

## CONTEXTS OF SCIENTIFIC DISCOURSE: SOCIAL ACCOUNTING IN EXPERIMENTAL PAPERS\*

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Almost all analysis in the sociology of science has involved attempts to describe scientists' social actions and 'technical' beliefs. For example, much effort has been devoted to investigating whether scientists, in the course of their research, act in a detached, impersonal, universalistic manner and whether these forms of action are required for the regular production of valid scientific knowledge (1). Other investigators have sought to provide definitive descriptions of the 'main features' of particular scientists' beliefs as a preliminary to explaining the beliefs as having been moulded by the actors' socially derived interests (2). In recent years, however, there has been a growing although by no means widespread recognition that neither social action nor technical belief in science can be identified unequivocally for the purposes of sociological analysis (3). This is because it has become increasingly clear that different scientists can and do give quite divergent, yet equally plausible, accounts of the 'same' act or the 'same' belief; and that particular actors tend to alter their accounts of their own and of others' actions and scientific ideas as they respond to new social situations (4). As a result, some sociologists concerned with the study of scientists' meaningful actions, as distinct from scientists' 'behaviour', have come to see that meaning does not reside in the actions themselves but in the context-dependent procedures of social accounting whereby actions are interpreted. It has also become increasingly clear that scientists' beliefs, as distinct from the variable context-

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dependent formulations which scientists produce, are inaccessible to the sociologist. Consequently, a few sociologists have begun to focus, not on the description of scientific action or belief itself, but on the ways in which scientists construct varying accounts of their actions and beliefs in different social settings. We intend below to use this approach in order to begin to provide a sociological analysis of experimental research papers.

We will bring research papers within the scope of sociological analysis by showing that they are, at least in part, a form of social account. Our central claims are: that when scientists write experimental papers, they make their results meaningful by linking them to accounts of social action and collective belief; that the accounts of social action and collective belief presented in the formal research literature employ only part of the repertoire of social accounting used by scientists informally; that these formal accounts seem to be constructed in accordance with a traditional conception of scientific rationality; that this version of rationality exists alongside other views of scientific rationality; and that these other views are excluded from the realm of formal discourse. By showing how scientists selectively portray their social actions and beliefs in the course of formal publication, we will begin to reveal how scientists themselves create that appearance of impersonality, detachment and universality which sociologists have customarily regarded as literally descriptive of social action and technical belief in science.

Full-length experimental papers are usually divided into the following separate sections: Abstract, Introduction, Methods and Materials, Results, Discussion. Although we believe that all these sections involve some kind of social accounting, we will concentrate upon Introduction and Methods and Materials sections. We will first of all present some passages from research papers to show that they do contain accounts of their author's actions and to draw attention to the way in which these actions are presented. We will then compare these formal accounts with those given informally in interviews by the same scientists. This comparison enables us to demonstrate that the two kinds of account differ dramatically in several respects and to offer some suggestions as to why this is so.

The evidence presented below is taken from interviews which we have carried out with 34 biochemists working in one specific problem area. Each interview lasted from 2 to 3 hours and was taped and transcribed in full. In preparation for each interview, we read and discussed at least two of the

interviewee's published papers and, in many cases, considerably more than that. Thus, in addition to our general reading in this area of research, we have closely examined in the region of 100 published papers. This is much too large a number to be analysed in the kind of detail attempted here. In the sections which follow we have, therefore, concentrated on material from just two interviews and on one published paper by each interviewee. The reader should not forget that evidence will be required from many more research papers and from other research areas in order to establish any degree of generality for our conclusions. In addition, although our analysis is comparatively detailed, it will need to be supplemented by even finer examination of smaller sections of text. We hope subsequently to extend the analysis in both these directions. The present study is just a beginning, an attempt to take a little further the very few previous studies of this aspect of scientific accounting (5).

The points we intend to make below do not involve any deep appreciation of the technicalities of biochemistry. The reader should not be deterred, therefore, by his failure to understand completely the quotation from an introduction to a research paper which begins the next section. We use only four quotations with such a high level of technical content and only two of them need to be understood by the reader in any detail. Both these quotations are followed immediately by a 'layman's gloss'.

## **Social Accounting in Introductions**

### *Introduction A*

A long held assumption concerning oxidative phosphorylation has been that the energy available from oxidation-reduction reactions is used to drive the formation of the terminal covalent anhydride bound in ATP. Contrary to this view, recent results from several laboratories suggest that energy is used primarily to promote the binding of ADP and phosphate in a catalytically competent mode (1) and to facilitate the release of bound ATP (2, 3). In this model, bound ATP forms at the catalytic site from bound ADP and phosphate with little change in free energy.

A critical test of this proposal would be to measure energy-dependent changes in binding affinities at the catalytic site for adenine nucleotides. However, such measurements are complicated by the fact that mitochondrial membranes have numerous binding sites . . . An inhibitor that specifically prevents substrate binding at the catalytic site would prove very useful since it would allow binding events directly involved in catalysis to be distinguished from other processes that require bound adenine nucleotide . . .

An indication that the new phosphorylation inhibitor, efrapeptin, might bind at the catalytic site comes from studies with aurovertin . . . .

In this paper, we report the results of studies on the mode of inhibition of oxidative phosphorylation by efrapeptin . . . It is difficult to accommodate these results in a single mechanistic scheme involving a single independent catalytic site for ATP synthesis and hydrolysis. As will be discussed, the data are more easily interpreted in terms of a multiple interacting site model, such as the one recently proposed by C, D and E.

### *Layman's gloss A*

ATP is one of a class of complex molecules called nucleotides. It is biologically important because it is a major source of energy in living organisms. ATP is formed by the combination of ADP and inorganic phosphate. The overall process whereby ATP is formed is called oxidative phosphorylation. The energy required for its formation (or catalysis or synthesis) is thought to be generated through a series of linked oxidation-reduction reactions. The point made by this author in the first paragraph is that, whereas many biochemists have believed that the energy produced by these oxidation-reduction reactions is used to bind together ADP and phosphate, there is now good evidence showing that this binding occurs at specific sites with little expenditure of energy. He refers to a new model of oxidative phosphorylation in which the free energy is used, not to *make* ATP, but to *release* it for physiological purposes.

