gross

REPRESENTING REALITY

Discourse, Rhetoric and Social Construction

Jonathan Potter

SAGE Publications
London • Thousand Oaks • New Delhi

Contents

Introduction

In virtually any situation appeal to the facts, to what really happened and what is only invention, can be a powerful device. Factual accounting is the stuff of arcane scientific disputes over whether neutrinos have been detected, of mundane domestic conflict over who last washed the dishes, and of ideological concern as particular versions of the economy are assembled and undermined. Descriptions are so bound up with our lives that virtually any conversation includes reports of events and actions. We read newspapers and watch television programmes which overflow with real life stories and varied factual claims. Factual reports are a commonplace currency of occupations as varied as doctors, teachers, engineers and police officers. And fiction, too, ironically but interestingly, is full of realist description striving to make characters believable and plots coherent.

This book is concerned with two closely related sets of questions. First, how are descriptions produced so they will be treated as factual? That is, how are they made to appear solid, neutral, independent of the speaker, and to be merely mirroring some aspect of the world? How can a factual description be undermined? And what makes a description difficult to undermine? Second, how are these factual descriptions put together in ways that allow them to perform particular actions? What kinds of activities are commonly done using descriptions? And why might descriptions be suitable for doing those activities?

There are three main objectives for the book. First, it provides an overview of the main traditions of work on fact construction: the sociology of scientific knowledge, the closely related perspectives of ethnomethodology and conversation analysis, and the 'structural tradition' of semiology, poststructuralism and postmodernism. The coverage is necessarily selective. I am trying to capture the main thrust of the arguments, and pull out the issues which are particularly relevant to the study of fact construction, while avoiding getting bogged down in unnecessary technicalities. The coverage is comparative and points of convergence and conflict are emphasized where possible. Anyone hoping for a full integration will be disappointed; however, I do draw on elements of all three traditions in the more specific discussion in the later chapters.

The second objective of the book is to give an account of some of the basic procedures through which the factuality of descriptions is built up, and how those descriptions are involved in actions. This combines a detailed discussion of a range of relevant research on fact construction – some derived

from the traditions reviewed earlier, some with more disparate roots – with novel analyses of my own. In particular, a set of themes in the construction of facts are identified and illustrated with sample analyses. My hope is that these will both provide an organizing framework for making sense of the different studies and highlight some issues which any research using descriptions would wish to discuss. Further, they should provide some considerations which anyone analysing descriptions and accounts of any kind are likely to find helpful.

The third objective is more diffuse, but perhaps more important. I hope the book will show both how significant the role of descriptions and factual accounts is in our lives and what a rich and fascinating topic it is to study. I have deliberately chosen to draw on materials from a wide range of factual descriptions to illustrate the generality of the questions I am raising. At the same time, many of the examples should be familiar to most readers (for example, newspaper reports and relationship disputes) which I hope will make the points more accessible as well highlighting their generality. I have come to see that factual discourse, even in casual, mundane settings, such as in an argument between a husband and wife, is organized in enormously fine detail and with great subtlety. If I can convey some of that subtlety and intricate organization here I will be very happy.

Before starting with Chapter 1, there are three tasks. First, I will provide a small number of brief examples to illustrate more explicitly what is involved in studying fact construction, and to raise some of the themes that will come up later. Second, I will comment on some background issues relevant to the book and discuss one or two precursors to the work described here. Finally, I will give a brief overview of the book as a whole.

Welcome to the Fact Factory

A Fictional Undercover Cop

In Quentin Tarantino's film, *Reservoir Dogs*, a central character is a young undercover cop, Freddy. His mentor, Holdaway, has helped get him access to a gang of jewel thieves and is teaching him a story that he can use to make his criminal identity convincing.

Freddy: I gotta memorize all this? There's over four fuckin' pages of shit here.

Holdaway: It's like a fuckin' joke, man. You remember what's important and the rest you make your own. You can tell a joke, can't ya?

Freddy: I can tell a joke.

Holdaway: Well just think about it like that. Now the things you hafta remember are the details. It's the details that sell your story. Now your story takes place in a men's room. So you gotta know the details about that men's room. You gotta know if they got paper towels or a blower to dry your hands. You gotta know if the stalls got doors or not. You gotta know if [*Holdaway continues . . .*] Now what you gotta do is take all them details and make 'em your own. This story's gotta be about you, and how you perceived the events that took place. (Tarantino, 1994: 71)

What lessons are there here? The first is very basic, and easily missed. It takes work to produce a description that is convincing; it can be done well and it can be done badly. There are more or less standard procedures that can be drawn on when establishing the veracity of an account. Note the emphasis that Holdaway places on detail. It is not the general pattern of events so much as the details that makes the story credible. These are the sorts of things that someone who was there to witness events would know but which are not intrinsic to the general narrative. This is a theme that will be explored in Chapter 6.

A further consideration is that Freddy is working up an invented story. It is tempting to consider this to be totally different from someone recounting an actual story. That is, we might consider the actual story as the standard, natural form and the fake one as a derived form or parasite. However, both the conversation analyst Harvey Sacks and the philosopher Derrida offer reasons for not accepting this hierarchy too readily. It may be that an authentic story draws on the same resources as a subversive alternative that pretends to authenticity. And maybe the way authentic stories are organized is partly a consequence of the possibility of inauthentic alternatives.

Two final issues are highlighted with this example: the contrast between fact and fiction and reflexivity. This is not a real dialogue between an undercover cop and his boss – it is invented, and it is part of a fiction where a whole set of considerations about the dialogue will be at work over and above whether it is the sort of thing that might actually be said in a real-life example of this kind (does it work dramatically? does it develop the characters? and so on). Indeed, there are all sorts of reasons to think that a real life conversation between cops like this would be very different. Most basically, a comparison between transcripts of actual conversations and playscript dialogue is likely to show the actual conversation to appear messier than the fictional one – it will be full of corrections, hesitations, pauses, ungrammatical constructions. However, that does not mean that actual conversation will not be organized in subtle and artful ways; nor, for that matter, that the fictional example is uninteresting. Both are fascinating, and both throw light on each other.

One of the interesting and paradoxical features of fiction is that it is a major domain for fact construction. Novelists and playwrights produce texts which need to be credible and believable on some level. For example, the vivid detail and witnessed perspective that Holdaway emphasizes is also a central feature in the literary skill of making a story convincing. Tarantino's text is both *about* the cop's process of learning fact construction and, simultaneously, *doing* fact construction as it vividly paints this interaction for us with its swearing, colloquialisms and displayed anxieties. And this reflexive relationship is repeated here in the introduction to the current book, where it

is both standing as an example of fact construction and contributing to the credibility of this text. Freddy is convincing the jewel thieves; Tarantino is convincing cinema goers; I am trying to convince readers.

Economies of Truth

In the course of the famous 'Spycatcher' court case in Australia, in which the British government attempted to prevent publication of a book claiming the security organization MI5 was run by a traitor, Sir Robert Armstrong famously responded that he had been 'economical with the truth' when examined by defence attorney Malcolm Turnbull. The following is reconstructed from partial reports in different newspapers:

Mr Turnbull: Did the letter contain an untruth?
Sir Robert: It does not say we already had a copy of the book.
Mr Turnbull: It contains an untruth.
Sir Robert: It does not contain that truth.
Mr Turnbull: It gives a misleading impression.
Sir Robert: It was a misleading impression in that respect, but a lie is a straightforward untruth.
Mr Turnbull: And what is the difference between an untruth and a misleading impression?
Sir Robert: The question is rather one of being *economical with the truth.*

This example illustrates a number of relevant themes. Note first that the phrase was produced as a response to cross examination. That is, it is part of the interaction, it is occasioned by its context where it is a response to an accusation. It addresses inconsistencies in testimony while resisting the implication that the speaker had been lying. The simple point here is that people do not produce descriptions out of the blue; they produce them for what they can do in some stream of activity. Sir Robert's claim should not be understood as an abstract claim about truth that he will stand behind in whatever future context he might find himself; it is produced *on* this occasion *for* this occasion.

A second point is related to the idea of being economical with the truth itself. It captures in a very neat way how the business done by a description can relate to both what is described and what is left out. The point of the being 'economical with the truth' in this version is that you can provide an answer to a question which does not contain actual falsehoods, but works by leaving out something that would give a very different impression. For example, in this case Sir Robert denied that the government had a copy of a book without telling the questioner that it had the proofs of the book; that is, although it did not have an actual bound copy it did know what it would contain. This is a feature of factual accounting that we will return to in detail in Chapter 7.

This example also shows up the sorts of skills that people have for undermining and resisting factual versions. Although this phrase was used as part of a distinction between lying and giving a misleading impression by withholding

information (Sir Robert later claimed to have been drawing on Edmund Burke's distinction between 'falsehood and delusion' and 'economy of truth'), it was widely taken as a softened admission of lying. Indeed, since then 'economical with the truth' has become a popular pejorative phrase for certain kinds of official lying and deception. Here are just three examples from around 50 thrown up by a brief search through just three months of two newspapers on CD-ROM.

> For ministers to point out that four out of five prescriptions are not paid for by individuals is surely being *economical with the truth.* Forty percent of the population pays for prescriptions. (*Guardian*, 19 February 1994)

> Mr Paul Marland . . . also disputed claims that Lloyd's never made Names bankrupt. He said the market was being '*economical with the truth*'. (*Daily Telegraph*, 17 January 1994)

> Grave doubts have been cast on the financing methods used to purchase Venables' stake at Spurs by indicating that he has been somewhat *economical with the truth* when he claimed he had put all his money into Spurs. (*Daily Telegraph*, 19 January 1994)

The phrase is not always directly quoted in this way; it can be modified to do different tasks. For example, an editorial about a minister's disputed hotel bill (the issue being whether his visit was an undeclared gift) describes him as being '*miserly with the truth*', and a cartoon about a government law officer's criticism of an official inquiry features one commuter saying to another, 'he feels they have been *extravagant with the truth*'. By modifying the phrase, reporters, cartoonists and others can draw delicately on the original and familiar meaning to ironize some claims and arguments.

More generally, the notion of an economy of truth serves as an appropriate metaphor for the topic of this book. Like money on the international markets, truth can be treated as a commodity which is worked up, can fluctuate, and can be strengthened or weakened by various procedures of representation.

The Anecdotalizer

This extract is from a light-hearted article where the author confesses to be a compulsive anecdotalizer.

> Anecdotalising. It's an addiction. Every minuscule detail of my life is transformed into another party piece. Pubs, bus stops, the office, are all turned into impromptu theatre spaces. . . .
>
> Often there is no incident. Having a point, an event or bizarre coincidence is reserved for beginners. Anyone can string out a tale about the time they were locked out of the house naked while a Salvation Army brass band marched down their street.
>
> Only a true raconteur will hold forth on a failed attempt to adjust a wall thermostat. (*Guardian Weekend*, 6 January 1993)

One point that this extract neatly illustrates is that descriptions are not just

involved in situations of conflict, or where there is a strong concern with factual accuracy. People in their everyday talk tell stories to one another; they construct narratives – anecdotes – to make points, for entertainment and laughter.

In the continuation of the article the writer tells a story about recklessly starting an anecdote and only halfway through realizing that there is no point or punch line. This again stresses the theme of reflexivity. The article about the compulsive anecdotalizer is *itself* constructed as an anecdote, where a relatively trivial matter – not having a good ending to a story – is turned into a major disaster: 'like the captain of a sinking ocean liner, I refuse to acknowledge defeat, tell the band to play on'. And again, note, this narrative is being set to work in the current text that I am writing.

Another issue highlighted here is the flexibility of descriptions. Descriptions are not determined by events but are worked up, and this working up can itself be skilful. Thus the achievement of making the failed thermostat adjustment is turned into an interesting and involving story. However, while the surface implication of the article is that the compulsive anecdotalizer is a rather special figure, I will argue that issues involving the construction of versions are endemic in conversation. People package their lives into narratives which they tell for a whole range of different purposes. For example, one of the materials which will be used in several chapters below is taken from a relationship counselling session in which a couple each gives versions of an evening where the woman may have been flirting and the man may have attempted suicide (as we will see, these are already highly contentious descriptions). The anecdotalizing in this case is geared to actions such as assigning blame and showing who needs to change their behaviour.

These three examples are intended to provide an initial orientation to the themes that will be explored in detail later in the book. Before then there are some final introductory issues that need airing.

Preparations

Philosophy

It is important to emphasize that this is not a work of philosophy. In particular I am not trying to resolve classic philosophical disputes between, say, advocates of realism and anti-realism. And I am certainly not trying to answer ontological questions about what sorts of things exist. The focus is on the way people construct descriptions as factual, and how others undermine those constructions. This does not require an answer to the philosophical question of what factuality is. Nevertheless, this approach cannot fail to have implications for broader debates about the status of realism and relativism, and about the appropriate ontology for social sciences. Work of this kind contributes to the respecification of the nature of philosophical discourse as rhetoric (following Richard Rorty, 1991). Conversely, one move in linguistic philosophy has been to rework unmanageable and enduring metaphysical

questions as issues which can be addressed through a consideration of people's discourse. For example, rather than trying to solve the philosophical question of free will, John Austin (1961) suggested it might be more constructive to consider the way people account for freedom and constraint.

Rather than arguing directly with realism, the sorts of rhetorical devices that are used to shore up a realist position have been analysed (Gergen, 1994; Potter, 1992). There are certain common tropes that realists use to attack the coherence of the sort of constructionist position developed in this book, most notably the *furniture argument* ('see this [bangs on table]; you're not telling me that's a social construction') and the *death argument* ('what about the victims of the Holocaust, the fleeing Iraqis on the Basra Road, victims of amnesia – surely you don't want to deny their reality'). Again, the response that Derek Edwards, Malcolm Ashmore and I (1995) developed to these arguments was not to argue directly against them, but to take apart the rhetoric on which they are based; decoupling the implied equivalence between relativism and lack of political commitment, and emphasizing that constructionist arguments are not aimed at denying the existence of tables (a very realist idea!) but at exploring the various ways in which their reality is constructed and undermined. Interesting though they are, these debates move away from the main issues of this book, and they will not be further explored here.

Definitions and Etymology

The simplest way of characterizing the main theme of this book is in terms of the way descriptions are made factual, and what those descriptions are used to do. However, the words *fact* and *description* (and related terms such as *report* and *account*) have a complex history, and their current sense is only the start-point for research. *Fact* in the sense of 'a thing done or performed' (*Oxford English Dictionary*, 2nd edition on CD-ROM; henceforth *OED*) goes back to the sixteenth century; while the seventeenth century starts to see the more familiar modern sense, 'something that has really occurred or is actually the case', and contrasts made between facts and inferences or fictions: 'a particular truth known by actual observation or authentic testimony, as opposed to what is merely inferred, or to a conjecture or fiction' (*OED*). The interest in facts in this book is attributional rather than actual. That is, the topic is what participants *count as* factual rather than what is *actually* factual.

The term *description* can refer to both action and object: on the one hand, it is 'the action of setting forth in words by mentioning recognizable features or characteristic marks' and, on the other, it is 'a statement which describes, sets forth, or portrays; a graphic or detailed account of a person, thing, scene, etc.' (*OED*). Both of these senses date back to the fourteenth century. The terms *account* and *report* are described in a similar fashion. To *report* something is 'to relate, narrate, tell, give an account of (a fact, event, etc.); while an *account* is 'a particular statement or narrative of an event or thing; a relation, report, or description' (*OED*). Note the way the definition of *description* uses the term *describes* as well as the term *account*, the definition of *report* uses

account, and the definition of *account* uses *report* and *description*. There is a lot of circularity. However, the contrast that I want to pick out is the way *fact* implies truth and real occurrence while *description* does not. This book covers the interactional space between these two notions, the business of building up a description as a fact.

Specificity versus Universalism

One of the tensions in this book is that between specificity and generality. I will argue that to understand the way factual accounts are constructed, and the way they are bound up with activities, it is important to understand their specific features, and the way those features relate to the setting in which they are used. Harvey Sacks (1992) has effectively shown the way much of the business of interaction is carried by what might at first sight seem to be the details. In talk, for example, this may be the selection of one specific word from a group of words with similar meanings, or the appearance of delays and overlaps, hesitations and corrections. Much of the book will be concerned with specific features of talk such as this, or with the particular constructions that appear in newspaper reports or texts of other kinds.

As a counterpoint to this focus on specificity, I have deliberately chosen to cover a very wide variety of forms of factual discourse. In the chapters that follow, I discuss scientific discourse, newspaper articles of various kinds, a couple's relationship counselling sessions, novels and films, everyday talk and talk amongst documentary film makers. My use of this wide selection of materials is driven by the conviction that there are general features of fact construction. That is, there are considerations that are likely to be attended to whatever the type of discourse. By casting the net widely in this way these general patterns are more likely to be revealed along with limitations on their generality. It is notable that the main traditions discussed in Chapters 1 to 3 combine major theoretical differences with differences in the kinds of material they are focused on: sociology of scientific knowledge obviously deals with scientific practices; ethnomethodology and conversation analysis have come to focus on talk in everyday and institutional settings; and work in post-structuralism and postmodernism has concentrated on literary and philosophical texts. I have opted for a comparative approach at both the level of theory and material.

Transcription

A number of the chapters below discuss examples of transcribed talk. Most use the increasingly standard system of transcription developed by the conversation analyst Gail Jefferson (1985; for an overview see Psathas, 1995). In some cases the origin is published articles; in other cases the examples are reproduced from original transcript. Either way there is a dilemma over its presentation. Many people find the sorts of detail, and the transcription symbols that go along with it, interfere with its readability. That would be a reason for simplifying the transcript: stripping off the extraneous symbols and

elements. However, given the sorts of arguments about specificity I have just noted, this kind of detail needs to be recognized as an intrinsic part of a good transcript. The transcribed detail is not just an empiricist flourish to demonstrate completeness or conscientiousness or rigour (although it might do those things – see Bogen, 1992); it is an intrinsic and essential part of the interaction. In addition, anyone wishing to evaluate the claims and interpretations I make about sections of transcript might want to do so without being handicapped by information lost through judgements about what is extraneous.

I have been mindful of both of these concerns, and I have retained transcription symbols and information unless it is a major handicap to the intelligibility of the example. I hope that those readers unfamiliar with the Jeffersonian system (briefly described in an Appendix) will soon come to see it as clear and, indeed, invaluable for giving a sense of the talk as situated, voiced and, most importantly, a co-constructed part of an interaction (Schegloff, 1995).

Reflexivity

This is a book about constructing facts. One of its themes is the way descriptions are organized to make some version seem credible and objective. This also is a book full of descriptions (of theory, of disciplines, of literatures, of findings, of bodies of belief, and so on). It is a book, then, that refers to itself. This immediately raises the issue of reflexivity. Let me put this in its sharpest form. If the book is revealing that facts are constructed by devices, what of the devices that it uses to construct the fact that facts are constructed by devices? Put another way, do the conclusions of the book have any implications for the book itself? Is it, for example, entirely self-destructive?

