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3 Contexts of scientific discourse

In chapter one, we argued that interpretative variability within participants’ discourse is sociologically important. In the second chapter, we illustrated this variability in relation to a couple of accounts of the development of research into oxidative phosphorylation. In this chapter, we will begin to show that participants' discourse, although varied, displays certain observable patterns. We will examine two contexts of linguistic production, namely, the experimental research paper and the semi-structured interview involving biochemists and non-biochemists. We will show that participants' accounts of action and belief are systematically different in these two settings. This will enable us to identify two major interpretative repertoires, or linguistic registers, which occur repeatedly in scientific discourse.

It is important to emphasise that when we use the phrase 'social context', we are not referring to phenomena which exist independently of participants' discourse. For social contexts are themselves *products* of discourse. It is through their recurrent patterns of language-use that participants construct such phenomena as the 'formal research literature' and 'informal' interaction. To describe the social context of the formal research literature as being distinct from the context of informal interaction is essentially to refer to systematic differences in the ways in which scientists construe their actions and beliefs. Phrases like 'the formal research literature' are labels employed by participants as well as analysts to distinguish variations in the forms of discourse by means of which members construct the meaning of action and belief.

Halliday makes this point more precisely as follows.

> Let us assume that the social system (or the 'culture') can be represented as a construction of meanings - as a semiotic system. The meanings that constitute the social system are exchanged through a variety of modes or channels, of which language is one . . . Given this social-semiotic perspective, a *social context*. . . is a temporary construct or instantiation of meanings from the social system . . . [the] components of the context are systematically related to the components of the semantic system; and . . given that the context is a semiotic construct, this relation can be seen

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> as one of realisation. *The meanings that constitute the social context are realised through selections in the meaning potential of language.*[1]

For the purposes of our analysis, reference to the existence of different social contexts in science is simply another way of drawing attention to patterned variations in the discourse through which scientists construct their social world.

In this chapter, then, we will begin to describe some of the systematic ways in which scientists select from the full meaning potential of their language as they construct formal and informal contexts within the scientific domain. We will illustrate how they draw selectively on two interpretative repertoires as they depict action and belief in ways which are appropriate to the different interpretative contexts they are involved in reproducing. More specifically, we shall show that when scientists write experimental research papers, they make their results meaningful by linking them to explicit accounts of social action and belief; that the accounts of action and belief presented in the formal research literature employ only one of the repertoires of social accounting used by scientists informally; that these formal accounts are couched in terms of an empiricist representation of scientific action; that this empiricist repertoire exists alongside an alternative interpretative resource, which we have called the contingent repertoire; that this latter repertoire tends to be excluded from the realm of formal discourse; and that the existence of these formally incompatible repertoires helps us to begin to understand the recurrent appearance of interpretative inconsistency in scientists' discourse.

Full-length experimental papers are usually divided into separate sections: the Abstract, Introduction, Methods and Materials, Results, and Discussion. Although we believe that all these sections involve some kind of social accounting, we will concentrate upon Introductions 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 authors' 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.

We will concentrate on material from just two interviews and on one published paper by each interviewee. We have chosen to proceed by means of a comparatively detailed examination of two papers, rather than by means of quantitative analysis of large numbers of texts, because it seems to us that in this way we can demonstrate most forcibly the capacity of

particular scientists to produce radically different versions of given actions. Nevertheless, 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. Fortunately, the relatively few previous studies of formal scientific texts in other areas have all furnished observation very similar to our own.

The points we intend to make below do not involve any appreciation of the technicalities of biochemistry beyond those introduced in the previous chapter. Non-biochemists should not be deterred, therefore, if they fail 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 in this chapter, 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 I (main author Leman)**
> 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 bond 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 Bradshaw, Willow and Stein.

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LAYMAN'S GLOSS I

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. In this respect introduction I, 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 are often 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. The significance of the authors' findings is established at least partly by the way in which the social element in the text is presented.