He then goes on to state that this model could be tested by measuring the relevant binding affinities. However, this task is complicated by the fact that the membranes of mitochondria, which are complex intracellular particles within which these processes occur, have numerous binding sites in addition to that where ATP is formed (the catalytic site). Efrapeptin is then identified as a substance which appears to act only on the catalytic binding site and which should help the experimenter to make observation of that site alone. Finally, the author claims that the results obtained with efrapeptin are inconsistent with older views of ATP formation and are best interpreted in terms of an extended version of the new model mentioned in the first paragraph.

Many commentators have drawn attention to the way in which scientific papers are written in an impersonal style, with overt references to the actions, choices and judgments of their authors being kept to a minimum (6). In this respect the introduction, about half of which is reproduced above, is typical of scientific writing. Although three other scientists are referred to by name in the full text, there is in every instance a rapid return to less personal formulations and the authors themselves only appear once through their use of the pronoun 'we'. At various points in the exposition verbs usually associated with human agency are employed, but we often find them combined with some non-human 'agent'. Thus, 'recent results' are said to 'suggest certain possibilities', and 'studies with aurovertin' are said to 'indicate others'. Despite this impersonal style, which minimises explicit mention of social actors and

their beliefs, it is clear that parts of the text implicitly offer accounts of the actions and beliefs of the authors and of their specialised research community. To this extent the introduction has a definite, albeit partly obscured, social component. We wish to suggest that the authors use the introduction to establish the significance of their findings at least partly by the way in which they organise the social element in their text.

If we consider the opening sentence we see that it is not a statement about the physical world, but about the customary nature of certain beliefs among a number of biochemists. This sentence could equally well have been written by a sociologist, trying to construct an interpretative analysis of social action in a research network. This similarity exists because the sentence is part of a subtle and purposeful social analysis. For the beliefs in question are presented in a way which enables the authors to contrast them unfavourably with those of another group of scientists, to which the authors themselves belong. What is particularly noticeable about the first sentence is how the beliefs which it summarises are prepared for immediate rejection. Thus, instead of presenting the central idea as a reasonable, though inconclusive interpretation associated with at least some experimental evidence, the authors choose to describe it as mere assumption. Furthermore, no supporting literature is cited. The impression is subtly conveyed that, although this idea may have been around for a long time, it has no firm scientific foundation and is not to be taken seriously.

The nature of the opening sentence prepares us to expect and to welcome the contrasting view which the second sentence reveals. Clearly the reader, as a scientist, is expected not to favour unsupported assumptions, but only views based on hard data. Consequently, in the second sentence the authors tell us that it is 'experimental results' which suggest a significantly different state of affairs from that previously assumed. They do refer implicitly to particular actors, when they use the phrase 'several laboratories'. But the authors do not formulate their argument, as they could have done with at least equal accuracy, in terms of two or more groups of scientists producing different experiments along with plausible yet divergent interpretations of those experiments, but in terms of one group's *results* undermining the other group's *assumptions*. Although in sentence two the conclusions deriving from these results are presented simply as suggestions, they are described more strongly in the third sentence as constituting a model; that is, a systematic

explanatory scheme with, as the next paragraph makes clear, a central proposition which can be put to the test. References are given for this model, in case the reader wishes to check its content or its empirical support. Thus, in the course of three or four sentences, the authors have conveyed a strong impression, at least for readers unfamiliar with the topic, that the paper to follow is based upon a well established analytical position which constitutes a major advance on prior work. They have done this, not directly by means of biochemical data, but by characterisation of scientific belief within their social network.

The formal account of collective belief offered by these authors would have been regarded as misleading by many of the scientists we interviewed. For many of them, at least informally, were highly critical of the model advanced in this paper and in particular expressed dissatisfaction with its supporters' failure to provide empirical clarification of the physical mechanisms involved. We put this point to the senior author during the interview (7).

**1. Interviewer.** The most frequent criticism of the idea of conformational coupling that people have talked about is that it doesn't tell you anything about mechanism. How would you respond to that comment?

*Author A.* I'd say they were right. We always feel a little embarrassed when we talk about conformational change. Because its a vague sort of thing. But I think it is an important idea. The data seems to indicate that you need energy to release ATP from the enzyme . . . I agree that it is aesthetically unpleasing not to have a very detailed account of what's happening. If it *is* an energy driven conformational change, that change will never, not in our lifetime anyway, be described in very discrete steps. (29–30)

In this passage Author *A* qualifies his formal description of the merits of the model he is advocating, in response to our version of the informal comments of other researchers. This shows clearly, not only that other scientists would probably have introduced these results quite differently, but also that a quite different account could have been given by the authors themselves of the state of scientific belief within the social network under consideration.

In order to show that the research paper considered so far is not unique, let us look at another introduction.

### *Introduction B*

The chemiosmotic hypothesis (1) proposed, *inter alia*, that each span of mitochondrial

respiratory carriers and enzymes covering a so-called energy-conservation site (2) is so arranged that  $2\text{H}^+$  are translocated across the mitochondrial inner membrane for each pair of reducing equivalents transferred across that span. Evidence in favour of this value of 2.0 for the ratio of protons translocated to reducing-equivalent pairs transferred (i.e.  $\rightarrow \text{H}^+/2\text{e}^-$  ratio) has come mainly from one type of experiment. In this, the length of the respiratory chain under study has been altered by changing either the oxidant or the substrate (3, 4).

In the present paper we describe an independent method for measurement of the  $\rightarrow \text{H}^+/2\text{e}^-$  ratio per energy-conservation site. The same substrate (intramitochondrial NADH) and oxidant (oxygen) is used throughout, but the number of energy-conservation sites is varied from one to three by using mitochondria from variants of [a particular yeast] with modified respiratory chains. We conclude that the  $\rightarrow \text{H}^+/2\text{e}^-$  ratio is 2.0 per energy-conservation site.

