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4 Accounting for error

The biochemists we interviewed, in talking about their research, devoted much effort to distinguishing between scientific truth and error in their field, and to explaining why particular scientists had adopted correct or incorrect theoretical positions. Their talk also contained 'good reasons' why they should pay particular attention to truth and error. For instance, they pointed out that when there are two or more competing 'theories' available in a given area of investigation, each will usually lead to the design of quite distinctive experiments. Accordingly, if a scientist is to do satisfactory experimental work, and all our respondents had published experimental results, it is of crucial importance that he chooses the theory which is most nearly correct. Similarly, in devising his experiments, he must decide on the adequacy of others' observational claims, because acceptance of some claims rather than others will have a direct bearing on what experiments should be undertaken next and what results one should expect to achieve.

During interview talk, then, these scientists regularly identified each others' scientific positions and took note of each others' theoretical and experimental errors. In addition, they often tried to account for their colleagues' errors, that is, they provided versions of participants' actions and beliefs which made these errors readily understandable. In this chapter, we will focus on our respondents' attempts to account for theoretical errors. It is probable that retrospective accounts, including accounts of error, occur more frequently in interviews than in 'ordinary' informal talk among scientists. Nevertheless, it is clear from studies of numerous types of conversational data that the orderly reconstruction of past action and belief, exemplified in this chapter by scientists' accounts of error, is a recurrent feature of ordinary talk. Silverman, in particular, has drawn attention to the similarity between interviews and informal conversations in this respect.

> A final feature of interviews that I want to address arises in a common characteristic of talk: in their accounting activities members concern themselves with displaying what will currently be understood as rational grounds for past actions and as rational explanations of past social

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> scenes, i.e. they seek to display their purported 'sensible' and 'reasonable' character. Furthermore, this sensible character is found in what 'finally' is seen to transpire - so that, for all practical purposes, *the meaning of the past is found in the present*. [Emphasis added]'

The present outcome of past action in relation to which scientists tend to organise their rational reconstructions is overwhelmingly the scientific correctness of the speaker's own current intellectual position. This feature is so widespread in our data that we propose it tentatively as the fundamental principle of social accounting in science. This does not mean, however, that each scientist's present intellectual position provides a fixed reference point for the construction of his discourse. For participants' scientific views, like their other interpretative resources, are continually reformulated in the course of ordinary talk and textual production. Thus the principle proposed above means that each scientist organizes his accounts of action and belief in ways, appropriate to the particular interpretative context, which explain, justify and validate the version of his scientific position furnished in a specific passage of talk or in a particular unit of discourse. We will see that in the present chapter this principle applies consistently in the case of scientists' accounts of error.

Our aim in the rest of this chapter is to document the recurrent interpretative features which appear in passages where scientists are making sense of theoretical error. We will try to identify the particular features of scientists' reconstructions whereby the occurrence of scientific error is made understandable. Although our data are primarily taken from interviews, we will also provide some indication that the same interpretative form is used by scientists in other kinds of discourse.

Some examples of accounting for error

In this section, we offer some instances of the kind of interpretative work by scientists with which we are concerned in this chapter. We ask the reader to examine them carefully and to reach his own preliminary conclusions about their interpretative structure before moving on to consider our analysis. After we have identified in the next section what we take to be the main features of such accounts, we will proceed to examine further examples in greater detail and to extend the analysis throughout the rest of the chapter.

> 4A
> I had no axe to grind. It's an advantage not being able to contribute in the theoretical sense. I mean, you don't feel that you have time and publications and reputation based on previous contributions and it's very easy to go the way the evidence seems to point. It leads to more flexibility.

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> People like Gowan and Fennell especially and Milner, certainly had many publications and they discussed one theory as they went along and they had a lot invested in that field and I think they were psychologically a little bit reluctant to follow the lead of - utterly new, strange and different coming from somebody else completely. Certainly that remains the case with Pugh. Miller, 22]

> 4B
> *This* is the effect of removing membrane potential. Now we ask what happens if we *now* prevent, within *this* system, prevent a hydrogen ion accumulation inside, when we don't think we can have any membrane potential. Now you will have, you will have people, particularly people at [a particular university], who will give you absolute hell about those experiments. But the people at [that university] are wrong. The people at [that university] are wrong because they are too damned dogmatic. They think this is an insuperable barrier to the chemiosmotic theory or at least it is beyond the range that's acceptable to the chemiosmotic theory. And that's no way to do science. The facts are

pretty clear experimentally and these people are sort of misquoting the fact. [Southgate, 20-1]

4C

Fennell had become quite an influential person actually and he was Professor of Biochemistry and very much an anti-Spencer man. I always remember when I was a post-doc that Fennell came down to give a talk about why the Spencer scheme was wrong and it was just a load of nonsense, you know. It really was and I remember it made me so angry. I remember having violent arguments with him. Of course, Snow [the speaker's supervisor] couldn't understand my arguments at all and certainly Fennell didn't, because he was putting up such ridiculous things. I might say that now Fennell believes in the Spencer scheme. But he'd built his whole career up on opposing it and he believed in the chemical intermediate hypothesis . . . in the case of people like Fennell, who were forceful people in bioenergetics, they didn't really understand the . . . simple thermodynamics, really. They didn't really understand it and because they were forceful people the controversy built up. [Grant, 23-4]

4D

That was another strength of the [chemiosmotic] theory. You could take somebody else's experiments and they could be entirely reinterpreted in a way which was more simple than the one offered by the authors... But Waters didn't believe any of it. None of it. He'd been brought up with the chemical theory. He'd made several contributions to that. He'd interpreted all his work on [a particular reagent] in terms of it, in a complicated way. He was a great friend of Watson's. He knew Gowan. It was America anyway. The chemiosmotic theory, as far as he was concerned, was a little bit of a joke. Perhaps an irritating one. And there was this damn Englishman interpreting everything. [Barton, 11]

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4E

There is, to my way of thinking, not a single piece of evidence that will bear close examination for the Spencer model. And the *crucial* piece of evidence - Ditchburn has written in the last *TIBS* on the membrane potential and he has looked for that membrane potential for the last 15 years. There is no membrane potential, period. That's a source of embarrassment to everybody, because the Spencer model requires it. He's never seen it and they have pilloried him. Everybody is looking under the bed: did you do this? And did you do that? And he goes back each year and he does all the controls that they claim he should do, and he does them. And he still gets the same answer - it isn't there. Now they say, well

- it's like religion. People don't know *why* they believe certain things. They believe them. Their fathers believed them. Their mothers believed them. So they believe them. Its purely irrational now. There's no-one I know can make a reasoned case for the Spencer model at the present time. [Pugh, 21]