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The opening sentence, for example, 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 organised social analysis. 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, it is depicted in the text as mere assumption. Furthermore, no supporting literature is cited. The impression is 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, the second Sentence informs us that it is *experimental results* which suggest a significantly different state of affairs from that previously assumed. Reference is made implicitly to particular actors, when the phrase 'several laboratories' is used. 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 differing interpretations of those experiments, but in terms of one group s *results* undermining the other group's *assumptions*. Although in sentence 2 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 text has conveyed a strong impression, at least for readers unfamiliar with the topic, that the rest of the paper is based upon a well established analytical position which constitutes a major advance on prior work. This has been achieved, not by the presentation of biochemical findings, but by the characterisation of scientific action and belief within the authors' social network.

The formal account of collective belief offered by these authors would have been treated as misleading by many of the scientists we interviewed.

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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.

> 3A
> *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?
> *Leman:* I'd say they were right. We always feel a little embarrassed when we talk about conformational change. Because it's 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 Leman 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 their social network.

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

> **INTRODUCTION II (main author Spender)**
> 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 2H+ 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. ->H+/2e- 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 ->H+/2e- 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 ->H+/2e-ratio is 2.0 per energy-conservation site.

LAYMAN'S GLOSS II

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 (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 ($2e^-$) 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 $\rightarrow H^+/2e^-$ 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 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 authors' contribution to knowledge, and thereby the meaning of their work in the laboratory, is established 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 in the latter setting that previous observations of these ratios were by no means widely accepted.

3B
Spender: . . . 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?
Spender: 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. Our first author, Leman, 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 Spender depicted his results in the formal paper as an advance in method; even though he says that 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.

It thus seems that, not only does the characterisation of action and belief vary from one scientist to another, but also that these scientists offer quite different versions of action and belief in formal papers as compared to informal interviews. In both these papers, the scientific conclusions being proposed are made to appear as if they followed unproblematically from empirical evidence produced by means of impersonal experimental procedures. In informal talk in the interview, however, the scientific views of the authors and their actions as scientists are sometimes allowed to appear much more personal, open to debate and generally contingent.

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What is left out of the formal account

A style is adopted in formal research papers which tends to make the author's personal involvement less visible; and the existence of opposing scientific perspectives tends to be either ignored or depicted in a way which emphasises their inadequacy, when measured against the 'purely factual' character of the author's results. As a consequence, the findings begin to take on an appearance of objectivity which is significantly different from their more contingent character in informal accounting. This formal appearance is strengthened by the suppression of references to the dependence of experimental observation on theoretical speculation, the degree to which experimenters are committed to specific theoretical positions, and the influence of social relationships on scientists' actions and beliefs, all of which are mentioned frequently in informal accounting.

Consider the following statements made by Leman during his interview. In these two quotations he is describing his reaction to the idea embodied in the model mentioned in introduction I, when it was first suggested to him by the head of his laboratory.

> 3C
> 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]

> 3D
> 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 is depicted as conceptual

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and speculative rather than empirical and controlled. It was the act of perceiving new meanings in data which 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 formulating 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 intuitive insights is emphasised.

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

> 3E
> 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]

3F

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 it's not proven, but I think it's pretty close. [19]

In quotation 3E, Leman 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 quotation 3F 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 'it's pretty close to being proven' to 'some nice good thermodynamic data would help to demonstrate it', express strong commitment to the model. Yet in the formal account, he does not refer to his own involvement in the

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formulation of the model and implies a considerable degree of critical detachment: 'A critical test of this proposal would be to measure . . .'

The last phrase in quotation 3C, 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 3G, he is referring to his own work following on from that presented in the paper under discussion here. In quotation 3H, he is talking about the response of one of his opponents to critical tests of *his* theory.

3G

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

Leman: 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. [35-6]

3H

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 it is 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 willing to accept as legitimate what he saw as a dogged, *ad hoc* defence of a false theory. In view of his uncertainty as to whether the work reported in his paper 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 only one possible description from several. 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 Leman, in informal discussion, treats his adoption of this particular model of ATP synthesis as being 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 may influence the course of individual scientists' research is excluded

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from the formal paper. Thus introduction **I** 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 no more than two laboratories seem to have been involved and that the author's presence as a co-author of one of the cited papers show him to have been a member of one of these laboratories. Yet in informal discussion, the author stressed how significant for his research career was the period in that laboratory and how he has retained strong social links with its members and with their research.