#### *Layman's gloss B*

This paper is concerned with the series of oxidation-reduction reactions which are believed to occur in the membranes of mitochondria. This series of reactions is referred to as the respiratory chain. It is the respiratory chain which is taken to generate the free energy required for the formation and/or release of ATP. The author takes it for granted that this energy is furnished by a gradient of protons ( $\text{H}^+$ ) which is created across the mitochondrial membrane by the action of the respiratory chain. He also takes it for granted that protons are carried across the membrane by pairs of electrons ( $2\text{e}^-$ ) at three sites. The issue which he addresses is: How many protons are carried across at each site by each pair of electrons, i.e. what is the  $\text{H}^+/2\text{e}^-$  ratio?

In the first paragraph, he states that a figure of 2.0 has been obtained previously by means of experiments in which the number of sites in the chain has been varied by changing the substrate (the reagent which donates the protons and electrons to the chain) or the oxidant (the reagent which receives the electrons after the protons have been transported). This is possible because some substrates and oxidants operate at different points in the chain.

In the second paragraph he states that he has also obtained a ratio of 2.0 per site, using the same substrate and oxidant, but employing mitochondria from three different strains of yeast with respiratory chains of varying length. Later in the paper he states that these mitochondria have chains with either one site, two sites or the full three sites.

This introduction seems straightforward and unproblematic. A quantitative aspect of a major hypothesis is first identified. The authors point out that, although this part of the hypothesis has been experimentally confirmed, only one kind of experimental design has been employed. An alternative technique is then briefly described. And the introduction ends with a statement that this new technique produces the same results as previous experiments and therefore provides further support for the hypothesis. In what sense does this passage involve social accounting? In the first place, an account is being

offered of the state of belief among those scientists concerned with the proton/electron ratio. Although it is not stated explicitly, it seems to be implied that there are no negative experimental findings which need to be considered and little, if any, disagreement about the scientific meaning of previous findings. Indeed, it is this form of presentation which enables the authors to depict their own results as primarily a contribution to experimental technique: 'In the present paper we describe an independent method for measurement of' the relevant ratios. Their actual observations can be treated as basically unproblematic, because they are portrayed as merely confirming what competent researchers already know. As a result, attention is directed to the novel techniques used to obtain these expected observations. In this way the author's contribution to knowledge, and thereby the meaning of their work in the laboratory, is defined by the manner in which the existing state of belief about this ratio is construed.

The content of this introduction differs considerably from the discussion of the paper in the interview. For instance, the senior author stressed informally that previous observations of these ratios were by no means widely accepted.

2. *Author B.* . . . There's always criticism of one method. There are very few methods that are bomb-proof . . . What we did was another way of doing it . . .

*Interviewer.* So there were people at that time who were casting doubt on *P* and *Q*'s figures?

*Author B.* You bet there were. And not merely on the *figures*, but on whether it happened at all. [Certain people] said, 'It just doesn't happen. There are no protons ejected'. (13–14)

In the introduction, no hint is given that the ratio mentioned in the text had been strongly criticised or the previous methods put in question. Whereas in the previous introduction the position of those opposed to the authors is briefly characterised in adverse terms, in this introduction it is entirely ignored. Author *A* presented his results as furnishing a test of and further support for a model already clearly superior to the previous, poorly worked out approach; even though informally he accepted other scientists' reservations about the central ideas of the model and their doubts about its empirical foundation as entirely reasonable. Similarly Author *B* depicted his results as



an advance in method; even though he was well aware that many scientists doubted whether previous observations were correct and even whether the phenomena which he was supposed to be measuring actually existed. Thus the characterisation of collective belief appears to vary from one scientist to another. In addition, each scientist offers quite different versions of collective belief in formal papers and in informal interviews. Thus in both these introductions an account of the current state of collective belief is fashioned which makes the authors' contribution seem much less problematic and open to alternative interpretation than it does in ordinary discourse.

### *What Is Left Out of the Formal Account*

A style is adopted in formal research papers which tends to hide the author's personal involvement; and the existence of opposing scientific perspectives seems either to be ignored or to be referred to in a way which emphasises their inadequacy, when measured against the 'purely factual' character of the author's results. As a consequence of such systematic accounting, the author's findings begin to take on an appearance of objectivity in the formal text which is significantly different from their more contingent character in much informal accounting. This formal appearance is strengthened by the rigorous suppression of certain other features which appear frequently in informal accounting. In particular, reference to the dependence of experimental observation on theoretical speculation, to the degree to which experimenters are committed to specific theoretical positions, and to the influence of social relationships on scientists' actions and beliefs, is regularly eliminated as scientists move from informal to formal accounting (8).

Consider, with respect to the relationship between theory and data, the following statements made by Author *A* during his interview. In these two quotations he is describing his reaction some years before to the central idea of the model mentioned in Introduction *A*, when it was first suggested to him by the head of his laboratory.

3. He came running into the seminar, pulled me out along with one of his other post docs and took us to the back of the room and explained this idea that he had . . . He was very excited. He was really high. He said, 'What if I told you that it didn't take any energy to make ATP at the catalytic site, it took energy to kick it off the catalytic site?' It took him about 30 seconds. But I was particularly predisposed to this idea. Everything

I'd been thinking, 12, 14, 16 different pieces of information in the literature that could not be explained, and then all of a sudden the simple explanation became clear . . . And so we sat down and designed some experiments to prove, test this. (8)

4. It took him about 30 seconds to sell it to me. It really was like a bolt. I felt, 'Oh my God, this must be right! Look at all the things it explains'. (14)

In the formal paper we are told that experimental results suggested a model, which seemed an improvement on previous assumptions and which was, accordingly, put to the test. In the interview, however, we hear of a dramatic revelation of the central idea of the model, which was immediately seen to be right, which revealed existing data in a new light and which led to the design of entirely new experiments. The author mentions in the interview that this major scientific intuition came to the head of the laboratory when he was 'looking over some old data'. But the essential step which so excited those concerned was conceptual and speculative rather than empirical and controlled. It was the act of perceiving new meanings in data that were already familiar. Furthermore, when the authors of the introduction refer to 'results which *suggest* a new model', they cite precisely those results which were, according to the informal account, actually produced *as a result* of the intuitive formulation of the central idea of the model. It appears, then, that the actions involved in producing and establishing the model are characterised quite differently in the formal and the informal accounts. Whereas in the former the model is presented as if it followed impersonally from experimental findings, in the latter the sequence is reversed and the importance of unique social events clearly revealed.