Without getting too far ahead of arguments that will be aired more thoroughly later, I do think that there are reflexive implications from work on fact construction for this book and for social sciences more generally. Indeed, I even think there is an element of self-destruction. At the end of the book the ideal reader should be able to turn their gaze back on the book itself and decompose the techniques and tropes that it draws on so freely. For I have opted to use a conventional mode of presentation. It is not a new literary form; no alternative voices will pop up to argue with the main authorial voice (Mulkay, 1985); and it is not (I hope!) a parody of a social science book (Ashmore, 1989). I hope that erratic, but persistent, references to reflexive issues in the course of the text will underline their pertinence.

That is not to say that a novel literary form would have been inappropriate; more than anything it is the sheer difficulty of achieving one without making the text reader-unfriendly that put me off. So, as it stands the book has a single authorial voice (although thinkers such as Mikhail Bakhtin, 1981, might dispute whether any book actually has a single voice) and draws on many of the familiar tropes of social science writing and fact construction more generally. It is (almost) unashamed of *drawing* on the kinds of visual metaphors that imbue recent western writing about knowledge: it is concerned with *throwing*

light on *murky* topics, tracing out a new *point of view*, and *seeing* how far a constructionist argument can be pushed (Derrida, 1982; Rorty, 1980).

Omissions

As I will discuss in detail later on, academic writing tends to draw on textual forms – tropes – which construct a god-like, all-seeing, all-knowing, all-comprehending stance, which is at the same time disinterested and fair. Real authors are, of course, located in history, in particular communities, constrained by their grasp (or lack of grasp) of bodies of ideas, by the quality of their libraries and so on. Writing is full of serendipity and is inseparable from academic biography. Even noting this can have the same quality: 'look, here is a stance *so* disinterested and so god-like that it can even understand and admit its own limitations!' Nevertheless, this is an opportunity to highlight (confess?) some limitations. (I am not going to confess prejudices – I am sure they will be all too apparent.)

The first limitation is in my coverage and use of the work of Mikhail Bakhtin. He makes a couple of brief appearances, but I have a strong sense that his work could be made much more relevant to a number of the arguments used here (cf. Shotter, 1992). The second limitation is in the failure to address seriously the Actor Network Theory developed by Bruno Latour, Michel Callon and John Law (for example, Callon, 1995; Latour, 1993; Law, 1994). This is an exciting approach to facts and knowledge which has important implications for any study of fact construction. Yet I have been unable to decide whether it provides an organizing frame within which some of the ideas that I discuss could be situated, or whether those ideas raise problems for that frame. My rather weak solution in this text is to attempt neither situation nor critique.

A third limitation is of a rather different order. I long envisaged this book to have a chapter on images or visual rhetoric. The fact that it does not is not because I do not think this is an important topic – I do; it is because the book has grown and was already in danger of becoming unwieldy. This chapter was the one that could be abandoned with least disturbance to the overall argument. If had been included, it would undoubtedly have covered the recent sociology of science work on practices of 'making visual' in research settings such as staining cells, graphing animal habitats and charting features of the sea bed (Aman and Knorr Cetina, 1988; Atkinson, 1995; Lynch, 1985, 1988; Myers, 1990; Goodwin, 1995 – see also references in Ashmore et al., 1995). A common theme here is the collaborative work that goes into producing observable images that provide for stable interpretations. The chapter would also have covered some of the classic work on semiology, such as Roland Barthes's essays on photography (Barthes, 1977, 1981) and more recent semiologically inspired developments (Hodge and Kress, 1988; Shapiro, 1988; Williamson, 1978). This body of work in particular provides a major attack on the idea of photography as an innocent medium of factual representation. Some other time . . .

Precursors

It is useful to situate what comes next in terms of two of its most important precursors: John Austin's speech act philosophy in *How to Do Things with Words* and Peter Berger and Thomas Luckmann's phenomenological development of the sociology of knowledge in *The Social Construction of Reality*. These two books are part of what made the current project possible.

Austin and Speech Acts

One of the main elements in Austin's philosophical project was an attack on views of language that made referential issues of truth and falsity paramount. In place of the overwhelming philosophical concern with the 'truth value' of statements taken in the abstract, Austin emphasised the *practical* nature of language. Language is used to do things; it is a medium of action.

Initially, he built a plausible distinction between two classes of utterances. On the one hand, there are utterances that state things: 'Loughborough is in the middle of England'; on the other, there are utterances that do things: 'I bet you five pounds that Labour win the election.' But in the course of a brilliantly argued set of lectures he showed that the distinction could not be sustained. He proposed a general theory of speech acts which treats all utterances to be both performing actions and having features that depended on issues of truth and falsity. Thus 'I bet you five pounds that Labour wins the election' is part of the act of betting – but depends on there being a sensible referent for the words '*Labour*' and '*election*'; at the same time 'Loughborough is in the middle of England' is a statement that may be evaluated for its truth or falsity – but when uttered performs the act of stating.

This is the crucial and radical point. Austin's work starts to move the discussion away from the idea of statements – descriptions, reports – hanging in some conceptual space where they can be compared to some feature of the world and focuses attention on statements as actions performed in settings with particular outcomes. As he put it 'the total speech act in the total speech situation is the only actual phenomenon which, in the last resort, we are engaged in elucidating' (1962: 148).

It would be most unfair to criticize Austin for not doing something that he was not trying to do; after all, his targets were particular traditions in philosophy. Nevertheless, it is worth noting some limitations in his work, and the burgeoning literature it spawned, for the enterprise I am concerned with.

First, despite the expressed commitment to elucidating the total speech act in the total speech situation, Austin worked with made-up examples, which tend to be typifications (the standard bet) or institutionally determined (*I do* in the marriage ceremony) and which are considered outside of their production in actual settings. Again, this is not a problem for Austin in so far as he is seen as developing a philosophical argument, but it starts to become an important problem when Austin's work is drawn on as the basis for an analytic programme for studying language practices in general and factual

language in particular (for example, see, Duranti, 1992). It cuts across the sorts of sense making that go on in everyday interaction by having the meaning of the target utterance determined by fiat. This approach will be discussed more in Chapter 2. Austin's emphasis on idealized cases as the best start point for understanding language has been effectively criticized by Jacques Derrida in a series of arguments discussed in Chapter 3.

Another problem lies with Austin's treatment of statements as actions. This is a radical first step in the study of fact construction, but the procedure of basing arguments on decontextualized invented examples leads him to miss one of the fundamental features of statements. Statements are used to do things. This can be seen as a subclass of one of the classic problems with speech act theory, that of indirection. Speech act theorists have struggled to account successfully for one of the most pervasive phenomena in language use which is, to put it crudely, the separation of form and function. Thus when we say 'can you pass the salt' we are not asking a question about abilities, we are making a request for the salt to be passed; while if we are making an offer we often couch it as a request: 'have a drink'. As we will see, statements are a more or less indirect way of performing a huge range of different actions: complimenting, complaining, inviting, blaming and so on. Showing that statements are actions is just the preliminary; what comes next is an examination of the many different actions that statements can do; where Austin finishes this book starts.

Berger and Luckmann and Social Construction

Berger and Luckmann's classic, *The Social Construction of Reality*, made a hugely influential contribution to the sociology of knowledge. It provided a systematic argument to the effect that the worlds in which we all live are not just there, not just natural objective phenomena, but are constructed by a whole range of different social arrangements and practices. For our present purposes it had the important role of establishing processes of social construction as a central topic of study.

A second important feature of the book is its emphasis on taking a 'symmetrical' stance to knowledge that is treated as true and false. As they put it: 'It is our contention, then, that the sociology of knowledge must concern itself with whatever passes for "knowledge" in a society, regardless of the ultimate validity or invalidity (by whatever criteria) of such "knowledge"' (1966: 15).

As we will see in Chapter 1 when discussing the sociology of scientific knowledge, this stance is an extremely important one when dealing with fact construction because it frees the researcher from taking sides with particular groups whose beliefs are better established than others and, more fundamentally, from deciding what should be counted as true or not. The social researcher is thus excused the difficult task of being a better physicist than the physicists that are being studied, a better surgeon, or whatever.

Like Austin, however, Berger and Luckmann were better at opening up the potential for analysing fact construction as a topic than following through that analysis. There are a number of potentially problematic features of their argument. First, the book is not an analytic book. It does not contain much in the way of analysis of how reality is constructed. Instead it provides general arguments for such construction and explores their implications for social life. Again, it would be unfair to criticize Berger and Luckmann for something they were not attempting, but this is an important difference from the approach to fact construction that I have adopted.

Second, Berger and Luckmann's study is focused on the phenomenology of individuals' experience. That is, rather than see processes of construction at work in talk and texts, it emphasizes people's perception and understanding:

> The reality of everyday life is organized around the 'here' of my body and the 'now' of my present. This 'here and now' is the focus of my attention to the reality of everyday life. What is 'here and now' presented to me in everyday life is the *realissimum* of my consciousness. (1966: 36)

The sorts of problems that this kind of 'cognitivism' generates are discussed in various places below, but particularly in Chapters 4 and 8. For the moment, I will just note that it tends to obscure the interactional and rhetorical nature of fact construction, while reifying a mental world which itself a major element in factual discourse. In other words, people produce versions of their mental life – their motives, their beliefs and so on – in the course of establishing the factuality of particular claims (see Edwards, 1996).

A final problem with Berger and Luckmann is that their constructionism is a rather limited affair. Although they spend a lot of time considering the various assumptions that a garage mechanic, for example, makes about 'his' world and its nature, they themselves can see round the edges of this construction without any problems. They do not, that is, consider the implications of treating social construction as a general feature of knowledge, including that of sociologists. I have already stressed the value of reflexivity; Berger and Luckmann ignore any epistemological troubles it faces them with. Despite these limitations, both Austin and Berger and Luckmann played a crucial role in opening up for study the issues that are the topic of this book.

Overview of the Book

The first three chapters in the book cover the main traditions of work involved in fact construction. Chapter 1 covers the sociology of scientific knowledge which exploded, especially in Britain, in the late 1970s and throughout the 1980s, stimulated by earlier developments in philosophy of science. This offered a radical reappraisal of traditional views of scientific facts and is still a site for heated debate between sociologists, philosophers and scientists. The chapter describes traditional sociology of science, and a range of challenges to it from philosophers. These challenges reconceptualized the nature of observation, stressed the interconnected nature of scientific

claims, and emphasized the importance of scientific community and practice. The work of Harry Collins and the 'Empirical Relativist Programme' is discussed in detail, particularly his studies of the social construction and destruction of replication, along with 'constructionist' and 'interest' theories of scientific knowledge. This chapter highlights the value of taking a relativist perspective which starts without preconceptions about what facts are true, and illustrates some of the ways in which rhetoric is both emphasized and underplayed in sociology of science.

Chapter 2 takes ethnomethodology and conversation analysis as its topic. Stimulated by the pioneering work of Garfinkel and Sacks during the 1960s, these perspectives offered a novel account of both social interaction and the procedures that people use to understand the nature of their world and to display their conduct as coherent. It laid a particular emphasis on the way the stable, orderly nature of human life is achieved by people's practices. The chapter describes the central ethnomethodological concepts of indexicality, reflexivity and the documentary method of interpretation, and reviews some studies of the organized practices through which facts are made, concentrating on the example of suicide statistics. Another important topic is Melvin Pollner's work on 'mundane reason'; that is, the pattern of methods and assumptions that people use to sustain the sense of a stable and agreed underlying reality. Conversation analysis is introduced with a focus on the way it has conceptualized accounts as a structural element in particular kinds of interaction. This provides a developed research example where one class of descriptions (accounts) can be understood as performing a particular action, and having features that facilitate the performance of that action.

The loose tradition of semiology, post-structuralism and postmodernism continues to exert a major influence across the human sciences and wider cultural debates. Here the nature of human understanding has been redefined more than once. Chapter 3 introduces the basic ideas of semiology with a discussion of Ferdinand de Saussure's foundational work and some of Roland Barthes's later refinements to the approach. A range of post-structuralist thinkers are discussed including Roland Barthes (again), Michel Foucault and Jacques Derrida. I try and provide a feel for what is common and what is unique with respect to their work as it relates to fact construction, using the example of intertexuality and war to explore some of their insights. The section on postmodernism concentrates on Jean-François Lyotard's diagnosis of the postmodern condition and Donna Haraway's political and feminist exploration of the nature of factuality and stories given that condition. Some of the issues raised are addressed through a discussion of David Byrne's movie *True Stories*.

Chapter 4 provides a transition from the reviewing and systematizing in the first three chapters to the focus on specific procedures that makes up the chapters that follow. It outlines a set of considerations that need to be taken into account, and distinctions that need to be made, in research on fact construction. Some of these derive from the earlier traditions and some are new. One of its roles is to describe different ways in which the metaphor of construction

has been used in linguistics, ethnomethodology and post-structuralism. It suggests that a complete constructionist account of fact construction will need to consider both the procedures through which versions are stabilized and made credible and the resources that those procedures draw on. The chapter develops an argument for taking an analytic approach to fact construction which focuses on texts and talk in action (discourse) rather than mental models, representations and ideas (cognition), and for treating that discourse as having two rhetorical orientations: an offensive orientation concerned with undermining alternative descriptions and a defensive orientation concerned with resisting discounting. Chapter 4 ends by introducing a distinction between the action orientation of descriptions (what the description is doing) and the epistemological orientation of descriptions (how the description attends to its own factuality). It is also intended to serve as a compact introduction to the themes that will be explored in the next three chapters.

Chapters 5 and 6 concentrate on the various procedures that are involved in constructing (and undermining) factual accounts. In Chapter 5 the topics of interest management and category entitlement are discussed. The referencing of a speaker's interest in their description is one major procedure for discounting it. The discussion focuses on a range of different ways in which writers and speakers resist such discounting. Categories of persons are often closely connected to their epistemological rights (doctors know about medicine, people with good memories can be trusted to give accurate accounts, and so on), and building a category entitlement for the producer of a description can be an important way of building up its factuality. This chapter also discusses the notion of footing – for example, is the speaker claiming or merely reporting? This has an important role in fact construction. The discussion here concentrates on the way neutrality with respect to a claim may be built up or undermined by various techniques of quoting.

In Chapter 6 the general concern is with the procedures that people use to separate descriptions from their own interests and produce them as neutral and external; that is, to give them a quality of out-there-ness. There are a range of these procedures, or externalizing devices. I focus on the use of empiricist discourse (impersonal constructions characteristic of science and some news reporting), constructions of consensus and corroboration (independent observers agree in their descriptions), and the use of narrative involving either rich detail or general formulations (rich detail can be used to sustain the category entitlement 'witness', general formulations can be used to resist easy rebuttal). In each case the emphasis will be on the way they can be worked up or undermined; these devices are not 'plug and play' modules that work irrespective of context.

While Chapters 5 and 6 concentrate on the epistemological orientation of descriptions, Chapter 7 is focused on their action orientation. As this is such a huge topic I restrict coverage to three themes. The chapter discusses the connected issues of categorization and ontological gerrymandering. A lot of the business of a description is done through its categorization; different categories imply different stories of motive and responsibility and have different

implications for what should come next. At the same time categorizations can work to exclude potentially relevant considerations; they can gerrymander what is taken into account in a way that contributes to business at hand. A second theme is extrematization and minimization. This involves building some description to present bigness or smallness, violence or passivity, goodness or badness and so on. The third and final theme is normalization. How can some event or conduct be made out as normal and commonplace, or how can it be undermined as strange or deviant?

The final chapter returns to the nature of constructionism and asks how it should be conceptualized in the light of the arguments in the book. It also considers the significance of these arguments for the conduct and presentation of social science, taking work on public opinion and on social representations as two contrasting examples where a concern with the business done by descriptions can have important consequences. Finally, the chapter explores the broader implications of these arguments for politics and practice, highlighting tensions between different kinds of criticism and the reflexive exploration of social science texts.

The book is organized in two groups of chapters (1–3, 5–7) and two individual chapters. Chapters 1, 2 and 3 focus on the main theoretical and analytic traditions, and each could be read separately as a review which emphasizes the way descriptions are established as factual. Chapter 4 is a linking chapter which provides brief illustrations of the themes that are developed in detail in the following three chapters. This could be read as an introduction to what comes later and stands as a relatively compact summary of the perspective on fact construction developed in the book. Chapters 5 and 6 concentrate on procedures for fact construction, while Chapter 7 focuses on the way descriptions are fitted to activities.

These three chapters contain much of what is novel in the book, and they can be read as a relatively self-contained whole without losing too much. Although they draw on a range of different arenas where factual descriptions are used, for simplicity they will return repeatedly to a small number of examples: the relationship counselling sessions of a couple called Connie and Jimmy, the talk of various people involved in making a film about the failure of cancer research, and a range of different kinds of television and newspaper reporting. These will be combined with repeated discussion of two path-breaking studies – Dorothy Smith's (1990) study of the workings of an account describing someone's mental illness, and Robin Wooffitt's (1992) study of the construction of accounts of paranormal experiences. Chapter 8 ends the book by raising the broader questions of constructionism, social science representation and criticism.

4

Discourse and Construction

Although the traditions of work discussed in the last three chapters contain a wealth of material relevant to a systematic study of factual accounts, they each have their limitations and blind spots, as well as specific arenas of development and application – namely, science, everyday talk and literary texts. This chapter can be thought of as a crossroads in the book. It will start to lay out a scheme for understanding the operation of factual accounts, synthesize some of the features of the perspectives that were reviewed in the preceding chapters, and draw on a range of specific research studies which will be described in detail in later chapters. The chapter is intended to serve as both a frame and organizing introduction to the next three chapters. It will also introduce a range of more specific questions. Why do people use descriptions or factual accounts? What sorts of activities can they be used to achieve? How are accounts made to seem solid, factual and independent of the speaker? What procedures are used to undermine factual accounts? However, before we tackle these questions there are some fundamental theoretical and analytic precursors that must be addressed.

Some Stories of Construction

The Mirror and the Construction Yard

One way of conceiving the arguments of this book is as organized around the clash between two metaphors: the mirror and the construction yard. With the mirror metaphor there are a set of things in the world which are reflected onto a smooth surface, but in this case the surface is not glass but language. Language reflects how things are in its descriptions, representations and accounts. And as these are circulated in the world of human affairs they may be treated as accounts which are reliable, factual or literal, or, alternatively, the mirror may blur or distort in the case of confusions or lies. This metaphor is familiar in stories about science and a whole range of more 'mundane' human practices. It is a metaphor which makes descriptions passive: they merely mirror the world. Yet like a mirror image or a photograph, they can also stand in for that world and be as good as the world for many purposes.