> 3J
> I went to Bradshaw's lab. He had a very profound influence on me. That was really where I was educated. [4-5]

> 3K
> 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 Bradshaw; the Bradshaw, Willow and Stein 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. Bradshaw 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 discussion in the interview, 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 Bradshaw *et al*. A 'literal reading' of that final paragraph might be along the following lines: that the authors carefully examined the compatibility 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 says that he had already decided whilst in Bradshaw's laboratory that the traditional view was inadequate and that he never seriously considered interpreting his results within its frame of reference. Secondly, he reports that his experimental design was based on Bradshaw's original model, which he himself had played a part in formulating. Thirdly, he stresses that his acceptance in this paper of the revised version of the model proposed by Bradshaw *et al*. would have been impossible without fairly direct informal contact with Bradshaw.

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Once again, it is clear that certain elements which are prominent in the informal account are 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 and in interviews, 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 quotations 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 I (main author Leman)**
> Heavy beef heart mitochondria were prepared by the method of Wong and stored in liquid nitrogen. Well coupled mitochondrial particles were prepared by a modification of the procedure of Madden. These particles were used to prepare inhibitor-protein-depleted particles by centrifuging under energised conditions according to the method of Gale ...
>
> 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 II (main author Spender)

> [A particular strain of yeast] was grown in continuous culture under conditions of glycerol limitation (Conran and Spender) or sulphate limitation (Hill and Spender). A variant of this yeast that does not require copper and has a cyanide insensitive terminal oxidase Jason and Spender) was grown in continuous culture in a copper extracted medium . . . Harvested and washed cells (Conran and Spender) were converted

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> into protoplasts and mitochondria isolated as described by Castle. Protein was determined by the method of Sheridan. Measurements of respiration-driven proton translocation were made with the apparatus described by Mason and Spender in 1.0 ml of anaerobic 0.6 M-mannitol . . . Polarographic measurements of P/O ratios were performed as described by Shoesmith, by using the experimental conditions of Spender...

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 Wong', but also 'particles were prepared by a *modification* of the procedure of Madden'. where the authors' 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 **I,** 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 Spender stated: 'In the *Biochemical Journal* they have a separate section and you have to give sufficient detail there to enable any competent scientist 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 repeatedly contradicted. For instance, it is frequently noted that exact compliance with another's methods and exact replication of their results is virtually impossible.

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3L

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. [Spender, 16]

Or as another respondent put it:

3M

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. [Richardson, 17]

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 judgements and intuitive understanding. They talked of particular researchers having 'good hands' or 'a feel' for laboratory work.

3N

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°C. It didn't matter a damn really, whether it was 35 or 40°C, 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. [Spender, 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. Such actions cannot be properly written down and they can only be understood satisfactorily through close personal contact with someone who is already proficient.

3P

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. [Spender, 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.

3Q
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?
Spender: 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. [Spender, 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 the actions relevant to the production of their results. 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.' [Spender, 20]

Scientists who belong to the same research network often claim, of course, to have succeeded in translating the content of their colleagues' formal methods sections into effective laboratory practice. But they also acknowledge that when these formal accounts 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.

3R
One is telling the general reader very roughly how the experiments are

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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 ...

> [Richardson, 20]

> 3S
> 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 . . . [Spender, 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 formal methods sections contain 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 scientists themselves are able to describe some of the differences between their formal and informal accounts of laboratory practice. They recognise that what they regard informally as crucial aspects of their actions are 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.

Empiricist and contingent repertoires

We have shown that, in the material examined above, two significantly different forms of social accounting are available to scientists and are selectively employed by them. The formal research literature is dominated, we suggest, by an empiricist repertoire. Although this repertoire is also used widely, as we shall see, in situations like informal interviews and ordinary conversations, on such occasions it is frequently supplemented by forms of contingent discourse not normally found in the research literature.