The formal and informal accounts also differ in their treatment of the author's degree of commitment to the model. No explicit mention is made in the introduction of the author's prior involvement in the model's formulation. And the impression is given that the author is engaged in subjecting the model to a detached, critical test. Informally, however, significantly different statements were frequently made.

5. When I arrived here, I thought that the clearest way of demonstrating that energy input served to promote ATP dissociation from the enzyme rather than the formation of a covalent bond, would be to show a change of binding affinity for the ATP upon energisation. Everything up to that point had been kinetic evidence . . . and I felt some nice good thermodynamic data would help. (31)

6. It is a kind of shocking idea. 'Hey, everybody has been taught it takes energy to make ATP and now you are going out preaching it doesn't take energy to make ATP, it takes energy to get it off the catalytic site.' It was hard to sell . . . I personally think that its not proven, but I think its pretty close. (19)

In quotation five, scientist *A* does not describe himself as testing the model or trying to disprove it. Rather he portrays his actions in terms of looking for new kinds of evidence to furnish additional support. Similarly, in passage number six he stresses that, although only the smallest degree of uncertainty existed in his own mind, it was difficult to convince other scientists, who had not shared that initial revelation, that this model was required by the available evidence. Several phrases used by the author in the interview, varying from 'Oh my God, it must be right!' and 'its pretty close to being proven' to 'some nice good thermodynamic data would help to demonstrate it', seem to imply that he was fairly strongly committed to the model. Yet in the formal account, he chooses not to refer to his own involvement in the formulation of the model and to imply a considerable degree of critical detachment: 'A critical test of this proposal would be to measure . . .'

The last phrase in quotation three, to the effect that he designed experiments 'to prove, test', the model, suggests that, informally, the interviewee did not distinguish clearly between testing and proving the model. The following additional passages illustrate how, in informal talk, he tended to approach the issue of testing theories. In quotation seven, he is referring to his own work following on from that presented in the paper under discussion here. In quotation eight, he is talking about the response of one of his opponents to critical tests of *his* theory.

7. *Interviewer*. Do you see your current work as testing out the alternating site model or filling in details?

*Author A*. I think, well no. We *may* come up with additional data for the alternating site. But basically the aim is just to learn something about the catalytic site and not to test this further. (35–36)

8. He's tenacious . . . He's trying to accommodate data that doesn't agree with it by constructing a fairly complicated explanation. I think eventually he's got to give it up because I think its probably wrong. What he does is, and this is not a bad type of technique, I'm not criticising him for it, when he hears something that doesn't agree with his ideas he tries to find an explanation. The problem is that he's constructed such a complicated explanation for this, that the whole thing should be dismantled and he should start again. (24)

We can see from these passages that, informally, the speaker did not insist that experimental work has to put the researcher's theoretical framework to the test and that he was even willing to accept as legitimate what he saw as a dogged, *ad hoc* defense of a false theory. In view of his uncertainty as to whether the work reported in the paper under study was or was not devised to test the model, and in view of his acquiescence in quite different versions of research strategy, the reference in the introduction to carrying out a critical test can be seen to be a somewhat selective use of terminology. In a context other than that of the research paper, it would have been quite appropriate to have characterised the same actions quite differently; for example, as an attempt to prove an interpretative speculation to which the author was strongly committed.

We have seen that Author *A*, in informal discussion, recognises that his adoption of this particular model of ATP synthesis was brought about by his experiences in a specific laboratory and by his close contact with a particular group of colleagues. This recognition of how social relationships influence the course of individual scientists' research is hidden from view in the formalised paper. Thus Introduction *A* refers simply to the fact that 'several laboratories' had produced results which supported 'the model'. Only by consulting the references could the reader observe that only two laboratories seem to have been involved and that the author's presence as a co-author of one of the cited papers shows him to have been a member of one of these laboratories. Yet in informal discussion, the author stressed how significant was the period in that laboratory to his research career and how he has retained strong social links with the members of that laboratory and with their research.

9. I went to *C*'s lab. He had a very profound influence on me. That was really where I was educated. (4–5)

10. We were struggling with it. My students and I had all these diagrams all over and I wonder what would have happened if we hadn't gotten something in the mail. I wonder if we would ever have stumbled on it ourselves; probably not. But I got a preprint from *C*; the *C*, *D* and *E* paper, in which he proposed two cooperative catalytic sites and as soon as I saw it I liked it . . . This hadn't been published and we had the advantage of knowing it before it came out. *C* was very kind and kept us up with what he was doing (33–4).

This intellectual indebtedness and informal contact, which form natural topics for ordinary discourse (9), are not revealed in the introduction. The

final paragraph of the introduction simply states that the empirical results which are to follow are difficult to assimilate by means of traditional assumptions and that they are more easily interpreted in terms of the model proposed by *C*, *D* and *E*. The strong social connections among those involved remain covert. A 'literal reading' of that final paragraph would be along the following lines: that the author carefully examined the compatability of their results with the major theoretical positions available in the literature and were led to conclude that one theory was shown to be clearly superior to the others in the course of an impartial appraisal. However, the account given informally is dramatically different. In the first place, the speaker had already decided whilst in *C*'s lab that the traditional view was inadequate and he never seriously considered interpreting his results within its frame of reference. Secondly, his experimental design was based on *C*'s original model, which he himself had played a part in formulating. Thirdly, his acceptance in this paper of the revised version of this model proposed by *C*, *D* and *E*, would have been impossible without fairly direct informal contact with *C*. Once again, it is clear that a certain element which is prominent in the informal account is left out of the formal introduction. There are, of course, bound to be differences of some kind between the informal and the formal accounts, if only because the former can be detailed and discursive whereas the latter are required to be brief and concise. Consequently, simply to show that differences exist does not itself take us very far. However, we have begun to show that these differences are not random, but systematic and meaningful. Certain clearly identifiable ways of characterising social action, which are treated as normal in ordinary discourse, are consistently omitted from formal accounts; whilst other, opposite, attributes are emphasised. The underlying rationale whereby informal accounts are transformed in the course of formal accounting will become clearer in the next section.