4F

I do think it was the little grey - first let me preface this by saying I am a good friend of Spencer's. . . So I don't choose to say anything - I just try to give you the facts as I see them and I do think that [Spencer's] little grey books, never having to go through a review, were much more extensive and comprehensive than you could have got into the literature. And I do think that they stimulated a tremendous number, well a lot of students, who really pushed the hypothesis and created an aura of fact, when there might not have been fact. You know, the group at [a particular university], Richardson, Crosskey and Burridge, did a tremendous amount to promote the idea, without ever questioning the things that Milner or myself or Lucas might have questioned about - 'Is it right?' Instead, they took the stoichiometry, ratios of hydrogen ions to oxygen, as OK and they were not OK. And things like that. But I do think that a lot was based upon the fact that the grey books were so comprehensive and well written. Spencer writes well, no doubt about it. . . I do think that for ten years the very strong support was forthcoming without coming down to the *critical* issues. [Gowan, 2]

4G
I think that there was just a tendency for people to try to give the impression that they were right. And a lot of us found that they were betraying us, you know, that they were really being very dogmatic about their views and they had very strong personalities and they were wrong. I think that that's one of the things that I probably discovered at an early enough age to where I could reorient my whole way of approaching things and not worry about what these people were saying and in fact attack them every chance I got and really to try to cut them to pieces to make them get down to just how you can say such and such. Where is the data for this? How can you exclude this? And then you found out that

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some of them had hearing problems. Perry could never hear what I had to say. He always had a hearing problem every time I asked him a question at the meetings. [Carless, 27]

4H
We usually use enzyme that's been depleted before we make the measurements. There are lots of things you have to take into account and there are very strong individuals in the field who want to interpret everything in terms of their theories. Of course, those are the other guys, not us. We're interpreting it even, balanced *[general laughter]*. The other ones are the ones who are doing that. When you try and bend the data like that sometimes you don't take into account everything, too. Its complicated. There are lots of unknown factors still to be discovered. [Hargreaves, 51]

The asymmetrical structure of accounts of error

During each of these passages the speaker (a) identifies the views of one or more scientists as mistaken and (b) provides some kind of account which enables us to understand why the scientist(s) adopted an incorrect theory or failed to accept a correct theory. Any passage which displays these two features is an example of 'accounting for error'. In all the passages above, the speaker's own view is taken to be synonymous with the correct scientific view. However, different speakers endorse different, and sometimes apparently diametrically opposed, positions. The first six quotations focus fairly clearly upon specific theories, in that they refer explicitly to the chemiosmotic or chemical intermediate hypotheses, to Spencerian or anti-Spencerian views, or to scientists who are frequently cited as advocates of specific theoretical positions. If we identify speakers' positions in these passages in relation to the chemiosmotic hypothesis, it appears that 4A, 4C and 4D are pro-chemiosmosis, that 4E and 4F are anti-chemiosmosis, and that 4B is difficult to categorise. Although 4G and 4H are rather more general in character, they also distinguish unequivocally between the speaker's correct view or scientifically proper research strategy, on the one hand, and a loosely defined collection of false views, on the other hand. Thus all these passages involve a marked contrast between correct and incorrect views of the phenomena of oxidative phosphorylation.

Another feature of these accounts is that speakers link the correct view directly to experimental evidence. In the sense in which we have used the word 'empiricist' in the previous chapter, each respondent presents his own position in empiricist terms. Each speaker presents his theoretical Position as an unmediated expression of the natural world, in so far as that

world has revealed itself in the findings of controlled experiments. For example, Miller says that because he had no axe to grind, it was 'very easy to go the way the [empirical] evidence seems to point' (4A). Similarly, Southgate begins his passage with an emphatic statement of what is shown to be the case experimentally and he states later that 'the facts are pretty clear experimentally', even though other scientists are unable to recognise them (4B). Barton in quotation 4D, having previously reviewed the more obvious experimental basis for the chemiosmotic theory, goes on to maintain that that theory is actually confirmed by what other people have (mis)interpreted as counter-evidence (4D). Pugh and Gowan, although they display totally different theoretical commitments to those of the previous speakers, also base their theoretical contentions directly on a personal reading of the empirical evidence which they present as if it were unproblematic. Pugh does this more dramatically, claiming that there is not a shred of evidence in favour of chemiosmosis and that one of the major constituents of the theory does not exist: 'There is no membrane potential, period' (4E). Gowan is more restrained. Nevertheless, he fits the general pattern in organising his account as if he had privileged access to the empirical world. Thus, the chemiosmotic theory was based on 'an aura of fact, when there might not have been fact'. Similarly, the theoretical claims of the chemiosmotic theory about stoichiometries and other matters are treated as being simply incorrect (4F). Other speakers draw attention to the importance of basing theoretical claims on the data and the widespread failure on the part of other scientists to do this (4G); or to the intellectual confusions characteristic of those who did not see the realities of the natural world with the accurate perception of the speaker (4C).

Although these speakers in aggregate are advancing a wide variety of conflicting views about a fairly narrow range of biochemical phenomena, in these passages they all speak as if their own position is an unproblematic and unmediated re-presentation of the natural world. In contrast, the actions and judgements of those scientists who are depicted as being or as having been in error are characterised and explained in strongly contingent terms. Their false claims about the natural world are presented as being mediated through and as understandable in terms of various special attributes which they possess as individuals or as certain kinds of social actor. For instance, scientists are presented as being in error because they are 'strong individuals who want to interpret everything in terms of their theories' and who, consequently, 'bend the data' (4H). Alternatively, they are characterised as 'strong personalities' (4C,4G), 'dogmatic' (4B,4E) and inclined to avoid awkward questions (4G), as being misled by publications which had not been subject to proper refereeing (4F), as

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irrational (4E), or as having too much invested in a theory to give it up (4A,4C). Even something as superficially irrelevant as being in America can be cited as a reason why a particular scientist got it wrong (4D). As we have seen in the previous chapter, the depiction of a scientist's actions in contingent terms does not in itself prevent those actions from appearing scientifically proper or from being associated with correct belief. Scientists do not necessarily undermine their accounts of laboratory practice, for example, by couching them in this manner. In accounts of error, however, the contingent representation of scientists' actions and beliefs is organised in such a way that it effectively removes the beliefs in question from the realm of scientific legitimacy. As Southgate puts it: 'that's no way to do science'. In other words, accounts of error are typically organised in a manner which not only explains scientific error by linking it to various 'non-experimental' factors, but in so doing explains it away.

In the passages above, these references to contingent factors are presented as if they explain, even though they do not spell out in detail, how it is that other scientists reached wrong conclusions. This is done through the employment of both the empiricist and the contingent repertoire within accounts which have an asymmetric structure; that is, the speaker's own empiricist speech is given interpretative precedence and provides an unquestioned context in relation to which other scientists' claims are to be classified, explained and repudiated. The speaker's presentation of his own views as identical with the discernible realities of the natural world furnishes the only viable, properly scientific frame of reference, in relation to which others' divergent views have to be taken as clearly false and in need of explication. To put this another way, each speaker who formulates his own position in empiricist terms, when accounting for error, sets up the following interpretative problem: 'If the natural world speaks so clearly through the respondent in question, how is it that some other scientists come to represent that world inaccurately? What is it about such speakers which prevents the natural world from representing itself properly in *their* speech?' This implicit question is resolved in accounts of error by the assertion that the views of these other scientists are being distorted by the intrusion of non-scientific, that is, non-experimental, influences into the research domain. The lexicon of the contingent repertoire is used to identify non-experimental factors which are probably mentioned regularly in the ordinary small talk of science and which can account plausibly for deviations from scientific accuracy. Thus the introduction of the contingent repertoire resolves the speaker's interpretative dilemma by showing that the speech of those in error, although it is not fully scientific, is easily understood in view of 'what we all know about' the typical limitations of scientists as fallible human

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beings. In accounts of error, then, the empiricist versions of correct belief provide instructions for the interpretation of contingent elements. Because contingent factors are mentioned only in the case of false belief, because they are directly contrasted with the purely experimental basis of the speaker's views and because their power to generate and maintain false belief is taken as self-evident, the contingency of scientists' actions and beliefs is made to appear anomalous and as a necessary source of, as well as an explanation of, theoretical error.

