociology of scientific knowledge: those sociologists whose main analytic objective is the revelation of social circumstances in science tend to miss consideration of the way in which scientists' routine invocation and attribution of social factors and interests is crucial to their work.

Practical reasoning in sciences encompasses a wide variety of activities. In the second part of this paper I illustrated the analytic perspective developed above in relation to just one type of activity, namely the work of textual organisation whereby a particular scientific accomplishment is reported. In the particular case of an account of a discovery, a number of features combine to make available to the reader a picture of the discovery process as a path-like sequence of logical steps towards the revelation of a hitherto unknown phenomenon. The perspective adopted here might be profitably developed and extended to an examination of a much wider range of scientists' accounting practices.

## Notes and References

1. I would like to thank a number of people who helpfully commented on an earlier draft of this paper. These include Peter Halfpenny, Karin Knorr, Roger Krohn, Bruno Latour and Michael Lynch.
2. S. W. Woolgar, 'What Can Ethnomethodology Tell Us About Science As A Topic?', Paper presented to Werner-Reimers Stiftung conference, Bad Homburg, 4–7th January 1979 and to BSA Sociology of Science Study Group, London, 13th February 1979.
3. In terms of its concern with issues of practical reasoning, fact production and the use of the documentary method of interpretation, ethnomethodology would appear to have a clear relevance for those interested in the nature of scientific inquiry. This relevance is implicit in the early work: H. Garfinkel, *Studies in Ethnomethodology*, Prentice Hall, New Jersey, 1967; issues more explicitly related to scientific work are raised in articles such as those by H. C. Elliot, W. W. Sharrock and D. H. Zimmerman in R. Turner (ed.), *Ethnomethodology*, Penguin, Harmondsworth, 1974. Most recently, scientific work has enjoyed extended and detailed treatment at the hands of a growing body of interested ethnomethodologists. See especially, M. Lynch, *Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory*, Ph.D. diss., University of California, Irvine, 1979; also M. Lynch, 'Technical Work and Critical Inquiry: Investigations in a Scientific Laboratory', Paper presented at conference on 'The Social Process of Scientific Investigation', McGill University 19–21st October, 1979.

4. Some of the earliest proposals to this effect can be found in S. B. Barnes and R. G.
   A. Dolby, 'The Scientific Ethos: A Deviant Viewpoint', *European Journal of
   Sociology* 11, 3–25 (1970); M. J. Mulkay, 'Some Aspects of Cultural Growth in
   the Natural Sciences', *Social Research* 36, 22–52 (1961); R. D. Whitley, 'Black
   Boxism and the Sociology of Science: a discussion of the major developments in
   the field', *Sociological Review Monograph* 18, 61–92 (1972).
5. The terms in which the debate is reported here derive from contributions to the
   seminar series: 'Rationality and the Sociology of Knowledge', Balliol College,
   Oxford, Summer term, 1979.
6. K. R. Popper, *Objective Knowledge*, Oxford University Press, Oxford, 1972.
7. R. Bhaskar, *A Realist Theory of Science*, Leeds Books, Leeds, 1975, p. 10.
8. B. Barnes, *Scientific Knowledge and Sociological Theory*, Routledge and Kegan
   Paul, London, 1974; *Interests and the Growth of Knowledge*, Routledge and Kegan
   Paul, London, 1977.
9. D. Bloor, *Knowledge and Social Imagery*, Routledge and Kegan Paul, London,
   1976.
10. A sociological explanation of the dispute between rationalism and strong programme
    might fashionably proceed by attributing to each party a difference in 'interests';
    differences in style and form of explanation, together with a fundamental difference
    in interests, would amount to the incommensurability of rationalism and the strong
    programme; since adequacy of explanation will depend in some complex way on
    the interests of the inquirer, it follows that any particular explanation will remain
    fundamentally inadequate as far as the other party is concerned. This line of
    argument might also account for differences between the strong programme as
    portrayed by rationalists, and the strong programme as described by its adherents.
11. See my comments below derived from the analysis by J. Gusfield, 'The Literary
    Rhetoric of Science', *American Sociological Review* 41, 16–34 (1976).
12. B. Latour and S. Woolgar, *Laboratory Life: The Social Construction of Scientific
    Facts*, Sage, Beverly Hills, 1979.
13. This is not to say that practising scientists are particularly 'good' or 'bad' sociologists
    (or philosophers). Rather, the suggestion is that they engage in essentially similar
    forms of 'practical reasoning'; that their competences with regard to basic explana-
    tory skills (for example, invoking causes, attributing motives) are no different from
    those of sociologists (or philosophers).
14. For purposes of making the analogy between the constructive work of scientists
    and that of the strong programmers, this last formulation pays insufficient attention
    to the work of scientists in establishing whether or not *any* social circumstances are
    present. Although, intuitively, we might expect scientists only to be concerned with
    social factors when the work of science 'goes wrong', it is possible that such an
    assumption detracts attention from continuous implicit monitoring by scientists
    of the potential intrusion of social factors.
15. This shift in focus amounts to a redefinition of the explanandum, a manoeuvre
    which, from the point of view of protagonists to the debate outlined above, might
    be thought of as something of a 'Pyrrhic victory'. The cost is that of seeing knowl-
    edge production from the perspective of either philosopher or sociologist, and yet
    the manoeuvre opens up possibilities of investigation relatively free from (or at least
    respectful of) the constraints (for example, inconcludeability) of this kind of debate.