The metaphor of construction works on two levels when applied to descriptions. The first is the idea that descriptions and accounts *construct the world*, or at least versions of the world. The second is the idea that these descriptions and accounts are *themselves* constructed. Construction here

suggests the possibility of assembly, manufacture, the prospect of different structures as an end point, and the likelihood that different materials will be used in the fabrication. It emphasizes that descriptions are human practices, and that descriptions could have been otherwise. There is nothing much that can be done about the reflection in a mirror; you can clean the mirror, make sure it is flat and smooth, but that relates only to its ability to receive an image passively. Yet a house is built by people, and it could have three chimneys and lots of windows, or it might have no chimneys and a set of French doors. It might be built with concrete, mud bricks, or girders and glass, and it might be very strong or rather delicate.

How strong is construction in this metaphor? The strongest version of the metaphor would have the world literally springing into existence as it is talked or written about. Ridiculous, surely! Perhaps, but I want to opt for something nearly as strong. Reality enters into human practices by way of the categories and descriptions that are part of those practices. The world is not ready categorized by God or nature in ways that we are all forced to accept. It is *constituted* in one way or another as people talk it, write it and argue it.

Now there is no sense in trying to decide whether one of these metaphors is true and the other false. It is most unclear how such a judgement could be made – although that has not stopped considerable philosophical energy having been expended on the problem for a long time. The difficulty is in formulating the question. To judge whether a description was mirroring or constructing reality requires the description to be compared to the reality. Yet reality (or 'reality') cannot enter this debate except as another description, which would beg the question of whether this new description is *itself* descriptive or constructive.

I have chosen the construction metaphor on *pragmatic* grounds. It is the more productive of the two because it allows a set of questions to be asked that do not make sense if we accept the mirror metaphor. If we treat descriptions as constructions and constructive, we can ask how they are put together, what materials are used, what sorts of things or events are produced by them, and so on. I do not see the main issue here to be philosophical discussions of ontology; that is, discussions of what sorts of things exist and what their status is. Instead, these arguments about metaphors are intended to clear the way for a focus on practical and analytic issues. Indeed, the abstract formulation of this problem can be positively misleading because it focuses on the relation between a description and 'reality' in the abstract, rather than considering the sorts of practices in which descriptive discourse operates.

Another way of thinking about this problem of construction and reality is to apply the requirement of methodological relativism discussed in Chapter 1. Methodological relativism is the claim that scientists' assertions or judgements about what should be treated as true or not should not be the start point for social analysis. It allows researchers to avoid the kind of tangle that results in the social researcher needing to know more about science than the scientists themselves. Their science needed to be better so they could properly assess what was true and what not as a prelude to social analysis. Not surprisingly, social analysts are not better at physics, say, than fully trained physicists. The attempt to do social analysis of science without adopting methodological relativism often resulted in what Michael Mulkay (1981) dubbed 'vassalage'; the situation where the sociological conclusions became parasitic upon the claims of a dominant group of participants. The sociologist becomes the vassal or servant of this group.

These sorts of tangles that result in vassalage are not restricted to work on scientific facts, although they are vividly apparent with that topic. In any area where the factual versions of some group are taken as a start point for analysis the analyst may end up as a vassal. Take, for example, Paul Willis' (1977) classic study of the transition from school to work of a group of adolescent boys. Willis built his story partly through selectively privileging certain accounts of a group of pupils he calls 'the lads'. These participants took on a role like that of central characters in a realist novel; they are rich and rounded, capable of irony and self-critique, and ultimately not speaking just for themselves but on behalf of a social class. In contrast, the girl pupils enter the text only as objects of 'the lads'' discourse, they have no independent voice. Neither do the pupils who will go on to take exams and are more accepting of the school culture; Willis adopts in his text 'the lads'' sneering description of them as 'earoles' (see Atkinson, 1990; Marcus, 1986; Potter et al., 1984). The point, then, is that Willis' sociological text becomes a vassal to the perspective of a particular social group, taking their evaluative descriptive constructions and treating them as a factual version of their social world. This in itself is not the problem – it could be defended on the grounds that it gives voice to a subordinate group (see Sampson, 1993b). The problem is the realist treatment of their categories as a neutral and objective picture of this set of social relations, disengaged from any local interactional business.

Having established something of the general value of embracing a construction metaphor, we need to go beyond that to be more specific. What sort of building are we talking about here: houses or bridges? And what types of manufacture? It is helpful to distinguish five different lines of work which can be described as constructionist. The Introduction briefly discussed Berger and Luckmann's *Social Construction of Reality* (1966), and Chapter 1 gave a rather more elaborate discussion of constructionism in sociology of scientific knowledge (SSK) (for example, Latour and Woolgar, 1986; Knorr Cetina, 1995b). In this chapter I will discuss constructionist work in linguistics, as well as making more explicit the constructionist themes in ethnomethodology and post-structuralism. The object is not to produce watertight lines of demarcation but to indicate areas of overlap and tension between the different forms of constructionism.

Linguistic Construction

The best-known linguistic constructionist is undoubtedly Benjamin Whorf (1956), who contributed, with the linguist Edward Sapir, to what has come to be known as the 'Sapir–Whorf hypothesis'. In psychology a large amount of

research has attempted to test the hypothesis that people's *perception* of the world is determined by the language they use. For example, Eskimo tribes (as they were then called) were claimed to be able to make very fine distinctions between different kinds of snow because of the wide range of different words they had available. They had separate terms for snow that has just fallen, for wet snow, for snow that has frozen hard and so on. Whorf worked for a company which assessed insurance risk, and he used his job to illustrate the hypothesis. He gave the example of employees of a firm who had described gasoline drums as 'empty' and therefore safe; yet the drums were actually full of highly inflammable vapour which had exploded and started a fire. If only they had described the drums as 'full' (of dangerous vapour) they would have seen how dangerous they were and taken more care when dealing them. In this constructionism, language constructs people's perception of the world.

According to Derek Edwards (1994b 1996) the problem with this idea is that it treats language as a system of classification lying between the static individual perceiver and the world. What it does not do is treat language as part of a set of social practices. For example, with the gasoline drums we can ask what the employees who talked to Whorf were *doing* with their descriptions? If we do so, another possibility becomes available. Perhaps the description 'they were empty' was not a simple report using language that influences perception, but an *account* offered in a situation where issues of blame ('who was responsible for the fire?') and its practical consequences ('should the insurers pay up?') are pressing? That is, what Whorf is not doing is examining the *reflexive* quality of descriptions which emphasizes their role in both describing the world and contributing to current activities.

Later linguistic work in this tradition is in some ways more sophisticated, as well as being more integrated with developments across the social sciences. Nevertheless, this kind of limitation is still apparent. For instance, we can see this same assumption at work in Roger Fowler's interesting study of language in news reporting:

> Language and other codes . . . have a cognitive role: they provide an organized mental representation for our experience. Whatever the 'natural' structure of the world . . . we handle it mentally, and in discourse, in terms of the conventional meaning-categories embodied in our society's codes. (1991: 3)

Again, the story is of an inchoate and unformed world which is crystallized out into entities and processes somewhere in the perceiver's mind by a suitable set of linguistic spectacles.

One of the most ambitious recent attempts to spell out the process of linguistic construction comes from the linguist George Grace (1987). He suggested that the linguistic construction of reality involved three distinct stages. The first stage involves the 'specification' of a 'conceptual event'. Each language includes sets of terms, tenses, grammatical forms and so on, which allows for a range of possible events to be specified. For example, modern English makes it very easy to distinguish something that happened yesterday from something that happened last week or last year, in a way that the language of the Hopi Indians does not (Whorf, 1956). The second stage of the constructive process involves this 'conceptual event' being set into an ongoing stream of talk – Grace is not very specific about how this should happen. The third stage involves what Grace calls 'modalization'; that is, the event is framed as something that is being asserted, questioned, denied or whatever. Overall, then, the account of construction is this: the lexical and grammatical resources of English allow an object to be specified as a 'conceptual event', such as 'open door'. This 'conceptual event' can then be fitted into a conversation about the door, where it might be modalized as a question ('is that door open?'), say, or a request ('please shut the door').

The virtue of this model is that it is an attempt to characterize explicitly what might be involved in processes of construction. It also highlights the way different languages may provide different resources for performing actions. Nevertheless, it shares the flaws of other linguistic constructionisms. In particular, it is not attentive to practices of actual language use; rather, it treats language as a whole system and asks how it constructs a world. For example, it starts with the specification of events and treats what is done with those events once they are specified as secondary. Yet in practice this process may work in the reverse direction. Consider the interrogation of a murder suspect. The suspect may give a variety of descriptions of their victim, but it would be misleading to suppose that the nature of the victim is specified first and then fitted into some utterance that performs an activity. It seems much more plausible that the nature of the activity drives the nature of the description. For example, the victim may be described precisely in a way that mitigates the act of killing (Watson, 1978; Wowk, 1984). Generally, it may be simpler to say that talk involves categorization of persons, objects and processes, it tends to occur in interaction sequences and it is used to perform actions. Separating these things out as distinct and sequential stages causes more confusion than clarity.

Construction in Post-structuralism and Conversation Analysis

In the previous two chapters I spent some time discussing the various ways in which post-structuralist and conversation analytic work treats the construction of facts or the establishment of descriptions as realistic. At this point I will concentrate on making clear their basic assumptions about processes of fact construction.

In semiology, the central argument is that descriptions require a whole system of distinctions to work. This shows the word-object picture of description to be too simple. However, there is little in semiology which addresses the question of *how* a description is made to seem more or less factual. Post-structuralists have been more concerned with fact construction in the guise of the nature of realist forms of representation, particularly in literature. Both Barthes and Foucault focus on the way discourses or interpretative codes produce objects or descriptions which seem solid and unproblematic. Yet they have paid little attention to how such codes have their effect, although

they emphasize both their *familiarity* and their *authority*: that is, the codes have a taken-for-granted quality that makes their products seem natural or commonsensical and they are often associated with powerful and influential institutions such as medicine and educational psychology. Even Barthes's *S/Z*, for all the obsessional detail in its analysis of Balzac's text, describes the codes at work without saying why the use of any particular code will make the text seem more real. Derrida was certainly concerned with what might be called the textual mechanics through which arguments are made to seem obvious and effective. However, for the most part his arguments were directed at the truth or validity of philosophical arguments rather than at realism or factuality as such and, apart from a highly suggestive emphasis on the central role of tropes and metaphors, his approach is not easily applied to the construction of factual versions.

In contrast to the linguistic and post-structural stories of construction, conversation analysts treat reality construction as something that has to be *achieved* using some devices or techniques. That is, from a conversation analytic perspective the use of a particular descriptive term, or even a familiar discourse, may not be enough to construct a version of events which will be treated as real or factual. Rather, realism and factuality are worked up using a set of rhetorical devices and techniques which may be specific to particular settings. Moreover, these techniques do not guarantee that a version of an action or an event will be treated as factual. They can be deployed effectively or badly, and they can be undermined vigorously or accepted credulously.

Conversation analysis provides the final story of how fact construction gets done. It is particularly attractive because it opens up an area of research not strongly emphasized by the other approaches. The story of linguistic construction left little to explain; whenever words are uttered construction gets done. There is some value in this, as it is certainly the case that using descriptive language produces versions of the world. Yet it does not engage with the question of why some versions 'work' and some do not; that is, the question of why a version is treated as a factual depiction of how things are in some interaction, or why it is repudiated as biased, confused or self-serving. The post-structuralist story raises the important question of how particular interpretative codes or discourses came into being, and genealogical research in the Foucauldian tradition has attempted to provide answers to questions of this kind. Nevertheless, it too fails to open up the field of research on fact construction because it makes only general claims about familiarity and forms of understanding that have become habitual. It works less well when applied to the specifics of descriptions and to non-textual materials where the rigours of conversation analysis come into their own.

Let me assemble these different kinds of construction into an overall model. We can imagine the words and syntactical possibilities as the bricks and girders that are needed for any building. Post-structuralist discourses and codes can be thought of a prefabricated wall and ceiling sections that can be used as parts of very different buildings. The devices and procedures that are grist to the mill of conversation analysis make up the bolts and cement that hold the whole structure together. Nothing works without the stuff revealed by conversation analysis, but a study of fact construction will be limited without a close examination of bricks and prefabricated parts.

At the same time as elaborating this metaphor, let me briefly emphasize some problems. Its main shortcoming is that it treats the parts as solid prior to the building. What we actually need to imagine is that the bricks are soft and vague in outline, so that they only snap into shape as they are cemented into place. And the prefabricated sections must themselves be somewhat inchoate, with their solidity emerging as they are bolted together. Everything exists in a fuzzy and fluid state until crystallized in particular texts or particular interactions.

Discourse, Mental Furniture and Rhetoric

So far I have discussed a number of general features which characterize a constructionist approach to facts, starting from the traditions discussed in the earlier chapters. Before moving on it is necessary briefly to address three themes that have important implications for the understanding of facts and descriptions. The themes are anti-cognitivism, discourse and rhetoric; and they turn out to be closely bound together.

Anti-cognitivism

I have already discussed problems with cognitivist accounts of the operation of facts and descriptions in the context of de Saussure's semiology and other kinds of linguistic constructionism which see that what is constructed is inner pictures or representations of some kind. It is necessary to go into a bit more detail with the problems with cognitivist accounts to show why they are rejected here. There are now a range of general lines of criticism of cognitivism, mostly stimulated by Wittgenstein's later philosophy or by ethnomethodology (Costall and Still, 1991; Coulter, 1991; Edwards, 1996). Three problems are particularly pertinent here.

The first problem is with the notion of representations as 'inner' mental entities. Should they construed as concepts, or pictures, or what? The very coherence of the idea of inner representation is problematic (McKinlay and Potter, 1987). Moreover, inner representations are inferred from various representational practices involving talk and writing, and such inferences tend to circularity with the inner representations being used, in turn, to explain those representational practices. The straightforwardness of the notion of mental representations dissolves when it is examined closely, particularly in the context of actual interaction involving representations and descriptions.

A second problem with taking a cognitive focus is that representations become separated from the practices in which they are used and start to be conceptualized as static entities which individuals carry around with them. Put another way, the cognitive focus draws attention away from what is being *done* with representations and descriptions in the settings in which they are

produced. It prevents their reflexive and indexical properties being explored analytically. In terms of the metaphor of construction, then, the concern will be with descriptions and representations as they are built in the course of interaction; it will not be addressed to notional, in-the-head entities such as perceptions or representations, along with the cognitive apparatus of scripts, schemas and so on that go with such explanations.

Third is the problem that cognition is often the *topic* of description. In everyday life people spend a lot of time talking about their 'inner life': their thoughts, feelings, attitudes, goals and so on. When dealing with natural discourse, it is very hard to distinguish this kind of talk from discussions about whether the National Health Service is being run down or whether there is an invitation due for Saturday's party. Take the following extract from a relationship counselling session (C is the Counsellor, W is the wife; she mentions Jimmy who is her husband).

1 C: So you- you <u>seem</u> to be saying you're recognizing some kind of <u>pat</u>tern (0.6)
W: But the pattern <u>I</u> recognize is not (0.8) the pattern (.) that Jimmy recognizes about the situation. Y'know? (.) I just (.) <u>feel</u> that (2.2) I feel (.) that (.) he didn't (0.4) he says he didn't leave <u>me:</u> (.) for another woman (.) but I f- (.) believe (.) that if she hadn't've been <u>there</u> (.) this wouldn't have happened [*Continues*]

(DE-JF/C2/S2:2)

The wife's talk here moves fluidly between descriptions and avowals of her own mental life ('the pattern I recognize', 'I feel', 'I believe'), that of her husband ('the pattern that Jimmy recognizes'), and descriptions of actions and events ('he says . . .'). Now it is certainly the case that these different elements may be treated differently by participants: the procedure someone uses to undermine another's claim about his own feelings, for example, may be different from the procedure for undermining a report of what happened in a past event. However, from an analytic point of view, to start with the assumption that cognitive descriptions have a different *status* will lead to all sorts of tensions and confusions.

The approach to fact construction developed will, therefore, be just as interested in the construction of descriptions of the world of cognition as with descriptions of the world of actions and events. Indeed, as we will see later, there are often complex patterns of inference between these realms in ordinary talk (Edwards, 1996; Edwards and Potter, 1992; Potter et al., 1993). This can work in both directions. On the one hand, people can construct a description of how the world is that will warrant some cognitive state or event; a description of an insult can be used to warrant and make believable feelings of anger. On the other, descriptions of mental life may be used to warrant the existence of events in the world; a claim about having seen a flying saucer may be buttressed by noting a long-standing scepticism about such things on the part of the observer.

Discourse

The arguments against attempting to treat the business of fact construction as the business of building mental versions of the world, when turned around, become arguments for focusing on discourse. Indeed, we have already seen a discourse focus to be central in ethnomethodology, conversation analysis and post-structuralism; although there are important differences in the way discourse is understood in these different fields. I am taking a focus on discourse to mean that the concern is with *talk and texts as parts of social practices*. This is somewhat broader than the conversation analytic concern with talk-in-interaction, but rather more focused on the specifics of people's practices than the Foucauldian notion of a discourse as a set of statements that formulate objects and subjects. This sense of discourse will be exemplified in the next three chapters where the focus will be on actual materials – transcripts of conversations in different settings, newspaper articles, formal texts of various kinds – and on what is done in and through these materials.

It is important to emphasize that I am not arguing that a focus on discourse in the specific sense developed here is a prerequisite for producing sophisticated research on fact construction. There is plenty of high-quality research in this area that uses other approaches, some of which was discussed in earlier chapters. For example, sociologists of scientific knowledge have conducted revealing ethnographic studies of laboratory work (for example, Knorr Cetina, 1995a; Traweek, 1988); I have already quoted Karin Knorr Cetina's claim that 'ethnography furnished the optics for viewing the process of knowledge production as "constructive" rather than descriptive' (1995b: 141). Nevertheless, there are reasons for a discourse focus being particularly apposite for studies of fact construction.

Take the difference between what an ethnographic observer and a discourse researcher might make of the interaction reproduced in Extract 1 above. In ethnography the researcher is typically using their own participation, either actual or vicarious, as a basis for building understanding, and this will be supplemented with field notes (Hammersley and Atkinson, 1983). The goal is typically to generate an account of the actions and events that happen in a setting. So the ethnographic observer might make a number of observations about the woman's feelings, those of her partner, past events that have happened and so on. In contrast to this, the discourse focus proposed here will be concerned with the way in which the woman's account is established as literal and objective, and what it is being used to do. This will involve attending to what is often thought of as the (mere) detail of interaction: the hesitations, repetitions, repairs and emphases. Conversation analysts have shown just how important these things are to interaction – and they are virtually impossible for an ethnographic observer without a tape recorder and high-quality transcript to capture adequately.

