6 Constructing and deconstructing consensus

In the chapters above, we have begun to develop an analysis of scientists' accounting procedures; that is, an analysis of the procedures exhibited in various kinds of scientific discourse. We have stressed that even scientists' written technical discourse involves the representation of participants' actions and beliefs; and we have illustrated how scientists' actions and beliefs, like those of other social actors, can be characterised in numerous different ways by different actors and by the same actors in different interpretative situations. We have emphasised that scientists employ certain stable interpretative forms and repertoires, but that these recurrent interpretative resources are used with great flexibility to generate radically different accounts of social phenomena. We have found that participants' accounts are so contingent and variable that it is impossible to produce conventional sociological interpretations which are derived from these accounts in a satisfactory manner. We have suggested, accordingly, that instead of attempting to use participants' accounts as the basis for definitive analysts' versions of scientists' actions and beliefs, we should concentrate on identifying the principles in terms of which scientists' own accounts of action and belief are organised.

In the present chapter, we will extend this approach to deal with the issue of cognitive consensus in science. At first sight, this topic may seem to be difficult to reconcile with our stress on the variability of scientists' social accounting. For, by definition, consensus can only be said to exist when there is considerable agreement amongst the participants. We will show, nevertheless, that consensus is best conceived as a contextually variable aspect of scientists' discourse about action and belief. In so doing, we will try to allay any doubts that the reader may have about the possibility of using our form of analysis to deal with the *collective* phenomena with which sociology has been customarily concerned. For cognitive consensus in science is this kind of collective phenomenon *par excellence*. Our examination of consensus in this chapter is intended to show that our form of analysis does not stop at the description of participants' interpretative

methods, but can also reveal how participants use their interpretative resources to construct the realm of collective phenomena. Thus, we will begin to indicate in this chapter how discourse analysis could be used to revitalise an issue of longstanding sociological significance, the analysis of aggregate phenomena.[1]

Spencer's consensus diagram and its readings

In 1974, Spencer was awarded an important scientific medal for his work on bioenergetics. On receiving the medal, he gave an honorary lecture to the Biochemical Society. This lecture was subsequently published in a journal of biochemistry.[2] It is clear from our interview transcripts that all our respondents were familiar with its content; and in particular with the diagram reproduced here as 6B. The lecture is divided into two main parts. The first part presents the four 'basic postulates' of the chemiosmotic hypothesis, describes in chemiosmotic terms some of the detailed processes of oxidative and photosynthetic phosphorylation, and links the chemiosmotic 'rationale' to the wider biochemical literature. In the second part of the paper, Spencer reviews some of the evidence which has led to the 'establishment of the four fundamental postulates of the chemiosmotic hypothesis as experimental facts'.

In between the two main sections there is a short 'historical comment on trends of opinion' concerning the chemiosmotic hypothesis, accompanied by a graph. This graph and most of the written text of his historical comment are reproduced below. As in the last chapter, we have numbered the sentences in this passage for ease of reference and we will do so elsewhere in this chapter wherever necessary.

> 6A
>
> 1 When I first began to develop and advocate this chemiosmotic view of oxidative and photosynthetic phosphorylation in the early 1960s, the four fundamental postulates were almost entirely hypothetical, many of my most distinguished and respected colleagues, such as Tippert, Holst, Bridge, Brian, Parry, Dowland, Arnold, Bull and Purcell were persuasive supporters of coupling through energy-rich chemical intermediates and coupling factors; 2 and there did not, perhaps, seem to be much chance that the chemiosmotic hypothesis would survive the destructive experimental testing to which, we were all agreed, it should be subjected. 3 Nevertheless, it was incidentally my hope that the chemiosmotic view would survive, because, if it did, there was . . . the chance that it might end the debilitating lack of agreement between the experts in oxidative phosphorylation and related energy transductions by providing the foundation for a generally acceptable conceptual framework . . .4 As it turned out, the research sparked off by the chemiosmotic hypothesis in

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> many laboratories, including my own, produced much experimental evidence in support of the four fundamental postulates, which are now widely recognised as being experimentally established facts. 5 However, although there is now a relatively widespread acceptance of the chemiosmotic rationale in the field of photosynthetic phosphorylation, where there is a strong biophysical tradition, there has been more resistance in the field of oxidative phosphorylation. 6 Several of the more eminent authorities in the field of oxidative phosphorylation are reluctant to agree that coupling between the proton-translocating respiratory chain system and the proton-translocating ATPase system, plugged through the coupling membrane, is due to the proton current circulating between and vectorially through them. 7 They have preferred to believe, in keeping with the traditionally scalar origins of their conception of metabolism, that coupling is achieved by some unidentified energy-rich intermediates or by some direct interactions between components of the respiratory chain and reversible ATPase systems. 8 In [the figure below] I have plotted an assessment of the attitudes of some of the principal protagonists, based on that given by Cranmer in his excellent scrutiny of the chemiosmotic hypothesis (1969), and extended over the period from 1961 to 1973.9 Obviously, different research workers judge the same experimental knowledge differently, and opinions change as time allows improvement of comprehension and

> accumulation of knowledge. 10 Looking at the trend shown by this diagram, and bearing in mind the natural and inevitable predilections of the different protagonists, it does seem likely that the validity and usefulness of the chemiosmotic rationale in the field of oxidative phosphorylation and related energy transductions will be generally recognised in due course. 11 It is understandable, however, that some of the eminent biochemists, who have long championed the more traditional biochemical views of the coupling mechanism, have not found it easy or agreeable to acquire a taste for the relatively biophysical disciplines of membrane transport and vectorial metabolism that were not originally of their own choosing.

Diagram 6B is presented by Spencer in the text of his lecture as a straightforward description of the changing pattern of support for chemiosmosis. It is taken as documenting how the opinions of relevant specialists with respect to chemiosmosis have actually altered in the past and it is used (6A10) to suggest what is likely to happen in the future. The written text takes for granted the accuracy of the diagram and offers an interpretation of why the trend should have taken this form. The growing consensus about the scientific merits of chemiosmosis which is displayed in the diagram is attributed to the gradual accumulation of experimental evidence (6A4) and to the slow improvement of comprehension (6A9). The absence of a complete consensus is linked, in accordance with the