So far in this chapter, we have tried to give the reader an opportunity to scrutinise some of our data for himself and then to indicate in broad terms the kind of recurrent interpretative structure which we suggest is evident in that material and in the passages to be presented below. We will now explore some specific passages in more detail and begin to provide a firmer basis for our analysis of accounting for error. We should perhaps make it quite clear that terms like 'correct belief' and 'error' are intended to convey our understanding of particular respondents' statements, as expressed in interview transcripts, letters and papers, about the validity of their own and other biochemists' scientific views. They do not refer to *our* assessments of the biochemical knowledge-claims under discussion by participants.

The flexibility of accounting

A regular pattern in our biochemists' accounts of error, which we have already observed above (4B, 4C and 4F), is that the speaker contrasts his own experimentally based scientific appreciation with other researchers' 'failure to understand the issues' and then goes on to explain this failure by referring to various social and/or psychological characteristics of those concerned. The following quotation, from a relatively young researcher who described himself as having favoured Spencer's chemiosmotic ideas since he first entered the field, provides another example.

> 4J
> I was just one of a number of people who were working with these new ideas. It just seemed that everything that we did could be explained satisfactorily by Spencer's theory . . . So we said, if this idea is right then we ought to be able to show such and such a thing, and we would go ahead and do it and it would work... Maybe in some ways we were a little bit dogmatic. I found it very interesting because, as a Ph.D. student, I was meeting guys like Gowan and I was able to say to them. 'No, you've got this wrong. This can be explained much more easily in this manner.' I found that people like Gowan didn't really understand what was going on, in terms of this hypothesis. We were just able to explain a lot of things

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> and do a lot of things, all in terms of Spencer's theory. It was only later that we started to get a little bit more critical of it. . . My impression was that there was certainly a lot of prejudice involved. Gowan is a good example because he was at the forefront in those days, a very important man. He'd done a lot of good work in the 1950s and he'd got his own models of energy coupling. I think he was probably quite defensive about those ideas. So he was reluctant to accept the chemiosmotic hypothesis in the first place. But not only that, I think he was also reluctant to put effort into understanding the details of it. It was fairly complicated . . . Gowan definitely didn't understand it . . . He is a brilliant man and there is nothing there that he wasn't capable of understanding. I just don't think he was prepared at that time to put the effort into it, because of his earlier prejudices. [Crosskey, 4-5]

This kind of account is echoed by another advocate of Spencer's ideas from the laboratory where the researcher quoted above was trained.

> 4K
> I really only started to take things seriously when we started working on ion transport and then it became increasingly obvious that there was an economy in the chemiosmotic hypothesis describing what was going on which went right across the range of what we

> were doing... so that one became convinced that this really was more likely than the other thing. . Now the thing which convinced the world, or began to stun the world into taking notice of the Spencer hypothesis was that experiment in which he takes anaerobic mitochondria and adds a pulse of oxygen. Under those circumstances there is an ejection of protons. . . Gowan devoted himself to showing that the protons were ejected too slowly to be associated with the respiratory chain in the way in which Spencer had said. This was just to try and suppress the chemiosmotic hypothesis from another direction. But Gowan in fact never understood that hypothesis. This was very, very obvious to anyone who talked to him. He had such a dislike of it that he never bothered to think through what the consequences would be. [Burridge, 8,10]

A main point made in these passages is that Gowan got it wrong, he continued to accept erroneous ideas, because he never properly understood the chemiosmotic hypothesis. It seems to be suggested that anybody who *did* understand the theory would necessarily have accepted it. Because the correctness of the speaker's theory is taken for granted in the organisation of the account, any failure on the part of other scientists to accept that theory *must* be due to some misunderstanding. The other's failure to understand is then traced back to the action of various 'non-scientific' factors, such as undue commitment to his own model of energy coupling, a defensive attitude, prejudice, dislike and failure to put in enough effort. Thus these two accounts, like those examined above, are

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organised in a manner which displays how the speaker's theoretical conclusions were a simple, unmediated response to the evidence, whereas those of their opponent were influenced by extraneous or non-experimental considerations.

It is possible, in principle, that accounts 4J and 4K are closely similar because Gowan actually did not understand chemiosmosis and was defensive, prejudiced and unwilling to bother seriously with it. If one adopts this reading, there is nothing of general sociological interest about these accounts. They simply report the way things were in this specific case. However, this position cannot be held consistently with respect to *all* the accounts we have, because the accounts of the *same* actions offered by different scientists often appear to be incompatible, because the same individual can formulate significantly different accounts in different passages and because acceptance of our complete collection of accounts of error would lead to the awkward conclusion that virtually every contributor to the field, and every major contributor without exception, was scientifically incompetent and affected by non-scientific factors.

It is not possible, then, to accept at face value all the accounts of error in our material. But this is hardly surprising, for common sense tells us that scientists are likely to be sensitive about their errors and that, as a result, some of their interpretations will probably be affected by the desire to present a favourable self-image. However, neither common sense nor sociological method provide us with a way of sorting out the reliable from the unreliable accounts. As we pointed out in chapter one, not only have we no independent criteria to enable us to distinguish biased from unbiased respondents, but the testimony of each speaker generates an unending series of interpretative problems for the analyst who seeks to build up an accurate picture of what has happened in the research community.

Reconsideration of quotations 4J and 4K will help to illustrate such problems. For instance, in quotation 4J, Crosskey describes Gowan as a brilliant man who had earlier done excellent work in the field; and Crosskey notes that he himself at that time was perhaps a little dogmatic and that he has subsequently become rather more critical of certain aspects of the chemiosmotic hypothesis.[2] These points could have been used to argue that Gowan, being a researcher of great ability and much experience, may well have seen certain scientific defects in chemiosmosis as a result of which he rationally and scientifically decided that the hypothesis was inadequate or was in need of further investigation. Indeed Gowan himself, when we interviewed him, offered exactly this kind of account and buttressed it by suggesting that the judgement of *his* opponents had been swayed by non-scientific factors (4F). Moreover,

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several other respondents stressed that Gowan was a highly gifted scientist, scrupulous in his attention to detail and immensely industrious. There is, then, much evidence, some of which is furnished by Crosskey himself, which runs counter to the assertion that Gowan simply could not be bothered to put in the necessary effort or that he was misled by prejudice.