There is a final virtue of taking a discourse focus. If we have a transcribed record of discourse, rather than a set of formulations in note form, it places the reader of the research in a much stronger position to evaluate the claims

and interpretations. Harvey Sacks' goal of producing a form of analysis 'where the reader has as much information as the author, and can reproduce the analysis' (1992: I, 27) may be impossible to realize in practice. Nevertheless, there is an important sense in which this approach democratizes academic interaction. For example, the reader does not have to take on trust the sensitivity or acuity of the ethnographer. In the end, however, it is the relative success of these different approaches that is important. Are analyses with a discourse focus productive and convincing? Perhaps the answer to that question will be a bit clearer by the end of the book.

Rhetoric

So far I have stressed the value of focusing on fact construction in public discourse rather than mental pictures or subjective feelings of certainty. I wish to combine this focus on discourse with a stress on rhetoric. Recent work on rhetoric by Michael Billig (1987) has maintained that rhetoric should not be confined to obviously argumentative or explicitly persuasive communication. Rather, rhetoric should be seen as a pervasive feature of the way people interact and arrive at understanding. For example, he suggests that the social psychological notion of attitude needs to be rethought in rhetorical terms. Attitudes have traditionally been treated as individuals' isolated cognitive evaluations of parts of the world. Billig argues that they should be seen as public positions that are inseparable from current controversy; indeed, there is no role for attitudes except in issues where there is conflict and dispute. The implication of this is that 'every attitude in favour of a position is also, implicitly but more often explicitly, also a stance against the counter position. Because attitudes are stances on matters of controversy, we can expect attitude holders to justify their position and to criticise the counter position' (Billig, 1991: 143). This same argument can be applied to factual accounts. Indeed, Herbert Simons has argued that that 'part of the job of the rhetorical analyst is to determine how constructions of "the real" are made persuasive' (1990: 11). The consequence of emphasizing rhetoric here will be that, when descriptions are analysed, part of the interest will be in what alternative claims or arguments are being undermined. Put at its simplest, one of the features of any description is that it counters – actually or potentially – a range of competing alternative descriptions (see also Dillon, 1991).

Lyotard suggested that a characteristic of the postmodern condition is its emphasis on local rhetorical wars:

> In the ordinary use of discourse – for example, in a discussion between two friends – the interlocutors use any available ammunition, changing [language] games from one utterance to the next: questions, requests, assertions, and narratives are launched pell-mell into battle. The war is not without rules, but the rules allow and encourage the greatest possibility of flexibility of utterance. (1984: 17)

Without wanting to accept the postmodern assumption that there is something historically new about such fragmentation and dispute, the metaphor of war is useful. In a war it is possible to consider offensive as well as defensive weaponry. Many weapons serve both purposes, of course. Following this through for factual accounts, we can consider how a factual account can be inspected for its *offensive* and its *defensive* rhetoric.

On the one hand, a description will work as *offensive rhetoric* in so far as it undermines alternative descriptions. It may be constructed precisely to rework, damage or reframe an alternative description. On the other, a description may provide *defensive rhetoric* depending on its capacity to resist discounting or undermining. A whole range of techniques may be used to protect descriptions in this way, and they will be an important topic of later chapters of the book. The point, then, is that this rhetorical emphasis can serve as a counter to the more familiar approach to descriptions as primarily about the relationship between a particular set of words and a particular part of reality. Instead, it emphasizes the relation between a description and alternative descriptions, and the way such relationships may be worked up in argument.

The distinction between offensive and defensive rhetoric also emphasizes the value of taking a double analytic focus. Studies should look both at the procedures through which factual versions are built up, and the ones by which they are undermined. As we will see, these things are closely related. There is some terminology that will be useful here. I will refer to discourse which is constructing versions of the world as solid and factual as *reifying* discourse. *Reifying* means to turn something abstract into a material thing; and this is the sense I wish to emphasize, although material should be understood very widely. These are accounts which are producing something as an object, be it an event, a thought or a set of circumstances. In contrast, we will refer to discourse which is undermining versions as *ironizing*. The standard meaning of irony is to use words in the opposite way to their literal meaning. However, irony has come to have a more specific sense in SSK as an approach to discourse which treats it not as literal but as a product of interests or strategy (Woolgar, 1983). Falling somewhere in between these senses, I will treat ironizing discourse as talk or writing which undermines the literal descriptiveness of versions. It is the opposite of reifying discourse: it turns the material thing back into talk which is motivated, distorted or erroneous in some way.

Let us illustrate this by returning to Extract 1:

> Ia C: So you- you seem to be saying you're recognizing some kind of pattern (0.6)
> H: But the pattern I recognize is not (0.8) the pattern (.) that Jimmy recognizes about the situation. Y'know? (.) I just (.) feel that (2.2) I feel (.) that (.) he didn't (0.4) he says he didn't leave me: (.) for another woman (.) but I f- (.) believe (.) that if she hadn't've been there (.) this wouldn't have happened [*Continues*]
> (DE-JF/C2/S2:2)

The wife's talk is organized to reify a particular object: 'the pattern I recognize'. That is, it presents this as something that is actually the case. At the same time it ironizes another object: 'the pattern that Jimmy recognizes'. It presents this as a version claimed ('he says'), and even perhaps believed

('recognized'), by Jimmy – but nevertheless implausible because of the pattern of the events (which is itself a version that is reified in this talk).

There is a final point to note about rhetoric. Often rhetoric is treated as virtually synonymous with persuasion (Cockcroft and Cockcroft, 1992). However, this can easily turn the study of rhetoric into an exercise in cognitive psychology. It will treat the answer to the question of whether rhetoric is effective as dependent on an assessment of whether there has been a change in mental state in the audience. The way rhetoric is used here will not depend on psychological judgements of this kind. Instead, rhetoric will be treated as a feature of the antagonistic relationship between versions: how a description counters an alternative description, and how it is organized, in turn, to resist being countered. This conception of rhetoric meshes much better with the general approach to discourse introduced above. It is close to the traditional notion of 'suasive' rhetoric, which is discourse designed to elicit expressions of agreement from an audience.

The remainder of the chapter will introduce a scheme for understanding descriptive and factual discourse which will be elaborated on in the next three chapters. The basic argument will be that factual accounts have a double orientation. They have an *action orientation* and an *epistemological orientation*. On the one hand, a description will be orientated to action. That is, it will be used to accomplish an action, and it can be analysed to see how it is constructed so as to accomplish that action. On the other, a description will build its own status as a factual version. For the most part, the concern is to produce descriptions which will be treated as *mere* descriptions, reports which *tell it how it is*.

It is important to emphasize that the perspective developed here treats the epistemological orientation of accounts as *itself* a form of action; it is something built by speakers or writers – although it does not assume that this building is necessarily, even often, conscious or strategic. This quality is a *constructed* element to descriptions rather than something they either possess or not. The study of the epistemological orientation of accounts is the study of this building process.

The Action Orientation of Description

Why Descriptions Are Used

The idea that people can and do use descriptions to perform actions, or as part of actions, is not novel, and it can be easily illustrated. Take the following extract, in which some students are discussing a noise outside their flat.

2 *Becky*: oi (.) sh shh (.) it could have been <u>that</u>
Neil: NO[that's not making a noise
Alan: [no (.) something outside (0.4) it was definitely outside
→ *Diane*: Neil you've got shoes on
(DSS-K:94:1)

At the end of the extract Diane addresses Neil with the utterance 'Neil you've got shoes on'. Now as competent conversationalists, and people familiar with cultures where shoes may be taken off indoors but are required out of doors, we do not have any trouble hearing the arrowed utterance as a request for Neil to investigate the noise. Crucially, the participants clearly treat it in this way; for the extract is followed by a slightly jokey conversation about the danger of meeting a burglar and the risk that they might be carrying a weapon.

My concern is not so much with the specifics of this example. It has two features that are interesting because they are characteristic of the use of descriptions in performing actions. The first is that there is no *explicit* formulation of the request. Diane does not say 'please investigate that noise, Neil' or 'can you see what is going on?' Instead, a description is offered ('Neil you've got shoes on') from which a request may be *inferred*. In this context, the description of Neil's shoes identifies him as someone who can most easily investigate.

The second feature is connected to the first. The action being done by the description is a somewhat *sensitive* one. Diane is requesting Neil to do something that involves effort and perhaps even risk. The sensitivity here is not just about the pressure it puts on Neil. There is also an issue about the identity that is displayed by Diane, the person doing the requesting. By asking that Neil investigate the noise, Diane opens herself to being seen as 'lazy' or even 'cowardly'. That is, the focus on shoes displaces attention from these troubling interpretations by focusing on the topic of who has shoes on, and therefore *can* go out, rather than who can be bothered to or who is not scared (see also Pomerantz, 1980).

It is no coincidence that this sensitive action is done indirectly. Quite the reverse. One of the principal reasons for doing actions indirectly by way of descriptions is that the actions are sensitive or difficult in some way. Commonly, they will involve a potentially undesirable or problematic identity; that is, they may be actions which display the speaker as selfish, cowardly, insensitive, racist, stupid, flirty, pushy or one of a whole range of possibilities which are negative in the relevant context. This may seem like something of a paradox at first, for descriptions are commonly associated with coldness, objectivity and neutrality. However, it is not a paradox if we consider that it is precisely this feature that makes factual versions so suitable when there is a conflict or sensitive issue. For example, Bruno Latour noted that when dispute between different groups of scientists gets vigorous, description gets more and more technical (1987); and Anita Pomerantz notes that in everyday settings it is precisely when there is some dispute that people start to provide detailed warrants for their claims (1984b). Or, to give a final illustration, take Extract 1 again.

1b C: So you- you <u>seem</u> to be saying you're recognizing some kind of <u>pattern</u> (0.6)
W: But the pattern <u>I</u> recognize is not (0.8) the pattern (.) that Jimmy recognizes about the situation. Y'know? (.) I just (.) <u>feel</u> that (2.2) I feel (.) that (.) he didn't (0.4) he says he didn't leave <u>me:</u> (.)

for another woman (.) but I f- (.) believe (.) that if she hadn't 've been there (.) this wouldn't have happened [*Continues*]
(DE-JF/C2/S2:2)

Note the way the wife accompanies her contradiction of her husband's version – which treats his affair as not relevant to their marital difficulties – with a description which links the affair and the difficulties together.

The Dilemma of Stake

One way of understanding these features to the production of descriptions is to see them as a way of managing what Derek Edwards and I (1992) called the *dilemma of stake*. The dilemma is that anything that a person (or group) says or does may be discounted as a product of stake or interest. The referencing of such a stake is one principal way of discounting the significance of an action, or reworking its nature. For example, a blaming can be discounted as merely a product of spite; an offer may be discounted as an attempt to influence. The Prime Minister's claim that tax cuts are needed to stimulate the economy can be discounted as an attempt to make people feel good just before an election. In the case of Extract 2 above, Diane's request that Neil should investigate the suspicious noise could be discounted as a product of her not wanting to do it herself. Diane resists this danger by making the request implicitly via a description.

It is important to emphasise what I am *not* claiming here. The argument is not that social researchers should interpret people's discourse in terms of their individual or group interests. There are all sorts of difficulties with such an analytic programme, not least of which is that it is very difficult to identify interests in a way that is separable from the sorts of occasioned interest attribution that participants use when in debate with one another (see, for example, Woolgar, 1981; Yearley, 1982; and the discussion of interest theory in Chapter 1). The argument here is that people *treat one another in this way*. They treat reports and descriptions *as if* they come from groups and individuals with interests, desires, ambitions and stake in some versions of what the world is like. Interests are a participant's concern, and that is how they can enter analysis.

Take this extract from an account of the deliberations of a jury:

3 It wasn't, in truth, much of a case. The only defence witness was a cousin of one of the defendants and she got her story muddled up anyway; and the prosecution witnesses, many of them passers-by with no conceivable axe to grind, were articulate and plausible. (*Independent on Sunday*, 15 May 1994)

The author's explanation of why the defence case was treated as unconvincing by the jury relies heavily on judgements about the stake of the different witnesses. By naming the defence witness as a *cousin* of the defendant, he provides information from which readers (and jurors) can infer a motive for her to lie on the defendant's behalf. In contrast, the prosecution witnesses are described as *passers-by*, and the implication of this is spelled out

by stating that they have 'no conceivable axe to grind'; that is, they have no prior relationship or stake in the fate of the defendant. As we will see, the management of stake is one of the central features in the production of factual discourse.

There are two final cautions to make. I have started with rather simple examples to make the argument as clear as possible, but such examples may not be characteristic of the sorts of cases we will consider later. The first problem is the nature of the agent who is assumed to have stake and interest. So far we have considered cases where stake is treated as a feature of individuals. Yet, stake attribution is by no means restricted to such cases; it is regularly attributed to social groups, nations, ethnic groups, on the one hand, and to parts of persons such as their unconscious, or ideal, self, on the other. Accounts can move fluidly between attributing stake at these different levels. Second, descriptions are bound up with the performance of actions in all sorts of complex ways. Sometimes a description is used *alone* to perform an action, as in Extract 2; at other times descriptions have a standardized role as *part* of an action, as is the case with the way accounts play a role in turning down requests and invitations (see pages 60–4).

How Descriptions Are Used

So far, then, I have argued that factual or descriptive discourse may be drawn on to manage issues of stake, in particular where the speaker or writer may be treated as having a negative or problematic identity. However, this only gives an account for *why* descriptive discourse may be used. This must be complemented by an account of *how* particular actions are performed by descriptions. In other words, how is a particular description constructed to perform a specific action? This question will be the focus of Chapter 7. For the moment I will just note some of the considerations which such an account will need to address.

Potentially, there are a huge number of ways in which the production of descriptions is involved with actions. Descriptions are closely bound up with the idiosyncratic particulars of settings. In Extract 2 there are references to Neil and shoes which are crucial to the workings of this account, but are likely to be irrelevant in almost every other piece of discourse it is possible to imagine. At first sight this might well make us sceptical about the very possibility of making general claims about the procedures through which factual accounts are used in actions. Nevertheless, when we start to study the describing it becomes possible to make some general observations.

A central feature of any description is its role in categorization; a description formulates some object or event as something; it constitutes it as a thing, and a thing with specific qualities. The description presents something as good or bad, big or small, more violent or less violent, although often with more subtle options. Another common role of descriptions is to present some action as routine or, conversely, exceptional. Sometimes, the success of a description in action will depend on its selective management of the realm of

objects and events that are to be considered. The point, then, is that although the details of what is talked about may be endlessly varied, the sorts of procedures for constructing and managing descriptions may be much more regular, and therefore tractable in analysis. We have already seen an example of this with the discussion of accounts in Chapter 2, where we noted the way that accounts for turning down invitations and offers have a highly regular overall structure.

The Epistemological Orientation of Description

Referential talk on its own carries no guarantee that it will be treated as factual; producing a text with descriptions in it does not, on its own, constrain the reader to treat those descriptions as literal. This is particularly true in situations of conflict, or where there are delicate issues of identity, where descriptive discourse is common. People have a wide range of resources that they can use for ironizing descriptions, including notions of lies, delusions, mistakes, flattery, deceptions and misrepresentations, all of which can be drawn on to undermine the adequacy of a description. Given that there are these resources for undermining factual versions, it is not surprising that there are a developed set of counter-resources that are used to work up the facticity of a version and make it difficult to undermine. These are the resources that are used to construct a description as a factual account.

Bruno Latour and Steve Woolgar conceptualize this process in terms of a hierarchy of modalization (Latour, 1987; Latour and Woolgar, 1986 Woolgar, 1988b). This is illustrated in Table 4.1. At one end of the hierarchy there are descriptions whose status is considered highly suspect or provisional and may be treated as the lies or confusions of the speaker; at the other end, there are descriptions which are treated as solid and unproblematic, and quite separate from the speaker. At this end, some statements may be treated as so unproblematic that they do not even need to be explicitly formulated; they can be presupposed.

Table 4.1 *A hierarchy of modalization*

[...]
X
X is a fact
I know that X
I claim that X
I believe that X
I hypothesize that X
I think that X
I guess that X
X is possible

The process of fact construction is one of attempting to reify descriptions as solid and literal. The opposite process of destruction is one of attempting

to ironize descriptions as partial, interested, or defective in some other way. Often, of course, these things are combined as one version is established at the expense of another, as in Extract 1, for example. If we think of the hierarchy as a ratchet, processes of reification attempt to ratchet the description up through the hierarchy; processes of ironizing attempt to ratchet it back down.

The brief overview that follows is intended to illustrate the issues rather than deal with their complexities. It will divide processes of fact construction into two. On the one hand, there are resources that work on the identity of the describer; their descriptions may be undermined by reference to stake and they may be built up by reference to knowledge entitlements. This will be the topic of Chapter 5. On the other hand, there are a range of resources that contribute to the independence of the speaker from the description. These will be the topic of Chapter 6.

Interest Management

Interest management is one of the most fundamental approaches to fact construction. In Gaye Tuchman's well-known ethnographic study of newspaper reporting, she lists attention to interests as the first thing a reporter will note when assessing a source: 'Most individuals, as news sources, have an axe to grind. To be believed, an individual must prove his or her reliability as a news source.'(1978: 93). I have already noted some of the discounting through interest imputation in the discussion of the dilemma of stake. Indeed, one of the basic arguments here is that descriptions are often used *precisely* because they manage issues of interest. This point can be developed by returning to the two examples we used previously. By describing Neil's state of dress ('Neil you've got your shoes on'), Diane in Extract 2 provides a reason for him to investigate the suspicious noise which focuses attention on Neil – it does not involve Diane addressing any of her own possible reasons (fear, laziness) for Neil going. Put simply, a description like this can attempt to take the sensitive concern away from the speaker and make it a part of what is described.

The report of a jury deliberation in Extract 3 shows the writer reporting a pattern of stake to represent one side of a case as strong and the other weak. The defence witness is suspect because she is a relative of the accused; the prosecution witnesses' accounts are likely to be accurate because they have no stake in the outcome: they are mere passers-by. Just mentioning these particulars is treated as sufficient to cast doubt on the claims of the defendant. This again illustrates the power that interest invoking can have.

Extract 3 also illustrates a more general feature of fact construction. Claims about stake are *themselves* descriptions, and as such they are subject to the same concerns about fact construction. People may need to work up the factual nature of the elements in the accounts that are used, in turn, to work up the factual nature of what is being described. Put another way, the resources for reifying descriptions can work recursively. Just as much effort may be needed to construct the facticity of the resource as goes into using the resource to construct the facticity of what is at issue.