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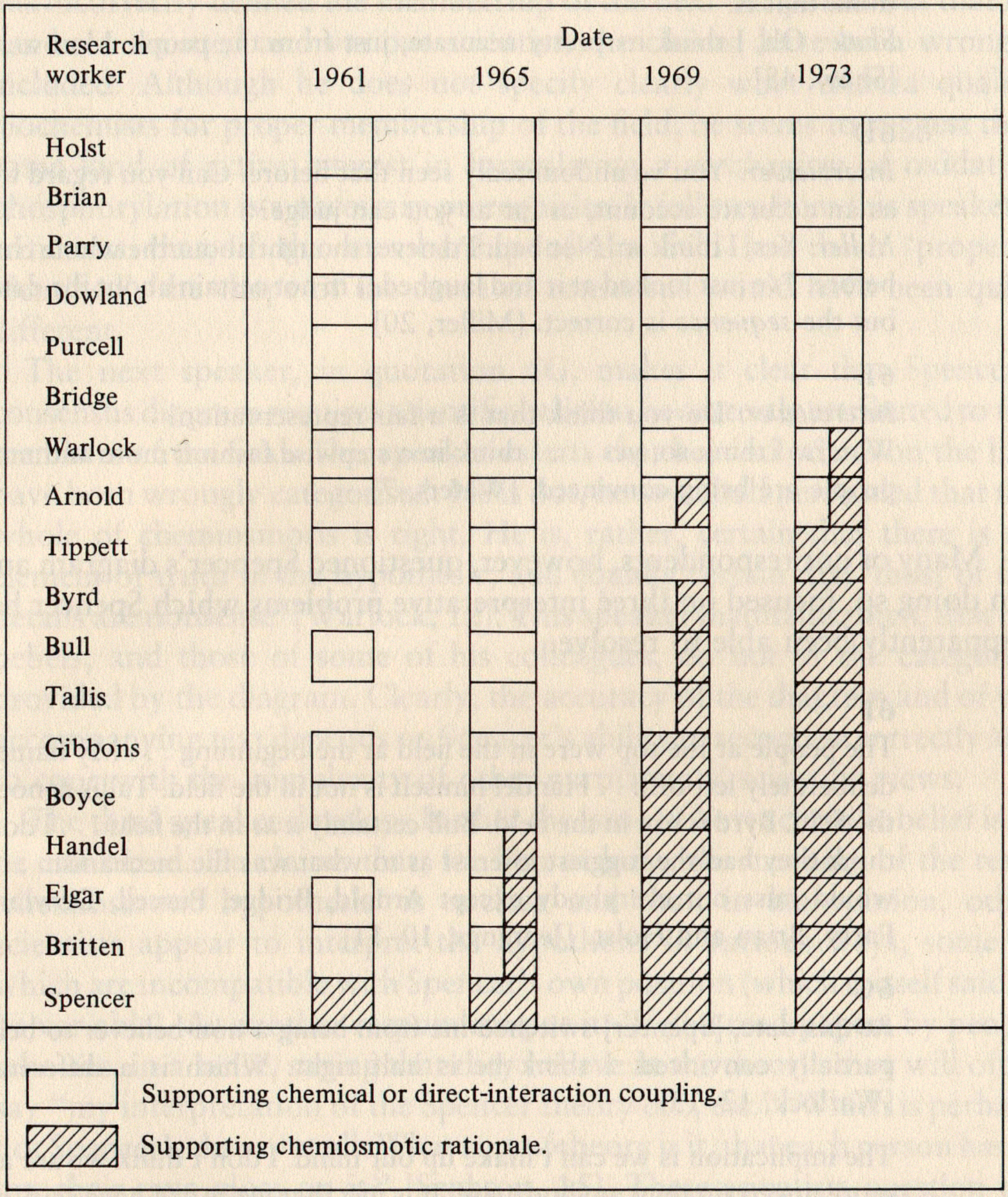
6B
Trend of support for chemiosmotic rationale



asymmetrical structure of accounting for error, to reluctance on the part of certain eminent scientists to adopt a theory which they themselves had not originated and which they found rather difficult to understand (6A5-7 and 11).

In the course of our interviews carried out in 1979, several of our respondents mentioned this diagram or similar diagrams which Spencer seems to have produced at various times. We asked these and our other respondents for their comments on the diagram. Some of them accepted it as an accurate, literal description of the growth of cognitive consensus in the field.

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6C

Interviewer: I wondered if you'd like to comment on how accurate you think that is.

Shaw: Oh, I think it’s pretty accurate, just from the people I know . . . [Shaw, 48]

6D

Interviewer: You've undoubtedly seen that before. Can you regard that as an accurate account, as far as you can judge?

Miller: Yes, I think so. Not bad. I'd never thought about the whole chart before. I've just looked at it and laughed. I'm not certain about the dates, but the *sequence* is correct. [Miller, 20]

6E

Interviewer: Do you think that is a fair representation?

Waters: I think so, yes . . . I think in a stepwise fashion more and more people are being convinced. [Waters, 7]

Many of our respondents, however, questioned Spencer's diagram and, in doing so, focused on three interpretative problems which Spencer had apparently been able to resolve.

6F

The people at the top were in the field at the beginning. . . My name is deliberately left off. . . Handel himself is not in the field. Tallis is not in the field. Byrd is not in the field. Bull certainly *was* in the field... I don't think they had the foggiest interest as to what was the mechanism . . . I would miss out everybody except Arnold, Bridge, Purcell, Dowland, Parry, Brian and Hoist. [Jennings, 1O-11]

6G

At this date, [Spencer] switched me from being a non-believer to being partially convinced. I think he is half right. Which is a difference. [Warlock, 12-13]

The implication is we can't make up our mind. I don't think we are any more uncertain than anybody else. It is just that we're as I have said, not that I am half convinced, but I am fully convinced he is half right. [Warlock, 52]

6H

People in this field . . . will say something that is really contradictory to the Spencer hypothesis and they will still declare that they are Spencerians

. Spencer himself has different versions and that's I think one of the confusing things. That when somebody says that Spencer is right or chemiosmosis is right, you

> would really have to nail down exactly what is meant... You can't decide who is a Spencerian and who is not, if you don't first of all define exactly what the doctrine is. [Hinton, 10-11]

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Each of these speakers objects to Spencer's diagram and states that it is misleading in a particular respect. Jennings (6F) maintains that Spencer has incorrectly defined the membership of the field. He points out that he has been omitted and various scientists, he claims, have been wrongly included. Although he does not specify clearly what criteria qualify biochemists for proper membership of the field, he seems to suggest that some kind of active interest in formulating a *mechanism* of oxidative phosphorylation is a necessary prerequisite. It follows from this speaker's criticisms that, if the membership of the field had been 'properly identified', the shape of the curve of consensus would have been quite different.

The next speaker, in quotation 6G, makes it clear that Spencer's consensus diagram requires scientific belief to be correctly attributed to the members of the field. This speaker asserts that he, and others on the list, have been wrongly categorised. He is not, he says, half persuaded that the whole of chemiosmosis is right. He is, rather, certain that there is an element of truth in the hypothesis; and equally certain that 'most of the details are nonsense' [Warlock, 12]. This speaker maintains, then, that his beliefs, and those of some of his colleagues, do not fit the categories provided by the diagram. Clearly, the accuracy of the diagram and of the accompanying text depends on Spencer's ability to recognise correctly and to cope with the complexity of other participants' scientific views.

The third speaker develops further the issue of how scientific belief is to be attributed. He claims that, for him at least, the meaning of the term 'chemiosmotic hypothesis' is unclear and that, in his opinion, other scientists appear to interpret the hypothesis in various ways, some of which are incompatible with Spencer's own position (which is itself said to be variable). As another respondent put it: 'If you read papers by people who are, in a sense, saying that they believe in the theory, they will often say "my interpretation of the Spencer theory etc., etc.". Which is perhaps not Spencer's theory at all. What sort of theory is it, that each person has to put their own gloss on it?' [Sephton, 15]. These scientists question the value of the consensus diagram on the grounds that, although over time more scientists may have come to profess acceptance of something which they call 'the chemiosmotic hypothesis', these scientists are hiding important differences of scientific opinion behind a superficial terminological agreement. If the chemiosmotic hypothesis means something different for each participant, then its increasingly widespread verbal endorsement in no way indicates that there is growing uniformity of scientific belief within the network.

The interview quotations above show that, although Spencer's consensus diagram *can* be read as a simple, literal description of what has

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happened in the field (6C to 6E), it can also be read as being seriously misleading (6F to 6H). The last three quotations illustrate the grounds on which typical objections to the diagram were based in our interview material. We suggest, however, that these latter quotations have a wider significance than this. For it appears that they address three basic interpretative issues which have to be resolved in *any* claim to describe the state of cognitive consensus in a scientific field. First, in claiming consensus it is implied that the speaker has identified all the relevant members of the field, that is, all those competent scientists who must be considered as working within the area of investigation in question. Secondly, it is implied that the speaker can attribute scientific belief correctly to each individual scientist. Thirdly, it is implied that the cognitive content of the consensus can be specified accurately and shown to coincide with the views of all those who are said to belong to it.

Analysts' and participants' consensus claims

These interpretative issues are not only faced by scientists as they construct their accounts of consensus, but also by any sociologist who attempts to formulate claims about scientific consensus in general or about the degree or nature of consensus within specific research networks. Consider, for example, Ziman's assertion that scientific 'facts and theories must survive a period of critical study and testing by other competent and disinterested individuals, and must have been found so persuasive that they are almost universally accepted. The objective of Science. . . is a *consensus* of rational opinion over the widest possible field.'[3] In order to verify such a claim, one would have to review all those - specialties included under the rubric 'science', identify the competent specialists in each case, and either formulate or locate formulations of the bodies of knowledge to which each of those groups of specialists are committed.