In addition, although Crosskey states in this passage that the chemiosmotic hypothesis was complicated, implying that this was one reason why Gowan failed to grasp it, he says elsewhere that chemiosmosis was basically quite simple. Both these apparently conflicting judgements were repeated many times in the interviews. It appears that many interviewees were, like Crosskey, able to conceive of the hypothesis as both complex *and* simple; and that they were able, at any one time, to select whichever of these characteristics fitted in with the structure of the particular account they were constructing.

Furthermore, there is the fact that Gowan can be shown to have understood the chemiosmotic hypothesis well enough to produce experimental observations which, at the time, appear to have posed very severe interpretative problems for its supporters. One of these is mentioned by Burridge in quotation 4K. Subsequently, it is said, these observations have been generally judged to be experimentally inconclusive or untenable. But speakers such as Burridge and Crosskey acknowledge elsewhere that the inadequacy of these observations was by no means obvious then and that it became established only after considerable further work by both pro- and anti-Spencerians. Thus, in other passages provided by these informants, Gowan's lack of understanding is much less obvious. In these stretches of talk, Gowan seems to have 'understood' chemiosmosis and its observational implications as well as anybody else.

We should stress that we are not trying here to disprove the accounts provided by any particular scientists, nor to show that specific accounts are intrinsically incompatible. We accept that participants, if they were given the opportunity, would be able to carry out interpretative work on their own and others' accounts to repair apparent inconsistencies. Our aim, rather, is to draw attention to the *flexibility* with which accounting is accomplished. Thus chemiosmosis was both complex and simple. It was empirically grounded, yet based only on an aura of fact. Similarly, Gowan was highly gifted scientifically yet incompetent in various respects, enormously industrious but also unwilling to make the necessary effort on a fundamental issue, putting forward criticisms of chemiosmosis which clearly showed that he did not understand the hypothesis yet which required much further experimental exploration before their inadequacy could be demonstrated.

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The priority of the speaker's version of his scientific position

The two following quotations are from Wisbech, an inorganic chemist often described as being on the margins of this research network. The views proposed by Wisbech and Spencer about the nature of oxidative phosphorylation are sometimes treated as very similar, indeed as basically identical, yet on other occasions as different in various important respects. Whereas Spencer maintains that protons are transported across the inner membrane from the inside to the *outside,* thus building up a gradient which returns through specific channels in the membrane to create and set free ATP on the inside of the membrane, Wisbech argues that the protons remain *within* the membrane. Nevertheless, although the two men appear to differ considerably over most details, they can also be said to agree about the basic idea that protons and electrons become separated in the membrane and that the release of protons is essential for the synthesis of ATP. Thus, in some passages, participants treat the two men's scientific claims as essentially the same. However, Spencer's analysis is treated as having been much more influential than that of Wisbech. Spencer has written and experimented much more on the topic of oxidative phosphorylation, and it is he who has received the credit and the prizes. It is necessary to appreciate this background in order to understand Wisbech's remarks.

In the two passages below, Wisbech employs different formulations of his scientific position and, as his interpretation varies, so does his categorisation of other actors and the substance of his accounts. In 4L, Wisbech is commenting on the defects in Spencer's model, that is, on those features with respect to which Spencer differs from Wisbech. In account 4M, however, Wisbech treats Spencer's ideas and his own as identical and he contrasts 'their model' with the views of those who failed to accept the central idea which the two of them had in common.

> 4L
>
> People were beginning to think that Spencer's hypothesis and mine were very similar. Well, the truth is they *are* similar. But the difficulty is that although both of us said that the proton and electron would escape from one another and come back and make this pyrophosphate [ATP], neither of us had a machine for doing that. Unless you invent that machine, I don't think you've solved the problem... And that's where the problem still is today. That machinery is not understood. In my opinion Spencer's description of that machinery is a thermodynamic impossibility, and I'm with some very good friends on that. But the biologists cannot understand why this machine is a thermodynamic impossibility. I don't believe that most of them understand this field at all . . . Spencer is an extremely naive man. He doesn't understand this thermodynamic

> problem. Neither does he understand any molecular chemistry, because he's not interested in that. He's a biochemist interested in bulk levels of various things and he doesn't understand the complexity of a protein as such. So he's not a chemist. He's much more a biological man. So he *couldn't* be bothered with those properties in the least. The machine didn't bother him. The chemistry he writes down, everybody writes back immediately, not just *me,* to say, 'Well, that won't do for chemistry. The chemistry is wrong.' [Wisbech, 28,31]
>
> 4M

> I said that nobody should get the prize except Spencer. (I was leaving myself out of it, because I genuinely believe I should have shared it.) The reason for that is that it was a very exciting hypothesis and his name had been associated with it. He'd worked on it when it was most unpopular, worked on it in face of a barrage of aggressive bad manners by a large number of people who didn't want it to be true, because it affected their status, I felt. And he had shown by his experiments that the ideas were basically correct. [Wisbech, 34]

These accounts both exhibit the asymmetrical structure with which we are now familiar. In both cases the speaker's version of correct belief is treated as relatively unproblematic. In 4M, it is presented as having been shown to be (basically) correct by experiments. The qualification 'basically' allows for the fact that Spencer's views are not identical with those of the speaker and cannot, therefore, have been *completely* confirmed experimentally. In 4L, those features of Spencer's theory which do not coincide with the speaker's are chemically wrong or thermodynamically impossible, or the relevant phenomena are simply not understood. The speaker acknowledges that he is offering a personal opinion. But this opinion is immediately strengthened by a reference to the 'very good friends' who endorse Wisbech's opinion; and by the subsequent observation that *everybody* objects to Spencer's chemistry and that the latter's attempts in this direction simply 'won't do for chemistry'. This portrayal of the scientific validity of the speaker's views and the support they enjoy among all competent scientists contrasts strongly with the representation of incorrect belief and the social and psychological characteristics of its perpetrators. In 4M, the latter are described as aggressive, bad-mannered and as unwilling to accept the truth 'because it affected their status'. In 4L, Spencer's errors are explained as arising from an extreme personal naivety combined with an inappropriate professional training. In addition, the perverse views of large numbers of other biologists are also attributed to their trained incompetence.

The particularly interesting feature of these two accounts, however, is the way in which, as Wisbech changes his representation of the degree of

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similarity between his own and Spencer's views, so the content of his accounts alters. In 4L, Wisbech begins by stating that people were beginning *to* think that his and Spencer's hypotheses were very similar and, indeed, that they *are* similar. But after the first two sentences, he concentrates on identifying the differences. In particular, he stresses that there is no evidence for Spencer's proposed machinery for ATP production. The rest of the passage confirms that Spencer's machinery is wrong and it explains, by reference to naivety and so on, how it is that the error has not only occurred, but has become so widespread. In contrast, in quotation 4M, Wisbech identifies himself scientifically with Spencer when he says 'I believe I should have shared the prize.' In this passage, he appears to be treating 'the hypothesis' as something which was common to them both and he goes on to describe and explain the response given to the 'basically correct' idea which they had both advocated. Consequently, there is no reference here to Spencer's naivety, to his failure to understand fundamental issues or to his narrow disciplinary perspective. Instead, Spencer's ideas, in so far as they are also Wisbech's, are simply presented as having been demonstrated by experiment. If this characterisation of correct belief is to be effective, it is necessary for the speaker to treat Spencer's scientific voice, for the moment, as (almost as) immaculate as his own. Thus, the pejorative sociopsychological characterisation is reserved, in this account, for those who opposed the essential truth embodied in the work of both Spencer and Wisbech; and the failure to recognise its validity is explained away, once again, as a result of non-scientific influences.

