

Problems and Possibilities in the Sociological

Analysis of Scientists' Accounts

(or The Why and the How in Reading Scientists' Accounts)

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Paper presented at 4S/ISA Conference on the Sociology  
of Science, Cornell University, Ithaca, New York, U.S.A.,  
November 4-6th, 1976.

Working Draft - Not to be quoted without author's permission

The three studies I want to review are Wynne's analysis of the reception by the scientific community of work by C.G. Barkla,<sup>3</sup> Mulkey's discussion of the ideological influences upon statements and descriptions offered by scientists<sup>4</sup> and my own attempt to use accounts of the early days of pulsars to characterise the process of discovery of a new phenomenon.<sup>5</sup> Although the studies by Wynne and Mulkey are not necessarily typical of current sociological use of scientists' accounts, they do make unusually explicit their reliance on such accounts. My aim is to draw from these studies certain features of the methodological procedures which sociologists have used in dealing with scientists' accounts.

Wynne has provided an extremely comprehensive description of the events surrounding the reception of the 'J phenomena' in the early part of this century. Barkla was a highly eminent physicist working at Edinburgh between 1913 and 1944. In 1917, Barkla won a Nobel Prize for his earlier discovery of sets of characteristic X radiations (the so-called K and L series radiations) at a time when the nature of radiation and the fundamental structure of matter was a central theoretical problem in physics. In addition to his discovery of the K and L series, Barkla proposed a theory which entailed the existence of a further series, the J series. At the time of its first full exposition, in 1916, this theory appeared quite plausible and was initially widely accepted. By 1923, however, a number of articles had appeared which argued against Barkla's conclusions and, in the face of fairly widespread opposition, Barkla confessed his J theory to be in error. At this stage, the consensus of scientific opinion favoured an explanation of Barkla's experimental results in terms of the Compton effect. But Barkla, while admitting an error in his formulation of the J theory, steadfastly resisted this alternative explanation. Indeed, Barkla committed himself to a fairly substantial research programme designed to explore further the experimental results of the J phenomena and to discredit explanations in terms of the quantum structure of radiation. By 1933, when Barkla finished publishing results, this research effort had produced 20 research papers and 14 Ph.D. theses. In the long run, however, Barkla's work on the J phenomena

'came to nothing', merely incurring the firm rejection by the scientific community.

Wynne argues that, in fact, no consensual account of why the J phenomenon was a scientific error was ever produced. Instead, according to Wynne, there appeared a number of accounts, journal papers and review articles, which used various arguments to discredit Barkla's work, but none of which invoked rational scientific grounds to do this. Some reviews claimed that the J theory had never been fully worked out and others highlighted Barkla's earlier admission of an error. Wynne points out that these kinds of arguments were not generally regarded as sufficient rational justification for a convincing rejection of a body of work. Yet some of these reviews were widely proclaimed as definitive refutations of Barkla. Wynne suggests that, in fact, these reviews acted as rhetorical confirmations of a rejection already well under way at the time they were written. There was no need for such accounts to do any more than 'rubber-stamp' the general feeling among the scientific community that Barkla's work was in error. Given the prevalence of such feeling it was unnecessary to 'prove' Barkla wrong. Circumstantial evidence of his guilt was sufficiently damning.

Wynne argues that these reviews presented misleading characterisations of Barkla's work. Furthermore, these characterisations served a number of important social functions. By drawing attention to circumstances and events preceding Barkla's investigation of the J phenomena, the reviews obviated the necessity of taking into account the work itself. The emphasis on Barkla's previous mistake, for example, detracted attention from his subsequent research effort by implying that his past record was good enough indication of its worthlessness. This, Wynne argues, had the effect of 'backgrounding' any radical uncertainty that potentially existed. Given that there was no way of ultimately proving Barkla wrong (and his opponents right), the reviews deflected attention from the real differences between Barkla and his opponents by invoking circumstantial reasons for doubting any of his claims. By concealing the fundamental nature of the differences between Barkla and others, the accounts avoided drawing attention to the existence of an alternative paradigm and

helped conceal the possibility that various experimental effects still required explanation.

This brief review scarcely does justice to the richness of Wynne's analysis. In particular, I have omitted the reasons Wynne gives for the emergence of the conflict between Barkla and supporters of alternative explanations of the J phenomenon. Nevertheless, the above enables us to outline some basic features of Wynne's method. The analysis comprises three distinct steps. Firstly, Wynne provides an extremely detailed description of the circumstances giving rise to the emergence of two conflicting schools of interpretation of a new phenomenon. He describes, for example, how Barkla's unwillingness to accept explanations in terms of the Compton effect stemmed, in part, from his deep-rooted objection to the use of the mass spectrometer, from his methodological commitment to absorption techniques (which had proved abundantly successful in Barkla's previous work) and from a fundamental belief in a realist epistemology. Secondly, having set the scene in this way, Wynne turns attention to the descriptions of this state of affairs as provided in various scientists' accounts. He then contrasts the contents of these accounts with the picture he originally arrived at and concludes that the accounts present a 'misrepresentation of Barkla's beliefs'.<sup>6</sup> When referring to a review by Dunbar, for example, Wynne states

... in spite of asserted claims that Dunbar scientifically refuted the J phenomenon, in fact he was unable to do anything of the kind...other reviews display similar contradictions, gaps and inconsistencies.<sup>7</sup>

Wynne comments in a similar way on a review by Gaertner:

in addition to the sanctions of having no fully worked out theory, having had previous convictions (the J series) and having pled guilty (which is quite false), wanting dogmatically to retain the classical theory (which again is not true), Gaertner exploits a mistake made by two of Barkla's research students as a sanction against the credibility of Barkla's whole research school.<sup>8</sup>

Wynne thus identified parts of what was said in the review accounts as being inconsistent with the 'real' picture of what happened. In addition, he noticed the

use of various devices which, although not themselves in direct contradiction to the real state of affairs, deflect attention from its serious consideration. In sum, these two noticeable features of the reviews, the outright contradictions on the one hand and the diversionary tactics on the other, act as powerful means of misrepresentation. The third step in Wynne's method follows. Having identified instances of misrepresentation, it is necessary to explain or make sense of them. What possible reason prompted the authors of these reviews to portray Barkla's work in such a misleading manner? The answers provided by Wynne have already been mentioned. Essentially, the characterisation of Barkla's work in an unfavourable light serves to minimise potential uncertainty within the scientific community by 'backgrounding' experimental phenomenon which may prove incompatible with the dominant research paradigm. The third step thus entails the specification of a number of sociological reasons which both explain the production of certain erroneous statements and which are consistent with the original description of existing scientific circumstances.

I want to keep this (albeit simple) characterisation of Wynne's method in cold storage while examining Mulkey's discussion of scientific accounts. Suffice to notice one point. A central part of Wynne's analysis (I called it the second step) is the identification of inconsistencies between what was said in some scientific reviews and what other evidence suggests was the case.