When one examines the secondary literature on science, however, it is evident that, despite the frequent assertion that science is unique in its attainment of cognitive consensus, there are no studies available which delineate in detail the nature and extent of consensus within any particular research network. Moreover, the few studies in which the topic of scientific consensus has been empirically explored show that *the analyst is ultimately dependent for his conclusions on the interpretative work carried out by participants*. Although scientists produce various texts, such as review articles, textbooks and research papers, which, it is suggested, can be used as unobtrusive measures of consensus, scientists' actual beliefs can never be inferred directly from these literary products alone. We have illustrated this in the preceding section.

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The usual sociologists' solution to this problem is to supplement or replace such documentary material with direct questioning of scientists. Such an approach is illustrated in one of the few systematic empirical studies of scientific consensus, in which respondents were asked the following question: 'If you consider the relevant literature on this *speciality field,* how much *agreement* is there with respect to the *theoretical* approaches which ought to be applied. . . those techniques and methods that can be considered as generally accepted. . . the *results* which so far can be considered as generally *accepted?'*[4] In this study by Knorr, 'degree of consensus' becomes equivalent to 'percentage of respondents claiming a high degree of consensus'.[5] The analyst presents respondents with a question which requires them to judge what is the relevant literature, what are their colleagues' scientific beliefs, what is agreed, and who is involved in the consensus; and then the analyst offers the aggregated responses as a measure of consensus.

Clearly, such an approach means that the empirical findings are generated out of scientists' own answers to the underlying questions of membership, attribution of belief and cognitive content. Scientists' reports of consensus could be used satisfactorily in this way, only if the reports could be taken as simple, literal descriptions of a state of scientific belief. But such an assumption is difficult to sustain. A similar assessment of the degree of consensus or the use of a similar label to identify the content of a supposed consensus may mean different things for different speakers. Furthermore, as we will see below, not only is it possible in a specific case for different scientists to give quite different views of the state of cognitive consensus, but each individual scientist, in different interpretative contexts, can furnish quite different accounts of the degree and nature of consensus in a field.

Such interpretative diversity occurs, we suggest, because the meanings of the underlying issues of membership, individual belief and cognitive content are themselves contextually defined and contextually variable. In view of the analytical position presented in our opening chapter and in view of the substantive analyses developed in subsequent chapters, this is hardly surprising. The particular significance of such interpretative diversity here is that it reveals how the data customarily used by sociologists as indicators of consensus are context-linked interpretative products, arising out of participants' solutions to the three underlying issues. Thus we begin to see that to adopt the traditional approach to the analysis of this collective phenomenon, as exemplified above in the writings of Ziman and Knorr, is to do no more than to present the aggregate results of scientists' own 'sociological theorising' about consensus as if it were itself the phenomenon of scientific consensus.

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One implication of this argument is that we need to know more about the way that scientists carry out their 'sociological theorising'. We need to investigate how participants create the appearance of shared belief and how they construct their solutions to the three underlying issues identified above. In the next section we will take a step in this direction by returning to Spencer's diagram and its accompanying text and by examining how Spencer deals with these issues. We will show that participants are able to furnish plausible denials as well as assertions of growing consensus, depending on the interpretative procedures they use to deal with the issues of competent membership, attribution of belief and the content of theoretical categories. The discussion which follows will begin to show how the collective phenomenon of scientific consensus becomes amenable to fruitful empirical analysis only when it is conceived, not as a social fact *sui generis,* but as a contingent product of participants' variable interpretative procedures.

Defining the field and identifying its members

In 6A, Spencer states that his diagram plots the attitudes of some of the principal protagonists in the field and that it is based on a review article published in 1969 by Cranmer (6A8). Cranmer describes his review as covering research on oxidative and on photosynthetic phosphorylation;

and Spencer's diagram includes scientists specialising in both these areas. It seems, therefore, that 'the field' to which Spencer's consensus diagram refers can be seen as being composed of at least these two smaller areas of research. Spencer himself draws attention to this division, when he claims that there has been more resistance to chemiosmosis in 'the field of oxidative phosphorylation' than in '*the field* of photosynthetic phosphorylation' (6A5). Furthermore, in interviews, respondents tended to describe themselves as specialists in one or other of these areas and, although often emphasising that the oxidative and photosynthetic systems are scientifically similar, they stressed that in many respects these sub-fields were intellectually and socially distinct.

On some occasions, then, our respondents treated oxidative and photosynthetic phosphorylation as parts of a single research area. On other occasions, these two realms of study were treated as being relatively separate. However, the shape and significance of Spencer's consensus curve depends crucially on his treating the two potentially separate fields as one in composing *this* diagram. For instance, the top six scientists named on the diagram, who are classified as remaining non-chemiosmotic throughout the whole period, are regularly identified as specialists in oxidative phosphorylation. In contrast, those at the bottom of the

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diagram, who are shown as quickly coming to support chemiosmosis, are said to be preponderantly involved in photosynthetic phosphorylation. Only Spencer and one other among the bottom five names are not defined in Spencer's own text and elsewhere as being mainly concerned with photosynthetic phosphorylation. Thus, it is only by treating the two 'fields' as one scientific entity that Spencer is able to produce his smooth curve of increasing cognitive consensus. If he had adopted in his diagram the distinction between the fields which he employs in his written text, he would have presented two distinct curves. Assuming the same personnel, one of these would presumably have shown very rapid conversion to chemiosmosis in the area of photosynthetic research. The other would presumably have depicted oxidative phosphorylation as remaining largely unaffected by chemiosmosis up to 1973. If this had been done, Spencer's claim, that merely looking at the trend makes it seem likely that his theory of 'oxidative phosphorylation and related energy transductions' will be generally accepted in due course (6A10), would have been seriously weakened, if not completely undermined.

This is by no means the only problematic aspect of Spencer's choice of persons to represent the changing trend of scientific opinion. The bibliography to Cranmer's review, on which Spencer's diagram is said to be based, contains 115 different first authors. Spencer's diagram plots the views of only 18 scientists. Spencer gives no clear indication of how the 18 were selected from the larger pool. He does suggest that he has focused on 'some of the major protagonists'. But no explanation is given of what exactly is meant by 'major protagonist' and no clear rules of inclusion/ exclusion are presented which would enable us to arrive at the particular list of scientists used by Spencer.

Spencer seems to have dealt differently with the top and bottom of the diagram in another respect. The top nine names, largely non-chemiosmotic, are all leading scientists and heads of laboratories who were well established in 'the field' before 1961, the first date on the diagram. In the bottom half, however, we have three scientists who were not in the field in 1961 and one of these, Handel, entered the field as a student of an older scientist also included in the bottom half of the diagram. It is by no means clear why these chemiosmotically inclined new entrants are included in the diagram; nor why some students of those scientists resisting chemiosmosis are not also included. Once more, the procedures for identifying the membership of the group on which Spencer's claim is founded seem equivocal and contingent. Yet, in so far as one reduces or expands Spencer's list, one necessarily alters the characterisation of the pattern of consensus embodied in his diagram.

One further possibility that has to be considered is that Spencer is simply adopting those explicit judgements of support for chemiosmosis which are

contained in the opening paragraph of Cranmer's review. But, although Cranmer's list overlaps with Spencer's, they are not identical. Spencer leaves out one of Cranmer's critics of chemiosmosis and two of his neutrals and introduces three opponents of chemiosmosis, as well as two scientists who are shown as becoming fully converted by 1973 and one who is shown as half convinced by that date. Another major difference between Cranmer and Spencer is that the former's listing of those for and against chemiosmosis is relatively casual and does not claim to be representative, while Spencer, in contrast, uses his similar sample to depict formally the 'trend of support for the chemiosmotic rationale'. The biggest difference, however, between Cranmer and Spencer is that whereas the former offers a sketch of the situation at a single point in time, the latter extends his diagram to cover the period 1961-73. In this respect Spencer goes far beyond Cranmer and it is clear that Spencer's text is 'based' on that of Cranmer only in the loosest sense.