Our reference to the existence of an empiricist repertoire is based on the observation that the texts of experimental papers display certain recurrent stylistic, grammatical and lexical features which appear to be coherently

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related. As we have seen, in research papers experimental data tend to be given chronological as well as logical priority. Neither the author's own involvement with or commitment to a particular analytical position nor his social ties with those whose work he favours are mentioned. Laboratory work is characterised in a highly conventionalised manner, as instances of impersonal, procedural routines which are generally applicable and universally effective. Although the content of experimental papers clearly depends on the experimenters' actions and judgements, such papers are overwhelmingly written in an impersonal style, with overt references to the authors' actions and judgements kept to a minimum. By adopting these kinds of linguistic features, authors construct texts in which the physical world seems regularly to speak, and sometimes to act, for itself. Empiricist discourse is organised in a manner which denies its character as an interpretative product and which denies that its author's actions are relevant to its content.

When the author is allowed to appear in formal texts, he is presented either as being forced to undertake experiments, to reach theoretical conclusions, and so on, by the unequivocal demands of the natural phenomena which he is studying or as being rigidly constrained by invariant rules of experimental procedure which are, in turn, required by the nature of the physical world. Each scientist's actions and beliefs, no matter how inconsistent they appear to be with those of other researchers, are presented as those of any competent scientist. The guiding principle of this repertoire appears to be that speakers depict their actions and beliefs as a neutral medium through which empirical phenomena make themselves evident. The stylistic, grammatical and lexical resources of the empiricist repertoire can be seen as related to this guiding principle in the sense that they are necessary features of texts which are consistently depicting participants' professional actions and scientific views as inevitable, given the realities of the natural world under study. we call this repertoire the 'empiricist repertoire' because it portrays scientists' actions and beliefs as following unproblematically and inescapably from the empirical characteristics of an impersonal natural world.

The selectivity of this kind of representation becomes evident when we compare scientists' formal accounts with the characterisations of the same acts given by the same scientists as they engage in informal discourse. As we have seen, at certain points in their interviews, scientists presented their actions and beliefs as heavily dependent on speculative insights, prior intellectual commitments, personal characteristics, indescribable skills, social ties and group membership. Not only was the general style of participants' informal discourse much more personal and idiosyncratic, but in certain passages they used the wider range of stylistic, grammatical

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and lexical resources to be found in informal talk to construct accounts of their own and others' actions and beliefs that were radically different in content from those appearing in comparable formal texts. In particular, the wider range of interpretative resources employed in informal talk allowed scientists to construct accounts in which the connection between their actions and beliefs and the realm of biochemical phenomena appeared much less direct and much more dependent on other variable influences.

Thus scientists informal talk about action and belief was often much more contingent, in the sense that speakers gave accounts in which it was accepted that their professional actions and scientific views could have been otherwise if their personal or social circumstances had been different. We refer to this form of discourse as the contingent repertoire. Its guiding principle is in direct opposition to that of the empiricist repertoire in that it enables speakers to depict professional actions and beliefs as being significantly influenced by variable factors outside the realm of empirical biochemical phenomena. when this repertoire is employed, scientists' actions are no longer depicted as generic responses to the realities of the natural world, but as the activities and judgements of specific individuals acting on the basis of their personal inclinations and particular social positions.

The identification of these interpretative repertoires is a first step in making sense of the ordered variability of scientific discourse. It helps us to begin to understand how scientists, as they reproduce different kinds of context within the social world of science through the use of different linguistic registers, come to generate discrepant versions of action and belief. At this point, however, before we take the analysis further, we need briefly to reflect on the nature of the context within which our respondents' informal talk has been generated. We have to consider whether the discourse of sociological interviews is likely to be special in some way and whether we can properly claim to be comparing scientists' formal with their informal discourse. Clearly, it follows from our general analytical position that interview talk will differ to some degree from that occurring on other kinds of occasion. It has to be accepted in principle, therefore, that the contingent repertoire could be a resource on which scientists draw only when they are engaged in interviews or that it is used only during discussion between scientists and laymen. If the contingent repertoire appeared only in these contexts, the analysis would have limited significance. However, as we shall see more clearly in later chapters, this seems in fact not to be the case.