Mulkey's interest in the statements made by scientists arises from his concern with the apparent discrepancy between descriptions of the normative structure of science and the marked deviation from Mertonian norms evident in recent case studies. Despite the notion that members of the scientific community act in accordance with the principles of universalism, communality, disinterestedness and so on, Mulkey suggests that there is now considerable evidence to show otherwise. Large numbers of scientists do not behave in accordance with these norms and accredited research findings can result from non-normative behaviour. One possible solution to this discrepancy was proposed by Merton himself.<sup>9</sup> Apparently

'deviant' behaviour in the scientific community can be accounted for by the existence of two sets of norms: 'norms' and 'counter-norms'. In a similar way to Merton, Mitroff had identified the existence of 'counter-norms' in a study of scientists engaged in research connected with the moon.<sup>10</sup> According to Mulkey, this identification relied heavily on the selection of parts of scientists' statements about themselves which appeared to contradict the original set of norms. However, Mulkey argues, the mere use of particular verbal formulations by scientists is no reliable indication of the existence of institutionalised norms. In addition, there is little to suggest that professional recognition rewards conformity to such norms. But if 'norms' and 'counter-norms' are not components of an institutionalised normative structure, what is the value of the evidence presented by Merton and Mitroff? Mulkey suggests that norms and counter-norms in fact represent

relatively standardised verbal formulations which are used by participants to describe the actions of scientists, to assess or evaluate such actions and to prescribe acceptable or permissible kinds of social action.<sup>11</sup>

The relationship between these verbal formulations and social interaction is certainly more complex than envisaged by those who took them directly indicative of institutionalised norms. Mulkey suggests that statements taken as indicators of norms in fact form part of an 'evaluative repertoire' which can be referred to and drawn upon by scientists according to the particular context in which its use is required and according to the particular interests of its users.

... the standardised verbal formulations to be found in the scientific community provide a repertoire which can be used flexibly to categorise professional actions differently in various social contexts and, presumably, in accordance with varying social interest. It seems to me misleading to refer to this diffuse repertoire of verbal formulations as the normative structure of science or to maintain that it contributes in any obvious way to the advance of scientific knowledge.<sup>12</sup>

The implied programme is one of relating scientists' use of this evaluative repertoire to their particular interests. At the same time, Mulkey notes that evaluative

repertoires are drawn on by scientists in discussions both within science and in the course of their interaction with non-scientists. It is likely that outsiders will find it particularly difficult to challenge the descriptions of science they are offered. This, according to Mulkay, accounts for some of the misleading descriptions of science passed on by sociologists interested in identifying norms prevalent in the scientific community.

A large part of Mulkay's discussion describes how scientists have depicted science in their attempts to justify a special relationship with government. In order to ensure that science was adequately financed, for example, differing scientific spokesmen emphasised the importance of basic values and attitudes (such as disinterestedness, impartiality and objectivity) in science. Although scientists have disagreed as to the political action most appropriate to sustain these basic features of science,<sup>13</sup> they have been more or less in agreement as to the aspects of science which they should bring to the attention of interested government agencies. The manner in which science is depicted, argues Mulkay, is another example of the selection by prominent scientists of features of science which will best serve their interests. The depiction of science which results, just like the description offered by scientists to non-participant sociologists, must not be taken at face value, but should be regarded as a description highly influenced by the particular social interests of participants and by the particular social context in which it is presented.

Two main points of difference between Wynne and Mulkay require comment. Firstly, although Wynne's study deals exclusively with written sources (scientific articles, reviews and so on), Mulkay bases his discussion on both verbal statements and written sources. Clearly, there is considerable variation in the style and content of these two types of material.<sup>14</sup> Nevertheless, both types constitute summaries or reports made by scientists. More importantly, for present purposes, both act as a resource in the construction of arguments by sociologists of science. For this reason I shall refer to both written and verbal material as accounts

throughout the rest of this paper.<sup>15</sup> Secondly, there are some differences in the nature of the reasons given by Wynne and Mulkay for the discrepancies they identify. For example, Wynne describes the effects of reviews written about Barkla's work. One such effect was the "backgrounding" of potentially disruptive disputes about the fundamental features of the dominant paradigm. It is unclear, however, to what extent Wynne supposes this effect was consciously intended by the authors of reviews. Mulkay, on the other hand, more clearly attributes intention to the users of evaluative repertoires. The general point here, however, is that Mulkay and Wynne are both concerned to offer explanations of the origins or causes of discrepant accounts. These explanations depend on the initial recognition and identification of discrepancies and consist of various sorts of reason derived from knowledge of the scientific community being studied.

In sum, there are several important similarities between the approaches adopted by Wynne and Mulkay when dealing with scientists' accounts. Firstly, they suggest it is possible to determine significant differences between accounts of science. Secondly, the accounts are not just different. They constitute either more or less correct versions of particular scientific circumstances or of relationships within the scientific community. Wynne did not merely identify differences between accounts. He regarded certain accounts as distortions or misleading characterisations. In a similar way, Mulkay did not argue that scientists' accounts of their behaviour may simply differ from any other account. For Mulkay, scientists' accounts can be incorrect. Thirdly, as a result of their potential inaccuracy, it is clearly implied that scientists' accounts should not be taken at face value. To rely on these accounts is to adopt a potentially misleading picture of scientists' activities. Fourthly, the source of error or bias in accounts can be explained with reference to the social location or vested interests of their authors. On the face of it, Wynne and Mulkay reach this position in rather different ways. Wynne starts by describing in detail the scientific circumstances surrounding Barkla's work and then contrasts this description with the version

offered by various scientific accounts. Mulkay, on the other hand, begins by considering the way in which various scientists' statements have been mistakenly taken as an accurate description of norms within the scientific community. He then ascribes various social interests to the genesis of these misleading statements. In fact, however, Mulkay's argument also stems from a discussion of various methodological problems encountered during a study of the emergence and growth of radio-astronomy.<sup>16</sup> In this earlier work, Mulkay noted a definite disjunction between prevalent notions, current among scientists, of circumstances surrounding the initial emergence of radio-astronomy and the situation as revealed by sustained empirical investigation. Similarly, there were found to be definite inconsistencies between the accounts of past scientific work volunteered by scientists and the versions arrived at by the investigators through careful evaluation of a variety of available evidence. Mulkay explained these inconsistencies in terms of the irrelevance for scientists of a historical perspective, or, put more charitably, by their confusion between scientific and historical accuracy.<sup>17</sup> In his most recent piece, however, Mulkay specifically nominates a rather different sort of reason for the discrepancy between scientists' statements and their actual behaviour. He suggests that scientists intentionally select statements according to particular social interests. In other words, statements are made so as to portray scientific activity in a way which will have beneficial consequences.

I have so far shown how Wynne and Mulkay adopt similar research strategies when dealing with scientists' accounts. Now I want to summarise some recent work of my own and to examine some difficulties which arose in following the strategy adopted by Wynne and Mulkay.