### **Social Accounting in Methods Sections**

The existence of social accounting in experimental papers is most obvious in the sections on 'methods and materials'; for these sections consist mainly of highly conventionalised accounts of what the authors did in their laboratories in the course of producing their empirical results. The following quotes reproduce parts of the methods sections of the two papers discussed above.

These sections are typical of the great majority of experimental papers in this area of biochemistry.

#### *Methods Section A*

Heavy beef heart mitochondria were prepared by the method of *E* and stored in liquid nitrogen. Well coupled mitochondrial particles were prepared by a modification of the procedure of *F*. These particles were used to prepare inhibitor-protein-depleted particles by centrifuging under energised conditions according to the method of *G* . . .

In order to establish that ADP formation is the only rate-limiting step in our spectrophotometric assay for ATP hydrolysis, the following test was performed for each preparation of assay medium. Hexokinase and glucose were added to give a rate of absorbance change equal to or greater than that of the fastest ATP hydrolysis activity to be measured. The amount of hexokinase was then doubled, and the assay medium was considered adequate if the rate of absorbance change doubled . . .

#### *Methods Section B*

(A particular strain of yeast) was grown in continuous culture under conditions of glycerol limitation (H and B) or sulphate limitation (I and B). A variant of this yeast that does not require copper and has a cyanide insensitive terminal oxidase (J and B) was grown in continuous culture in a copper extracted medium (H) . . . Harvested and washed cells (H and B) were converted into protoplasts and mitochondria isolated as described by K. Protein was determined by the method of L. Measurements of respiration-driven proton translocation were made with the apparatus described by M and B in 1.0 ml of anaerobic 0.6m-mannitol . . . Polarographic measurements of P/O ratios were performed as described by N, by using the experimental conditions of B . . .

There is insufficient space here for a full analysis of methods sections. We shall, therefore, simply try to illustrate some general points which are related to the discussion above. One of the most noticeable features of these passages is the way in which the specific actions of the researchers in their laboratories are expressed in terms of general formulae. Constant reference is made to methodological rules formulated by other scientists; and many of the authors' actions are not described at all, but are simply depicted as instances of these abstract formulae. This is sometimes so, even when the authors recognise that they have actually departed from the original formula. Thus not only do we find, 'mitochondria were prepared by the method of *E*', but also, 'particles were prepared by a *modification* of the procedure of *F*.' Where the author's laboratory procedures are seen as introducing new practices for which existing rules do not provide, these practices are themselves presented as rule-like

formulations as in the second paragraph of Methods Section *A*, which can then be used by other scientists in the course of *their* work. Informally, the principle which was usually said to guide authors in writing methods sections was that they should provide enough information for other scientists to repeat the authors' relevant actions and get the same results. As Author *B* stated: 'In the *Biochemical Journal* they have a separate section and you have to give sufficient detail there to enable any competent scientists to reproduce your experiment'. Thus the form of accounting used to depict scientists' actions in methods sections seems to be more or less explicitly an attempt to extract certain invariant dimensions from the unique, specific actions carried out by particular researchers in particular laboratories and to embody these dimensions of action in general, impersonal rules which can be followed by any competent researcher.

Methods sections, then, appear to be formally constructed as if all the actions of researchers relevant to their results can be expressed as impersonal rules; as if the individual characteristics of researchers have no bearing on the production of results; as if the application of these rules to particular actions is unproblematic; and as if, therefore, the reproduction of equivalent observations can be easily obtained by any competent scientist through compliance with the rules. In the course of informal talk, however, each of these notions is continually undermined. For instance, it is regularly noted that exact compliance with another's methods and exact replication of their results is virtually impossible.

11. When you write the paper which says how you did it, the ground rules are that you write it in such a way that other laboratories could reproduce your work and your conditions. Now that of course is impossible. There are all sorts of things that you don't know about, like 'finger factor', the local water, built-in skills, which you have taken for granted. But you try and do it anyway (Author *B*).

Or as another respondent put it:

12. Ideally, the scientific paper should make it possible, assuming that a library is available, for a Martian to come and do your experiment. But that's largely wishful thinking (Author *O*).

Thus scientists themselves are well aware of the gap between the formal appearance of methods sections and what they see as the practical realities of research.

Methods sections give the impression that the application of methodological procedures is a highly routinised activity, with little room for individual initiative and variability. Informally, however, scientists stressed that carrying out experiments is a practical activity requiring craft skills, subtle judgments and intuitive understanding (10). They talked of particular researchers having 'good hands' or 'a feel' for laboratory work.

13. You get a feel for what you need. I can tell you a story about this. I went to the workshop once to get something made. There was no way they could do anything for me for a week or a month. They were making something for Dr. *X*. I said 'What are you making for Dr. *X*?' 'Dr. *X* requires his water bath to operate at 36.5°C and *nothing else*'. And they were having a hard time actually. I said, 'That's ridiculous'. And I consulted with Dr. *X* and he produced this paper showing that in this experimental protocol, they'd worked at 36.5°. It didn't matter a damn really, whether it was 35° or 40°, as long as you stayed roughly where you were. Dr. *X* was not an experimenter and no longer does any. If you are an experimenter you know what is important and what is not important (Author *B*, 24).

When discussing laboratory practice informally, authors emphasised that dependence on an intuitive feel for research was unavoidable owing to the practical character of the actions involved. For such actions cannot be properly written down. They can only be understood satisfactorily through close personal contact with someone who is already proficient.

14. How could you write it up? It would be like trying to write a description of how to beat an egg. Or like trying to read a book on how to ski. You'd just get the wrong idea altogether. You've got to go and watch it, see it, do it. There's no substitute for it. These are *practical* skills. We all know that practical skills are not well taught by bits of paper. Could you write a dissertation on how to dig your garden with a fork? Far better to show somebody how to stick the fork in and put your boot on it. (Author *B*, 26).