The contingent repertoire and the distinction between the two repertoires appear to play a major role in various kinds of naturally

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occurring informal discourse among scientists. It is clear, for example, that scientists employ both the repertoires identified above in a similar manner m conference discussions as well as in interviews. Moreover, we will see below that certain kinds of scientific humour trade upon participants' informal use among themselves of the two repertoires. Thus, although the forms of discourse appearing in interviews and in 'naturally occurring' situations undoubtedly differ, these differences seem irrelevant to the broad observations we have made so far. We will accordingly, for the moment, treat interview discourse as typical of a loosely defined and wide-ranging context of informal interaction involving scientists. Nevertheless, we emphasise that this is no more than a preliminary definition. Undoubtedly interview talk will be found to differ systematically in certain as yet unknown respects from other forms of scientific discourse. Eventually, therefore, particularly as more direct recordings of naturally occurring talk among scientists become available, it should be possible to refine our concepts and to begin to specify which facets of interview talk are peculiar to interviews and which are generic to informal discourse in science.

Scientists' accounts of interpretative variation

If there are systematic variations in scientists' discourse, of the kind we have suggested, it would be surprising if participants were entirely unaware of and unable to comment upon them. Our respondents were, in fact, not only able to describe some of the contextual differences in discourse which we have illustrated, but they were also able to furnish what they presented as adequate explanations of these differences. These explanatory accounts themselves exemplify how scientists draw flexibly on both the empiricist and the contingent repertoires in the course of informal talk. We can, therefore, extend our analysis by examining these accounts. In the following passage, Leman is explaining why the scientific literature omits to mention the author's personal involvement. We have numbered each statement in this and the following quotation for ease of reference.

> 3T
>
> 1 Everybody wants to put things in the third person. So they just say, 'it was found that'. 2 If it's later shown that it was wrong, don't accept any responsibility. 3 '*It* was found. I didn't say I *believed* it. *It* was found.' 4 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 ... 5 Of course, everybody knows what's going on. 6 You're saying, 'I think'. 7 But when you go out on a limb, if you say 'it

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> was shown that' or 'it is concluded' instead of 'we conclude', it should be more objective. 8 It sounds like you are taking yourself out of the decision and that you're trying to give a fair, objective view and that you are not getting *personally* involved. 9 Personally, I'd like to see the first person come back. 10 I slip into it once in a while. 'We found.' 11 Even then I won't say 'I'. I'll say 'we' even if it's a one-person paper. 12 Can spread the blame if it's wrong *[laughs]*. [Leman, 57-8]

Attention is drawn in this passage to the use of impersonal formulations and the tendency to avoid revealing one's personal involvement. The reason the speaker gives for this stylistic convention is that it enables authors to avoid accepting any responsibility for errors (3T2). Yet he appears almost immediately to contradict this explanation, when he suggests that nobody is ever actually misled into thinking that no one was responsible for a paper's contents because it made little or no mention of human agents (3T5-6). Thus the rationale offered by this interviewee for this feature of formal texts seems to be internally inconsistent and unconvincing. The inconsistency seems to occur because the speaker combines an explanation of formal discourse in contingent terms (3T5-6,11-12) with an interpretation which stays close to the guiding principle of the empiricist repertoire (3T4,8).

Our second author also emphasises the impersonal character of research papers. But he argues that scientific papers have this characteristic because they are devised in terms of a mythical conception of scientific rationality.

> 3U
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> Spender: 1 I think the formal paper gets dehumanised and sanitised and packaged, and becomes a bit uninteresting. 2 In some ways I like the old ones, where a chap says, 'I did this and it blew up in my face'. . .3 Some of the charm has certainly gone.

Interviewer: 4 Why do you think that is?

> Spender: 5 One is a myth, that we inflict on the public, that science is rational and logical. 6 It's appalling really, it's taught all the way in school, the notion that you make all these observations in a Darwinian sense. 7 That's just rubbish, this 'detached observation'. 'What do you *see?*' 8 Well, what *do* you see? God knows, you see everything. 9 And, in fact, you see what you want to see, for the most part. 10 Or you see the choices between one or two

> rather narrow alternatives. 11 That doesn't get admitted into the scientific literature. 12 In fact, we write history all the time, a sort of hindsight. 13 The order in which experiments are done. All manner of nonsense. 14 So the personal side does get taken out of this sort of paper. 15 Maybe it's felt that this isn't the place for it to be put. I don't know... 16 Sometimes you get more of the personal side in reviews. 17 Some of them are quite scandalous actually, once you can read between the lines.