These two accounts bring out in a striking manner how characterisations, not only of other participants but also of scientific positions, can be varied, through appropriate selection of descriptive phrases, through selective comparison and through omission. They also reveal, once again, how the asymmetric structure remains constant even though the substance of the speaker's assertions differs dramatically from one passage to another. At the same time, they show clearly how the accounts of error furnished by a single speaker within a period of a few minutes can vary. As a result, they add further support to our previous arguments about the extent of interpretative variability and about the impossibility of using such accounts as sources of sociological evidence for the nature of social action and belief. These accounts seem to be best understood, not as providing descriptions of participants' prior actions, but as interpretative reconstructions which can portray events in many different ways, depending on the particular interpretative accomplishments in which the speaker engages in specific passages. These accounts also confirm that the crucial component in any speaker's reconstruction is the adoption of a specific version of correct belief. These conclusions will be further

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strengthened in the next section, where we look at accounts of error in process of construction.

Accounts of error under construction

In quotation 4N, the respondent is talking about one of the major issues under debate at the time of our interviews, that of stoichiometry. For our purposes, we can treat this issue crudely as equivalent to: 'How many protons are transported across the membrane per ATP formed?' The answer to this question has important theoretical implications, because it was frequently asserted that any figure other than two would entail major changes in the detailed mechanisms of proton movement contained in the chemiosmotic hypothesis. On the whole, the speaker talks as an enthusiastic supporter of Spencer. But on this issue he accepts that Spencer may be wrong, although personally he doubts it, and he constructs two alternative accounts of what is happening in current research into stoichiometries.

> 4N
> We are seeing many experiments done now on stoichiometry. I don't think the question is solved yet, so let's keep an open mind and let's pursue both possibilities: (a) that Spencer is right and (b) that he is wrong, in the stoichiometry matter. If Spencer turns out to be right, I think the analysis will go as follows: that we are getting a lot of people who basically understand the theory who are rushing in a little prematurely with experiments. There have been all sorts of little tiny things like how soluble is oxygen in saline and are there temperature artefacts on mixing solutions and on the electrode - technical matters which Spencer would be better on. I have never seen people do better experiments than Spencer. There are lots who are now doing as precise and beautiful

experiments, but I have not seen him surpassed in this kind of detail.

So that would be one solution. The alternative is that Spencer is being misguided by his intuition into thinking that there must be two protons because there are two electrons. . . [This possibility] does not detract, but more or less cements the chemiosmotic hypothesis. [Spencer's opponents] are showing that there *are* protons and that they *are* being translocated, and in using the criteria of Spencer, it's all complementary. [Roberts, 19]

As usual, there are parts of this account which seem clearly inconsistent with other respondents' views. In particular, various researchers, including several who professed to be Spencerians, maintained either that Spencer was actually a rather poor experimenter or that his experimental skills had declined in recent years; and this opinion was based, in some

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cases, precisely on adverse judgements of the results Spencer was getting on stoichiometry.

However, what is specially interesting about quotation 4N is the way in which the various characterisations of participants and their actions are made explicitly dependent on the rightness or wrongness of their knowledge-claims. If Spencer is right, says Roberts, then it follows that other researchers are being over-enthusiastic or in too much of a rush to produce results on a hot topic. Consequently, they are being insufficiently careful in their experiments as well as premature in their rejection of Spencer's theoretically based reinterpretation of their findings. However, he implies, we cannot know if this is an accurate characterisation of their actions until we know *who* is right. Thus, for Roberts, how to characterise the actions of Spencer's opponents does not seem to be an empirical matter, to be decided on the basis of observation and questioning of those concerned. It seems, in general terms, to be a matter of logical necessity. If Spencer is right, then his opponents *must* have been misled by *some* kind of extraneous, non-scientific influence. If Spencer is wrong, then *something* must have interfered with his normally scrupulous experimental practice.

The same technique for constructing an account is evident in the next quotation:

4P

Barton: And there have been occasions when people have said, 'Oh, him' instead of, 'Oh, that.' Sometimes people have been out to prove that somebody else is wrong, rather than *[unclear]*. But I think that inevitably things were seen in that way. I've seen other fields where things have been much more bitter. But science generally does progress very well and objectively, despite the subjective element. I think there *is* a subjective element.

Interviewer: Do you have any idea how this personal element gets eliminated?

Barton: Only because a sufficient number of experimenters try to make the position clear. If other people are interested enough, if it's important enough, then the work will be done again or, more likely, its ramifications will be pursued. Predictions will be followed up, more experiments done, and in the fullness of time a much clearer position will become apparent. Just as happened with the chemiosmotic theory. And then, any personal rivalry will be seen for what it was, in relation to the facts, as they become more fully established.

Interviewer: So the experimental evidence . . .

Barton: At the end of the day solves everything *[general laughter]*.
Interviewer: Overwhelms these private antagonisms.
Barton: That's right. [Barton, 62-3]

According to Barton, personal rivalries will only become evident for what

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they are when the truth has become clearly established. Such rivalries, presumably like the other forms of social distortion employed in accounts of error, cannot be definitely identified until the speaker 'knows what the scientific facts are'.

The underlying structure of accounts of error becomes visible with exceptional clarity in quotations 4N and 4P. Both accounts show clearly how speakers' formulations of error depend upon a prior formulation of correct belief. The portrayal of scientific error which we see exemplified in these accounts is a necessary implication of scientists' formulation of correct belief. It is the reverse side of the same coin. It is for this reason that Roberts and Barton are unable to decide *which* scientists have acted improperly until the scientific truth is known, *even though they already have a good idea of how those involved may have acted improperly*. In both cases, the researcher has available one or more plausible 'ready-made' accounts of error which can be applied to almost all participants and which can either be brought into play or abandoned as soon as correct belief is established. Because correct belief itself is depicted as deriving fairly unproblematically from the experimental facts, in the long run, each speaker is able to maintain that sooner or later his interpretation of others' actions will become as reliable as his knowledge of the objective realm of biochemical phenomena.

Using the contingent repertoire

It is clear from the analysis so far that whilst scientists' empiricist formulations of correct belief take a narrow range of interpretative forms, their portrayal of incorrect belief and its causes are much more varied and flexible. Not only has each speaker to be able to use his contingent repertoire to construct plausible, ready-made accounts of error to suit the circumstances of a potentially indefinite series of different individuals and situations, but each scientist has also to be able to vary his stock versions of action in accordance with the interpretative changes occurring in extended passages of talk. One feature of scientists' contingent repertoire which contributes significantly to this flexibility is the vagueness and imprecision of its terms.