In addition, scientists pointed out that many aspects of laboratory practice are traditional; in the sense that they are done because they are customary and are assumed, without detailed analysis, to be adequate for the task in hand.

15. *Interviewer*. One of the things we find difficult in reading those papers is understanding just *why* you have done certain things or used certain chemicals. Is there a convention about that?

*Author B*. The convention is that you normally use what you used last time round. You



don't want to change. Let's take an example. We want to suspend mitochondria in some medium. Now if you put mitochondria in water they swell and burst. So they need support. Why did we use 0.6 ml? 0.6 ml is about right. Why did we use mannitol and not sucrose or something else? Well, because somebody in Japan 10 years ago had published the first paper on making mitochondria and he used mannitol. I don't know *why* they used mannitol. They may have been given it. Or they may have found it was better. Or maybe it was what they started with and they didn't want to change. So that's why we used mannitol. We saw no good reason to change from the original recipe. And 'recipe' is the right word. It's like cooking (Author B, 18).

As a result of their emphasis on the role of customary practice and on learning by example, it is not surprising that many authors said that it is often extremely difficult to specify in full those aspects of their research actions relevant to the production of their results. For so much of their knowledge and skill is tacit. Even when it is possible for a scientist to work out from the formal paper what, for practical purposes, counts as a repetition of another's methods, unless he is working on something very similar it is likely to 'take him an awful long time. Because there are so many mistakes you can probably make, I suspect. And I wouldn't even know what they are, you see, that's the snag. They'd be things I'd take for granted' (Author B, 20).

Scientists who belong to the same research network do, of course, often succeed in translating the content of formal methods sections into effective laboratory practice. But when these formal accounts of scientists' actions pass outside the small, specialist community to scientists who do not share the same background of technical assumptions and who have not experienced close personal contact with its members, this process of translation becomes much more difficult. Although the formulations contained in methods sections appear to be impersonal and independent of social context, they can only be put into practice by those who have participated intimately in the social life of the relevant social grouping.

16. One is telling the general reader very roughly how the experiments are done and the specific reader, that is anyone else who is working in the same field, which of the things he already knows about you have chosen to do. I mean, it would not enable an intelligent scientist in another field to set about doing those experiments . . . (Author N, 28)

17. From my own experience of trying to read back old papers, it can be a nightmare sometimes, trying to work out what they actually did . . . People outside the field don't even know where to get the reagents from. If I look at a paper in molecular biology, where they're using all these fantastic antibiotics, I wouldn't know where to start unless they listed the sources of supply . . . (Author B, 16).

It is clear, then, that the accounts of scientists' actions which appear in the methods sections of research papers differ radically from the accounts of the same actions offered informally. Whereas the formal methods section contains highly abstract versions of scientists' research activities in the form of impersonal rules, with no attempt to specify how these rules are interpreted in practice in particular instances, scientists' informal accounts emphasise that these rules depend for their practical meaning on the variable craft skills, intuitions, customary knowledge, social experience and technical equipment available to individual experimenters. It is also clear that, to varying degrees, scientists themselves are aware of the divergence between their formal and informal accounts of laboratory practice. They are aware that what they regard informally as crucial aspects of their actions are entirely omitted from the versions given in research papers. They stress that the ostensible objective of methods sections is unattainable and they imply that the meanings given to the rule-like formulations employed in methods sections vary in accordance with readers' membership of specific social groupings. Indeed, one can go further than this and show that scientists sometimes explicitly depict the accounts of social action given in research papers as 'artful accomplishments' and that scientists themselves theorise about the interpretative processes involved. In the next section, we will look at some of the comments of our two authors which seek to explain the divergence between informal and formal accounting.

### Scientists' Accounts of Their Accounting Procedures

The author of the first paper examined above gave the following rationale for the formulation of social accounts in research papers.

18. Everybody wants to put things in the third person. So they just say, 'it was found, that'. If its later shown that it was wrong, you don't accept any responsibility. '*It* was found. I didn't say I *believed* it. *It* was found'. So you sort of get away from yourself that way and make it sound like these things just fall down into your lab notebook and you report them like a historian . . . Of course, everybody knows what's going on. You're saying, 'I think'. But when you go out on a limb, if you say 'it was shown that' or 'it is concluded' instead of 'we conclude', it should be more objective. It sounds like you are taking yourself out of the decision and that you're trying to give a fair, objective view and you are not getting *personally* involved. Personally, I'd like to see the first person come back. I slip into it once in a while. '*We* found'. Even then I won't say 'I'. I'll say

'we' even if its a one person paper. Can spread the blame if its wrong (laughs) (Author A, 57–8).

The aspect of accounting on which this speaker focuses is the use of impersonal formulations and the tendency to minimise the author's personal involvement. The reason he gives for this stylistic convention is that it enables the author to avoid responsibility for errors. Yet he appears almost immediately to contradict this explanation, when he suggests that everybody is aware of what is going on, that scientists recognise the conventional nature of this denial of responsibility. In our interviews, there is an enormous amount of material showing that researchers are, in practice, held personally responsible for published mistakes. It is clear that when scientists *read* papers, they translate the impersonal conventions of the formal report into their informal vocabulary. Nobody is ever misled into thinking that, because the research report made little or no mention of human agents, no researchers were actually responsible. Thus the rationale offered by this interviewee is unconvincing. Moreover, it gives little promise of providing analytical purchase on the range of material we have presented in previous sections.

Our second author also emphasised the impersonal character of research papers. But his central point was that scientific papers have this characteristic because they are devised in terms of a particular conception of scientific rationality.

19. I think the formal paper gets dehumanised and sanitised and packaged, and becomes a bit uninteresting. In some ways I like the old ones, where a chap says, 'I did this and it blew up in my face' . . . Some of the charm was certainly gone.

*Interviewer.* Why do you think that is?

*Author B.* One is a myth, that we inflict on the public, that science is rational and logical. It's appalling really, its taught all the way in school, the notion that you make all these observations in a Darwinian sense . . . That's just rubbish, this 'detached observation'. 'What do you see?' Well, what *do* you see? God knows, you see everything. And, in fact, you see what you *want* to see, for the most part. Or you see the choices between one or two rather narrow alternatives. That doesn't get admitted into the scientific literature. In fact, we write history all the time, a sort of hind-sight. The order in which experiments are done. All manner of nonsense. So the personal side does get taken out of this sort of paper. Maybe its felt that this isn't the place for it to be put. I don't know . . . Sometimes you get more of the personal side in reviews. Some of them are quite scandalous actually, once you can read between the lines.