My overall argument is that stake is both a potential problem for those wishing to establish the facticity of accounts and a resource for those wishing to undermine it. I have deliberately left the specific nature of stake and interest vague. Stake is a participants' issue which may be constructed in many different ways. It may be treated as something to do with features of a specific individual, or as something to do with their broader group allegiances; it may be as 'trivial' as a concern not to look foolish, or as 'important' as a desire not to be identified as a murderer. The role of analysis is not to assess whether these are right or not, but to explore the practices through which stake is established and discounted. A range of these practices will be explored in the next chapter.

Category Entitlements

If interest management is generally involved with the problems of accounts being undermined, category entitlements are the other side of the coin. Knowledge is culturally and normatively linked to categories of actors in a variety of different ways. Certain categories of actors are treated as entitled to know particular sorts of things, and their reports and descriptions may thus be given special credence. At its simplest, a person visits the doctor because she is expected to know something about illness. She is in a category of people who are treated as *entitled* to have such knowledge; she knows about illness *by virtue of the fact that* she is a doctor. That is, we assume that her category membership is a product of training, knowledge and so on.

The role of category entitlements can be seen in materials from a study by Jack Whalen and Don Zimmerman (1990). They analysed calls to an emergency switchboard of a large North American city and noted the ways in which different kinds of caller were treated. Some sorts of caller were asked about how they knew about what they were calling in, and others were not. Take two examples. The call in Extract 4 comes from an 'ordinary caller', an unknown member of the public (CT stands for Call Taker, C for Caller):

4 *CT*: Mid-City emergency
C: Would you send thuh police to eleven six oh Arvin Avenue North?
CT: Eleven six oh Arving Avenue north?
C: Yes there's been raping goin' on
CT: WHERE
C: Eleven si[x oh
CT: [Inside ur outside?
C: Inside thee house.
CT: There's somebody being RAPED?
C: Yup=
CT: =How do you know this?
C: I live next door. Two ladies bein raped, eleven six oh=
CT: = Di- How do you know they're Being raped inside that house.
C: Because . . . [*Call continues for 15 more lines*]
(Whalen and Zimmerman, 1990: 473)

In this call we see the caller being questioned in some detail about their grounds for the claim that someone is being raped and the police should attend. Now contrast this to the brief and smoothly managed call in Extract 5:

5 *CT*: .hh Mid-City emergency
C: Hi .hh This iz General – there's been an over dose (.) twenty-six twenty-six .hh Columbia: hh upstairs apartment num:::ber two: .hh
CT: O:kay thank you
C: umhm bye
Whalen and Zimmerman, 1990: 483)

In this case the caller from the hospital is treated as someone entitled to know about drug over-doses and their location. They are not asked how they know.

Now it is tempting to think that these categories are merely a feature of the world, and that speakers and writers will be assessed according to their membership in a straightforward way. However, this is too simple. As we have noted, fact construction processes need not just work on the facts, they can also work on the resources that build up the facts. Thus participants can work up their category entitlements in a variety of ways. And conversely, of course, they can undermine the entitlements of others. We can speculate, for example, about the way the caller in Extract 5 builds up his or her category entitlement, not just by naming ('This iz General'), but also by the familiar greeting at the start and the air of routine ('General' rather than 'the General Hospital'). Interests and category entitlements, then, are pervasive participants' concerns as they work up, and undermine, factual accounts.

Interest management and the building of category entitlement both involve constructions of the person who is making the report. That is, they are not focused on the content of what is being reported, but on the status of the reporter. In effect, they are addressing two relevant questions that can be asked when descriptions are offered. Does the person making the report have an *interest* that discounts the report? Does the person have an *entitlement* that increases its plausibility? These are not the only sorts of constructions that can help ratchet descriptions up or down through the hierarchy of modalization. Other approaches can involve the way the topic of the report is described (empiricist discourse, detail and narrative) and also the relationship between different reports of the same event (consensus and corroboration).

Empiricist Discourse

In Chapter 1, I discussed a range of sociological approaches to science. However, discussion of the strand of this work that has focused on scientists' discourse was postponed until later because of its direct relevance to issues of fact construction. One of the main conclusions of work on scientific discourse is that scientists draw on different vocabularies or 'interpretative repertoires' when they are describing their work. Interpretative repertoires are

systematically related sets of terms, often used with stylistic and grammatical coherence, and often organized around one or more central metaphors. They are one of the major resources that scientists use when constructing versions of their worlds. Nigel Gilbert and Michael Mulkay (1984) call the repertoire that predominates when scientists are describing their own work, and work that they take to be true, the 'empiricist repertoire'. Discourse of this kind treats data as primary and provides only generalized, inexplicit formulations of the actions and beliefs of the scientist. When the scientist does appear he or she is depicted as forced to undertake actions by the demands of natural phenomena or the constraints of rules.

6 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 and to facilitate the release of bound ATP. (Gilbert and Mulkay, 1984: 41)

We will explore this discourse in more detail later. For the moment there are two points to note. First, although this discourse is characteristic of formal science writing, many of its features are familiar in other settings, including everyday conversations. Second, we can make sense of some of the features of this kind of discourse by considering the general role of fact construction and its relation to dilemmas of stake. Although science, as I have already noted, tends to be taken as an arena where there is disinterested discussion of pure facts, a whole variety of interests are potentially invokable to discount a scientist's claims (Gilbert and Mulkay, 1984, ch. 4; Potter and Wetherell, 1987: 151–2). Empiricist discourse manages the dilemma of stake by directing attention away from the scientist and on to what is being reported, in much the same way that we saw Diane in Extract 2 focus on Neil's shoes rather than her own reasons for not investigating the potential burglar. Indeed, the empiricist repertoire takes this to extremes by not merely focusing on the data, but also by constructing the data as having its own agency. The reporter becomes a passive responder to the requirements of the facts.

Constructing Corroboration and Consensus

Notions of corroboration (is there another witness to this event?) and consensus (do the different witnesses agree?) are familiar in legal settings. However, they have a much wider currency. As with category entitlements, it is tempting to think of these things merely as straightforward and sensible features of reasoning. Independent witnesses, particularly if they are all saying the same thing, make an account more credible. The argument here is not that this intuition is a mistaken one. Rather, it is that it is too simple on its own because it ignores the way that witnesses can be *constructed as* independent and the way that their versions can be *constructed as* the same. And this is not simply a technical, analytic observation. This is also a participant's

concern. That is, participants orientate to both corroboration and its construction as they build and undercut such accounts.

For example, in a study of arguments about what went on at an off-the-record press briefing (Potter and Edwards, 1990), we found examples of the press's case being bolstered by emphasizing the consensus of a set of different reports. The following is from an MP's question posed as part of a parliamentary debate to do with the content of the briefing; it comes from *Hansard*, which is the official parliamentary record:

7 *Mr David Winnick (Walsall, North)*: As all the Sunday newspapers carried *virtually the same story*, is the Chancellor saying that *every journalist* who came to the briefing – he has not denied that there was one – misunderstood what he said?

(*Hansard*, 7 November: 26, emphasis added)

Here the consensus across the range of different stories is invoked as a warrant for their truth in a setting where the truth of an account has been challenged. However, our emphasis on rhetoric and the constructed nature of versions leads us to expect that there can be commonplace counters to such forms of warranting. One such counter is to suggest that the consensus across a set of accounts is a product of collusion rather than a set of people independently witnessing the same thing, and thus providing the same description. The following extract is from shortly after in the same parliamentary debate. The speaker, Chancellor Lawson, has attempted to deny the newspaper stories.

8 *Mr Lawson*: [. . .] the statements that appeared in the press on Sunday bore no relation whatever to what I in fact said. [. . .] they will have their shorthand notes and they will know it, and they will know *they went behind afterwards* and *they thought there was not a good enough story and so they produced that.*

(*Hansard*, 7 November: 26, emphasis added)

The minister discounts the idea that the reports are the same because they are true by introducing the notion that they are the same because the reporters made them up to be the same. Note, also, that this claim about collusion is itself a description which is open to dispute, and the minister warrants it by reference to the *interests* of the reporters. Their story was not good enough so they made one up. This reminds us that these procedures of fact construction are not working in isolation. They are equally likely to be drawn on together, as in this case.

Detail and Narrative

The *detail*, the *specifics* of a description, are crucial for the activity that the description is used to do. However, vivid detailed descriptions can also be used to build up the facticity of an account. They can provide an impression of being there by sketching features which, although not substantial to the claim or argument, would have been apparent to someone who actually witnessed

some event. Again, to treat this as a rhetorical construction is not to argue that detail is not an important element in fact construction; rather, it is to note that such detail can be produced and worked up for its fact-constructional properties. After all, this is one of the primary skills of novelists as they tell a story in a believable way.

Take the dispute about what went on in the controversial off-the-record press briefing I have just discussed. As the dispute continued, with further questions being posed about the news reports of the briefing, several of the papers responded with lengthy articles which included detailed descriptions of the briefing, containing much contextual detail.

> 9 Mr Lawson sat in an armchair in one corner, next to a window looking out over the garden of No 11 Downing Street. The Press Secretary, Mr John Gieve, hovered by the door. The rest of us, notebooks on our laps, perched on chairs and sofas in a circle around the Chancellor. It was 10.15 on the morning of Friday, 4 November. . . .
> (*Observer*, 13 November)

Few of the details in this account are directly relevant to the substantive issue of dispute which was focused on potential change in the government's arrangements for the payment of benefits. Neither the armchair, the garden, the hovering by the door of Mr Gieve, or the perching on chairs is consequential for the benefits change and none of these particulars had been denied, or even commented on, by Chancellor Lawson. However, such descriptive features are characteristic of the way scenes are built in novels (Fowler, 1977). They work to make what is described graphic and believable by warranting the speaker or writer as a proper witness.

It is here that a concern with detail blurs into a concern with narrative and narrative organization. Details of this kind can be organized to provide narrative structure to an account; the order of events, who the characters are and so on. Narrative organization can be used to increase the plausibility of a particular description by embedding it in a sequence where what is described becomes expected or even necessary.

Although detail can be used in this way, there are times when detail can be ineffectual and descriptions that are vague or global can be the preferred pathway to a sustainable account. One of the problems with providing rich detail is that it may be undermined in various ways: details may be picked apart, or inconsistencies identified which cast doubt on the credibility of the speaker. The use of vague or formulaic descriptions may provide just enough material to sustain some action without providing descriptive claims that can open it to undermining.

Combining Action and Epistemology

So far, for the purposes of this initial exposition, I have separated out the action and epistemological orientations of descriptions, as well as treating the various styles of epistemological warranting as themselves separable from

one another. However, in actual situations these different types of warranting are generally blended more or less seamlessly together and bound up with the action itself. It is important to stress again that these styles of fact construction do not work in a mechanical way. Rather, they have to be worked up and fitted to the specifics of the situations they are used in and there is always the potential for their being undermined.

To end this chapter, I will devote a bit more time to a single example to show how the various elements and considerations I have introduced can be meshed together. The following extract comes from the first five minutes of the initial relationship counselling session involving a couple whom we will call Connie and Jimmy. We have already met them briefly in Extract 1, which was taken from their second session. One of the points of contention in this first session is precisely what the couple's relationship problems are. The Counsellor asks about the sequence of events that led up to an abortive move to start counselling.

10
C: Wha- (.) what happened at that point.
W: At that poi:nt, (0.6) Jimmy ha- (.) my- Jimmy is
extremely jealous. Ex- extremely jealous per:son.
Has a:lways ↓been, from the da:y we met. Y'know?
An' at that point in time, there was an episo:de,
with (.) a bloke, (.) in a pub, y'know?
And me: having a few drinks and messin'. (0.8)
That was it. (0.4) Right? And this (0.4)
got all out of hand to Jimmy according to Jimmy
I was a:lways doin' it and .hhh y'know a:lways aggravating him.
He was a jealous person I: aggravated the situation. .h
And he walked out that ti:me. To me it was (.)
totally ridiculous the way he (.) goes o:n (.)
through this problem that he ha:s.
(DE-JF/C2/S1:4)

There are a lot of fascinating features of this account, but I will concentrate especially on Connie's description of Jimmy, and her description of the events that preceded his 'walking out' (for analyses that deals with these materials in more detail, see Edwards, 1995, 1996). Note first the way Connie breaks off a direct response to the Counsellor's question to insert a description of Jimmy (2 3). She describes Jimmy as an 'extremely jealous person'. The specifics of the description are crucial here. Jealousy can be something to do with the person who is jealous, or it can be caused by someone else. Connie's description paints Jimmy's jealousy firmly as something to do with him: he is an extremely jealous '*person*' and he had been like that 'from the day we met'. This description counters the possibility that the jealousy was caused by something that Connie did – flirting with men in pubs for example, as Jimmy later claims. The description of Jimmy does two things. In terms of the dispute over why Jimmy left her, it counters the inference that he left because of her actions. In terms of epistemological entitlements, it provides a sceptical frame for hearing Jimmy's subsequent version of the same event. A pathologically jealous man's

description of his wife's good time in a pub is unlikely to be fully objective.

Now take Connie's construction of the event itself (5–8). One of the notable features of this description is its lack of detail. *Episode* here is the sort of term that can be used to remain neutral to issues of cause and effect; critical linguists have identified language use of this kind as characteristic of newspaper reports where causality is being questioned or recast (Fowler, 1991; Hodge and Kress, 1993). 'Bloke' and 'pub' specify minimum particulars. And in line 7 Connie characterizes her activity in a minimal fashion. In this speech community 'messin' is part of a familiar idiom, 'just messing'; that is, being playful or non-serious. And even this is further mitigated by noting that she had had a 'few drinks'. What one does after 'having a few drinks' is often (although not always – the rhetoric can be worked either way) less criticizable than what one does when sober.

Having strongly built up Jimmy's jealousy as a feature of his personality, and provided a description of an event which downplays its seriousness, Connie is in a position to disparage his (reported) claim that he left her because of the event as 'totally ridiculous'. It is particularly important to see just how subtle and sophisticated what is going on here is. It is easy to think this sort of discourse, full of hesitations and repairs as it is, is an example of poor argument or general clumsiness of expression where inarticulate people are stumbling over their words. Yet it becomes apparent when examining examples of this kind how the different features contribute to the task at hand. For example, the global formulations Connie uses in her descriptions of the event are robust against undermining. Some of the effort Jimmy puts in shortly afterwards is shown by the way his description of the evening extends to 130 lines of transcript compared with the 3 that Connie devotes to it (some of his version will be discussed in Chapter 7). The general point, then, is that descriptions are closely fitted to particular activities, and their epistemological basis is attended to in a variety of different ways.

Overall, this chapter is intended to prepare the ground for a more systematic and elaborate discussion of procedures for fact construction in the two chapters that follow. It started by arguing that exploration of the manufacture of factuality will be facilitated by pressing the metaphor of construction to its limits, and by selectively combining elements from the constructionism in linguistics, conversation analysis and post-structuralism. Such a constructionist approach will be facilitated by three analytic emphases. First, it is anti-cognitivist. Its concern is not with construction understood as a mental process, involving the cognitivist apparatus of schemas, memory stores and social representations. There are a variety of reasons for eschewing such a perspective. However, one of the principal ones is that it draws attention away from how factual accounts are organized and how they are fitted into particular interactions. Whether derived from cognitive psychology (Neisser, 1976), social representations theory (Moscovici, 1984) or critical linguistics (Hodge and Kress, 1993), cognitive theorizing tends towards an individualistic perspective and away from the human practices in which fact construction are embedded. The second emphasis is on discourse; that is, talk and texts as

social practices, rather than, say, ethnographic reconstructions of these things. The focus on discourse makes it more straightforward to retain the order of detail that is involved in establishing factuality. The third emphasis is on fact construction as rhetorically organized. That is, analysis will work on two closely related dimensions. It will be concerned not only with fact construction (*reification*) but also with fact destruction (*ironization*); and it will be dealing with both the *defensive rhetoric* through which an account is protected against attack, and also the *offensive rhetoric* through which a contrasting description is undermined.

Following these preliminaries, I developed a distinction between the *action orientation* and the *epistemological orientation* of descriptions. The point here was to show that descriptions can be analysed both in terms of the sorts of actions that they are performing or contributing to, and in terms of fact construction; that is, the process by which description is built up into accepted fact. Different features of descriptions may be involved in each of these things. It should be emphasized, however, that this distinction is more heuristic than actual. After all, in many cases it is precisely through fact construction that actions get done. The epistemological orientation is not an abstract, philosophical concern with truth; it is a practical, situated concern with making a description credible.

Finally, a number of different elements that can contribute to fact construction were broken down into two clusters. On the one hand, there are practices which involve different constructions of the agent, such as constructions of interest and disinterest and the building of category entitlements. On the other, there are practices that make descriptions separate and external to the actor, such as the use of empiricist discourse, the manufacture of corroboration, and the organization of descriptions into narrative. These will be the topic of the next two chapters.

1

Social Studies of Science

If we are asked to think of something that epitomizes the world of facts, before very long we are likely to consider science. Colossal investments of time, money and people seem to have led to facts which are sharply specified and precisely defined, sustaining the weight of prodigious technological advances. Looked at in this way science becomes a 'hard case' against which to test an argument about the constructed nature of facts. If you can succeed in showing that scientific fact generation deviates from idealized models, then you expect that fact generation in other realms is likely to be even further from those models (Collins, 1985). If even white-coated scientists, with all their training and technical back-up, produce facts which are in some way problematic, then what hope for barristers, newspaper reporters or 'ordinary people'?

This argument makes many assumptions and can easily be viewed as a piece of rather transparent rhetoric used by some social researchers into science to build up the importance of their work. However, with the proviso that we should not take the 'hard case' argument too seriously, the social study of science is an excellent place to start examining fact construction. Not only has it been a melting pot for different theories of knowledge generation but it has also led to numerous detailed case studies of the work of scientists. Many of the themes and problems that arise in science recur in other fields of fact making.

Social studies of science have broadly based origins. Although much of it is characterized as the sociology of science, or the sociology of scientific knowledge, in the last two decades one of the notable features of the field is the wide interdisciplinary collaboration among sociologists, philosophers and historians of science, psychologists, linguists and literary analysts. Indeed, the traditional sociology of science, which held sway until the 1970s, now seems striking in its conservatism and its resistance to a thoroughgoing exploration of the social basis and context of facts. It is worth briefly considering the nature of this earlier work to provide a contrast to what came later.

Traditional Sociology of Science

Typically, traditional sociology of science was concerned with two questions. How is science organized as a social institution in such a way that scientists regularly and successfully produce objective facts? And, conversely, what distorting social factors might result in the production of scientific errors? The

figure in the sociology of science who was most involved in formulating and attempting to answer these questions was Robert Merton (1970, 1973). I will take his solutions in turn.

Norms and the Scientific Ethos

Merton wanted to understand the way particular social conditions paved the way for the emergence of modern science. He suggested that the rise of Puritanism in the seventeenth century generated an ethos characterized by values such as utility, rationality, empiricism and individualism which was ideally suited for science. Merton argued that, when they conformed to these values, people were starting to view their world more in the manner of modern scientists, and thus starting to act in a way that facilitated the production of objective facts.