The accounts of error in our collection rely heavily on notions such as prejudice, pig-headedness, strong personality, subjective bias, emotional involvement, naivety, sheer stupidity, thinking in a woolly fashion, fear of losing grants, threats to status and so on. All of these and many other similar conceptions appear in our data. Although the general drift of an account of error in which such phrases are used is generally clear enough in common-sense terms, it is very difficult to pin down their precise meaning.

For example, what exactly does a speaker mean when he describes an eminent, multi-prizewinner as naive or stupid? In addition, speakers do not hesitate to withdraw classifications of this kind during subsequent talk, if prior attributions begin to interfere with respondents' subsequent interpretative work. For example, in the following short extract the speaker simply abandons an earlier account of error as it begins to appear inconsistent with the version of events he now finds himself constructing.

> 4Q
> Obviously, before you make an effort like that, you have to be convinced that it's going to be worthwhile in terms of both your own self-interest as well as research interest and the field itself. I think for most people it wasn't clear that that was the case. I don't think it was deliberate obtuseness or that people were really pig-headed in the sense that I might have suggested. [Richardson, 3]

There is, then, a great deal of uncertainty and conceptual vagueness about the contingent characterisations employed in accounts of error. However, this vagueness allows the speaker room to adapt and change his position as he engages in informal discourse, without having continually to repair obvious inconsistencies. Nevertheless, speakers do sometimes run into difficulties. When this happens, it is possible to observe how the very ambiguity of the initial account can be turned to the speaker's advantage.

> 4R
> *Pugh:* There's a technology of perpetuating mythology. It's very elaborate, the system of reviewing, the way in which certain people control the meetings. If you want to write a fascinating book, I advise you to deal with the techniques by which that's done. That provides you with an absolute technique by which you can perpetuate error for an indefinite period. If you say, 'Look, I now have evidence that the Spencer model doesn't bear close examination, 1,2,3,4,5', they'll send it out to a Spencerian and he'll give you a list of things about a mile long to do and he'll wear you out. You can't win. Every experiment you do, he's got another one that you are going to have to do. He can make it impossible. But if you write it from the standpoint of a Spencerian, he'll just say, beautiful . . .
> *Interviewer:* Do you think it is true that Spencer himself had to face up to that kind of situation?
> *Pugh:* Of course. He fought another dogma and now he has become the dogma and he knows it and is not very happy about it.
> *Interviewer:* How do you think he managed to resist the dogma, so to speak?
> *Pugh:* Well it took a long time. Violent battles. And it was better. It could

> explain certain things, proton gradients, which nobody had been able to explain and he introduced really revolutionary new ideas . . .
> *Interviewer:* Do you see any signs of your own theory coming to be accepted?
> *Pugh:* No, none. Zero. And that's because nobody is interested. They hated the problem in the first place. It's beyond them. Now they feel it's been buttoned up, they don't want

> to hear about it. It's the ostrich approach. But this is an abandoned generation. They will be criticised severely by the historians as unequal to the task . . . they control the means and so on and they will do it until the whole thing will be like the Emperor has no clothing. It will take 20 years to find that out, by which time they will have become Lords and Princes. [Pugh, 22, 34, 35]

Account 4R is a selection from a much longer passage. The feature that we wish to bring out is the way in which Pugh appears to change the meaning of his main 'explanatory' concepts as he proceeds. In the first paragraph, he conforms to the asymmetric pattern when he describes how false beliefs are perpetuated by the technique of peer review and so on, not only in his own area, but throughout science in the past as well as the present. The notion is then used to account for his failure to get his own theory accepted, even though, as he stressed throughout the interview, it is scientifically superior. If one takes at face value Pugh's concept of the 'absolute technique' for maintaining existing dogma indefinitely, one might think that no new ideas and, in particular, no ideas which are 'really revolutionary' like Spencer's would ever be successful.

At this point Pugh is asked how Spencer managed to overcome the dogma which preceded *him*. He deals with the interpretative task of reconciling Spencer's success with his own description of the politics of science by stressing that there was 'violent resistance' to Spencer and that it did take 'a long time'. But in order to provide some positive reason for Spencer's success, Pugh falls back on the explanatory power of Spencer's theory, even though elsewhere Pugh describes that theory as 'preposterous, unbelievable' and 'non-explanatory'. However, if Spencer's theory succeeded because it explained the experimental facts better than the chemical theory, despite the operation of the 'absolute technique', this reopens the question of why Pugh's theory, which he views as an advance on chemiosmosis, shows no signs of winning converts. Pugh deals with this in the final paragraph by returning to the control exercised by the dominant Spencerian elite. But he supplements that now-weakened notion with a series of additional factors, only a few of which appear in the quotation above.

There is clearly no point in trying to establish exactly what Pugh means by concepts such as 'technique for perpetuating mythology' or 'abandoned

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generation'. They have no clear referents. Nor is there much point in the listener raising what could be regarded as counter-instances, for, with slight adjustments *to* the account, these can always be plausibly incorporated. Like the speakers in the preceding accounts of error, Pugh employs a repertoire of interpretative resources which can be made to fit loosely but plausibly with events. These notions can be used to explain, adequately enough according to the undemanding requirements of fast-moving and unreflexive ordinary conversation, why his theory is rejected. In other words, the main characteristic of these resources is that they can easily be expanded or contracted, withdrawn or supplemented, without creating glaring inconsistencies, to meet the exigencies of each new conversational exchange. They enable speakers to carry out complex and subtle interpretative work in a way which always leaves them room for further manoeuvre and which always seems to allow the speaker's own scientific views to emerge unscathed.

Throughout this chapter we have described scientists' accounts of error as being couched in terms of that contingent repertoire which we previously observed in scientists' informal versions of laboratory practice. It may appear that we are using the notion of 'contingent repertoire' rather loosely in applying it to both these kinds of talk. It is quite possible, for example, that the lexicon for talking about laboratory practice is systematically different from that used by scientists in accounting for error. It certainly seems likely that the former topic will not feature so many references to strong personalities, to manipulation of the refereeing system, and so on; whilst the latter topic will focus much less on craft skills and intuition. However, although there may well be differences in the incidence of certain kinds of phrases in these two interpretative contexts, in both, scientists' actions and judgements are depicted as those of specific individuals acting on the basis of personal inclinations and particular social positions. Furthermore, in both topics the distinction between empiricist and contingent formulations is clearly observable, highly recurrent and recognised by participants. But the relative clarity of this interpretative boundary is highly unusual. Within the broad realm of contingent discourse, interpretative divisions are much weaker, more blurred and open to creative modification. What is distinctive about scientists' accounts of error is not so much the employment of a standard range of substantive characterisations, although such stock interpretations do seem to reappear in our material, but the pronounced tendency to organise such accounts around an asymmetrical counterposition of empiricist and contingent versions of action and belief.

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Symmetrical accounts

Although the great majority of accounts of error are asymmetrical, it is not logically impossible to provide symmetrical interpretations of error and correct belief in the same account. In the next quotation, the speaker is referring to disagreements between himself and Spencer with respect to the interpretation of stoichiometry experiments.