*Interviewer.* Do you think there would be any disadvantages in allowing that sort of thing back into the formal literature?

*Author B.* I don't know. It depends what the purpose of the literature is. If the purpose of the literature is to describe what you did, why in scientific terms you did it – I mean, not because you want to do some bloke down or you want to advance your own career or get a quick paper out just because there's a grant application coming up soon. All these are valid reasons, but they're never admitted to. If the publishing reason is to present the science, what you did and what the conclusions were, then there really isn't much room for the emotive side. If I'm writing a paper [I don't say] 'I don't think that Bloggins understands electro-chemistry because he's a dum-dum'. I might say, 'This was overlooked by Bloggins *et al*'. I won't say *why* I think they overlooked it. I'm afraid its *gone* and its not going to reappear here. Probably it shouldn't reappear. I guess it reappears in other places. And we still *know* what's going on. We just don't make it public (Author B, 32–3).

Like the previous speaker, this scientist stresses that researchers can 'read between the lines'. For example, they will read Bloggins' paper bearing in mind that he is, in their opinion, a dum-dum. In other words, they translate the formal text into their more extended informal vocabulary. However, the explanation he offers for the existence of two vocabularies is slightly more developed than that of Author A. In the first paragraph, his central claim seems to be that when scientists construct research papers they reinterpret and re-order their prior actions so as to make them appear to fit an inductivist version of scientific rationality. This interpretation is consistent with the material we have presented above (11). For we have shown in some detail how these two researchers eliminate from their formal accounts any reference to those personal, social, contingent aspects of their actions which feature so prominently in their informal talk; how laboratory practice is presented as governed by impersonal routines; and how data are apparently removed from dependence on human judgment or theoretical commitment. All of these features are highly appropriate to the inductivist conceptions of 'letting the facts speak for themselves'. In the first paragraph of quote 19, scientist B begins to formulate, in a very preliminary fashion, an alternative conception of scientific observation and rationality which is intended to be more suited to the content of his informal repertoire. But he fails to take this line of thought very far beyond the basic assertion that science in practice differs considerably from the conception embodied in research papers. Moreover, in the second paragraph he clearly returns to the more traditional view of

science and scientific rationality. When he says that 'if you publish in order to present the science, there isn't much room for the emotive side', the speaker seems to have forgotten what he had just said about scientists 'seeing what they want to see' and constructing acceptable, non-emotive versions of their actions after the event. It is as if this speaker has at his disposal two rather different vocabularies for giving accounts of scientific action and belief, and that each vocabulary implies a different conception of scientific knowledge and rationality. In this passage B generates an apparent inconsistency by moving suddenly from one form of discourse and one conception of rationality to the other.

It is impossible to use the material presented in this section to reach any strong conclusions. What we wish to do instead is to use this material as a springboard, from which to make certain analytical suggestions. We suggest that social accounting in the formal domain is carried out in accordance with a traditional version of rationality. Within this perspective, genuine scientific knowledge is seen as being built up unproblematically by means of accurate, reproducible observation of the natural world. Observation is conceived as the act of recording the 'unembroidered evidence of the senses' and is regarded as reliable only in so far as it can be repeated 'identically' by 'everyman', that is, by any neutral, unprejudiced and technically competent person. It is allowed that scientific thought sometimes goes beyond the experimental facts, but such controlled speculation is seen as being guided by clear criteria of rationality. Accordingly, observational differences and differences of interpretation are seen as being due necessarily to the encroachment of non-scientific or non-technical factors; in other words, to the influence of personal or social factors, which are deemed to be separable from and irrelevant to the de-personalised propositions and practices of science.

There is much evidence in our transcripts that this traditional view of scientific knowledge is familiar to scientists and that it is frequently used by them. This conception of scientific rationality would lead scientists to dissociate their knowledge-claims from personal and social elements, to depict their research actions in terms of impersonal rules, to stress the autonomy of data, and so on, in exactly the way we have seen that they do in the realm of formal discourse, because to do otherwise would put their claims in jeopardy. Thus, it is reasonable to suggest that this idea of rationality is dominant in formal scientific accounting; that it is firmly institutionalised within the

system of scientific publication; and that it is the systematic use of a suitable vocabulary of accounting that produces the conventional characteristics that we have observed in experimental papers.

Informally, however, this version of rationality, although still of major importance, is by no means so all-pervasive. Scientists not infrequently challenge the appropriateness of this conception of science in the course of informal talk. They also regularly employ a vocabulary of social accounting which differs radically from that used in formal papers, in that such elements as personal commitment, the priority of theoretical insight, intuition and practical skills, and so on, are seen as essential to the process of knowledge production and are routinely mentioned in describing the course of events. Few experimenters have thought reflexively enough about their informal interpretations of social action in science to have devised a coherent alternative to the institutionalised view. Nevertheless, they do translate the contents of the formal literature into their informal repertoire, in order to make them meaningful in practical terms. As a result, they are aware of the gap between that literature and what they regard as the realities of research.

In the light of these suggestions, we can begin to understand why our respondents found it difficult to provide consistent answers to the interviewers' questions about research papers. For they were being asked to talk informally about and offer informal justifications for their formal vocabulary. Given that this latter vocabulary is based on a fairly coherent view of scientific rationality, we would expect that its use within the formal context would be entirely consistent. However, we would also expect that it would inevitably generate interpretative problems when used as part of the full, and much more diverse, repertoire of informal resources. These interpretative problems are hardly noticeable in the course of fast-moving, everyday discourse. But when the discourse is recorded and examined in detail, we can observe the tendency for speakers to produce what seem to be somewhat contradictory claims. Author *A* offers an informal explanation of the style of research papers which stays close to the traditional view, in that the use of impersonal forms of expression is treated as having a real influence on scientists' understanding of science. Within a few sentences, however, he points out that in practice nobody takes these appearances seriously. Similarly, Author *B* moves uneasily between, on the one hand, repudiating all that is excluded from research papers as unscientific and unimportant and, on the other hand,

criticising research papers for giving an unrealistic impression of how science actually operates. Thus, in their attempts to interpret the character of research papers, both authors can be seen to move between two rather discrepant views of scientific discourse as they themselves vacillate between two vocabularies of scientific discourse.