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> *Interviewer:* 18 Do you think there would be any disadvantages in allowing that sort of thing back into the formal literature?
> *Spender:* 19 I don't know. It depends what the purpose of the literature is. 20 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. 21 All these are valid reasons, but they're never admitted to. 22 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. 23 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.' 24 I might say 'This was overlooked by Bloggins *et al.*' 25 I won't say why I think they overlooked it. 26 I'm afraid it's *gone* and it's not going to reappear here. 27 Probably it shouldn't reappear. I guess it reappears in other places. 28 And we still know what's going on. 29 We just don't make it public. [Spender, 32-3]

Like the previous speaker, this scientist stresses informally 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 our terms, participants are being represented as translating the formal text into their more extended informal repertoire. Initially, Spender's 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 empiricist myth of scientific action. He seems to suggest that this myth has been devised for public consumption (3U5-6,29). Spender's description of the observable features of formal discourse, like that of Leman, is entirely consistent with our own (3U6-7,11-14, 23-5). Furthermore, Spender be-gins to formulate, in a very preliminary fashion (3U7-14), an alternative to the empiricist conception of scientific action and belief, which is presented as a more accurate portrayal of what actually happens in science and which is couched in terms of the contingent 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 and that in practice scientists are greatly influenced by personal factors. Moreover, in the second half of the passage he clearly returns to the more traditional, empiricist view of science (3U20-2). 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 be ignoring 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. Thus, like the previous respondent, Spender fails to produce a coherent, overall account of the differences between formal and informal discourse. Both

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speakers generate inconsistencies as they move somewhat erratically between empiricist and contingent versions.

we suggest that these interpretative difficulties arise because the speakers in these passages present interpretations of the formal realm in contingent terms along with interpretations couched in its own empiricist terms. Because both our respondents had been giving extended contingent accounts of their research practices immediately before being asked to reflect on the character of formal discourse, it seems reasonable to suppose that it was difficult for them to avoid mentioning the 'conventional and unrealistic' features of that discourse. In other words, both respondents are inclined by the discursive situation created during the interview to furnish contingent accounts of the actions supposedly lying beyond and behind formal scientific discourse. As a result, as we can see in the passages above, the characteristics of empiricist discourse are to some extent made to appear merely rhetorical. Yet, if our analysis is broadly correct, the empiricist repertoire is such an important interpretative resource, in informal as well as formal settings, that its rejection is likely to generate interpretative problems in any subsequent talk about science. Moreover, as we will see in the next chapter, speakers continually construct empiricist accounts of their own scientific position. we would, therefore, expect scientists to draw quickly back from or to qualify any talk in which they can be heard as generally undermining empiricist versions of scientific action. Thus we can understand the interpretative inconsistencies in our respondents' explanations of the nature of formal discourse as following from their socially generated (i.e. discursively generated) use of two formally incompatible interpretative repertoires to provide accounts of action.

The passages contained in this section begin to reveal some of the interpretative complexity and variability of informal scientific discourse, in so far as interviews can be taken to represent such discourse. This apparent variety of informal discourse poses an important issue for our analysis. For, although it is clear that the restricted and highly conventional formal discourse of science does display evident interpretative regularities, it is by no means so obvious that informal discourse is systematically organised to anything like the same extent. On the basis of our analysis so far, it could be argued that what we have termed the 'contingent repertoire' is merely a residual category, containing a *melange* of disparate elements which have in common only the fact that they do not appear in the formal literature and are sometimes difficult to reconcile with its empiricist vocabulary. Our next step, therefore, must be to begin to look in more detail at scientists' informal discourse, in order to observe whether it displays recurrent interpretative forms and to explore whether

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scientists employ the contingent repertoire as a coherent discursive resource. In short, we will now start *to* move from a fairly crude identification of broad interpretative repertoires to a more fine-grained examination of the orderly production of social meaning in science.