> 4S
>
> We think we've done experiments with NEM which test the interpretation proposed by Spencer and show it's not right. He does not, presumably, believe those experiments, although he hasn't specifically said why. Instead, he offers some experiments of his own, which he would take to demonstrate that NEM is working in some different way. Again, we haven't criticised those experiments specifically to him. But we either think they're not relevant (and although he sees certain effects, they're not relevant to the problem), or he's misinterpreted things.
>
> Both camps, I think, believe that the other is emotionally involved in the answer and, therefore, there's not much point in rational argument. I don't think it's worthwhile having a rational argument with Spencer about it, because I'm fairly sure I shan't change his mind. . . I find it quite difficult to argue about this, because I cannot see how he cannot accept that our arguments and experiments are right. I suspect that he has the same problem. So I don't think it's a problem of straight science. [Beamish, 23-4]

In this account, without relinquishing his claim that he has got it right and that Spencer has got it wrong, Beamish maintains a fairly detached stance toward his opponent's views. Thus, he begins by offering a general description of two different, but experimentally-based, perspectives on the issue of stoichiometry. If he had stopped at the end of the first paragraph, we would have been left with an account which was symmetrical in the strong sense that both correct and incorrect belief were presented as scientifically legitimate. In the second paragraph, Beamish goes on to suggest that those involved do not in practice regard their opponents' work as having the same scientific status as their own. Both camps are depicted as having devised parallel, and asymmetrical, versions of what is happening. Both sides, it is said, view their opponents' interpretations as distorted by emotional commitment.

Beamish's recognition that his opponents would probably describe him in exactly the way that he describes them is an unusual personal achievement; and it is this which enables him to maintain in this passage a precarious symmetry in his interpretation of both sides of the dispute. Nevertheless, he does not deviate very far from the standard accounts of

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error examined above. For he also portrays participants as endorsing the strongly asymmetrical view that it is always the other side which is 'emotionally involved in the answer'. Moreover, the asymmetry is clearly linked in this passage to an empiricist formulation of correct belief. It is precisely Beamish's empiricist contentions which make it difficult for him simply to accept that all parties are emotionally committed to their own positions. In this passage, Beamish seems to treat what he calls 'straight science' as consisting of the rational appraisal of reliable evidence leading to unequivocal conclusions. Yet, on one side or the other, false beliefs persist in relation to stoichiometry. Clearly, then, this is not 'straight science' in Beamish's sense. It follows necessarily, therefore, that something unscientific is happening, that non-scientific influences are somewhere at work and that, given the validity of his own scientific views, it cannot really be the speaker who is at fault: 'I cannot see how he [Spencer] cannot accept that our arguments and experiments are right.'

The quotation from Beamish is typical of speakers who attempt to construct symmetrical accounts in that such speakers, whether they are explaining true and false belief equally in experimental terms or in psycho-social terms, tend to revert back quickly to the dominant asymmetric structure. In other words, symmetrical accounts tend to be unstable. Another example of this can be found in quotation 4H, where the speaker, having referred to the very strong individuals who want to interpret everything in terms of their theories, formulates his own empiricist practice in a humorous manner: 'Of course, those are the other guys, not us. We're interpreting it even, balanced *[laughter]*.' The speaker's jokey delivery seems to imply that he is well aware that other scientists, including those he has just criticized, might also insist on presenting their views in empiricist terms and that they might also wish to explain away *his* errors as personal aberrations. To this extent, the speaker in this passage implies an equality of status amongst competing accounts and, in this sense, organises his account of correct and incorrect belief symmetrically. Yet the speaker's words, if read literally instead of being heard as a joke in accordance with his vocal inflections, remain strongly asymmetric: 'Those are the other guys, not us.' Moreover, although the speaker's tone seems to have made those words ironic at the time and to have suggested that the listeners should hear them as meaning the exact opposite of their literal sense, the speaker himself, in his very next sentence, appears to take for granted their literal meaning. He seems entirely to disregard his own humorous interjection and to proceed as if he had in no way departed from his customary empiricist voice. When the laughter ceases, he returns immediately to the theme of how other scientists seek to 'bend the data' in ways which provide spurious support

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for their erroneous theories. In other words, this speaker's move towards a symmetrical treatment of truth and error, like that of Beamish, is almost instantaneously abandoned.

Not only do symmetrical accounts tend to be unstable, but they are also very few in number. If we confine ourselves solely to material from our 34 interview transcripts, we find that out of a total of 65 accounts of error and correct belief, no more than five are symmetrical; whereas 60 exhibit the pattern which we have called asymmetrical accounting for error.

Accounts produced outside the interview

All the material above has been taken from transcripts of our interviews with biochemists. It is necessary to ask, therefore, whether this form of accounting occurs only in interviews. It is conceivable that there is something about the interview situation in general, our position as sociologists interviewing scientists, the particular questions we used or the inclinations of scientists within interviews which generated this kind of asymmetrical accounting. We have been unable, however, to discern any general pattern in the way in which accounts of error are produced in our transcripts or to identify any particular category of respondents as especially responsible for such accounts. Accounts of error sometimes occur early in a passage, in direct response to a question from the interviewer. But just as often they appear in the course of a more extended stretch of talk, as the speaker builds further interpretations upon his own prior discourse. They undoubtedly occur frequently in passages where 'chemiosmotic speakers' are making sense of the 'resistance' of those who are deemed not to have adopted this theory despite its experimental warrant. But they are employed with no less enthusiasm by 'non-chemiosmoticists' as they deal with the task of making understandable the apparent popularity of this 'obviously inadequate' theory. With one .exception, we can find no way of consistently linking the appearance of asymmetrical accounts to variations in the form of question, the interpretative context or the type of respondent.

The only discursive feature of the interview transcripts which does seem to be clearly related to the occurrence of accounting for error is of a very general kind, namely, the predominantly retrospective character of the talk recorded in these transcripts. Because accounts of error are mainly retrospective, the retrospective orientation of interview talk probably means that the frequency of accounts of error is higher in interviews than elsewhere. But, as we pointed out earlier in this chapter, retrospection is no way confined to interviews. It is, rather, a recurrent and organised feature of ordinary discourse. Thus, if we make the reasonable assumption that

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retrospection is widespread within *scientists'* ordinary discourse, and if we accept that

asymmetric accounts of error do not appear to arise from any unique interpretative situation being created in the interviews, then we have grounds for assuming that asymmetrical accounting will occur in the course of ordinary interchange between scientists.

The line of reasoning is strengthened by the following observations. In the first place, there is direct evidence that the same interpretative pattern is employed in social contexts other than that of the interview. It occurs, for example, in an historical article about the Pasteur effect written by one of the leading figures in the field for his fellow experts. In this article the author, quite removed from the influence of any sociological investigator, constructs an account of error with exactly the same structure as those examined above. He takes for granted that his own scientific understanding of the Pasteur effect is objectively correct and that he can see clearly what was wrong with the views of his predecessors. Because he takes the correct interpretation to be obvious, it follows that previous views were obviously wrong. He is led to suggest, therefore, that non-scientific factors were at work: 'For interesting psychological reasons this explanation of the Pasteur effect was widely accepted in spite of the fact that it was patently inadequate.'