It seems, therefore, that the preliminary analytical suggestions we have offered in this section may throw some light on scientists' own attempts to theorise about scientific accounting as well as helping to make sense of the divergences between informal and formal accounting documented above. Our main suggestion has been that in the formal domain social accounting is carried out by authors, editors and referees in terms of one internally consistent, traditional version of scientific rationality and its associated vocabulary. Informally, however, a much wider vocabulary is used and more than one view of scientific rationality is implicit in social accounting. By comparing these two realms of scientific discourse and by examining the complexity and diversity of informal accounting, we have begun to see how the conventional characteristics of the formal domain are socially accomplished.

### **Concluding Remarks: Contexts of Scientific Discourse**

Although sociologists and philosophers have accepted that there are 'irrational' as well as 'rational' actions in science, there has been a tendency to treat these two classes of action as quite distinct and to allocate them to separate social contexts. Intuition, speculation, idiosyncrasy, personal commitment, social interest, and so on, have been seen as characteristic of acts undertaken in the context of discovery or in the private phase of scientific work. But in the public context of verification or testing, these elements have been thought to be largely eliminated and replaced by impersonal, technical actions and judgments (12). In several recent studies, however, it has been shown that this distinction between independent contexts of action is difficult to maintain (13). Scientists appear not to abandon their personal commitments, their 'biases and prejudices', when writing up their own results; nor, it seems, do they regard other scientists' personal and social attributes as necessarily irrelevant to evaluation of the cognitive and technical claims presented via the public media of science. The formal claims in the research literature are

interpreted and given meaning in the course of private reflection and informal discussion, where the contingent factors supposed traditionally to be separated from the constitution of scientific knowledge come clearly into play.

It is misleading, therefore, to treat as independent the realms of discovery and testing or the private and public phases of research. Nevertheless, although these concepts do not reflect genuine differences of social action in science, they do correspond quite closely to systematic differences in scientists' *discourse* about action. Thus we suggest that the notion of 'contexts of scientific action', which has been the source of much unprofitable debate, should be replaced by the notion of 'contexts of scientific discourse'. It is not that the character of scientists' actions changes radically as they move from having ideas to testing those ideas. What changes is the way in which scientists choose to portray their actions, as they engage in differing social relationships. The material presented above amply illustrates this latter point.

We showed that experimental papers do contain accounts of social action and of collective belief. However, the social element is partly hidden by authors through the use of an impersonal style of writing and by a tendency to underplay the diversity of scientific opinion, thereby giving the misleading impression that the interpretation of experimental findings is not socially variable. The effect is to make the authors' claims appear relatively unproblematic.

We then showed that there are systematic differences between the accounts of social action and belief in research papers and those provided by the same authors informally. Thus experimental data tend to be given chronological as well as logical priority in formal accounts; although informally these same data may be described as following on from a speculative insight. Similarly, the author's own involvement with and commitment to a particular analytical position is not mentioned in research papers, nor his social ties with those whose work he favours, although informally these may be emphasised and their influence on the author's view of scientific issues clearly recognised. Laboratory work is characterised in the methods sections of research papers in a highly conventional manner, as instances of impersonal, procedural routines which are universally effective. Informally, however, this kind of characterisation is frequently repudiated and great stress is placed on the impossibility of expressing methods verbally, on individual researchers' practical dexterity and personal intuition, on close contact with skilled



practitioners, and on the need to *interpret* methodological rules in accordance with tacit knowledge.

Scientists are themselves aware that writing research papers involves social accounting and that their formal versions of their acts differ systematically from their informal accounts. Nevertheless our respondents were somewhat confused in their attempts to make sense of the divergence between the social content of formal papers and their informal, everyday interpretations which, naturally enough, they take as representing 'what really happens'.

We have suggested that not only are there systematic differences in social accounting but also different conceptions of rationality in different contexts of scientific discourse. A traditional view of scientific rationality is generally available among scientists as an interpretative resource. This view of rationality and its associated social vocabulary are firmly institutionalised within the formal organs of scientific communication. In contrast, in the course of everyday discourse, a much wider repertoire for social accounting is employed and alternative versions of scientific rationality are frequently suggested or implied.

If these suggestions are broadly correct, they direct our attention away from attempts to characterise scientists' social action as such, towards a concern with exploring how and why scientists produce varying versions of their actions. They also imply that several longstanding questions in the sociology of science are unanswerable in their customary form; for instance, whether or not scientists' actions are predominantly universalistic and whether or not action in the context of verification differs from that in the context of discovery. But most important of all, the analysis offered here begins to throw some light on the social procedures by means of which researchers construct the public discourse of science so that it appears to have the attributes of impersonality, objectivity and universality that have come to be widely accepted as characteristic of a 'genuine scientific community'. We have been studying some of the processes within science whereby the features which have provided the basis for taken-for-granted knowledge about science are socially produced.

## Notes and References

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6. G. N. Gilbert, 'The Transformation of Research Findings into Scientific Knowledge', *Social Studies of Science* 6, 281–306 (1976); Knorr, *op. cit.*, Note 4.
7. Both the papers we examine here had two authors. We interviewed the senior author in each case. It is these senior authors that we will refer to as Author A and Author B.
8. We are clearly assuming here that informal accounting in interviews is similar to informal talk between scientists. For some evidence indicating that this is so, see our *Accounting for Error* (unpub.). Nevertheless, this point does need further study.
9. Although Author A recognised the importance of this relationship with C, he nevertheless maintained informally that he accepted C's theory because it was the best theory. Even in informal discourse, social factors are not usually portrayed as determining correct scientific belief. Incorrect belief, however, is another matter. See *Accounting for Error*, *op. cit.*
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