16. The term 'social' may appear conspicuously absent from this last phrase. Its absence is intended to denote my reservations about the practice of premature "conceptual-isation of actions in social terms." For an elegant discussion of the way in which some sociological arguments 'fill out' analysis by the introduction of 'disengaged' analytical apparatus see Lynch, *op. cit.*, 1979, (Note 3).

17. H. M. Collins, 'The Seven Sexes: A Study of the Sociology of a Phenomenon, or the Replication of Experiments in Physics,' *Sociology* 9, 205–224 (1975).

18. In terms of the example of the discovery of pulsars referred to below, it was only possible to know that pulsars existed when you knew something about what they were. Of course, the *amount* that needs to be known about a phenomenon before its existence can be claimed to be (or accepted as) genuine is a crucial issue for scientists. In the case of pulsars, participants' prevarication on this point was exten-sive. See S. W. Woolgar, *The Emergence and Growth of Research Areas in Science with Special Reference to Research on Pulsars*, unpub. Ph.D. diss. University of Cambridge, 1978.

19. A point pursued at length in Latour and Woolgar, *op. cit.*, 1979, (Note 12) Chap. 4.

20. There are some similarities here with the argument of what Attewell calls the 'radical situationist' wing of ethnomethodology (P. Attewell, 'Ethnomethodology Since Garfinkel', *Theory and Society* 1, 179–210 (1974)). In particular, Attewell cites the work of Zimmerman and Pollner (for example, D. H. Zimmerman and M. Pollner, 'The Everyday World As a Phenomenon'. In J. D. Douglas (ed.), *Under-standing Everyday Life*, Routledge and Kegan Paul, London, 1971, pp. 80–103.

21. D. E. Smith, 'K Is Mentally Ill', *Sociology* 12, 23–53 (1978); previously published as 'K ist geisterskrank. Die Anatomie eines Tatsachenberichtes'. In E. Weingarten, F. Sack and J. N. Schenkein (eds.), *Ethnomethodologie: Beiträge zu einer Soziologie des Alltagsleben*, Suhrkamp, Frankfurt, 1976.

22. *Ibid.*, p. 23.

23. By 'notion of mental illness' I refer to the particular notion or version of mental illness made available to and used by the reader in making sense of the text. The programme is *not* that of attempting to specify the 'essence' or 'universal character' of mental illness.

24. It is important that Smith's argument is not understood as an application of 'labelling theory'. Smith does *not* claim, for example, that what was 'actually not' mental illness was given that label by reason of accounting practices. Rather, those practices are constitutive of the phenomenon. For a critique of 'labelling theory' from an ethnomethodological perspective see M. Pollner, 'Sociological and Common Sense Models of the Labelling Process'. In R. Turner (ed.), *op. cit.*, 1974, (Note 3), pp. 27–40.