The next quotation also indicates that asymmetrical accounting for correct belief and error does occur outside sociological interviews. It is taken from a letter written to reply to our inquiry asking whether the author would be willing to be interviewed. Our initial letter said very little about our interests and was phrased in the most general terms.

> 4T
> I was pleased to be approached by you, especially since I have not been able to propound my own ideas in print... As you may infer I do not accept Spencer's various schemes and basically regard the ox phos process as arising from [technical details omitted to prevent identification]. I send a MS rejected by pro-Spencer referees but I would make the point strongly that when a body of scientists have committed themselves in print to a theory they become biased towards rejecting other ideas lest they be made to look foolish. This is a weakness of our system. I would suggest for your attention the proposal that many ordinary biochemists have been bemused and confused by Spencer's interwoven assumptions; rather than say that they do not understand them they accept them. [Sephton]

The asymmetric account contained in this letter is of interest to us here, not only because it appears outside the interview and was in no way directly solicited by the investigators, but also because there is good evidence that Sephton actually described his situation in similar terms

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when talking to his colleagues. In the course of another interview, a scientist who knew Sephton informally and was familiar with his work and opinions told us, without our asking, that Sephton thought he was sometimes victimised by pro-Spencerian referees but that this was not in fact so. The informant had a quite different interpretation of what was happening.

The significance of this alternative view is not that it shows Sephton to have been wrong. The most that one can conclude in this respect is that, as we have seen many times, it is quite normal for one participant's account to be treated by others as obviously unconvincing. Its significance here is rather that it shows that the response Sephton gave to us was similar to the account he had given to his colleague. It provides a clear indication, therefore, that the form of asymmetrical accounting which we have documented above does occur in the course of informal interaction among scientists.

It would be misleading to pretend that the evidence and considerations advanced in this section can lead us to more than the most tentative conclusions. For the moment, we can only conclude that the marked and dominant interpretative pattern observed in our material must occur to some as yet unknown degree in naturally occurring discourse within science; that it certainly does occur frequently in some forms of discourse involving scientists and outsiders like ourselves; and that further comparative studies of scientists' discourse are needed if we are to begin to understand more fully the social production of scientific error.

Scientists and mundane reasoning

We stressed in the last section that the restricted nature of our empirical evidence on biochemists prevents us from making strong claims about the incidence of asymmetrical accounting for error in naturally occurring situations. Furthermore, because so few studies have carried out the kind of analysis we are attempting here, there are at present no other published studies of scientists' discourse which can be used to explore the wider relevance of our observations. There is, however, one description of non-scientists' discourse which bears a striking resemblance to our own and which indicates that something akin to the phenomenon we have been discussing in this chapter is observable, not only in science, but also in quite different realms of social life. This is contained in Pollner's study of what he calls 'mundane reasoning about reality disjunctures'.[3]

Much of Pollner's analysis deals, not with particular kinds of social actor, but with the generic figure of the 'mundane reasoner', that is, any actor whose speech presupposes that there is an objective world which can

be shared by, and identically reported by, all other competent participants. Clearly, the scientists quoted above are usually mundane reasoners in Pollner's sense. But so are non-scientists in certain types of situation; for example, people testifying in traffic courts. What is particularly interesting about Pollner's analysis is his suggestion that participants tend to deal with reality disjunctures, that is, 'disjunctive experiences and/or accounts of what is purported to be the same world'[4] in a manner which closely resembles the interpretative practice of accounting for error.

> *For a mundane reasoner, a disjuncture is compelling grounds for believing that one or another of the conditions otherwise thought to obtain in the anticipation of unanimity, did not*. For example, a mundane solution may be generated by reviewing whether or not the other had the capacity for veridical experience. Thus, 'hallucination', 'paraonia', 'bias', 'blindness', 'deafness', 'false' consciousness etc., in so far as they are understood as indicating a faulted or inadequate method of observing the world serve as candidate explanations of disjunctures. The significant feature of these solutions - the feature that renders them intelligible to other mundane reasoners as possibly correct solutions - is that they bring into question not the world's intersubjectivity but the adequacy of the methods through which the world is experienced and reported upon. *The application of such designations declares, in effect, that intersubjective validation of the world would obtain*

were it not for the exceptional methods of observation and perception of the persons identified as employing them.[5]

Although Pollner's empirical material is actually rather limited, it does draw our attention to the possibility that the asymmetrical accounting for true and false belief among our biochemists is part of, or is linked to, much wider discursive regularities than at first seemed likely. Nevertheless, non-scientists' accounts, as documented by Pollner, are by no means identical with those of our respondents. For example, Pollner's subjects do not seem to employ any highly standardized devices, such as our biochemists' 'strong personalities' or 'manipulative refereeing'. This may simply mean that those suspected of traffic offences do not form a linguistic community in the way that certain groups of scientists probably do. Similarly, Pollner's subjects appear not to have any shared interpretative repertoire in terms of which they can formulate and warrant their own versions of the natural world. Pollner's mundane speakers seem to have to develop the grounds for their own claims in a relatively *ad hoc* manner out of their own idiosyncratic and defeasible experiences. For example:

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Defendant: From the time I got on the freeway until when he pulled me over, I was checking my speedometer constantly . . .
Judge: You ever have your speedometer checked?

Defendant:. . . so I parked my car and I went over and I was inside their car talking for a few minutes and then the police barricaded both ends of the street off so we couldn't leave, then they charged me with aiding and abetting a drag racing contest, and there was no drag racing at all taking place.
Judge: Well, the officers appeared at the scene of extensive drag racing

Unlike these laymen, who are dealing with relatively isolated events in the unfamiliar linguistic context of the law court, our respondents are reconstructing accounts of experiences which have played a central part in their professional biographies, about which they and their colleagues have probably talked many times, and in retailing which they employ well established and appropriate interpretative repertoires. In particular, when formulating their own claims about the phenomena of the natural world, scientists have at their disposal the interpretative forms of the empiricist repertoire, which enable them to translate their idiosyncratic and defeasible experiences into the impersonal linguistic currency of 'experimental evidence'. As a result, scientific speakers seem to be peculiarly able to construct accounts in which they appear to have privileged access to the realities of the natural world: indeed, no matter what the diversity of views, each scientist manages to convey the strong impression that his voice and that of the natural world are one and the same.

It would be premature to conclude, however, that scientists' accounts of error will always be marked by a more definite contrastive pattern than those of laymen or that laymen never have access to some linguistic equivalent to the empiricist repertoire. Our evidence is too fragmentary at present to decide on these questions. For example, if each of Pollner's defendants had been recorded without the presence of the police officer who had brought the charge, as our biochemists were recorded without their opponents being able to overhear them, a significantly different series of explanatory accounts might well have been obtained, perhaps closer in their structure to our scientists' interpretations. It seems more reasonable, therefore, to conclude this chapter by stating that, so far as we can judge from our data, scientists' accounts of error appear to have a well-defined interpretative structure, that this structure depends on the existence of the two repertoires previously identified, and that there is some evidence of somewhat similar structures occurring in one other area of discourse.