My initial reliance on a number of scientists' accounts posed various problems for my construction of an intellectual history of pulsar research.<sup>18</sup> The extent of variation between accounts seemed to necessitate a complex process of evaluation of their relative reliability and historical accuracy. It was thought that those accounts assessed most trustworthy would subsequently form the basis for a

definitive account of the pulsar discovery, while the remainder would be discarded. Attempts to reconstruct 'what actually happened' would thus involve the eventual rejection of a large proportion of the available data. In fact, the grounds for rejecting particular versions (or for using only those features common to a number of accounts) could not easily be made explicit and were, in any case, thought to be distinctly dubious. It was suggested, therefore, that the difference between accounts be retained and that emphasis on historical accuracy be modified.

If we focus on the difference between accounts instead of trying to reach a judgement of their relative validity, we have another means of presenting the development of a specialty. For we can regard these differences not as sources of possible "inaccuracy" or "distortion", but as potentially fruitful forms of data in themselves. Perhaps the very difference between two accounts can tell us about the essential process of the development of ideas.<sup>19</sup>

It was envisaged that a detailed examination be undertaken of the ways in which participants' reviews differed, both from one another and over time.

My first task was to describe the nature and types of variation between accounts and this work took up the bulk of the discussion in my earlier paper.<sup>20</sup> In particular, I suggested that a close description of differences between accounts could be achieved by identifying 'component features' of the discovery process and recording their inclusion and sequential location in a number of selected reviews, interviews and other statements. An examination of thirteen accounts of the initial discovery of pulsars revealed two related types of variation between them. Firstly, there was considerable difference in the inclusion or exclusion of component features which make up accounts of the discovery process. Secondly, there was considerable variation between accounts in the sequence in which component features were presented.

More specifically, the following characteristics of differences between accounts were noted. Firstly, certain types of accounts of the discovery provided only limited types of information. Secondly, it was limited in the sense that accounts which result neither from specific enquiries, nor from the questioning of apparent discrepancies by an observer, tended to give the impression of a relatively

straightforward progression through a series of logical steps, successive observations, ideas and interpretations leading to the discovery. The version of the discovery process which includes information gained in response to direct enquiry in interview contrasts with that based predominantly on accounts written for publication. Thirdly, the contrasts between accounts do not result solely from differing abilities of participant to recall what took place. Fourthly, the pursuit in interviews of specific points of contrast revealed the existence of periods of uncertainty during the investigations, and hinted at the way other experiments running concurrently affected participants' decisions to pursue the investigation of new phenomena. These aspects of the discovery process were not immediately apparent from published accounts.<sup>21</sup>

Like Wynne and Mulkey, I identified discrepancies between a number of accounts produced by scientists. Unlike them, however, I did not suggest that one account could be regarded as any more reliable or accurate than another. Nevertheless, my study had thus far followed in broad outline the approach adopted by Wynne and Mulkey. Having identified various differences between accounts my next task was to explain them. At this point I referred to the problem of precisely describing differences between accounts.<sup>22</sup> I presented extracts from six reports of the pulsar discovery in order to illustrate difficulties in describing differences between them. The reports were all written by the same participant, Professor Hewish, who together with his research student, Jocelyn Bell, was centrally involved in the pulsar discovery.<sup>23</sup>

A: ...the earliest record suitable for position measurement was obtained on August 13th 1967.

(9 February 1968)

B: The recordings were mostly as expected but one day in August Miss Bell noticed something odd...By the end of September (however) the same thing was clearly apparent on six different occasions...

(August 1968)

C: Towards the end of August (Bell) had plotted, on more than one occasion, what appeared to be a scintillating source which was conspicuous...it was not until November 28th that the first satisfactory observation was made. Pulses could be distinguished on this recording, which may be said to mark the discovery of pulsars, but it was only several days later, when the pulses had been confirmed, that the results were taken to be genuine.

(August 1968)

- D: Soon after we started in 1967 August, we noticed a rather peculiar looking signal...We made a faster recording of this source on December 4 1967, and it was clear we had something very strange in the radio telescope.  
(December 1968)
- E: Radio signals from the first pulsar were, in fact, obtained on 6th August although it was not until December 1967 that the unique pulsed character of the radiation was recognized and the latter date therefore marks the discovery.  
(January 1969)
- F: The discovery of pulsars dates from November 28th 1967 when the first pulses were recorded from the source now known as CP1919.  
(August 1970)

At first sight, it appeared there were a number of differences between extracts to be described. However, two considerations led me to realise that my problem was not so much how to describe the differences between these extracts but how to decide whether they were different. Firstly, when on a number of occasions these extracts formed part of my presentation of the original paper, I was interrupted by requests that I make clear whether I intended the extracts as different or not. Some members of my audience assumed that such a juxtaposition implied points of distinction but themselves saw no discrepancy between the extracts. I was told, for example, that although I might have intended to contrast these extracts, there was no real difference between them. In one sense, this reaction is not unexpected. By placing together six extracts in the above manner, it could be assumed I was legitimately claiming them all to have relevance to the topic that was the focus for discussion, that is, they were all about the pulsar discovery. At the same time, it was clear that the extracts were made up of rather different strings of words. The question therefore arose: do these different strings of words convey the same or different meanings in relation to the topic of pulsar discovery? Clearly, the juxtaposition of six extracts consisting of identical strings of words would be unremarkable; their similarity in meaning would be taken for granted. The question of their potential difference thus arises, on the one hand, because the extracts are lexically different and, on the other, because the extracts are placed together so as to invite comparison. Secondly, the general question of

whether or not accounts of the pulsar discovery were consistent also concerned participants themselves. Some participants were highly critical of an apparent 'delay' in the release of news of the first pulsar discovery. It was claimed, for example, that although the announcement was not made until February 1968, Cambridge radio astronomers had made the discovery 'in about a two month period up to September 1967'.<sup>24</sup> This, it was alleged, unfairly denied other radio astronomy groups opportunities to study the new phenomena and thus contribute to scientific knowledge through use of technical equipment better and more suitable to the task than that at Cambridge. Hewish, on the other hand, maintained that such allegations arose, among other reasons, from a 'genuine misunderstanding' of when the discovery was made.<sup>25</sup> Uncertainty as to the date of the discovery thus provided the focal point for much of this dispute and led to complaints about a lack of clarity in reports of the discovery. The following comment was made by Hoyle:<sup>26</sup>

Information concerning the observation of November 28th is still vague. Hewish does not make the situation at all clear, either in his introduction to the Nature Pulsar volume, or in his Scientific American article.<sup>27</sup>

These considerations made me realise that the determination of the difference or similarity between references to the discovery was as much an issue for members of my audiences and for participants in the controversy as for myself. By this I mean that each of these parties could quite competently render these extracts as the same or as different. It thus appeared that a number of ways of making sense of these extracts were available, both to participants and to others. In the above case, ambiguity in the meaning of the extracts became apparent precisely because different readings were accomplished by different readers. In principle, readers could always arrive at different readings of any one account. But despite this potential ambiguity, readers routinely arrive at similar readings. So it occurred to me that whether or not persons reach similar readings of the same account, they are always faced with the task of accomplishing a reading. The unhesitant, routine selection or accomplishment of one reading goes on all the time. This led me to

ask how readers carry out this task. What are the ways in which readers achieve, or arrive at any one particular reading of an account?