We suggested in the previous section that traditional sociological analysis of the 'collective phenomenon' of scientific consensus was dependent on prior interpretative work carried out by participants in relation to three underlying issues, one of which was group membership. We have now seen that Spencer's portrayal of the pattern of consensus is intimately linked to the way in which he selects particular scientists to represent the field; that various quite different lists could have been compiled with equal plausibility; that the choice of different scientists, given unchanged attribution of scientific opinion to each individual, would have produced a significantly different picture of the changing pattern of cognitive consensus; and that Spencer's procedures for making his selection remain unspecified.

These observations suggest that Spencer did not first resolve the issue of membership through the application of clear-cut, objective criteria and then find that he had an upward consensus curve. Rather, it seems that the task of constructing such a curve informed all his judgements about membership. In other words, Spencer's consensus curve is a creative display of his own interpretation of the changing nature of consensus in ox phos', which is achieved partly through tacit judgements about group membership and which achieves an appearance of facticity partly by treating the issue of membership as unproblematic.

Spencer, of course, never claims that the scientists named in 6A constitute a definitive or comprehensive membership list. Nevertheless, the upward consensus curve, which is utterly dependent on Spencer's choice of personnel, *is* treated in Spencer's written text as an accurate depiction of the actual movement of scientific opinion within a genuine field of biochemical research. Although the diagram appears analytically

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to be highly contingent, it is presented in Spencer's text as a literal description of an underlying social reality. For sociological purposes, however, it is clear that we cannot regard Spencer's diagram as documenting a consensus which exists independently of the interpretative work embodied in that diagram. The appearance of consensus exists only through this interpretative work; which has to be conceived analytically, not as a description of a collective phenomenon 'out there' in the field of bioenergetics, but as an interpretative accomplishment achieved by Spencer on the occasion of his honorary lecture and available as an interpretative resource to others as they create *their* versions of the history of 'the field'.

In the previous section we saw that, despite the apparent contingency of Spencer s consensus claim, many scientists were able to treat it as an accurate description and indeed, like Spencer himself, as merely stating the obvious. For such scientists Spencer's diagram, whatever its possible inadequacies in detail, is treated as documenting a real, underlying pattern to be observed in their field. Spencer's diagram is taken simply as displaying 'what everyone knows'.

> 6J
> *Interviewer:* I wondered... whether you thought his overall trends were accurate.
> *Perry: [Looking at 6B]* I would think so. Has anybody objected to it? I wouldn't expect it. I think I can tell you who would object to it probably . . . I don't think that's important. I think that basically it is accurate. [Perry, 12]

When scientists respond in this way and express agreement with Spencer, they never raise questions about his interpretative procedures. Like the speaker in 6J, they sometimes recognise interpretative problems, but they dismiss these as minor imperfections which in no way detract from the overall accuracy of Spencer's consensus claim. When respondents deny Spencer's claim, however, they can trade upon underlying interpretative issues as grounds for wholesale rejection of Spencer's account. In so doing, speakers can focus on the issue of membership of the field, as in quotation 6F. But more frequently they focus on the issues of the content of the supposed consensus and the attribution of belief to individual scientists.

Attributing scientific belief

In the review articles by Spencer and Cranmer, the views of individual scientists with respect to oxidative phosphorylation are identified only by means of allocation to categories such as 'supporting chemiosmosis',

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'supporting chemical coupling', or 'adopting a notably neutral attitude'. The allocation of individuals to these categories is either undocumented or is warranted by references to one or a few of their published papers. The attribution of scientific belief is treated as unproblematic, in so far as no reservations are expressed in either text about its accuracy.

The method of attributing belief adopted by the authors of these reviews seems to involve several interpretative procedures. First, attributions are made as if each scientist, at any particular point in time, has a specifiable scientific view with respect to a range of biochemical phenomena. Secondly, it is taken for granted that each scientist's position can be accurately identified by other scientists from his research publications. Thirdly, it seems to be assumed that differences between individuals' beliefs are relatively unimportant and that the similarities between their views coincide neatly with the major hypotheses in the field. These general procedures appear to provide part of the interpretative resources which participants use to construct accounts of cognitive consensus.

Although these procedures are regularly employed when participants' interpretative work entails the unproblematic attribution of belief, they can be explicitly abandoned in other interpretative situations. Consider the following exchange.

> 6K
>
> *Interviewer:* Did Waters fully understand the chemiosmotic theory?
>
> *Barton:* Not when I got there, no... he probably would have liked the chemical theory to have come through, after all. Because that's what he'd been personally committed to. But he was certainly absolutely objective and well capable of recognising the force of any other theory and I'm sure, by the time I left, he was in favour . . .
>
> *Interviewer:* I'm not very clear about Waters. Is there a point at which Waters comes to accept the chemiosmotic theory?
>
> *Barton:* I couldn't tell you that. You'd have to ask him that. I left in 1971 or so. [Barton, 14]

In this passage, the respondent initially has no qualms about attributing beliefs to his erstwhile supervisor, nor about categorising his views in terms of the two major theories. Waters is said not to have understood the chemiosmotic theory when Barton first arrived in his laboratory and he is described as having been personally committed to the other theory. Then Barton, as a way of illustrating Waters' scientific objectivity, proceeds to assert that he is sure that Waters was in favour of the chemiosmotic theory by the time he left the laboratory. In the first paragraph, therefore, the speaker adopts an approach to the attribution of belief which is similar to that used by the authors of our review articles and by those scientists who accept Spencer's diagram at face value. Yet subsequently, on the same

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page of transcript, he denies the very claim he has just made about Waters having come to favour chemiosmosis. And he does this, not by stating that he had made a mistake about Waters' specific scientific beliefs, but by maintaining that it was quite impossible for him to ascertain at all what Waters' scientific ideas were. Thus, having confidently attributed various scientific views to Waters (I'm sure . . . he was in favour'), the speaker questions the very possibility of making any such attribution ('You'd have to ask him that').

What are we, as analysts, to make of such apparently inconsistent assertions? We suggest that scientists' ability, on occasion, to repudiate their own attributions of scientific belief, in the course of conversation, makes it difficult for analysts to accept such claims as anything more than contingent formulations which are devised in accordance with variations in interpretative context. Thus Barton's claim that Waters had come to favour chemiosmosis can be seen as a way of re-establishing Waters' scientific objectivity, which had been put in question by Barton's prior statements about Waters' personal commitment to the chemical theory and Waters' failure to adopt, at least initially, the theory which Barton portrayed as firmly established by the empirical evidence. This example shows, then, that the interpretative procedures identified above, by means of which participants attribute scientific belief, can be variably implemented and it suggests that they are used by a given speaker only in certain kinds of interpretative situations.

If attributions of belief in interviews are treated as context-dependent interpretative accomplishments, there seems no reason to regard them differently when they occur in review articles. The relative absence of obvious contradictions among the claims advanced in particular written texts is probably due to the care with which such texts are prepared, to the use of a restricted interpretative repertoire and to the absence of that direct interaction with other actors which elicits variable responses in so many subtle ways. Of course, even in the transcripts of informal interviews, the kind of blatant and immediate retraction exemplified in quotation 6K occurs only rarely. Taken alone, therefore, this passage cannot justify our claim that the attribution of scientific belief is highly variable and socially contingent. However, the divergent attributions made by different participants provide much additional evidence of this phenomenon.

We have a great many instances where the beliefs of particular scientists appear to be characterised very differently by different speakers. This material is illustrated in quotations 6L and 6M. These two passages have been chosen because they both refer directly to Spencer's consensus diagram (6B) and because they both deal succinctly with a number of leading members of the field.