In an extension of this argument, Merton suggested that modern science is sustained by a more developed set of puritan values, which he called the *norms of science*. The argument is that modern science is constrained by four particular institutional imperatives: universalism, communism, disinterestedness and organized scepticism. While historically developed from the Protestant ethos, the role of these imperatives is to generate the conditions which allow facts to be produced in a reliable way. Communism requires that knowledge is freely and openly shared; organized scepticism, that all knowledge claims are assessed for their theoretical coherence and empirical adequacy; disinterestedness and universalism, that everyone's knowledge claims are assessed by essentially the same impersonal criteria and thus that scientific status is gained through merit rather than patronage or social position.

This account of science has continued to generate a large critical literature (for recent discussion, see, for example, Fuller, 1995; Lynch, 1993). And stories have been told about the origins of science which are strikingly different from Merton's (Shapin and Schaffer, 1985; Latour, 1993). The major point of interest for us is the way in which the problem of fact production was initially constructed in Merton's work. Essentially, it started from a received view of the nature of scientific facts – that they are impersonal, empirically warranted, rigorously tested and then asked what kind of social organization could produce such things. In what has often been called a storybook (Mitroff, 1974) view of science, scientific activity is taken as given, and the problem for the sociologist is to outline a social system that will explain it.

The problem with starting with the storybook view of scientific facts, as many subsequent analysts have pointed out, is that it is just that: a storybook account which does not describe the actual practices of scientists. For example, it is possible to view norms such as universalism very differently, by not treating them as clear-cut constraints, but as symbolic and open-ended resources that have to be interpreted differently according to the context in which they are used (Mulkay, 1976, 1980). Moreover, it is possible to consider scientific accounts which invoke such norms as vocabularies of justification

(Mulkay and Gilbert, 1981; Potter, 1984). That is, norms can be seen as one element in the persuasive armoury that scientists draw on when they are arguing with other scientists or attempting to legitimate the practice of science as a whole.

This brings us to the question right at the heart of this current book. How are descriptions made to seem literal and factual? In this case, how can scientists describe their individual activities in a way that presents them as following from the impersonal rule of 'proper' science? Before focusing directly on that, however, I will turn to the other major feature of Mertonian sociology of science, which is its concentration on error.

Sociology of Error

Although Merton stressed the importance of the set of norms for guiding the scientific activity of fact finding, he also stressed that scientists do not always conform to these norms. At times there is fraud; scientists may keep results to themselves or pass them only to selected associates; there may also be prejudice against particular individuals or groups. However, these deviations were treated as exceptions – indeed, for Merton they *must* be exceptions, for without their general effectiveness scientific facts would not have the special status they do.

Merton suggested that these deviations from the norms provide a sociological or psychological way of explaining scientific error. Prejudice against a group of researchers may result in the maintenance of a mistaken theory in the face of a correct alternative, or individual ambition may lead a scientist to falsify findings to fit into a desired model. What is interesting here is the asymmetrical way in which researchers in the Mertonian tradition approached what they construed as true and false belief. False belief could be directly explained through a 'social fact' (personality, prejudice and so on) disrupting the proper operation of scientific norms. True belief was dealt with quite differently. For scientists governed by the norm system, true belief arises directly from a careful investigation of how the world is. Put simply, in this view of science, the facts themselves determine truth, while error is explained by processes of a psychological or sociological nature. The consequence of this is that with true belief there was nothing to explain save for how the conditions for proper scientific inquiry came about and how those conditions are undermined. Social researchers only come into their own when they apply their skills in understanding group processes and psychodynamics to understand how false belief came about. This set of assumptions has been most effectively identified and criticized by the sociologist David Bloor (1991).

In effect, then, the tradition represented by Merton and others bracketed off the study of facts themselves and contented itself with examining their sociological context. A full sociological analysis of the *content* of science – of scientific ideas, theories, methods and so on – was reserved only for falsehoods. With the benefit of hindsight, we can see that these sociologists embraced scientists' own stories about the distinctive and privileged nature of

their knowledge and were led to focus their attention on facts that scientists had already discarded as mistaken for one reason or another. Given this self-imposed limit on analysis it is perhaps not surprising that the crucial developments that paved the way to a full social study of scientific facts came from philosophy and history of science rather than sociology.

Philosophy and Scientific Facts

It is important not to give the impression that philosophers and historians of science have been more sceptical about scientific activities than sociologists. For, with some notable exceptions, they have found scientists' stories as congenial and as self-evident as Merton. Philosophers have been primarily concerned with the justification of scientific knowledge; while historians have been traditionally interested in the thoughts and procedures that led 'great' scientists to 'great' discoveries. Philosophers in particular have traditionally taken it as given that scientific knowledge is special; and have seen their role as being to show how this special nature can be rigorously demonstrated. However, in their attempts to provide such a demonstration through the detailed exploration of classic scientific episodes, philosophers and historians started to build a radically different view of science. I will focus here on three facets of this novel view: the breakdown of the distinction between observation and theory; the notion that scientific beliefs are bound together in complex networks; and an emphasis on scientific communities and practices.

Observations and Theories

One of the most powerful and bewitching ways of understanding facts has been to think of them as observations of how the world is. Do I see a table there or not? Was that a blip on the photon scintillator or not? Observation has been thought to provide two basic rewards. First, it appeared to offer direct and unmediated access to the world and its features. It is the evidence of one's own eyes. Second, it seemed to allow for a basic process of corroboration: any observer who takes a particular viewing position ought to be able to see the same thing. Taken together, these appear to allow observation to work as a foundation for knowledge building; whatever else might be going on, we can see some particular properties of the world, and also others can check our observations by substituting themselves for us (Mulkay, 1979).

The idea that facts are a product of observation (the doctrine of empiricism) is so taken for granted, and so fundamental to scientists' understanding of their current practice, that it is difficult indeed to resist viewing it as self-evident. Indeed, both our scientific and everyday language of knowledge and understanding are permeated with visual metaphors: *looking* for the truth, *seeing* the point, *viewing* it as self-evident, and so on. Yet this idea of knowledge based on observation has a complex historical pedigree. Its self-evidence to us now is not something natural; it has been built up over a long period of time.

For example, Steven Shapin and Simon Schaffer (1985) documented the way that, in the middle of the seventeenth century, Robert Boyle drew on ideas from the judicial process to provide a new way of justifying his scientific claims about air pressure and the existence of a vacuum. They quote from Boyle:

> For, though the testimony of a single witness shall not suffice to prove the accused party guilty of murder; yet the testimony of two witnesses, though but of equal credit . . . shall ordinarily suffice to prove a man guilty; because it is thought reasonable to suppose, that, though each testimony single be but probable, yet a concurrence of such probabilities, (which ought in reason to be attributed to the truth of what they jointly tend to prove) may well amount to a moral certainty, *i.e.*, such a certainty, as may warrant the judge to proceed to the sentence of death against the indicted party. (1985: 56)

The truth of the scientific claims is established for Boyle, then, through using a number of witnesses who can concur in their support. It should be noted, however, that not just any witness would do; for Boyle, reliable witnesses were members of the appropriate communities, while 'Papists and atheists' were apt to find their stories questioned. Note also that, for Boyle, this way of understanding scientific observation was not self-evident. He had to argue for it and he imported the practice from the more familiar legal setting.

In this century the utility of observation as a foundation for scientific knowledge has started to come under threat from philosophical, historical and sociological analysis (for example, Barnes, 1977; Hacking, 1983; Kuhn, 1970; Rorty, 1980). In questioning the idea that visual experience is somehow a direct and unproblematic facsimile of aspects of the world, philosophers drew on psychological research on visual perception, and in particular work showing the sorts of reversals in how one sees an image that take place with visual illusions along with the role of cultural expectations in categorizing what is seen. We are all familiar with line drawings that can be seen as either a duck or a rabbit, or as either the top or bottom of a set of steps. Here the visual experience changes, although the drawing stays the same, and this is taken to raise the possibility of fundamental disagreements over the meaning of the same scene (Hanson, 1969; Kuhn, 1970). Cultural expectations are shown to operate in contexts where, for example, people quickly have to identify playing cards from a pack in which the colour of the ace of spades has been changed to red. The tendency is to report the ace of spades as black in line with expectations. The lesson, and the problem for empiricism, is that we may see what we *expect* rather than just what is there.

These are rather artificial examples and their relation to actual scientific practice is open to question. Practices of observation in the sorts of settings that scientists actually work in are much more complex than these simple, isolated visual exposures imply (for example, Goodwin, 1995; Lynch and Woolgar, 1990; Knorr Cetina and Aman, 1990). For example, Michael Lynch (1994) notes the way in astronomy the term *observation* serves as a rather loose device for collecting together a range of actions such as setting up the

position of a telescope, connecting a particular sensor to it, building up patterns of dots on an oscilloscope, converting a series of these into a chart and then gaining the support of colleagues over a particular interpretation. Unlike the snap judgement made of a single projected image, observation is 'temporally extended, socially and equipmentally distributed, and contingently fated' (1994: 138). Nevertheless, the sorts of psychological examples used by Kuhn and others worked as powerful rhetorical counters to the idea that what is seen is determined by the object, or even its image on the retina.

The problem with the idea that perception provides a firm and unproblematic foundation for knowledge becomes more apparent when we consider that, whatever the images on scientists' retinas, when observations enter the currency of science they do so in terms of utterances or some form of written discourse. Even at its simplest this involves some form of categorization; it is not just seeing what is before the eyes but seeing it *as something*; not just a particular colour sensation but a descriptive choice: red, brown with golden speckles, or whatever. And in science, as with 'common sense', our categories are not some neutral and abstract set of descriptive pigeon-holes; they are derived from theories and broad cosmologies. Philosophers such as Mary Hesse (1974) have argued that scientists work with descriptive terms – mass, mitochondria, muscle fibre – that presuppose a whole set of theoretical assumptions; and if we try to unpack these assumptions, and ground them in terms of other observations, these too are theory dependent (see Chalmers, 1992; Mulkay, 1979, for useful summaries of these arguments).

In addition to this range of problems with observation there is another issue which is increasingly apparent with modern science. Much of the time scientific 'observations' (and, as I have noted, this term starts to become increasingly misleading) are dependent on intricate recording apparatus such as electron microscopes, oscilloscopes and bubble chambers whose workings are themselves dependent on a range of elaborate theories which are presupposed in every observation (Feyerabend, 1975). Take Karin Knorr Cetina's (1996) ethnographic study of the use of a particle detector in high energy collider experiments at CERN. The detector is immensely complex, and the physicists spend much more of their time trying to make sense of *its* behaviour than they do looking for the hypothesized and almost unimaginably small particles that are their research topic. They run the huge equipment over and over again to find out about its blemishes and idiosyncrasies. The term *observation* here stands only in the loosest relation to research practices in this community.

The Web of Belief

Another facet of this critique of empiricism considers the way scientific statements or beliefs are connected together in a network. In the early part of the twentieth century the philosopher of science Pierre Duhem argued that scientific claims were never evaluated purely in relation to the findings of particular experiments. Instead, claims are evaluated by considering a whole

range of issues, including experimental findings, theories, ideas about method, statistics and so on (Duhem, 1962). For example, if a finding is consistent with a well-established body of theory, it is more likely to be accepted without discussion than if it is thought to contradict an established theory. An 'observation' of 'dark matter' in space is more likely to be rigorously repeated if it is seen as contradicting basic postulates of modern astrophysics; astronomers will search for alternative interpretations that sustain the coherence of their general account of the universe. In contrast, an observation that meshes with a large body of theory may be accepted with relatively little discussion.

In the 1950s, the American philosopher Willard van Orman Quine developed Duhem's ideas about the interconnection of beliefs and the role of experience into a famous metaphor, often elaborated as the Quine Duhem thesis (1961; see also Hesse, 1974; Quine and Ullian, 1970). Quine suggested that scientific beliefs should be regarded as stretched in a fabric, rather like the skin of a drum. The fabric is pulled toward the edge of the drum by experience; however, this experience does not determine the organization of the fabric, for it adjusts all the time to ease tension. Sometimes adjustments arise because of new observations pulling from the edge; at other times theoretical developments lead to reorganization of the fabric.

The crucial, and radical, point of the metaphor is that no *single* scientific observation will have a determinate effect on the web of belief. The impact of observations will depend on the state of the fabric as a whole. This way of understanding science suggests that there could never be a crucial experiment, a study which *on its own* definitively forced the choice between two competing theories; indeed historical work has suggested that experiments commonly thought of as crucial are often viewed in this way only some time *after* the abandonment of the earlier theory (Collins and Pinch, 1993). The general consequence is to undermine the idea that observation provides a conclusive foundation for knowledge. At the same time it provides a novel pragmatic emphasis on issues like the coherence of one belief with others and the overall simplicity of the system.

Community and Practice

A final and somewhat ironic consequence of these philosophical reassessments of science was an increasing recognition of the crucial role of scientific community and practice. While sociologists were led away from concerns with the content of scientific knowledge by Mertonian ideas, philosophers were finding that their concerns led them to psychology and sociology. The best-known proponent of this view was the philosopher and historian Thomas Kuhn (1970), whose ideas can be usefully seen as an extension of the Quine Duhem thesis. The notion of a web of beliefs is a very abstract one. Kuhn's important modification was to stress that such a network does not hang in some abstract conceptual space, but is embodied in the knowledge and practices of specific groups of scientists. Scientific beliefs are expressed in debate and inscribed in scientific writing.

If the network is going to be readjusted in the way Quine suggests, this will involve groups of scientists changing their theoretical commitments, learning new methods, abandoning favoured and laboriously acquired standard models of problem solving ('paradigms') and so on. Kuhn argued, on the basis of historical case studies, that instead of putting the network into a state of minor but continual disruption, the community of scientists will carry on doing 'normal science' in the face of anomalies and problems that arise from research until at some point the whole system will be so stressed that it will be forced to undergo radical readjustments. Only after this period of 'revolutionary science' can the serenity of normal science return. Kuhn not only claimed that science actually worked in this way, but also that it was sensible for it to do so.

Kuhn's community-based model of science was not the only one developed by philosophers. For example, Imre Lakatos (1970) argued that the central social unit for doing science is the 'research programme': a developing series of studies organized around a set of more of less basic theoretical assumptions. Others, notably Karl Popper (1959), suggested that it is not the social and intellectual organization of science which is important but the *manner* in which scientists carry out their activities. For Popper, science is distinguished from non-science by the *activity* of trying critically to test hypotheses and resisting the temptation to make continual *ad hoc* modifications to keep hypotheses going in the face of counter-evidence. For example, he argued that scientists should not have postulated new but invisible planets to keep Newtonian theory going in the face of seeming deviations in planetary orbits from predictions. In fact, Popper was strongly critical of Kuhn's suggestion that periods of 'normal', stable and unquestioning science are necessary for its development; for him this was simply bad science (Popper, 1970).

This brief characterization of developments in the philosophy of science does scant justice to the complexity and richness of what has been one of the most exciting areas of modern philosophy. Controversy continues, and there are many in philosophy who would reject some or even all of the points above. I have covered it in this way because it fits into the general narrative I am constructing about facts in two ways.

First, it is intended to show the way that even philosophers, whose basic concern has been the justification of the unique status of scientific facts, have raised fundamental problems for simple storybook models of science and its development. The simplicity of empiricism – the lone contemplative scientist and the world ready for inspection – is compromised by observations blurring into theories, by theories being interconnected, and by the recognition of how this is dependent on a community of scientists and their actions. The value of this work does not arise from a demonstration of how facts are warranted – for it has left only a vague outline of how this is done – but from its revelation of the limits of the classic empiricist story of science.

Second, this work shows how an abstract epistemological concern with the relation between an observation statement and some part of reality has

turned into a psychological and sociological concern with the role of expectations, machineries and communal practices. Unlike the traditional sociology of science, which effectively locked the content of factual knowledge away from the prying eyes of analysts, the new philosophy of science was an invitation to open the box and grapple with the specifics of scientific knowledge. And, particularly in Britain where Mertonian theory had anyway not been so well established, the invitation was accepted with relish.

Sociology of Scientific Knowledge

Modern sociology of scientific knowledge (or SSK, as it is widely called) is characterized by a number of overlapping theoretical concerns, analytic methods and research focuses. It is an area of lively internal debate, quite apart from sporadic controversies with more traditional sociologists and philosophers of science (Bunge, 1992; Laudan, 1990) as well as with scientists themselves (Labinger, 1995; Wolpert, 1993). I will start by discussing Harry Collins' work because it raises effectively many of the fundamental issues in SSK, as well as providing some powerful examples of this position in analytic practice.

The Empirical Relativist Programme

The clearest way to introduce this work is through a contrast to the traditional sociology of science, which focused on the social conditions or norms that enable the generation of true knowledge, and on the way particular social or psychological factors such as prejudices and personal ambitions led to scientific errors. For Harry Collins, the problem with this work is that it adopts scientists' own distinctions between what is true and what is false and sets itself the twin problems of explaining how the errors came to be made and what the social conditions are which sustain the truth. His point was that this traditional work legitimated any current *status quo* by presupposing the correctness of any current state of belief. It assumes that what scientists take as valid scientific knowledge needs no *social* explanation, for it is adequately accounted for by the nature of the *natural* phenomena that are being studied (Collins 1981; Collins and Cox, 1976).

Collins argued that, if they are to avoid becoming public relations managers for science, then social analysts need to adopt a *relativist* stance. Now relativism is a complex and fiercely contested notion in the social sciences, and one which is often treated as a straightforward term of abuse: someone has 'fallen into' a relativist position; the 'spectre of relativism' has to be avoided (Edwards et al., 1995; Smith, 1988). Collins wanted to rescue the notion from its theoretical dungeon.

Collins proposed that a form of *methodological* relativism is crucial for SSK. That is, scientists' claims about what is true and false should not be taken as the start point for analysis but should become a topic of analysis in their own right. One of the most striking consequences of approaching scientific

knowledge from a stance of methodological relativism is that it immediately frees up the whole scientific field for study. The social analyst is no longer restricted to picking up the scraps rejected from the scientific table as false beliefs or having to be content with routine studies of its organizational psychology. Furthermore, the analyst no longer has to sort out the scientific issues in a more definitive manner than the scientists themselves. In fact, what might have seemed at first sight to be an unnecessary and even rather eccentric start point for social research quickly comes to seem sensible and, indeed, indispensable. The value of methodological relativism immediately becomes apparent as we concentrate on the sorts of tangles that we can easily get into as we attempt to make unproblematic judgements about scientific truth and falsity. It is worth briefly noting some of these difficulties before we proceed.