The formulation of this question represents a major point of divergence from the approach adopted by Wynne and Mulkey. The major interest for me at this stage was not whether the extracts actually were consistent or contradictory. Instead I was concerned to find out how extracts could be read as consistent or as contradictory. At the same time it did not seem so important to determine whether or not extracts actually had been read in one way or another. Rather I was interested in ways they could be read. Since it was not my intention to specify the readings which had been produced, it was obviously less possible to speculate why participants had chosen to read extracts in one way or another.

These points require considerable elaboration. In order to examine precisely how the new approach differs from the treatment of accounts outlined previously, I shall represent the new approach schematically. Before doing this, however, it is worth giving a bare outline of the way the analysis might begin.

One way to approach the above question is to make explicit one particular reading of the six extracts and then to examine the methods which readers might employ to accomplish this reading. Let us start with the reading that these extracts are contradictory. I have already indicated that a particular presentation of extracts can be interpreted as intending something unusual or remarkable about their content. The above juxtaposition of these extracts can be taken as saying that their comparison is at issue, so that their inconsistency becomes a possibility in the eyes of the reader.<sup>28</sup> A way of then reading the extracts as inconsistent is by using a metaphorical concept of discovery which takes a discovery to be a point occurrence in time. If any report of the discovery fails to nominate exact points in time, it can be said that the report is unclear as to when the discovery 'happened'. Alternatively, where exact dates are mentioned but not referred to as the discovery date, the report can also be read as unclear about when the discovery actually happened. It is this kind of reading which leads to the complaint about

a lack of clarity. The author of the quote above admits that November 28th has been specifically mentioned. But, he complains, it is unclear whether this refers to the actual discovery or to a mere observation. If individual reports of the discovery can be read as unclear, a collection of such reports can easily be read as contradictory. If, for example, both extract A and extract B are regarded as imprecise because they do not nominate a point in time when the discovery happened, it is possible to select a date for A and a date for B which each represent estimates of 'the discovery date' but which do not coincide. However, this selection of specific dates is not the only way to read the extracts as inconsistent. It may be sufficient to imply that their consistency is in doubt simply by drawing attention to the 'vagueness' and 'imprecision' of the extracts. For example, a determined barrister might use this technique to discredit the general trustworthiness of all the extracts. His cross-examination of the extracts' author might include the following:

You mention so many different dates in these extracts: 6th August, August 13th, December 4, November 28th. It is entirely unclear which you would have us believe is the correct date. You have referred to 'the end of September', 'one day in August', 'soon after we started in 1967 August' and 'not until December'. Are you really so unsure of your dates? What exactly do you want us to believe?....(etc.)

Interestingly, the author of the extracts alluded to this possible usage when asked whether he might in retrospect have reported the discovery differently.

Q: Is there no other way of putting it that you would have preferred?

A: If I had known there were nasty-minded people like Fred Hoyle around I would have adopted a more legal jargon perhaps, but one doesn't lead one's life on the assumption that it's populated with (....).<sup>29</sup>

Let us now consider the reading of the extracts as consistent. In interview, Hewish provided three main arguments for reading the extracts as consistent. Firstly, he claimed that an ability to understand the scientific issues involved was a prerequisite to seeing consistencies. When asked (somewhat blandly) for his comments on the six extracts, he was initially unclear what was

required but then himself posed the possibility that they were contradictory.

Are you as a sociologist puzzled by statements like that which seem contradictory? Is that why you've got them there?<sup>30</sup>

The argument here might be summarised as follows: any scientist worth his salt can tell that the extracts are consistent. The above response can thus be seen to be saying that persons other than scientists are precluded from making competent assessments of the consistency of the extracts. Sociologists may well be puzzled, according to this view, because they simply do not understand what a scientist would understand when he reads the extracts. Non-scientists are unable to draw upon specialised knowledge (available to the scientist<sup>31</sup>) which would allow them to see that the references 'actually are' consistent. In addition, the qualification 'worth his salt', allows for the incompetence of scientists not fully conversant with the specialised knowledge required to understand the consistencies. For example, a 'green' research student who had not assimilated sufficient expertise to see the consistencies could be excluded from the definition of 'any scientist worth his salt'.

This argument suggests that scientists may make more of the extracts than a lay reader. In particular, they may be able to draw upon knowledge of the ways observations are made so as to 'fill in' any apparent gaps and inconsistencies. For example, they may be able to recognise references to one kind of experiment or observation as 'in fact' meaning something rather different. In the same way, one might expect a scientist who reads the extracts as consistent to claim that the extracts are 'actually' about different aspects of the discovery and thus that their comparison is inappropriate.

Hewish's second, related, argument was that it was incorrect to treat the extracts in isolation from their original context. Furthermore, he argued, it was impossible to find out 'what happened' except by considering the accounts as a whole.

... if you want to find out what happened, you go and read the accounts. You don't pull little statements out of articles.<sup>32</sup>

According to this argument, extracts may be unrepresentative and are thus likely to lead to an incorrect or distorted picture of what really took place. With particular reference to the date of the discovery, this implies that there are other items of evidence in the rest of the report which give a fairer indication of when the discovery actually happened. Finally, Hewish argued that the determination of the exact day of the discovery was of no relevance to scientists.

...of course, it doesn't say on what day it happened.  
So what? No one was interested on what day it happened at this stage. Late November seems a perfectly reasonable thing to say.<sup>33</sup>

It just doesn't strike a scientist exactly when he makes a measurement as being very significant. It might be to the historian, but not to the scientist.<sup>34</sup>

These arguments for reading the extracts as consistent or as contradictory provide a number of clues to the sorts of methods employed in the accomplishment of readings. We have noted, for example, that the juxtaposition of extracts can be used to decide about their consistency. A metaphorical concept of discovery can be used to argue that the extracts lack clarity or that they are generally vague. On the other hand, readings of inconsistency can be discredited by pointing to the scientific incompetence of a reader. Competent readers may be able to fill in the missing information on the basis of shared knowledge about how science is done. In addition, readers may be able to read items in accounts as references to the discovery date even though no precise calendar date is mentioned.

Clearly, these considerations represent only a very preliminary attempt to examine methods employed in accomplishing readings. It is now necessary to specify more systematically the differences between the approach suggested here and that previously adopted when dealing with scientists' accounts.

Let me summarise schematically the main steps in alternative approaches to the use of scientists' accounts. In these schemes ' $X_i$ ' represents any individual account.<sup>35</sup> The first scheme represents the method adopted by a particularly

naive researcher.

- Scheme 1
- (i) Accounts  $X_1$   $X_2$  .....  $X_n$  purport to describe what happened.
  - (ii) Account  $X_1$  is the most reliable, accurate version of what happened.
  - (iii) A description/understanding of what happened can be arrived at by drawing preferentially upon  $X_1$ .

The implication of this scheme is that a large number of accounts (that is, all accounts except  $X_1$ ) are discarded because they are less reliable or accurate and would therefore produce a distorted picture of the actual events of interest.

It was suggested earlier that this be replaced by a modified scheme.