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> *6L*
>
> [The consensus diagram] was partly just to indicate rather pleasantly and lightheartedly that perhaps Max Planck wasn't right and that this thing [the curve of support for chemiosmosis] might eventually get to the top in not too short a time, which as it happened is exactly what it did. Well, it hasn't quite got to the top. I don't suppose it ever will . . .
>
> People like Brian, who was very much involved with the other theory, were working hard to make sure that the chemiosmotic hypothesis was accepted as a theory, for the time being. . . I would have said his attitude, Purcell's attitude, Holst's attitude, all the big people . . . the people involved are marvellously willing *in the long run,* or they have been in my experience, marvellously willing to be altruistic and to work for the beauty of our subject, in the end. [Spencer, 61 and 57]

> *6M*
>
> 1 Many people have accepted it grudgingly, but many others - I think you will find that the people who were the most committed to other things haven't actually accepted it. 2 They have redefined their own theories. 3 But I mean, in fact, if you *now* take what they say and translate it into his terminology, they are actually saying the chemiosmotic

> hypothesis. 4 But most of them would not admit - I am sure that, looking down this list, that Holst hasn't; that, Brian is in a funny situation, he in many ways is perhaps the most objective of scientists, but he is I suppose a fence sitter. 5 Party, well he's redefined everything, he's saying the Spencer thing but in very metaphysical terms. 6 Dowland I don't think has been converted at all. 7 Purcell certainly hasn't. 8 Bridge just somehow avoids it, he just talks about other things completely. 9 Arnold doesn't. Arnold is now more antagonistic than he ever was. 10 So if you take the top half of those people [on the diagram], they haven't really been converted. [Harding, 21-2]

In quotation 6L, Spencer brings the consensus curve up to date. He suggests in the first section that the curve has now (1979) reached the top, that is, that almost everybody accepts chemiosmosis. In the next section, he mentions three leading figures explicitly and he refers to 'all the big people' as having acted altruistically, that is, as having abandoned their previous incorrect views, as having ultimately admitted the scientific superiority of the chemiosmotic hypothesis and as having come actively to advocate its general acceptance. Thus the current views of virtually all the major researchers in the field are depicted as being basically similar and basically chemiosmotic.

In contrast, the speaker in 6M gives a much more complex account of the current situation as he rejects the idea of a strong chemiosmotic consensus. He allows far greater variation between individuals. He treats the identification of belief as more problematic. And he summarises the

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overall situation as one of continued resistance, rather than conversion, to chemiosmosis. Harding contends at the outset that, not only have people adopted chemiosmosis grudgingly, but that many of them have not actually accepted it at all (6M1). He then seems to make a distinction between what people *say* they believe and what they really do believe. He suggests that, if you translate or restate the scientific claims of some of these people into Spencer's terminology, their views appear to be identical with the chemiosmotic hypothesis (6M2-3). But, he suggests, most of them will not admit this (6M4). Thus Harding is proposing here that the identification of individuals' scientific views is by no means straightforward and that it may sometimes involve a process of translation into terms rejected by the scientist himself (6M3).

At this point (6M4) the speaker, after a pause, refers to Spencer's list of names and produces brief summaries of their scientific views. He seems, in this sequence, to be describing what each of these scientists really believes and not merely what they say. Thus, Parry is 'saying the Spencer thing but in very metaphysical terms'. The others, however, and this includes all the 'big people' described by Spencer as accepting chemiosmosis, are depicted as not really believing at all in that theory (6M6-9). Moreover, Harding attempts to discriminate between different shades of scientific opinion among this collection of scientists. Some of them, he suggests, can be translated into chemiosmoticists (6M3 and 5). Some of them are indifferent (6M4). Some of them are actively opposed (6M7 and 9). And the views of others cannot be ascertained (6M8). Thus, whereas Spencer, in the positive consensus claim above (6L), treats the views of this group of scientists as similar, as essentially chemiosmotic and as illustrating the completion of the trend identified in the consensus diagram, Harding emphasises the current diversity of views, the lack of consensus with respect to chemiosmosis and the failure of Spencer's consensus curve to continue up to the present day.

In making this comparison between passages from Spencer's and Harding's interview transcripts, we have been able to illustrate how radically different scientific views can be attributed and frequently are attributed to given participants by different speakers; and to show that the identification of individual scientists' views by other participants is a complex and potentially variable interpretative achievement. We saw in the last quotation, as we saw in 6F to 6H, how scientists tend to make explicit and to undermine the kind of interpretative work involved in presenting a positive consensus claim as they warrant their own rejection of such a claim. Thus, as Harding deconstructs Spencer's account of consensus in 'ox phos', he comes to question whether scientists' views can be easily discerned in what they say or write. He also questions the

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assumption that each scientist has a single, coherent scientific position. In other words, he begins to repudiate some of the interpretative procedures identified above which are treated as unproblematic in Spencer's and others' positive consensus claims.

We have also seen that the attribution of individual belief is, at least sometimes, closely associated with the use of the names of specific theoretical positions. This feature of our data may be due, to some extent, to the fact that our interviewees tended to talk about others views in connection with Spencer's diagram, which is organised around a simple division between the chemiosmotic theory and all other theories. Thus, there is no guarantee that their accounts of consensus in other everyday situations would closely resemble in this respect those forthcoming in the interviews. Nevertheless, nobody objected to Spencer's diagram on the grounds that its categories were inappropriate or unfamiliar. Moreover, every speaker identified numerous scientists who were chemiosmoticists and others who were, or had been, committed to the chemical theory. Indeed, these simple theoretical labels were used time and time again by our respondents to identify groups of participants whose views could be treated as scientifically equivalent or identical, and who therefore constituted a scientific consensus which, in the limiting case of 'chemiosmosis', was sometimes presented as virtually coinciding with the research network itself. The terms 'chemiosmoticist', 'chemiosmotic hypothesis', 'chemiosmotic theory', 'chemiosmosis', 'Spencer's hypothesis', and 'Spencerian theory' were especially pervasive in our interviews. Let us examine how participants interpreted and used these terms in relation to the topic of consensus.

The meanings of chemiosmosis

Let us begin this section by looking at the meaning given by Spencer to the term 'chemiosmotic hypothesis' in his Nobel lecture. This lecture took place five years after that in which Spencer presented the consensus diagram examined above and shortly before his interview with us. Although Spencer does not offer any graphical representation of changing opinion in the later lecture, he does give a verbal account of growing scientific consensus which brings that diagram up to date. One of the themes in this lecture is that research on biological energy transduction disproves Max Planck's famous dictum that new ideas are accepted only after their opponents die (see also 6L above). In his Nobel lecture, Spencer begins by stating that 'what began as the chemiosmotic hypothesis has now been acclaimed as the chemiosmotic theory'. This theory, he suggests, is designed to answer three elementary questions about respiratory chain

systems and analogous photoredox systems: 'What is it?', 'What does it do?', 'How does it do it?' He continues, 'we can now answer the first two [questions] in general principle, and . . . considerable progress is being made in answering the third'. Thus 'with few dissenters, we have successfully reached a consensus in favour of the chemiosmotic theory'.

In the course of the lecture, a body of experimental evidence is reviewed and clear-cut answers provided to the initial questions. Although Spencer notes that there is still much to be understood about the details of the biochemical processes involved in energy transduction, he emphasises that chemiosmosis is an empirically concrete and experimentally validated series of propositions which describes in some detail the structure and functioning of respiratory and photoredox chain systems, which also explains the coupling between respiration and oxidative phosphorylation, and which extends beyond this limited range of phenomena to provide general principles applicable to other significantly different biochemical systems.

These points were repeated in our interview with Spencer, in which he referred once again to the existence of a 'pretty broad consensus' in favour of chemiosmosis. However, immediately after making this point, Spencer went on to point out that other scientists did not always fully understand what 'chemiosmosis' means. In other words, their versions of chemiosmotic theory differed from his.