25. L. Bloomfield, 'Linguistic Aspects of Science', *Philosophy of Science* 2, 499–517 (1935).

26. L. Bloomfield, 'Language Or Ideas', *Language* 12, 89 (1936).

27. Garfinkel, *op. cit.*, 1967, (Note 3).

28. Collins, *op. cit.*, 1975, (Note 17).

29. P. McHugh, 'On the Failure Of Positivism'. In Douglas, *op. cit.*, 1971. (Note 20), pp. 337–354.

30. Certainly, Smith (*op. cit.*, 1978, Note 21) makes no attempt to substantiate her assumption of isomorphism.

31. Latour and Woolgar, *op. cit.*, 1979, (Note 12).

32. Cf. D. C. Anderson, 'Stories and Arguments: Narrative Assembly and Contrastive Characterisation as Contributive Features in the Local Organisation of a Sociology Text', unpub. paper, no date.

33. A. Hewish, 'Pulsars and High Energy Physics'. In *Les Prix Nobel En 1974*, The Nobel Foundation, Stockholm, 1975, 69–79. For reasons of space, the analysis focusses on the first 24 lines of this text.

34. Woolgar, *op. cit.*, 1978, (Note 16a) pp. 79 ff.

35. For purposes of present analysis, I have no interest in the 'accuracy' of the data. That is, I am not concerned to establish whether or not the lecture address is an adequate portrayal of the actual facts of the matter. This might be a legitimate line of investigation in another context, although there are strong arguments for suggesting that the actual facts of the matter are essentially irretrievable in any incorrigible sense (see, for example, S. W. Woolgar, 'Changing Perspectives – a chronicle of research development in the sociology of science'. In J. Farkas (ed.), *The Sociology of Science and Research*, Akadémiai Kiadó, Budapest, 1979, pp. 421–437). Here I have no wish to evaluate Hewish's claims, to downgrade the achievements alluded to in the lecture nor to detract from the perceived significance of his scientific work. This declaration is necessary for two reasons. Firstly, it seems that almost any attempt to examine a document (or any other artifact) without wishing straightforwardly to build upon, or use 'the facts which reside in the document' is greeted with suspicion. It is assumed that if the analyst does not accept the factual status of the document, he must be questioning its veracity. To repeat, my motive is not evaluative. Secondly, in the particular case of pulsars, much controversy followed the initial announcement in 1968 of discovery and was rekindled by Hewish's subsequent receipt of the Nobel Prize. See Woolgar, *op. cit.*, 1978, (Note 34). Here I am not examining the lecture address for evidence bearing on this dispute.

36. An analysis (along similar lines to the one here) of contemporaneous discovery accounting practice could attempt to discover how much needed to be known about a 'thing' before its existence could be taken as genuine (see Note 18). In the present text, the retrospective character of the account reveals the discovery as something that was on the cards all the time (even from 1948 – see below).

37. Anderson, *op. cit.*, no date, (Note 32); Gusfield, *op. cit.*, 1976, (Note 11); Smith, *op. cit.*, 1978, (Note 21). The attentive reader will notice that I gratefully employ many of the terms and expressions used by these authors.

38. Where the wording remains entirely unchanged I have acknowledged this by quotation.

39. Smith, *op. cit.*, 1978, (Note 21) p. 26.

40. Gusfield, *op. cit.*, 1976, (Note 11).

41. Line references to annotated text.

42. My usage of 'provides for' is recurrent in the subsequent analysis and merits some elucidation. By saying that a feature of textual organisation 'provides for' a certain reading, I mean simply that a certain interpretation or set of meanings is made possible. This is not to claim that this is the only reading which might in practice be arrived at, nor am I able to cite evidence categorically demonstrating that 'most people' did read it this way. Rather, my aim is to specify the characteristics of the

text which could lead to one particular and plausible reading. In the present context, it remains possible, for example, for a sceptical reader to notice lines −4 to −1 as merely an *attempt* to legitimate the whole story, 'knowing full well that this is just the kind of dubious tactic which people at that institution (l. −1) would employ'. An analysis of this alternative reading is possible, but would be complicated by the need to specify the source and applicability of a reader's 'knowledge about the use of tactics'.