- Scheme 2
- (i) Accounts  $X_1$   $X_2$  .....  $X_n$  purport to describe what happened.
  - (ii) Account  $X_j$  differs from account  $X_k$ .
  - (iii) A number of reasons can be provided to explain the differences between accounts  $X_j$  and  $X_k$ .

Whereas scheme 1 depends on the positive identification of the most reliable account, this second scheme depends on the positive identification of a difference between accounts. There may be aspects in which accounts are thought to be more or less different from each other, but there exists at least some difference which might bear explanation. This scheme corresponds to the methods adopted by Wynne and Mulkay. For Wynne, and perhaps less obviously for Mulkay, one of the accounts ( $X_j$ ) corresponds to his own version of events derived from the use of a variety of evidence. Deviations from this version come in the guise of other accounts ( $X_k$ ) whose writing is explained in a                      of ways. I have already mentioned how Wynne explained the occurrence of misleading characterisations of Barkla's work. Mulkay explained statements giving rise to a 'Mertonian' depiction of science in terms of ideological interests of scientists. Hewish also gave reasons for the difference between his own version and that of one of his critics, largely in terms of the latter's incompetence.<sup>36</sup> In this scheme the major emphasis is on the adequacy of reasons provided for the perceived differences between accounts. As a result attention is deflected from the process by which differences are

recognised in the first place. When, in my own work, I realised that participants engage in this process in much the same way as sociologists, the task of recognising differences itself became of interest. This would seem to imply the following procedural scheme.

Scheme 3 (i) Accounts  $X_1$   $X_2$  .....  $X_n$  support to describe what happened.

(ii) Participants  $P_1$  .....  $P_m$  recognised accounts  $X_j$  and  $X_k$  as different (or the same).

(iii) A number of reasons can be provided to explain why these accounts were recognised as different (or the same).

There are two main difficulties with this scheme. Firstly, the procedure depends on the unequivocal identification of a participants' recognition of differences. In other words, a prerequisite to following this scheme is the provision of an incorrigible version of 'something having actually taken place', where the something is the recognition of accounts as different. We avoid having to specify that  $X_j$  and  $X_k$  'actually were' different but merely replace this with statements about participants 'actually having recognised differences'. It is possible to provide evidence that certain participants did recognise inconsistencies between accounts. It was partly for this reason that I introduced the comment by Hoyle. But my use of this comment depended on my reading it in a particular way, that is, I used the comment having decided that it demonstrated a complaint about lack of clarity. This may seem a reasonable reading of the comment. But to accept it as reasonable or otherwise merely neglects consideration of the very process which forms the main focus of interest in this scheme. It is the process by which readings are achieved which is of primary interest. The provision of an incorrigible version or reading as part of our argument only undercuts our major concern. The scheme therefore requires reformulation so that it is not necessary to produce a particular reading of some data as evidence of a participant's recognition of differences. The second difficulty hinges on the use of the word 'why' in part (iii). It is important that reasons given for a particular reading are in terms of the structure

of the accounts themselves. We need to ask what it is about the accounts themselves that enables a particular reading to be accomplished. This is so as to exclude reasons based on factors external to the accounts. The reason for this is that factors external to the accounts, such as the ideological interests of particular groups of scientists, themselves derive in a complex way from a series of readings of a variety of sorts of accounts. This is not to say that the use of any such reason is 'wrong' or 'inadequate'. The objective is rather to understand the ways in which such readings are achieved in the first place. Rather than ascribing motive or intention to a participant who is seen to produce a particular reading, the objective is to show how such a reading might be achieved. This does not attempt to answer why a participant should choose to read an account in this way.

In the light of these difficulties the procedural scheme requires modification as follows:

- Scheme 4. (i) Accounts  $X_1$   $X_2$  .....  $X_n$  support to describe what happened.  
 (ii) Account  $X_j$  and  $X_k$  could be read as different  
 (iii) By focussing on the structure of the accounts it is possible to show how  $X_j$  and  $X_k$  could be read as different.

In this scheme it is not necessary to commit oneself to a positive identification of an instance of the recognition of two or more accounts as contradictory. Whether or not a certain participant did recognise inconsistencies is not of central importance. It is the possibility of reading accounts as inconsistent which is important. In addition, the possibility of recognition need not be confined to participants. Finally, the analysis of how readings are done does not require the use of reasons connected with the reader's (or the writer's) motives, interests or intentions. This is mainly because ascription of motive to a reader itself depends on the accomplishment of certain readings of evidence or data which is taken to be about the reader.

In the last part of this paper I shall present brief comments on aspects of

a possible analysis entailed by the scheme just outlined. Firstly, however, it is necessary to slightly modify, yet again, the above procedural scheme for purposes of illustration.

All of the above procedural schemes contain an identical first line:

Accounts  $X_1$   $X_2$  .....  $X_n$  purport to describe what happened. It is clear, however, that to bring together a number of extracts and claim them to be 'about the same thing' is to do considerable interpretative work on the extracts even before the 'proper' analysis begins. In other words, the collection of reports on the same topic requires a number of readings of their sameness. Part of Hewish's earlier argument about the consistency of the extracts was that this reading of their sameness was mistaken. He thus claimed the extracts to be about different aspects of the discovery. For present purposes, however, it is irrelevant whether or not a reading of their sameness is correct. The point is that work has to be done to accomplish such a reading. The task of reading extracts as the same or different can therefore be legitimate topic for analysis.<sup>27</sup> However, such analysis would be fairly complex. It would have to focus on how readings of each extract are accomplished, to consider the various combinations in which comparison between readings could be done and finally specify the way in which comparison between specific combinations was done. For this reason my brief illustration will concentrate on one single account and on certain observations about utterances (or strings of words) within the account. The account chosen is Hewish's Nobel Lecture entitled 'Pulsars and High Density Physics'.<sup>38</sup>

The first section of the account is sub-titled 'The Discovery of Pulsars' (lines 1 - 159).<sup>39</sup> Clearly, the sub-title and the title together tell the reader what to read the account for. Without these headings and without line (1) it would not be immediately clear what the first fifty or sixty lines were about. The headings and first lines can thus be read as establishing the main concern of the paper, that is, it is about the discovery of pulsars. If the account was being narrated, it would seem inappropriate to interrupt at (12), for example, to raise

a question about the 'solar corona'. The reader should already have taken notice of particular devices which instruct him to read the first part of the account (up to (58), at least) as a 'lead-up'. The narrator could well rebuff a reader'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. In other words, the reader will be normally expected to have understood that the first section be read not for itself but as 'providing the background' or 'acting as a lead-in'. However, there are no explicit instructions to this effect in the text. It does not say, for example, 'The first fifty lines are simply intended as introduction'. It may therefore be the case that certain phrases and sentence formulations can be read as providing such instructions. Consider line (1): 'The trail which ultimately led to the first pulsar began in 1948 when I joined Ryle's small research team.....'. This can be read as performing a number of functions. Firstly, as noted already, this helps to reinforce the work done by the headings. It establishes the central topic of the account. Secondly, it indicates that the story to be told is not only about the discovery of pulsars but also about the author's involvement. The utterance draws attention to the coincidence of the start of the pulsar discovery trail and something that happened to the author. Thirdly, it indicates how the story of the pulsar discovery will be told. The idea of a 'trail' can thus be taken as a cue that a path of successive events is about to be described and that this path culminates, perhaps after some considerable time ('ultimately'), in the discovery. Fourthly, the notion of trail diverts attention from the social construction of the new phenomenon. The trail can be read as existing quite independently of those who followed it. It thus appears, that once the path was followed it was inevitable that, sooner or later, the discovery at the end of the path be reached.<sup>40</sup> In this way emphasis is placed on the suddenness of becoming aware that the phenomenon already existed, rather than on the process by which the reality of the new phenomenon was constructed.