> 6N
> Of course, people tend to take a very simplified view of a theory and say that *is* the theory. People have tended to say the chemiosmotic theory says that protons must go right out into the bulk [aqueous phase] and come back from the bulk. Well, it never said anything of the kind. . . I couldn't possibly fail to know that the surface conductance [at the outer surface of the membrane] is likely to be considerably higher than the bulk conductance. So I would never have been fool enough to say that they normally go right out. [Spencer, 70]

In this passage, Spencer identifies a particular 'misunderstanding' which some scientists have about chemiosmosis. In the passage which follows we find a scientist apparently exemplifying this misunderstanding.

> 6P
> 1 Spencer will just not consider anything about surface phenomena at all . . 2 The interface between the membrane surface and the bulk just doesn't exist and you know, it damned well does!... 3 But I don't worry myself about it too much, while some other people will go around saying: 'Oh, the whole Spencer scheme is wrong because he's forgotten the interface.' 4 Of course, at the end of the day, it's relevant . . . 5 [But] I

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> don't think it matters that much . . . 6 What difference does it really make? 7 The concept is that you use the thermodynamic gradient of protons to make ATP. [Grant, 69]

In 6P, Grant describes Spencer's hypothesis in a way which we have just seen Spencer himself expressly repudiating; that is, he claims that chemiosmosis ignores the issue of surface conductance (6P1-2) and he proffers a highly simplified version of the concept of chemiosmosis (6P7). On these specific occasions, therefore, the meaning of chemiosmosis seems to differ for these two scientists. Grant refers to other scientists who treat Spencer's supposed failure to deal with surface conductance as integral to his theory and, therefore, as grounds for rejecting the theory (6P3). But Grant portrays himself as not agreeing with these scientists about the centrality of the phenomena of surface conductance to the chemiosmotic theory (6P5-6). Chemiosmosis, he suggests, can be stripped down to the basic notion that ATP is made by a gradient of protons (6P7). By redefining chemiosmotic theory in *this* way, Grant is able to separate himself from its critics and to present himself as a chemiosmoticist; as part of the consensus recognised by Spencer (6P3-6). Yet, in so doing, not only does Grant propose a version of chemiosmosis which differs in detail from Spencer's in relation to the specific issue of surface conductance, but he also promulgates precisely the kind of grossly simplified view of chemiosmotic theory which Spencer condemns at the beginning of quotation 6N. In other words, the appearance of scientific agreement between these two researchers is maintained only at the terminological level. It is accomplished by their both using the same theoretical label, namely, 'chemiosmosis', to refer to scientific interpretations which differ considerably in substance.

In our interviews, almost every scientist clearly operated with at least two versions of chemiosmotic theory. On the one hand, chemiosmosis was depicted as a theory dealing in some detail with the processes involved in oxidative and photosynthetic phosphorylation. At this level, the scope of the theory was similar to that covered in Spencer's Nobel Lecture. The content of the theory, however, and the degree to which it was taken to be experimentally validated, differed from one speaker to the next. There was little evidence of a uniform version at this level persisting from one speaker to another and much evidence of scientific disagreement. On the other hand, there was a highly simplified, basic version of chemiosmosis. This version was widely used by our respondents and can be seen as constituting, in some sense, a consensus. However, the scientific content of this basic version was minimal; so much so that it could be and frequently was endorsed by those who described themselves and were described by

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others as strongly opposed to chemiosmosis as well as by those who claimed to be fervent supporters of the theory.

Although Spencer and many others noted the widespread use of a simplified consensual version of chemiosmosis and although speakers frequently remarked on the diverse interpretations of chemiosmosis to be found in the research network as a whole, none of our respondents commented on their own use of two versions of chemiosmosis. Rather, each respondent moved implicitly from one version to the other, as he talked about the theory and the degree of consensus, in accordance with the interpretative requirements of his discourse. In the following quotations, as in 6P, we can clearly see the speaker moving between a detailed and a basic version of chemiosmosis.

6Q

(a) 1 Even now there is this huge craziness [about stoichiometries]. 2 I think it is of relatively minor importance what the stoichiometry is . . . [although] it is an important question, because it has to do with the mechanism and generation of the electro-chemical gradient. 3 And of course Spencer's concept of loops is beautiful in its simplicity. 4 There's very little evidence for it and in my own case, I mean that business with the electron donors, finding inhibitors working at different steps, that still doesn't fit his loop theory and its hard to see how it could fit a loop theory ... 5 People do not want Spencer to be right all the way. 6 They are willing to say 'OK well, the proton gradient has something to do with ox phos, but is it the only thing and is his stoichiometry right?' 7 That's nonsense. 8 There's *no* question in my mind that the overall theory is correct . . . 9 I can take every piece of data that I couldn't explain, except for the things that have to do with the loop and have to do with the generation of the electro-chemical gradient, I can take every piece of data and explain it ... [Cookson, 15]

(b) 10 [The evidence] certainly doesn't *favour* a loop mechanism. 11 Now I don't make a big thing out of that right now because, from *my* point of view which has always been the translocation of substrates, it's not that important. [Cook-son, 20]

(c) 12 I think that without question the overwhelming percentage of what Spencer has to say is right and I think there is very little that one can argue against that [Cookson, 21]

(d) 13 I don't see the kind of dialogues taking place at meetings

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that did take place 10 years ago. 14 There was a lot more open discussion and debate 10 years ago than there is now. . . 15 One of the reasons is of course that the truth of the Spencer business has become very apparent, so there is not a lot to argue about. [Cookson, 32]

In quotation 6Qa, Cookson begins by defining the current debate over stoichiometries, that is, over the number of protons crossing the membrane, as crazy. It is crazy, he suggests, to engage in heated debate because the implications for chemiosmosis are fairly trivial. Yet he goes on to state almost immediately that the outcome of what he has called the 'battle over stoichiometries' will actually have major consequences for participants' conception of the basic mechanism of energy transduction. Thus, this controversial aspect of chemiosmotic theory is both important and unimportant (6Q1-3). These remarks are confusing, if they are read literally as referring to a single theory, namely, *the* chemiosmotic theory. They become more understandable, however, in the light of the speaker's subsequent adoption of two versions of chemiosmosis, one of which excludes all those elements which he defines as still being scientifically problematic (6Q9). Like the previous speaker, Cookson changes the scope and content of the theory as he speaks in such a way that the essential validity and general acceptance of one of these versions can never be in doubt.

Spencer's position on stoichiometries is often linked in the research literature to his conception of protons being moved across the membrane by a configuration of loops. Consequently, Cookson comments in passage *a* on Spencer's formulation of the loop mechanism. He states that there is very little evidence in support of that mechanism (6Q4). Nevertheless, he claims, this does not mean that 'chemiosmosis' is reduced to the kind of crude, basic version that we found in quotation 6P (6Q6-7). Chemiosmosis, he maintains, is more than the assertion that 'the proton gradient has something to do with 'ox phos'. Cookson proposes that there is something called 'the overall theory' which has actually been shown to be correct (6Q8). He then suggests that this theory has a very detailed and specific biochemical content (6Q9). However, he then formulates this overall theory in such a way that the loop mechanism and anything to do with the generation of the electrochemical gradient are expressly excluded (6Q9). Furthermore, in sentences 10-11, he identifies the aspects of the chemiosmotic theory with which he is concerned as those dealing primarily with transport of substrates rather than with the manufacture of ATP. Thus, for this speaker in these passages, chemiosmosis is presented as some kind of general, yet detailed, theory which to a considerable degree has been experimentally confirmed; yet, at the same time,

supposedly the same theory is interpreted in idiosyncratic terms as covering only those phenomena with which the speaker himself is centrally concerned and as composed only of those theoretical claims which he takes as validated.

Like Grant (6P), Cookson presents himself as a Spencerian or as in agreement with chemiosmotic theory, yet as differing in various significant respects from Spencer and as endorsing only a fairly narrow selection from Spencer's published claims. In quotations 6Qc and d, Cookson makes strong assertions about the degree of consensus with respect to chemiosmotic theory; and he goes on to use this supposed consensus to account for the lack of public argument over chemiosmosis in recent years. It is quite unclear, however, what Cookson means in these passages by such phrases as 'what Spencer has to say' and 'the Spencer business'. Which aspects of chemiosmotic theory is he referring to?