43. In more formal ethnomethodological terms: the indexicality of all followed terms can be repaired by selecting that sense compatible with the collection of senses implied by the heading.

44. This scheme is loosely adopted from H. Sacks, 'An Initial Investigation of the Useability of Conversational Data for Doing Sociology'. In D. Sudnow (ed.), *Studies in Social Interaction*, Free Press, New York, 1974, pp. 31–74.

45. Gusfield, *op. cit.*, 1976, (Note 11).

46. The circularity of this scheme can be thought of as somewhat similar to assuming the answer in a mathematical proof.

47. Smith, *op. cit.*, 1978, (Note 21) p. 37. In a similar way, Garfinkel shows how the documentary method of interpretation can involve a search for the proper sense of questions to which answers are offered: Garfinkel, *op. cit.*, 1967, (Note 3) p. 90.

48. As will become clear, it is possible to think of what are described here as 'devices' in terms of the 'modalities' referred to by Latour and Woolgar, *op. cit.*, 1979, (note 12) Chap. 2.

49. Gusfield, *op. cit.*, 1976, (Note 11).

50. *Ibid.*, p. 21.

51. See M. Roche, 'A Durkheim Reader: Rules for Loud Speech and Silence', *Writing Sociology* 1, 9–23 (1976).

52. In this text, membership of *a* community appears to be more important than specifying *which* community in this text. It is noticeable, for example, that the "we" of line 3 is different from the we implicit in "small research team" (l. 2).

53. Cf. W. W. Sharrock, 'On Owning Knowledge', pp. 45–53 in Turner (ed.), *op. cit.*, 1974, (Note 3).

54. Although for reasons of space it is not possible to pursue this here, it would be instructive to examine the consequences of textual organisation of community and of the relationships established in the text between the actors depicted as participating in the discovery. The textual presentation of contributions made by assistants would be of particular relevance in the context of a subsequent dispute over which individual should rightfully be accredited with the discovery: the 'anyone else' of this last sentence has a special significance in the terms of this dispute. See Woolgar, *op. cit.*, 1978, (Note 34) Chap. 3.

55. T. S. Kuhn, *The Structure of Scientific Revolutions*, Chicago University Press, London, 2nd edition, 1970, pp. 1–9.

56. In the course of an extended study of the emergence and growth of research areas in science, requests for interviews were made to over 40 participants who had worked on pulsars during the five years after the announcement of their discovery in 1968 (see Woolgar, *op. cit.*, 1978, Note 34). In many case, the first reaction of these participants was that "it all began in the 1930's" or "to know about pulsars you really need to go back to the work of Baade and Zwicke before the war". The

accounts of some observers make an explicit case for linearity of development by excluding the relevance of certain sociological variables. See, for example, M. N. McMorris, 'The Ancestry of Pulsars', unpub. paper, University of West Indies, 1973.

57. Smith, *op. cit.*, 1978, (Note 21) p. 28.

58. Although, of course, there are no 'explicit' or 'literal' instructions to this effect.

59. Smith, *op. cit.*, 1978, (Note 21) p. 37.

60. Gusfield argues in a general way that because the comment on or significance of results appears at the end of a sequence: problem-methods-results-conclusion, the conclusion appears as the culmination of the use of methods in order to produce results. Gusfield, *op. cit.*, 1976, (Note 11) p. 19. Although both Smith and Gusfield describe the important effect of sequencing devices, neither specify in detail how sequences are provided for.

61. A. Bullock and O. Stallybrass (eds.), *The Fontana Dictionary of Modern Thought*, Fontana, London, 1977, p. 355.

62. Of course, this is not necessarily exhaustive of the requirements of logicality: conditions for adequate entailment are endlessly programmatic. That is, we can always imagine further conditions that could be specified as necessary for adequacy: for example, that $Y_1$ is not just another descriptive version of $X_1$, that $Y_1$ 'really is' distinct from $X_1$ and so on. It follows that any instance of 'having been logical' is a practical accomplishment involving the repair of inconcludability by invocation of ad hoc and etcetera clauses.