The account appears to have a marked etiological structure: the story consists

of a long sequence of cause and effect. There appears to be no single suggestion or line of argument that does not lead to another; there are no dead ends and no *cul de sacs*. Once again, there are no explicit instructions that the account should be read in this way. Instead readers notice devices which tell them how the account is to be read. One such device is the author's use of the passive. For example:

....I was able to show..... (7)  
 My fascination.....brought me to the solar corona (10-11)  
 ....I was eventually able to trace.... (13)  
 In my notebook....there is a comment that.... (15)  
 This source later turned out to be..... (47-48)

The use of such formulations tend to give the impression that events were occurring quite independently of the author and that his involvement was relatively incidental to the on-going accumulation of knowledge. To get an idea of the importance of this device, different formulations can be substituted for those above. Whereas in other circumstances it might be quite appropriate, for example, to use 'I showed' instead 'I was able to show', the sustained use of the active form makes the present account read quite strangely.

As already noted, the account is organised as a recognisable sequence. The story starts in 1948 (1) and ends in 1974 (153). Clearly it is impossible to report everything that happened during this time. Yet the reader gets the impression that the available time is adequately filled. There is no obvious gap such that a reader could ask what had gone on between certain times without giving the appearance of 'pressing for details'. It appears that the fullness of the story depends in some complex way on the manner by which items or events are sequentially tied. This tying appears to be done so that the reader is not encouraged to enquire about the events themselves. In addition, activities are described so as to provide a reasonable occupancy of the time period to which they are allocated.

References to dates also prompt interesting questions. There are various references to years during the 'lead-up' section - '1948' (1), '1954' (15), 'eight

years later' (21), '1965' (42 and 49), 'Later that year' (54). Thereafter, however, dates are referred to in months and year (from 59). Every month is mentioned from 'July 1967' (59) through to 'January 1968' (127). The only references to exact day are 'November 28th' (84) and 'February 8th' (130). Why do dates need to be more precise on some occasions than on others? What does this do to the way the paper is read? What happens if we substitute more or less exact dates on each of the occasions dates are used? What exact dates would violate 'around the middle of August 1967' (71)? Do references like 'by the end of September' act more as sequencing or tying devices than attempts to specify exact dates?

These brief observations provide no more than a very tentative illustration of the kinds of issues and questions which might be addressed in the analysis of accounts outlined above.

### Summary and Discussion

I started by using studies by Wynne and Mulkey to outline the main features of an approach commonly used by sociologists when dealing with scientists' accounts. I then reported my own attempts to specify differences in scientists' accounts, focussing in particular on six extracts of the first pulsar discovery. I originally intended to follow the approach adopted by Wynne and Mulkey. This involved first specifying the nature of differences and then explaining them in terms of knowledge about the circumstances surrounding the pulsar discovery. However, it was not at all clear how I should decide whether or not the six extracts were inconsistent. In addition, participants' differing perceptions of accounts of the discovery figured prominently in a bitter dispute over the timing of the release of news of the discovery and, subsequently, over the award of the Nobel Prize to the leader of the discovery group. Clearly, some participants were able to read discovery accounts as consistent, others could read them as contradictory. It thus occurred to me that the methods used in reading accounts are themselves an

interesting and potentially fruitful topic for study. A preliminary survey of arguments for and against particular readings revealed a number of clues which could be used to shape an analysis of how readings are accomplished.

The distinguishing features of this alternative programme were detailed in a schematic portrayal of approaches to the analysis of accounts. In particular, it was shown that the main concern of the alternative programme is how possible readings could be accomplished rather than why certain readings had been made by participants. In other words, the alternative programme entailed an examination of the methods used to construct different readings rather than an investigation of the origins and causes of different readings known to have been made. Finally, a tentative attempt was made to outline some of the questions which might be tackled under the rubric of this alternative programme.

The relative advantages of the two programmes described in this paper require careful examination. Although this is beyond the scope of the present paper, I shall end with some suggestions.

The original programme comprises a series of steps, each of which involves various interpretations and inferences made on the basis of different items of data. For example, the characterisation of an account as 'misleading' requires the unequivocal reading of a contrast between at least two pieces of data. The further explanation of this difference necessitates the use of additional evidence which inevitably consists of further accounts; and these accounts themselves need to be recognised as relevant and interpreted so as to throw light on the perceived difference between accounts. Explanations for the genesis of a misleading account often consist of the ascription of motive or intention to participants. This ascription also involves a further complex process of interpretations of accounts used as evidence. It can thus be seen that throughout the construction of the final explanation, the sociologist relies heavily on assumptions about the adequacy of interpretations that he makes. At the same time, this approach neglects to examine the very processes of interpretation on which the sociologist relies.

In investigating the methods by which readings are accomplished, it appears that the alternative programme largely avoids assumptions about adequacy of interpretation. The investigation concerns the possibility of particular readings of data rather than the provision of incorrigible versions based on such readings. At the same time, however, the alternative programme seems to introduce problems concerning the relevance of its findings to my own original objective of understanding processes of scientific growth and development. Clearly, the alternative programme of analysis addresses questions quite distinct from those previously discussed. But it is less clear in what sense the results of the analysis will bear upon current problems in the sociology of science. To what extent is this programme of analysis indifferent to the topic of study? For example, in what way will the methods of accounting which emerge from this analysis be peculiar to scientists? Is it possible that the insights of this analysis can provide indirect confirmation of current hypotheses? Hopefully, these questions can be profitably answered through an elaboration of the preliminary analysis undertaken in this paper.