We suggest that in quotations 6P and 6Q, the speakers clearly alter the meaning of such terms as 'chemiosmotic theory' and 'Spencer's hypothesis' as they proceed. By varying the meaning of these terms, they are able to allow for the existence of a range of scientific disagreements among their colleagues, often with respect to apparently fundamental issues, and for marked differences between their own formulations and those proposed by Spencer in the literature, without giving up their claim that there is a substantial, even overwhelming, cognitive consensus. Grant achieves the appearance of consensus by reducing chemiosmosis to a basic, consensual version and by 'showing' that his own detailed differences with Spencer are not inconsistent with acceptance of this essential chemiosmotic concept. Cookson explicitly rejects the use of any grossly simplified version of chemiosmosis; a device which he has presumably encountered in the course of informal discussion with his colleagues. Instead, he is able to construct his strong consensus claim by blurring the distinction between Spencer's 'overall theory' and his own personal and partial interpretation of that theory. Cookson's use of the notion of 'the overall theory' is in practice equivalent to Grant's notion of 'the basic version'. Both concepts are employed to exclude from chemiosmotic theory whatever the speaker does not accept and whatever the other members of the field are said not to accept; whilst at the same time, both concepts are used as if they refer unequivocally to some theoretical entity, namely, the chemiosmotic theory, which exists independently of speakers' highly variable interpretations. Thus we can observe these speakers sustaining an appearance of consensus in their discourse through the subtle deployment of various versions of a theory which is said to be generally accepted.

Let us offer just one more illustration of the variable meaning of 'chemiosmosis'.

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6R

(a) 1 I think people in general accept chemiosmosis up to a point: that electron transport certainly generates a potential across the membrane, certainly generates a movement of protons when measured under certain circumstances. 2 And the evidence certainly shows that a membrane potential or a proton gradient or both can make ATP. 3 But it doesn't hang together when you take it much further than that. [Milner, 22]

(b) 4 Spencer postulates a movement of the protons between the two bulk phases. 5 Now recently, a number of experiments have come out that suggest that the pathway for the protons is not between two bulk phases, and maybe only between one bulk phase and the membrane or maybe even within the membrane itself . . . 6 So a bulk pH gradient across the membrane does not appear to be a high-energy intermediate state that's required to make ATP. 7 That was predictable anyway because Jarvis had earlier found . . . [Milner, 23]

(c) 8 Spencer pointed out that you don't get oxidative phosphorylation unless you have a complete vesicle. 9 Now that's been taken as an article of faith for many years and it made a lot of sense. 10 The only trouble is that the force of the argument now looks in retrospect not very great. 11 Because. . . there is no way to make a piece of membrane that is not a vesicle. 12 The only way you can get any pieces of the whole system that is not a vesicle is to put it into a strong detergent. 13 The detergent replaces the membrane. 14 So . . . you no longer have a membrane and you no longer have oxidative phosphorylation. 15 But that isn't a very good test, because you haven't got a piece of membrane any more and furthermore the detergent is inhibiting all these enzymes [which make ATP] . . .16 So the force of that argument [in favour of chemiosmosis] is now lessened a great deal. [Milner, 24]

(d) 17 Well, I think [these results] mean that the original form of the chemiosmotic hypothesis, that a complete membrane vesicle is required and that a proton gradient is required across the membrane, if these results can all be confirmed, they imply that what might be involved is a proton cycling. 18 *I would still regard this as chemiosmotic, although it's an unfortunate name then* [emphasis added]. 19 I would tend to want to rename the idea as an electro-chemical proton

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> mechanism ... 20 That's the way the wind is blowing currently. [Milner, 25]

These four fragments are taken from a longer passage in which the speaker identified a series of difficulties with the chemiosmotic hypothesis. Two of these are partly reproduced in sections 6Rb and c; and their theoretical implications are summarised in 6Rd. In 6Ra, the respondent offers a typical consensual version of chemiosmosis. He briefly summarises those aspects of chemiosmosis which people in general accept and his version, although short, seems roughly in line with Spencer's own published formulations; that is, it refers to electron transport creating a trans-membrane gradient which produces ATP.

In the two following sections, however, Milner brings into question two of the fundamental claims of chemiosmosis, namely, that a closed membrane (6R8-16) and a bulk gradient across the membrane (6R4-7) are required for ATP synthesis. He asserts that there are now good grounds for abandoning these features of chemiosmotic theory. The speaker recognises that further confirmation of recent experiments is required (6R17). But he suggests, unlike Spencer in the Nobel Lecture of the previous year, that the trend of opinion is currently moving away from these chemiosmotic assumptions (6R20). Nevertheless, he avers, he would still regard the processes involved as chemiosmotic (6R18). In making this claim Milner seems to stretch the meaning of the term chemiosmosis to its limits. He stresses that he is moving away from the central assumptions of chemiosmosis to such an extent that the very word no longer seems appropriate (6R19). Yet, he maintains, his approach is still in some sense chemiosmotic. Like the two previous speakers, Milner maintains an appearance of general acceptance of chemiosmosis by subsuming radically different scientific claims about specific biochemical processes under a highly general interpretation of that term.

Slightly later in the interview, having proposed further necessary alterations to chemiosmotic theory, he offered another consensual version of chemiosmosis. This version is even more basic than that offered in 6Ra. It is formulated in such a way that the essence of chemiosmosis can be seen clearly to include even the radical innovations which he has just recommended.

> 6S
> Everyone accepts that the fundamental particles of oxidative phosphorylation are the electron and the proton. If there was nothing else to the chemiosmotic hypothesis than this it would still be a very important contribution. . . I think the whole fraternity working in the field feels that Spencer has been very doctrinaire in his attitudes towards his own

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> hypothesis. When you look back on the history of many scientific hypotheses, they've all had to be modified in one way or another . . . [Milner, 26-7]

In this passage, the basic contribution of chemiosmosis is taken to be that of adding the proton to the electron as one of the fundamental particles involved in the production of ATP. From this point of view, any analysis of oxidative phosphorylation which includes both these particles can presumably claim to be chemiosmotic. There can be no doubt that this definition of 'the chemiosmosis which everyone accepts' is a far cry from the versions given by Spencer in his papers and lectures and in his interview. However, it is Spencer who is taken to task here for refusing to redefine his version of the chemiosmotic theory so as to bring it into line with the 'whole fraternity' of scientists working in the field. Spencer is criticised for being 'doctrinaire in his attitudes towards *his own hypothesis*'; that is, in this passage, for treating chemiosmosis as a concrete, detailed theory, rather than as a basic claim that protons as well as electrons are important. In this quotation, the speaker clearly treats 'Spencer's own hypothesis' as identical to the particular interpretation of chemiosmosis which he happens to formulate at this juncture. This respondent, like those quoted above, presents himself as uniquely able to speak on behalf of the theory in question. It is through *his* voice that the chemiosmotic theory which is coming to be agreed makes itself known.

In this section, then, we have seen how the meaning of such terms as chemiosmotic theory', 'Spencer's hypothesis', and so on, vary from one speaker to another. We have also seen that each respondent employs more than one interpretation of chemiosmotic theory in the course of the informal talk occurring in interviews. We have suggested, as a first step in analysing this kind of data, that we treat each actor as moving between an idiosyncratic version of chemiosmosis and a consensual version.

The interpretative variability of 'chemiosmosis' is not easily discerned in the ordinary course of events. Much of the time, it is hidden by the character of scientists' discourse about consensus. For researchers regularly speak as if 'chemiosmosis' is an entity held in common with most other colleagues. They each proceed as if the specific version of the theory that they are engaged in proposing is 'the real chemiosmosis' which is coming to be accepted or rejected by the field. They continually construct their accounts as if they are referring to 'a theory' which exists independently of their interpretative work. However, the detailed comparisons between accounts carried out above reveal that the apparent facticity of chemiosmosis and its apparently widespread endorsement are illusory in the specific sense that they exist, not as objective entities in an

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external social world, but only as attributes of participants' contingent consensus accounts.