In many of the most exciting areas of contemporary science there is no consensus over what is correct or not; instead there is heated controversy. Indeed, it is often the controversy that generates the excitement. There are also large scientific fields in which there is apparent consensus over matters of truth and falsity. Yet even here the analyst often does not have to look too hard to find an appreciable number of dissenting voices. Furthermore, the content of the consensual view may be varied; that is, scientists may espouse the 'same' theory, but what they mean by that theory may be radically different (Gilbert and Mulkay, 1984; Latour, 1987). Moreover, if we take a longer historical perspective, many scientific claims which are widely accepted at one time have later been drastically revised or entirely abandoned (Feyerabend, 1975; Kuhn, 1970). Sometimes the process has happened in reverse; initially ridiculed ideas become accepted.

Collins has focused much of his research on scientific fields in which there is ongoing controversy. This has two benefits. First, in controversies the rules and competencies that underlie science are thrown into question and may thus be explicitly formulated in ways rare in more consensual areas. That is, the researcher can use the controversy to bring into the open what is elsewhere often tacit. Second, the researcher is better able to maintain the relativistic stance of indifference to the way things 'really are' because this is precisely what is in dispute.

The advantage of controversies is underlined by the use of a powerful analogy, which has implications for the analysis of fact production more generally. Collins (1985) suggested that when we deal with scientific knowledge it is often like studying the sorts of traditional ships in bottles that sailors make. After all the glue has dried and the strings have been cut they seem almost magical. There is no easy way of seeing how they came to be made. The advantage of looking at controversies, according to Collins, is that they are situations where we might be able to catch a glimpse of the glue being inserted and the strings being pulled.

Collins suggests that there are three stages to providing a sociological account of a controversy (Collins, 1983a). The first stage involves documenting the flexible ways in which experimental results can be interpreted. How can particular findings be made out as supporting a theory or not? How can a replication be constructed as confirming a finding or dismissed as incompetent? Such flexibility is just what is to be expected in the light of the Quine–Duhem thesis which stresses that the findings of an individual experiment will be judged against bodies of theory as a whole. There are all sorts of ways in which tensions in the network introduced by novel findings can be reduced.

The second stage focuses on the way this open-endedness is dealt with so that one particular outcome results. How, ultimately, is the controversy settled? Here Collins departs from the Quine–Duhem view. Recent versions of the Quine–Duhem thesis (Hesse, 1980; Knorr Cetina, 1982a; cf. Kuhn, 1977) suggest that although there may be varied responses to the findings of individual experiments, there can be an orderly and rational response to accumulations of findings from a range of studies. This response depends on the application of general criteria which encourage the network to change in ways that emphasize coherence, say, or simplicity. However, for Collins the flexibility in dealing with research results combined with the holistic nature of scientific belief systems provides an opportunity for a variety of rhetorical devices and techniques of persuasion to be used. The debate is not closed by these rational considerations but by the sorts of strategies that might be used to sell a political programme to an electorate.

The third stage of the programme is much less developed in Collins' work. It concerns the attempt to relate the closure of controversies to wider social and political structures in society. I will return to this theme later in the chapter, when examining another tradition in sociology of scientific knowledge which has tried to relate the choice of theories and development of controversies to scientists' group allegiances and, ultimately, the broader societal context. For the moment it will be useful to move away from these rather abstract and programmatic claims and illustrate what they add up to when Collins researches a specific controversy. There are a number of case studies of controversies that have been carried out within this framework (for example, Collins and Pinch, 1982; Pickering, 1981; Pinch, 1986); I will concentrate on one of Collins' own studies which is well known and well respected and concerns a dispute over the detection of gravitational radiation (Collins, 1975, 1981, 1985).

Gravitational Radiation and the Sociology of Facts

One prediction adduced from Einstein's theory of relativity is that gravity ought to be detectable as a form of radiation. The movements of large objects should create a flux or discharge of this radiation. The problem for researchers is that this flux is almost unimaginably weak, making detection an exceptionally difficult task. However, massive galactic events such as exploding stars should generate quantities of radiation that might be detectable on the Earth. In 1969 the American physicist Joseph Weber claimed to have been the first person to detect it. Put very simply, he had hung up a very large aluminium bar in a sealed chamber and measured minute vibrations using

strain gauges. The bar is like a stick floating at the edge of a pond; if there is a big splash near the middle of the pond it should bob up and down.

Following Weber's claim, a number of other groups of scientists attempted to find gravitational radiation using similar apparatus. None of them found success. Collins examined the published papers and disputes between these scientists as well as interviewing a number of the key figures. Much of his argument was directed against what might be called the orthodox scientific interpretation of what went on — namely, that a number of studies had tried and failed to replicate the original, and thus the original was mistaken. Collins raised two difficulties with this orthodox view.

First, in practice what was described as 'repeating the experiment' did not involve using precisely the same apparatus and measurement techniques as Weber's original. So-called replications generally attempt to improve on the original apparatus, or address its potential shortcomings; there is often no profit for the researcher in doing a mere duplication. In fact, scientists often move between two different ways of characterizing a replication. When considering its methodological role in demonstrating the trustworthiness (or not) of some findings they often characterize the replication as a mere duplication; but elsewhere they may emphasize its novelty or sophistication when compared with the original (see also Ashmore, 1988; Mulkay, 1985).

Collins' study raised a second and more fundamental problem with the orthodox account of replication. For it turned out that there was a lack of agreement over what counted as a competently conducted experiment. Collins documented a range of 'extra-scientific considerations' which were drawn on as evidence of scientific competence or lack of it. It could be the personality and intelligence of the experimenters, a previous history of failures, the prestige of their home university, and so on. He notes that these judgements seem also to be closely related to the scientists' prior beliefs about the existence of gravity waves. Thus scientists who *believed* in measurable gravity waves tended to treat replications claiming to find them as *competent* and replications failing to find them as *incompetent*. Mirroring this, those scientists who did *not* believe gravity waves were measurable thought that the replications which did *not* find them were *competent*, while those which *did* find them were *incompetent*. In this situation the status of replications does not stand outside the controversy in a way that can neutrally close it down in one way or another; rather, the controversy extends to the status of replications.

Collins concluded that the best way to understand what was going on was not to think of it in terms of simple attempts to replicate but as a negotiation about *what counts as* a competent experiment in the field. Rather than replication being an arbiter of dispute, it becomes a focus of dispute in its own right. And as judgements of the competence of experiments were bound up with judgements about the nature of gravitational radiation, in effect the experiments were negotiations about the nature of the phenomena. Collins expressed this perspective on replication in gravity-wave research in the following manner:

> the most fruitful way of interpreting the activity of scientists . . . is not as attempts to competently replicate, or competently test . . . findings, but rather as *negotiations about the meaning of a competent experiment* in the field. *Ipso-facto*, they are negotiating the character of gravitational radiation and building the culture of that part of science which may become known as 'gravitational wave observation' (Collins, 1975: 216).

This element of the research can be seen as part of the first stage of the empirical programme which demonstrates the potential flexibility in the interpretation of experimental findings.

In a later part of the study, Collins (1981, 1985) went on to the second stage of the programme and tried to show how particular strategies had been used to close the controversy down to the point where it was essentially dead. His crucial sociological point was that there were no purely *rational* or *scientific* reasons that compelled gravity-wave scientists to disbelieve Weber's claims. The incredibility of these claims had to be *socially produced* by the use of a range of different rhetorical strategies.

According to Collins, the critical actor in the controversy was a scientist he called Quest (the pseudonym is to protect his anonymity). Quest was instrumental in closing down the controversy. However, this was not because of the technical merit of his work, or the novelty of his evidence, or the sophistication of his experimental design; rather, it was because Quest devoted himself to a high-profile campaign, using skilful rhetorical presentations in both scientific and more popular outlets. Collins quotes different scientists in the field in support of this interpretation of the effect of Quest's work.

> 1 . . . as far as the scientific community in general is concerned, it's probably Quest's publication that generally clinched the attitude. But in fact the experiment they did was trivial — it was a tiny thing . . . but the thing was the way they wrote it up. . . .
>
> 2 Quest had considerably less sensitivity so I would have thought he would have made less impact than anyone, but he talked louder than anyone and he did a very nice job of analysing his data.
>
> 3 [Quest's paper] was very clever because its analysis was actually very convincing to other people, and that was the first time that anyone had worked out in a simple way just what the thermal noise from the bar should be. . . . It was done in a very clear manner, and they sort of convinced everybody.
>
> (All from Collins, 1985: 92)

Collins' general conclusion is that, although there were a variety of findings and studies that went against Weber, it was not these that were crucial; it was the *manner* in which they were marshalled by a particular scientist, Quest, alongside his own work, in such a way that they appeared to settle unambiguously the non-existence of measurable gravity waves. With the campaign a success, the controversy was effectively over, pushed out of the market (one might say) like a weak brand in the face of a sustained bout of television advertising from a stronger competitor.

Collins, Relativism and Facts

At this juncture it is useful for us to take stock of what is valuable in Collins' approach in order to consider how far it could provide the basis for a more general account of fact making. There are two important features of this work which I will carry forward throughout this book. The first is the stance of methodological relativism. Collins does not start from the assumption that Weber's research is flawed or that his critics were misguided. The empirical programme of relativism is intended to be indifferent to both of these possibilities. When he considers the success of Quest and failure of Weber, he is not wanting to argue that this is because Quest is, in fact, *right* or, indeed, that Weber is *wrong*. The alternative to methodological relativism would be to assume a knowledge of astrophysics greater than the participants (a tempting but unlikely claim!) or simply to treat the beliefs of whoever is currently successful as right. That would mean that the social analyst would be forever providing a sociological gloss on the current scientific *status quo*: that is, they would be repeating the Mertonian sociology of error discussed above.

The second feature of Collins' approach I want to emphasize and support is his more general emphasis on deriving conclusions from a detailed analysis of specific fact-making practices. He is avoiding the theoretical or conceptual stipulations that are present in much philosophy of science along with much of the sociology of knowledge tradition (for a useful discussion of that tradition, see Dant, 1991). Such stipulations may be useful if the enterprise is the normative one of specifying what *should* count as a good fact; if, however, the concern is with what *participants* count as factual in particular social settings, and how their versions are warranted, then a strongly analytic stance will be indispensable. Throughout this book I will emphasize the virtues of understanding fact construction through considering actual cases.

Both methodological relativism and the analytic focus are very important. However, Collins' work raises some equally interesting but much more problematic issues (see also Ashmore, 1989; Mulkay et al., 1983). It is worth spending time on them as they have implications for how my argument will continue. I will take three areas of difficulty in turn: Collins' use of a realist perspective to understand the social world; the leaking away of his relativism in analytic practice; his treatment of accounts of rhetoric as non-rhetorical. To address these points we will need to become more and more entangled in some of the rich but complex detail involved in the study of fact making. In fact, we can start to see that much of the descriptive language that we have used up to now in talking about science is far from neutral in its implication about what is going on.

The Problem of Social Realism Collins adopts a realist stance when conceptualizing the activities and beliefs of scientists. His scientific world is furnished with individual scientists; these scientists have specific beliefs and are organized into collectivities within which there are controversies with sides; they may be persuaded by rhetoric or by evidence; and a controversy

can be up and running or closed down. Collins is treating science as you might a car engine: here is the distributor, over there the spark plugs, when the pistons move they turn the crankshaft, and so on. Thus Collins takes issue with the stories that scientists tell about gravity waves, the quality of experiments, and more generally how science progresses; yet, at the same time, he is accepting their general common-sense understanding of the relevant categories, objects and processes.

This is an important point, so it is worth spelling out carefully what is being suggested. Take categories of scientists, for example. It is possible to take a category such as 'gravity-wave scientists' as a neutral descriptive term that collects together all the scientists who actually work on gravity waves. That would be to treat the category realistically. However, the category can also be treated as a construction; that is, as a category that different scientists use with different boundaries, say, and as part of different activities. Some versions of the category may be widely accepted, while others may be fiercely contested.

There are numerous theoretical reasons for questioning the kind of social realism Collins uses, some of which derive from traditions of social analysis that we will cover in later chapters (such as ethnomethodology and deconstruction). But for the moment I will focus on the analytic problems that confront Collins in his attempt to produce a unitary and realist version of what is happening in the social worlds of 'gravity-wave scientists'.

To understand these problems better it is necessary briefly to consider Collins' analytic method. Although the gravity-wave study was conducted mainly through two major interview tours, along with extensive reading of the gravity-wave literature, Collins characterizes what he is doing as developing his *participant comprehension* of the field (Collins, 1983b). That is, he is not treating his interviews simply as a means of finding out what is going on in the field; instead he is using them as a setting in which to develop his participants' understanding of what is involved in gravity physics. He learns what is involved in being a member of this community, albeit somewhat vicariously, and then uses this developing understanding to guide his analysis. This means that when he presents extracts from interviews they are not meant to be the *data* on which the analysis is based but an *exemplification* of his participants' understanding. Given that they are meant to be ideal cases, is it possible to re-read them in a way that reveals a different story? That is, is it possible to trouble Collins' realist story? I want to argue that it is.

When we start to look for it, it is not hard to find considerable variability in the way events, people and developments in the gravity-wave field are constructed by the participants. For example, I quoted above some participants who present Quest as having been effective in closing down the controversy; others characterized their response to Quest very differently:

> 4 [Quest and his group] are so obnoxious, and so firm in their belief, that only their approach is the right one and that everyone else is wrong, that I immediately discount their veracity on the basis of self delusion. (Collins, 1981: 47)

Variability of this kind is profoundly troubling for the kind of realist story Collins is telling. It poses the question of how Collins decided on his particular version of Quest's effectiveness, or on his version of what is happening in the field more generally. To construct his realist account in the face of variability of this kind Collins is forced into selectively reifying (reading as literally true) some accounts and ironizing (treating as mistakes, lies or rhetoric) other accounts. I will discuss these two terms in more detail in Chapter 4. Collins was led to work in this way by the demands of producing a realist version from the competing and fragmentary texts at his disposal. This also leads him into difficulties sustaining his relativist stance.

The Problem of Relativism Leakage Although the relativist position allows Collins to be disinterested in the truth or otherwise of scientists' utterances about the natural world, his need to provide a definitive version of what is going on in the social world forces him to make exactly such judgements concerning scientists' utterances about the social world. The difficulty with this is in keeping the two sorts of judgements separate. For example, Collins claimed that the technical arguments against Weber's experiments were not sufficient to end the controversy – it had to be finished off rhetorically. However, this appears to be moving beyond a mere judgement about what is happening in the field socially towards the provision of a definitive version of the adequacy of particular experiments. Indeed, it is the sort of version that Weber could use in defence of his position: he might say: 'They have not shown me to be wrong scientifically; I am a victim of a political vendetta.'

The particular concern with the way relativist analysis may favour one party in a dispute has been developed by Brian Martin, Evelleen Richards and Pam Scott (1991). They give examples of the way relativist studies of scientific controversies – for example, over the value of vitamin C in cancer treatment – can be treated as asymmetrical by the participants. As studies of this kind are showing the flexibility in the interpretation of experimental findings, and the rhetorical means through which disputes are closed down, they can be drawn on by the participants on the weaker side in the controversy to help criticize the stronger side. The demonstration of the social contingency of an argument is more troubling for an argument that has been established as solid and rationally justified than for one that has been widely treated as unreliable and inadequately justified. Martin et al. (1991) refer to the phenomena of participants in a controversy taking over SSK arguments within the controversy as *capturing*.

The point I am making is slightly different from that of Martin et al.; it is not that Collins' work has been captured by participants but that it is inevitably involved in making judgements about the content of science, because these are inseparable from judgements about what is happening socially. Accepting evaluations of the relative worth of sets of experiments (for instance, that Quest's research was trivial but rhetorically effective) amounts to the same thing as evaluating the relative worth of different versions of the natural world. To paraphrase Collins' own conclusion about replication:

negotiations about the value (rhetorical or genuine) of a particular experiment are, *ipso facto*, negotiations about the character of gravitational radiation.

The Problem of Non-rhetorical Rhetoric The final issue is with the way Collins uses the notion of rhetoric. Rhetoric is central to his account because it provides closure to controversies; without it the indeterminate nature of experimental findings would allow the controversy to drift on and on. Yet although rhetoric is one of his major concepts for understanding social life, he does not explore its senses or develop an elaborate theoretical account of the notion. When he provides examples of rhetoric they are often in the form of rhetoric *attributions*. That is, he gives examples of people claiming that such and such an experiment or publication had its effect through rhetoric – but takes the claims themselves as *non*-rhetorical. Michael Billig (1989) has suggested that rhetoric attributions of this kind are themselves a powerful rhetoric.

I will be devoting more space in later chapters to the role of rhetoric in social analysis. For the moment let me start with a preliminary, but useful, definition of rhetoric as discourse used to bolster particular versions of the world and to protect them from criticism. Using this notion, let us look again at the extracts (1–3 above) which Collins uses to illustrate his claim that it was Quest's rhetoric that was crucial in finishing the gravity-wave controversy rather than the intrinsic quality of his research findings.

1b . . . as far as the scientific community in general is concerned, it's probably Quest's publication that generally clinched the attitude. But in fact the experiment they did was trivial – it was a tiny thing . . . but the thing was the way they wrote it up. . . .

2b Quest had considerably less sensitivity so I would have thought he would have made less impact than anyone, but he talked louder than anyone and he did a very nice job of analysing his data.

3b [Quest's paper] was very clever because its analysis was actually very convincing to other people, and that was the first time that anyone had worked out in a simple way just what the thermal noise from the bar should be. . . . It was done in a very clear manner, and they sort of convinced everybody.

(All from Collins, 1985: 92)

In each of these extracts we see the speaker focusing on the effect of Quest's work on *other* scientists. These scientists do not characterize *themselves* as taken in by what they see as the work's style; indeed, they are keen to praise Quest's clarity, his very nice job of data analysis and his novel problem solution. We therefore have to take on trust that these speakers can give an accurate account, not only of the influence of Quest's work on a large number of other scientists, but also exactly what feature of the papers was responsible for the influence. The irony in Collins' analysis, then, is that he is elevating rhetoric to a position as the crucial lubricant for controversy closure – yet he is treating the accounts which supposedly show this as non-rhetorical.

This discussion has taken us some way from central features of Collins' Empirical Programme of Relativism. However, its value is in introducing issues which will appear repeatedly in different guises in the course of work on fact construction. If we think back to the use of a car engine as a metaphor for the social world of science, we can now be clear just how limited it is. Rather than there being carburettors, plugs and so on which are simply *there* to study (or so the garage tells us!), we would need to treat these things as *constructions*. The social world of science is produced in the talk and writing of the different scientists. And the production is very much a part of the business at hand. Weber can use the sort of construction that Collins has worked up as part of his defence that his research was undermined by rhetoric rather than proper scientific argument; Quest can characterize his arguments as effective because they so clearly show Weber's flaws. From this perspective it makes sense for us as analysts to treat *both* gravity waves *and* social processes equally as constructions.