Notes

1. In particular, I am interested in the possibility of extending ideas in conversational analysis to the analysis of accounts. See, for example, H. Sacks, 'On the Analysability of Stories by Children' in R. Turner (ed.) Ethnomethodology (Penguin, 1974) and Sacks, with E.A. Schegloff and G. Jefferson, 'A Simplest Systematics for the Organization of Turn-Taking in Conversation', Language 50 (December 1974) p.696-735.
2. For example, Andrew McAlpine and Allan Bitz, 'Some Methodological Problems in the Comparative Sociology of Science', Manchester Business School, paper read at B.S.A. Sociology of Science Study Group, 1974. See also John Law, 'Theories and Methods in the Sociology of Science: An Interpretive Approach', Soc. Sci. Inform. 13 (4/5) (1974) pp.163-72; John Law and D. French, 'Normative and Interpretive Sociologies of Science', Sociological Review 22 (1974) pp.581-95.
3. Brian Wynne, 'C.G. Barkla and the J phenomenon: a case study in the treatment of deviance in physics', Social Studies of Science 6 nos.3/4 (1976) pp.3-7-47.
4. M.J. Mulkay, 'Norms and Ideology in Science', Soc. Sci. Inform. (forthcoming, 1976).
5. S.W. Woolgar, 'Writing an Intellectual History of Scientific Development: The Use of Discovery Accounts', Social Studies of Science 6 no.3/4 (1976) pp. 395-422.
6. Wynne, op cit. (note 3) p.333.
7. Ibid., p.330.
8. Ibid., p.332.
9. R.K. Merton, The Sociology of Science (University of Chicago Press, 1973), Chapter 18.
10. Ian I. Mitroff, The Subjective Side of Science (Elsevier, 1974).
11. Mulkay, op.cit., (note 4).
12. Ibid.
13. Mulkay, op. cit., (note 4).
14. Woolgar, op. cit., (note 5).
15. I am using 'accounts' simply as a generic term of reference and not in the sense intended by Scott and Lyman. M.B. Scott and S.M. Lyman, 'Accounts', Am. Soc. Review 33 (1968) pp.46-62.
16. M.J. Mulkay, 'Methodology in the sociology of science: some reflections on the study of radio astronomy', Soc. Sci. Inform. 13 (2) (1974) p.107-19. The study itself is reported at length in D.O. Edge and M.J. Mulkay, Astronomy Transformed (Wiley Inter-Science, 1976).

17. The argument is that scientists do not ordinarily think of past scientific episodes in terms of a detailed chronology of successive experiments, observations and results. Instead, they tend to reconstruct these episodes in terms of logical progression through a series of investigations, specifically emphasising those results most relevant to the current framework of knowledge and largely neglecting instances of error, confusion and failure.
18. The following is a much condensed version of arguments presented in Woolgar, op. cit., (note 5).
19. Ibid., p.399.
20. Ibid.
21. Ibid., p.414.
22. Ibid., p.399.
23. A. Hewish: A) Hewish et al., 'Observation of a Rapidly Pulsating Source', Nature 217 (24 February 1968), 709; B) 'Pulsars', Scientific American (October 1968), 25-35; C) 'The Discovery of Pulsars', in The Pulsating Stars 1 (London: Macmillan/Nature, 1968); D) 'Pulsing Radio Sources', Journ. Brit. Astron. Ass. 79 (1969), 178-90; E) 'Pulsars', Endeavour 28 (1969), 55-59; F) 'Pulsars', Highlights of Astronomy 2 (1971), 3-19.
24. F. Hoyle, letter to The Times (8 April 1975).
25. A. Hewish, interview (14 April 1975)
26. Hoyle's part in the emergence of radio-astronomy and his previous relationship with the Cambridge group are described in Edge and Mulkay, op.cit., (note 16).
27. F. Hoyle, private communication (30 April 1975).
28. Hewish himself provided confirmation that attention is drawn to the possibility of discrepancy by virtue of the juxtaposition of extracts. A. Hewish, interview (20 May 1976).
29. Ibid., my emphasis.
30. Ibid.
31. The notion that a body of knowledge belongs to a particular collectivity is itself analysed as a member's accomplishment in W.W. Sharrock, 'On Owning Knowledge' in R. Turner, op.cit., (note 1).
32. Hewish, op. cit. (note 28).
33. Ibid.
34. Ibid.
35. For purposes of presentation, the single most accurate account is referred to here.  $X_i$  could also refer to a set, or select group, of accounts, all of which are regarded as more reliable than others.

36. Hewish, op. cit., (note 25).
37. I started by specifying similarities and differences between methods adopted by Wynne and Mulkay. The manner in which I read these studies as similar can also be taken as a topic for analysis. This is not to enter a spiral of analytic relativism, as if often supposed. It is simply to shift the analytic focus from making an incorrigible reading of an account to asking how such a reading is made.
38. A. Hewish, 'Pulsars and High Energy Physics' in Les Prix Nobel En 1974 (The Nobel Foundation, 1975) pp.69-79. The first section is reproduced in the Appendix.
39. Numbers in brackets refer to lines in the account as reproduced in the Appendix.
40. Similar metaphors picture the scientist as pulling back a curtain to reveal extant truths or as embarking on a voyage in an ocean populated with ideas waiting to be discovered. See, for example, G. Hotton, 'Models for Understanding the Growth and Excellence of Scientific Research', in S.R. Graubard and G. Hotton (eds.), Excellence and Leadership in a Democracy (New York: Columbia Univ. Press, 1962).

Appendix (see note 38).

# PULSARS AND HIGH DENSITY PHYSICS

Nobel Lecture, December 12, 1974

by

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## 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 propagation 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.<sup>(1)</sup>

10           My fascination in using extra-terrestrial radio sources for studying the  
11 intervening plasma next brought me to the solar corona. From observations 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.<sup>(2)</sup>

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 sufficient  
17 coherence to produce interference patterns at the Earth which would be detectable  
18 as a very rapid fluctuation of intensity. Unfortunately the information then  
19 available showed that the few sources known were more than one hundred times too  
20 large to produce this effect, and I did not pursue the idea. This was sad because  
21 the phenomenon was discovered by chance, about eight years later, by Margaret Clarke  
22 long after I had forgotten all about my comment. She was involved with a survey  
23 of radio sources at Cambridge and noticed that three particular sources showed  
24 variations of intensity. She pointed out that two of the sources were known to have

angular sizes of less than 2" and estimated that a scintillation mechanism required plasma irregularities at distances of thousands of km but she concluded that the fluctuations were an unsolved mystery.<sup>(3)</sup> During a group discussion I suddenly remembered my earlier conclusion and realised that, if the radio sources subtended an angle of less than 1", they might show the predicted intensity scintillation by plasma clouds in the interplanetary medium. With the assistance of Scott and Collins special observations of 3C 48 and other quasistellar radio sources were made and the scintillation phenomenon was immediately confirmed.<sup>(4)</sup>

Since interplanetary scintillation, as we called this new effect, could be detected in any direction in space. I used it to study the solar wind, which had by then been discovered by space probes launched into orbits far beyond the magnetosphere. It was interesting to track the interplanetary diffraction patterns as they raced across England at speeds in excess of  $300 \text{ km s}^{-1}$ , and to sample the behaviour of the solar wind far outside the plane of the ecliptic where spacecraft have yet to venture.<sup>(5)</sup>

The scintillation technique also provided an extremely simple and useful means of showing which radio sources had angular sizes in the range  $0''.1-1''.0$ . The first really unusual source to be uncovered by this method turned up in 1965 when, with my student Okoye, I was studying radio emission from the Crab Nebula. We found a prominent scintillating component within the nebula which was far too small to be explained by conventional synchrotron radiation and we suggested that this might be the remains of the original star which had exploded and which still showed activity in the form of flare-type radio emission.<sup>(6)</sup> This source later turned out to be none other than the famous Crab Nebula Pulsar.