Consensus as an occasioned interpretative product

The consensus accounts we have examined are always closely linked into the rest of the speakers' discourse.[6] For instance, Spencer used his consensus diagram in the early 1970s as a basis for revealing the dogmatism of some of his opponents as well as for prophesying about the future development of the field. His subsequent claim in the Nobel lecture that most of his major opponents had at last been converted to chemiosmosis, enabled him to characterise his erstwhile antagonists as basically altruistic and as willing, in the long run, to restrain the promptings of self-interest for the benefit of science. Others used assertions of a chemiosmotic consensus to explain why the field was closing down. Still others moved from denying that consensus to endorsing the scientific superiority of alternative theories and to identifying a range of emergent scientific problems which guaranteed that the field would be intellectually lively for years to come. Thus assertions and denials of cognitive consensus are important building blocks in scientists' discourse. They play a significant part in helping scientists to construct forceful and coherent characterisations of their social and intellectual world.

In the analysis above, however, we have been less concerned with studying how consensus accounts contribute to the meaning achieved in extended sequences of scientists' discourse, than with the interpretative structure of consensus accounts themselves. We have shown that Spencer's claim as formulated in his diagram could be read as contingent, as well as a literal description of the self-evident. Those scientists who challenged Spencer's claim drew attention to his 'inaccuracies' in identifying the membership of the field, in specifying individuals' scientific views and in describing the scientific content of the supposed consensus. These challenges made visible the three basic interpretative issues which can be seen to have been resolved in any consensus account.

We suggest that it is impossible for participants to furnish definitive solutions to these three interpretative issues. For such solutions involve unformalisable, practical judgements; judgements which are indirect, inferential and dependent on the particular interpretative context in which the judgement is being made. For instance, as we showed above, there is no single, unambiguous way of defining 'the field' in which our scientists work. Similarly, scientists' beliefs cannot be directly observed by their colleagues. Rather they are inferred from the published literature and from

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informal discussions, and then subsumed within a narrow range of conventional theoretical categories, the meaning of which appears to differ from one researcher to the next and from one occasion to the next.

This does not mean that scientists construct consensus accounts in a random fashion. Certain recurrent interpretative methods seem to be regularly employed in the accounts asserting consensus that we have examined. For instance, the following procedures appear to be in evidence **in** our material:

(a) treat each scientist as committed, at any given time, to a single scientific viewpoint or belief;
(b) treat each viewpoint as clearly evident in a scientist's written products and informal statements, yet as something separate from these products;
(c) treat each theoretical label as having a clear, invariant meaning;
(d) treat the view of (most) individual scientists as coinciding with one of the current theoretical labels;
(e) employ consensual and idiosyncratic versions of a theory so as to reconcile cognitive variation with the existence of consensus.

It is tempting to refer to these recurrent features of scientists' consensus accounts as resulting from the existence of a widely shared scientists' 'folk theory of cognitive consensus'. It seems to us, however, that the notion of 'folk theory' should be avoided in this case.[7] For the features we have identified are not explicitly stated by participants themselves. They are, rather, analysts' formulations describing certain interpretative procedures[8] which seem to occur regularly in a collection of accounts. There is no evidence to suggest that they depend on a theory held by participants. Thus, the features summarised above are best seen as recurrent interpretative procedures which are embodied in the collection of accounts under investigation, in the sense that they can be made visible by the kind of systematic comparison we have adopted.

These general procedures (a-e) appear to help scientists resolve the three underlying interpretative issues. Indeed, they can be described as ways of reducing or concealing interpretative contingency. For example, in treating the attribution of individual belief as unproblematic in giving an account of consensus, the scientist is ignoring those occasions, which are endemic in informal interaction in science, when scientists experience enormous difficulty in comprehending one another's technical arguments. Similarly, by treating each theoretical label as having a clear, invariant meaning, participants create an aura of facticity for each theoretical position and convey the misleading impression that those scientists subsumed under a given label actually endorse the same set of scientific beliefs. Nevertheless, although these procedures enable scientists to

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disregard huge areas of interpretative contingency, they do not place any narrow restrictions upon the precise content of consensus accounts. For, as we have seen, not only do the consensus accounts of different scientists differ considerably, but the accounts produced by a given scientist vary from one occasion to another. In this sense, consensus accounts can be seen to be 'occasioned', even though they reveal certain recurrent interpretative features which can be observed and formulated by the analyst.

It would be misleading, therefore, to interpret consensus accounts as following inevitably from participants' resolution of the three basic issues by the application of a set of determinate rules to particular cases. This was seen most clearly above in connection with Spencer's consensus diagram. We showed in that case that there seemed to be no readily identifiable procedures for identifying membership, attributing belief, and so on, which *led* Spencer unavoidably to end up with a smooth upward curve of consensus. Thus, scientists' consensus accounts are neither literal descriptions of an independent social reality, nor are they the necessary outcome of scientists standardised interpretative procedures. They are, rather, the means by which scientists make available to us, and to their colleagues, versions of the state of collective belief which are appropriate for specific interpretative occasions.

Analytical implications

Scientific consensus has been treated by sociologists as a typical collective phenomenon, that is, as a potentially measurable aggregate attribute of social groupings which exists separately from the interpretative activities of individual participants. Nevertheless, empirical study of scientific consensus clearly does depend on individual scientists' interpretative products. In the extreme case, like that of Knorr's study mentioned above, the sociologist establishes the degree of consensus simply by aggregating the consensus accounts of a number of individual scientists. But even less direct studies of consensus, for example, those using review articles or citations, depend unequivocally for their conclusions on scientists' own, context-linked and potentially variable symbolic products.[9]

In this chapter, we have examined several kinds of symbolic product which could plausibly have been used by sociologists as indicators of scientific consensus in 'ox phos'; for example, a review article, two honorific lectures and sections from interview transcripts. Moreover, the Nobel Prize has recently been awarded, a particular theory is coming to dominate the textbooks, prior theories appear to have been widely repudiated and strong consensus claims can be found in the interviews and

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elsewhere; all apparently clear signs of the existence of consensus in this research area. Yet we have shown that participants' consensus accounts are highly variable and that their meaning is linked *to* the interpretative situation in which they occur. We have shown that the consensual character of 'ox phos' can be either constructed or deconstructed, not only by different participants, but also by the same participants as they engage in new interpretative work. In view of these observations, it appears that, for the purposes of sociological analysis, a given field at a particular point in time *cannot be said to exhibit a specifiable degree of consensus*. Rather, the field must be said to exhibit varying degrees of consensus, depending on the discourse of those involved.

Scientific consensus, then, is neither distinct from members' discourse nor is it open, even in principle, to definitive measurement at any specific juncture in a field's history. Consequently, traditional analysis of this topic is doomed to failure. If consensus is open to various construals, there is no point in trying to show how a range of other social factors vary in accordance with *the degree* of consensus. Unlike traditional analyses, however, the form of interpretation we have begun to develop above is not undermined by the variability of discourse about consensus. For we focus analytically, not upon the highly variable consensus claims produced by participants, but upon the recurrent interpretative methods whereby variable symbolic products, such as consensus accounts, are contextually generated.

It is important to recognise that the interpretative procedures which we have identified are not the personal interpretative achievements of individual scientists; even though each text or utterance in which these procedures appear *is* a unique product. The objective of our analysis has been to identify recurrent, regularly used, and in this sense collective, cultural resources which are embodied in and visible in participants' discourse. Thus, in our treatment of the construction and deconstruction of consensus in 'ox phos', we have not been trying to replace the traditional analysis of collective phenomena with an individualistic perspective. Rather, we have been developing an alternative and more fruitful approach to the investigation of *social* regularities.