In 1965 I drew up plans for a radio telescope with which I intended to carry out a large-scale survey of more than 100 radio galaxies using interplanetary scintillation to provide high angular resolution. To achieve the required sensitivity it was necessary to cover an area of  $18,000 \text{ m}^2$  and, because scintillation due to plasmas is most pronounced at long wavelengths, I used a wavelength of 3.7 m. The final design was an array containing 2048 dipole antennas. Later that year I

55 was joined by a new graduate student, Jocelyn Bell, and she became responsible  
56 for the network of cables connecting the dipoles. The entire system was built  
57 with local effort and we relied heavily upon the willing assistance of many members  
58 of the Cambridge team.

59 The radio telescope was complete, and tested, by July 1967 and we immediately  
60 commenced a survey of the sky. Our method of utilising scintillation for the  
61 quantitative measurement of angular sizes demanded repeated observations so that  
62 every source could be studied at many different solar elongations. In fact we  
63 surveyed the entire range of accessible sky at intervals of one week. To maintain  
64 a continuous assessment of the survey we arranged to plot the positions of  
65 scintillating radio sources on a sky-chart, as each record was analysed, and to  
66 add points as the observations were repeated at weekly intervals. In this way  
67 genuine sources could be distinguished from electrical interference since the  
68 latter would be unlikely to recur with the same celestial coordinates. It is  
69 greatly to Jocelyn Bell's credit that she was able to keep up with the flow of  
70 paper from the four recorders.

71 One day around the middle of August 1967 Jocelyn showed me a record indicating  
72 fluctuating signals that could have been a faint source undergoing scintillation  
73 when observed in the antisolar direction. This was unusual since strong  
74 scintillation rarely occurs in this direction and we first thought that the  
75 signals might be electrical interference. So we continued the routine survey.  
76 By the end of September the source had been detected on several occasions, although  
77 it was not always present, and I suspected that we had located a flare star,  
78 perhaps similar to the M-type dwarfs under investigation by Lovell. But the  
79 source also exhibited apparent shifts of right ascension of up to 90 seconds  
80 which was evidence against a celestial origin. We installed a high-speed recorder  
81 to study the nature of the fluctuating signals but met with no success as the  
82 source intensity faded below our detection limit. During October this recorder  
83 was required for pre-arranged observations of another source, 3C 273, to check

84 certain aspects of scintillation theory, and it was not until November 28th that  
85 we obtained the first evidence that our mysterious source was emitting regular  
86 pulses of radiation at intervals of just greater than one second. I could not  
87 believe that any natural source would radiate in this fashion and I immediately  
88 consulted astronomical colleagues at other observatories to enquire whether they  
89 had any equipment in operation which might possibly generate electrical interference  
90 at a sidereal time near  $19^h 19^m$ .

91 In early December the source increased in intensity and the pulses were  
92 clearly visible above the noise. Knowing that the signals were pulsed enabled  
93 me to ascertain their electrical phase and I reanalysed the routine survey records.  
94 This showed that the right ascension was constant. The apparent variations had  
95 been caused by the changing intensity of the source. Still sceptical, I arranged  
96 a device to display accurate time marks at one second intervals broadcast from the  
97 MSF Rugby time service and on December 11th began daily timing measurements. To  
98 my astonishment the readings fell in a regular pattern, to within the observational  
99 uncertainty of 0.1s, showing that the pulsed source kept time to better than 1 part  
100 in  $10^6$ . Meanwhile my colleagues Pilkington, and Scott and Collins, found by  
101 quite independent methods that the signal exhibited a rapidly sweeping frequency of  
102 about  $-5 \text{ MHz s}^{-1}$ . This showed that the duration of each pulse, at one particular  
103 radio frequency, was approximately 16 ms.

104 Having found no satisfactory terrestrial explanation for the pulses we now  
105 began to believe that they could only be generated by some source far beyond the  
106 solar system, and the short duration of each pulse suggested that the radiator could  
107 not be larger than a small planet. We had to face the possibility that the signals  
108 were, indeed, generated on a planet circling some distant star, and that they were  
109 artificial. I knew that timing measurements, if continued for a few weeks, would  
110 reveal any orbital motion of the source as a Doppler shift, and I felt compelled  
111 to maintain a curtain of silence until this result was known with some certainty.  
112 Without doubt, those weeks in December 1967 were the most exciting in my life.

It turned out that the Doppler shift was precisely that due to the motion of the Earth alone, and we began to seek explanations involving dwarf stars or the hypothetical neutron stars. My friends in the library at the optical observatory were surprised to see a radio astronomer taking so keen an interest in books on stellar evolution. I finally decided that the gravitational oscillation of an entire star provided a possible mechanism for explaining the periodic emission of radio pulses, and that the fundamental frequency obtainable from white dwarf stars was too low. I suggested that a higher order mode was needed in the case of a white dwarf, or that a neutron star of the lowest allowed density, vibrating in the fundamental mode, might give the required periodicity. We also estimated the distance of the source on the assumption that the frequency sweep was caused by pulse dispersion in the interstellar plasma, and obtained a value of 65 parsec, a typical stellar distance.

While I was preparing a coherent account of this rather hectic research, in January 1968, Jocelyn Bell was scrutinising all our sky-survey recordings with her typical persistence and diligence and she produced a list of possible additional pulsar positions. These were observed again for evidence of pulsed radiation and before submitting our paper for publication, on February 8th, we were confident that three additional pulsars existed although their parameters were then only crudely known. I well remember the morning when Jocelyn came into my room with a recording of a possible pulsar that she had made during the previous night at a right ascension  $09^{\text{h}} 50^{\text{m}}$ . When we spread the chart over the floor and placed a metric rule against it a periodicity of 0.25s was just discernible. This was confirmed later when the receiver was adjusted to a narrower bandwidth, and the rapidity of this pulsar made explanations involving white dwarf stars increasingly difficult.

The months that followed the announcement (7) of our discovery were busy ones for observers and theoreticians alike, as radio telescopes all over the world turned towards the first pulsars and information flooded in at a phenomenal rate. It was Gold (8) who first suggested that the rotation of neutron stars provided the simplest and most flexible mechanism to explain the pulsar clock,

144 and his prediction that the pulse period should increase with time soon received  
145 dramatic confirmation with the discovery of the pulsar in the Crab Nebula (9, 10).  
146 Further impressive support for the neutron star hypothesis was the detection of  
147 pulsed light from the star which had previously been identified as the remnant  
148 of the original explosion. This, according to theories of stellar evolution, is  
149 precisely where a young neutron star should be created. Gold also showed that the  
150 loss of rotational energy, calculated from the increase of period for a neutron  
151 star model, was exactly that needed to power the observed synchrotron light from  
152 the nebula.

153 Now, in 1974, with more than 130 pulsars charted in the heavens, there is  
154 overwhelming evidence that the neutron star "lighthouse" model is correct. No  
155 other star could spin fast enough, without fragmenting, to account for the most  
156 rapid pulsars yet periods ranging from 33 ms to 3.5s are readily accommodated  
157 by the rotation theory. At the same time there is unfortunately no satisfactory  
158 theory to account for the radio emission generated by these tiny stars which have  
159 radii of only 10 km.