Oh, and there is another construction here, of course. It would be very odd, to say the least, for me to expend this effort in showing some of the difficulties of Collins' mixture of realism and constructionism, and then to repeat them precisely in my own text. My version of Collins' work is a story put together for the purposes of this text, it is designed to make a particular argument. Collins, the Empirical Programme of Relativism, philosophy of science – all these things are simplifying and clarifying categories that allow me to build a story. That is not to say that the story is wrong or untrue or inaccurate – for those judgements presuppose that there is a definitive 'Collins', a definitive philosophy, and so on that this account could be compared with. It is a story that works here.

For the rest of this chapter I will discuss, rather more briefly, two of the main contemporary alternatives to the Empirical Programme of Relativism: constructionism and interest theory. As well as being important players in contemporary SSK, they will allow us to address further central issues that are involved in the study of fact construction.

Constructionist and Interest Theories of Scientific Fact Making

These two approaches to the sociology of scientific knowledge have their own distinctive features; yet they share with Collins' work a rejection of the principal assumptions of traditional Mertonian sociology of science. They reject the view that a set of broad social norms will ensure the production of true knowledge, as well as the idea that the task of the social analyst is to account only for scientific errors. Constructionist work will be considered first. This is best exemplified by the work of Karin Knorr Cetina (1981, 1996) and that of Bruno Latour and Steve Woolgar (1986). While Collins' research is based largely on interview studies (although, as I have indicated, he gives them an ethnographic spin), Knorr Cetina and Latour and Woolgar derived their conclusions mainly from ethnographic studies performed in laboratories

involved in biochemistry and high energy physics. As Knorr Cetina put it, ethnography 'furnished the optics for viewing the process of knowledge production as "constructive" rather than descriptive; in other words, for viewing it as constitutive of the reality knowledge was said to "represent"' (1995a: 141). Such studies involved spending time with scientists watching their actions at the lab bench, sitting with them at their workstations as they analyse data, and trying to make sense of what is going on in much the same way that an anthropologist might make sense of an exotic culture. Indeed, it is hard to think of a culture more exotic than the high-energy physicists at CERN with their vast detection machines and extraordinary cosmologies.

The Construction of Knowledge

The term 'constructionism' is used with a number of distinct and sometimes contradictory shades of meaning across the social sciences and even within SSK itself. I have already briefly introduced the phenomenological variety of constructionism of Berger and Luckmann (1966), which was concerned with the life world of individuals; how a person's experience takes the form of solid and enduring entities and structures. In SSK a constructionist approach is typically contrasted to a descriptive approach. In this case what is being stressed is a contrast to parts of the standard or storybook view which treats science as producing increasingly accurate and powerful descriptions of an external reality. Knorr Cetina expressed this contrast as follows:

> Rather than view empirical observation as questions put to nature in a language she understands, we will take all references to the 'constitutive' role of science seriously, and regard scientific inquiry as a process of production. Rather than considering scientific products as somehow capturing what is, we will consider them as selectively carved out, transformed and constructed from whatever is. And rather than examine the external relations between science and the 'nature' we are told it describes, we will look at those internal affairs of scientific enterprise which we take to be *constructive*. (1981: 1, emphasis in original)

In opposition to the standard view, Knorr Cetina and others in this tradition have suggested that the products of science are fabricated through social interaction between specific individuals in accordance with *ad hoc* criteria in idiosyncratic circumstances that are dealt with in an opportunistic manner. Much of a researcher's time will be occupied with 'tinkering'; that is, using the local resources – apparatus, raw materials, available skills – to 'make things work', where the criteria of something 'working' will itself have been developed. The analytic studies in this area have concentrated on documenting the role of these different constructive activities in fact production.

This perspective has two related consequences for how epistemology is understood in practice; that is, for the status of scientific knowledge. On the one hand, the argument is that there is nothing epistemologically special about scientific work. Scientific knowledge production does not have principled differences from knowledge in legal or everyday settings. Not surprisingly, this is a claim that has not always been taken in good spirit by

practising scientists (for example, Wolpert, 1993). Knorr Cetina quotes Richard Rorty's provocative formulation: "'no interesting epistemological difference" could be identified between the pursuit of knowledge and the pursuit of power' (1995a: 151). On the other hand, the argument breaks up the supposed uniformity of scientific practices. Rather than science being characterized by a small set of methods that are followed, whether in biochemistry, astronomy or sociology, constructionists have stressed that scientific disciplines and sub-disciplines operate with a disparate set of epistemic cultures. For example, molecular biologists and high-energy physicists work with strikingly different notions of the empirical: compare the hypothetical events in the particle collider which may be reconstructed within several concurrently available theoretical systems and the search for change in bacteria growth on a high-protein culture in a petri dish (Knorr Cetina, 1995b).

Constructionists also emphasize the importance of negotiation in the making of scientific knowledge. Again, the point here is generally to build a contrast with the storybook view that scientists' decisions are governed in a mechanical or simple manner by the outcome of experiments, observations, replications and so on. Just as Collins argued that what counts as a competent replication should be seen as a product of negotiation, so various constructionist researchers have shown that a wide range of features of scientific life do not have universal determinate meanings, but are subject to processes of negotiation and interaction. For example, Michael Lynch (1985) has studied the interactions taking place where neuroscientists decide whether observations made by microscope are genuine phenomena or really artefactual. These interactions are not merely an adjunct to the decisions, but are part of the constitution of their nature.

Exactly what is being suggested about the nature of knowledge and truth by workers in this perspective is not always clear. For example, at times Knorr Cetina (for example, 1982b) has characterized her work as complementary to that of Collins; yet at other points she has distanced herself from his work, and seems to be more interested in following up the sociological implications of accepting the Quine Duhem thesis (Knorr Cetina, 1982a). Although she has adopted the methodological relativism which is widespread in SSK she did not follow Collins in hypothesizing that the 'natural world' makes no difference to science. As she puts it, 'facts are not made by pronouncing them to be facts but by being intricately constructed against the *resistances* of the natural (and social!) order' (1995b: 148, emphasis added). For her, the natural world is an emergent product of laboratory practices, but this does not mean that these practices are not revealing high quality, powerful or at least useful knowledge in some not quite precisely specified sense.

Whatever the detailed epistemological differences between an empirical relativist and a constructionist position, their difference in research emphasis is reasonably clear. While empirical relativists have looked mainly at controversies, constructionists have concentrated on 'unfinished knowledge'. This has involved them approaching fact making through ethnographic and

observational studies of scientists at work in laboratories. This emphasis fits, of course, with the theoretical stress on the opportunistic, *ad hoc*, situated nature of knowledge manufacture. If scientific products are closely dependent on the contingencies of their location of production then that is the sensible place in which to study them. The observer needs to be on the spot because that is where knowledge is actually manufactured. This is a contrast to Collins who, in his work on controversies, sees the end of a controversy as the moment when a fact is finally stabilized; that is, when the last strings are pulled out of the bottle, the glue is set and the ship is standing magically. Knorr Cetina sees facts as fabricated through procedures taking place somewhere within the laboratory; while the controversy is merely the place in which they are later argued over, rationalized, and accepted or rejected.

We have already noted that Collins is rather vague in his use of notions such as rhetoric and negotiation in the outcome of controversies. There are often similar ambiguities over the exact explanatory role of some of the features constructionists emphasize when studying the production of specific facts in laboratories. As Knorr Cetina herself notes, the notion of negotiation is used with a range of different inflections and the precise procedures at work in any situation are not always well specified. Part of the problem is that constructionists, like Collins, are often attempting to produce a unitary, realist version of how facts are manufactured out of idiosyncratic local resources; and as such they are subject to the same problems that we documented above with respect to Collins. In particular, constructionists are forced into a mixture of ironizing and reifying accounts as they produce a singular realist narrative, and this means that they have not always attended carefully to the rhetorical orientation of scientists' accounts. This is not to claim that the general perspective, or individual studies done within it, have not made important contributions to understanding fact making. Far from it. The work of Latour, Woolgar and Knorr Cetina has provided a powerful alternative to the accounts of science given by traditional philosophers and historians. Moreover, the general perspective I will be using in this book is a variant of constructionism. To end the chapter, let us now turn to the final SSK perspective concerned with social interests.

The Theory of Social Interests

The best-known researchers in this tradition are Barry Barnes (1977, 1982), David Bloor (1982, 1991) and Steven Shapin (1982; Shapin and Schaffer, 1985). These and others have tried to explain the content of scientific knowledge in terms of various kinds of interests. Put simply, perhaps rather too simply, these researchers suggested that scientists are making certain claims about reality because it is in their interest to make those claims. Some of these interests may be a product of the local disciplinary context in which a scientist works – scientists may have an interest in getting their work published, for example, as this will further their careers. And studies of scientific practice

which are concerned with interests of this type (for example, Pickering, 1984) overlap to a considerable extent with those in the empirical relativist and constructionist traditions. What is distinctive and provocative in the theory of social interest is the emphasis it places on the role of scientists' background culture and broader social allegiances – their group memberships and political viewpoints. And it is this aspect of the approach which I will concentrate on here.

The Quine–Duhem thesis has again proved a useful reference point for interest theorists (Barnes, 1982), although they refer to 'Hesse nets' in recognition of the important development of these ideas by the philosopher Mary Hesse (1980). As I noted earlier in the chapter, the Quine–Duhem thesis suggests we regard scientific beliefs as lying in an extensive interconnected network or web of belief. Although observations provide a boundary condition for this network, no individual observation has a determinate effect because of the interconnected nature of the network. Any particular belief statement may thus be retained in the face of a contradictory observation statement by making a readjustment somewhere else in the network: by modifying or abandoning a theory, say, or even in the extreme case a logical law (Quine, 1961).

One important consequence of the Quine–Duhem model, which I have not stressed so far, is that there will be a strongly *conventional* aspect to scientific judgement. When a novel observation or theoretical statement is introduced into the network there are a range of different ways in which adjustments could be made. The adjustments that are actually made will be dependent on some general notions about what sort of transformations in the network are acceptable. Interest theorists have argued that such general notions themselves are neither a product of 'observation' nor 'pure reason' (Barnes, 1981). In his original article Quine talked rather vaguely of the network tending towards 'simplicity' and 'conservatism'. Interest theorists have suggested that such general notions are not sufficient to force determinate choices between theories; they have to be supplemented by the operation of social interests. They will modify their networks in ways that serve their interests and they will also use their interests as standards against which to evaluate extensions of the network (Pickering, 1992: 4).

From this perspective certain scientific laws or theories will be retained, perhaps in the face of what some scientists would see as contradictory evidence, because of their perceived use in justifying social world views. That is, the social background of a group of scientists may lead them to see a certain configuration of theory as appropriate because it fits in with their social understanding. Interest theorists argue that this will result in homologies between the structure of knowledge and the structure of society (Bloor, 1982). Scientists will be literally rediscovering or redescribing the structure of their society in their test-tubes and cloud chambers.

This is all rather abstract – let me try and flesh it out using a highly regarded study of the relation of interests to scientific knowledge. Brian Wynne (1979) tried to demonstrate the crucial role of social interests in a

debate about the nature of the 'ether' in late Victorian times. Many astrophysicists in Victorian Britain believed in the existence of the ether, an invisible medium that filled space and explained a variety of physical and astronomical phenomena. Wynne claims that the theory of ether was drawn on by its proponents at Cambridge University as a part of a moral discourse to legitimate their own social ideals. Ether theory reflected their general social and religious beliefs which stressed 'the organic unity of knowledge, metaphysical realism, and the unseen world' (Wynne, 1979: 176). These social beliefs opposed the fast-growing secular ideology of scientific naturalism and individualism which, according to Wynne, was a by-product of industrialization and the increasing power of the bourgeois middle class. Thus there is a two-way causal connection: ether theory was influenced by broader social concerns, and it was also used to effect those concerns. Put simply, it was believed because of the ideology of its proponents, and these proponents used the theory to justify that ideology.

Interest theory has made an important contribution to SSK and has stimulated a number of fascinating case studies of scientific episodes. However, it raises some of the same issues that we noted above with respect to constructionism and, in more detail, with respect to the empirical programme of relativism (see also Woolgar, 1981; Yearley, 1982). It represents another attempt to develop a realist version of a particular arena of scientific work; indeed, it is even more ambitious, for not only does it depend on producing a realist account of scientific events, beliefs and groupings but it also has to coordinate this with a similarly definitive account of the nature of particular social groupings, classes and their ideologies. Whether such a version is *in principle* possible or not, in practice, interest analysts are also involved in processes of selective ironization and reification as they assemble an account from particular historical documents. In addition, because of their emphasis on the central role of group allegiances they are led to provide clear-cut group and sometimes social class categorizations for each scientist.

For example, Wynne uses a variety of what we might call 'homogenizing devices' to sustain his unified account of the beliefs of Cambridge physicists. The principal device is to treat everyone who taught at Cambridge or went to Cambridge at some point as sharing the same social beliefs. Furthermore, Wynne's attribution of social interests is particularly problematic. He is concerned to show that scientific beliefs are a product of interests and not purely understandable as produced by technical concerns. To warrant this he quotes scientific judgements that the ether theory cannot be warranted purely in terms of the contemporary state of theory and evidence; however, in doing this he is moving away from a relativistic stance and starting to side with critics of ether theory who at times argued exactly this. Yet rather than see these critics' accounts as themselves factual constructions designed for rhetorical purposes (showing the inadequacy of ether theory), Wynne is treating them, for the purposes of his own argument, as definitive documents of the actual situation for ether theory.

So we see again in the theory of social interests the combination of social

realism, relativism leakage and selective reification and ironization that we identified in Collins' work. Nevertheless, this is an ambitious theory, and one of the most successful existing attempts to relate the content of scientific knowledge to the broader social climate in which those ideas were produced.

Realism, Relativism and Rhetoric

I have not tried to provide a thorough review of SSK in this chapter. Sociology of scientific knowledge is now a major area of social research and one that has boomed during the 1980s and continues to grow, most recently with a major concern with technology (Bijker and Pinch, 1992). Research in SSK is conducted from several different perspectives of a complexity to which it is hard to do justice in a single chapter. (For very different summaries, see Ashmore, 1989; Jasanoff et al., 1995; Woolgar, 1988b). Some work that is often treated as part of SSK is dealt with elsewhere. For example, Donna Haraway's more postmodern explorations of the intersections between science and society appear in Chapter 3, while discourse analytic work done by Nigel Gilbert and Michael Mulkay is discussed in Chapter 6.

In this chapter I have tried to show how SSK emerged out of issues in the philosophy of science which raised questions about traditional images of science and its operation. Although it focused on the breakdown of the conventional distinction between observation and theory, the stress on the way scientific claims are organized together in interconnected networks, and the emphasis on scientific practice and its communal nature, there are several other themes in recent philosophy of science that would have led in the same direction (Chalmers, 1992). The sociological work which I have reviewed picks up this attack on the storybook view of science and develops it in various directions.

The Empirical Programme of Relativism stresses the flexibility in dealing with scientific findings and the central role of rhetoric in the ending (or sustaining) of controversies. Constructionist work stresses the local and *ad hoc* nature of scientific work along with the importance of negotiating the meaning of observations, methods, replications, policy implications and virtually everything else *in situ*, in laboratories and on work benches. Interest theory reconnects scientists to their broader social allegiances by suggesting that their choice of theory is related to their understanding of society.

Three theoretical and analytic themes will be taken forward into later chapters. First, the argument for methodological relativism is crucial not just for work on science, but for work on fact construction generally. Methodological relativism means that the analyst is not starting with a set of assumptions about what is true and false in any particular social setting and then trying to work out what led some people to get it wrong. Instead, the analyst will be indifferent to whether some set of claims is widely treated by participants as 'true' or 'false'. Truth and falsity can be studied as moves in a rhetorical game, and will be treated as such rather than as prior resources governing

analysis, to avoid subordinating the analyst to a current scientific orthodoxy.

Second, one of the positive points about SSK is its strongly analytic, or empirical, orientation. Even though it raises questions about traditional ways of understanding the nature of empirical research, it shows the value of conducting detailed studies of fact construction. One of the distinctive features of the field is its assumption that the best way to study fact construction is to research its operation in particular settings, and I will follow this principle throughout this book. So, although there are important and live philosophical questions to do with realism, epistemology, the nature of truth and so on, these will be bracketed off in favour of concrete investigations of factual accounts. As it turns out, many of them reappear as practical concerns for people as they construct and undermine versions of the world.

The third and final point concerns social realism. The discussion of the work of Collins focused on problems with his combined emphasis on social realism and rhetoric. Collins told a story of controversy resolution which drew on notions of sides, strategies, rhetoric and so on. His goal was the actual story of how the controversy was closed down. And his most central explanatory tool was rhetoric: the scientists were described as mobilizing political strategies. Yet Collins' realism became problematic precisely because of the importance of the rhetoric. Scientists will be constructing powerful arguments not just about gravity waves and experiments but also about the groupings that they fall into, about the rhetoric in each other's papers, and about the very closure of the controversy itself. The problem is in treating the deadness of the controversy as simply there, a social *fact*; when its deadness itself is part of what is at issue. That is, the deadness can be a rhetorical *accomplishment*, but not, as Collins claimed, through rhetorical strategies enabling one side to win, but instead attempting to constitute the controversy as dead is itself one move in the controversy.

Consider this another way. Collins has to limit carefully the effectiveness of rhetoric to make his social realism work. If the rhetoric is too weak, then it is either not strong enough to force the closure of controversies or, even worse for Collins' argument, the controversies are being closed because of the accumulation of rational considerations such as telling evidence, novel analysis of data, successful theorizing. However, if the rhetoric is too strong, the realism breaks down because the social furniture – the groups, closed debates, strategies – becomes subject to rhetorical reworking. In this book the consequences of a strong notion of rhetoric will be explored where nothing (the data, the sides in the controversy, the text I am currently writing) is excluded a priori from being considered as a rhetorical construction. The next chapter discusses the perspectives that have most to say about facts as an accomplishment: ethnomethodology and conversation analysis.

6

Constructing Out-there-ness

This chapter continues the focus developed in the last of the devices that are used to construct descriptions as factual. While the previous chapter concentrated on the way in which the nature of the producer of the description could be managed by discounting any potential stake they might have in it and building their category entitlement to be a competent describer, this chapter will concentrate on procedures which, for the most part, draw emphasis away from the nature or identity of the producer. These are procedures designed to provide a quality of what might be called *out-there-ness*. In other words, they construct the description as independent of the agent doing the production. More specifically, these procedures draw attention away from concerns with the producer's *stake* in the description – what they might gain or lose – and their *accountability*, or responsibility, for it.

One of the most basic and familiar forms of *out-there-ness* construction involves the use of what Nigel Gilbert and Michael Mulkay called empiricist discourse. This eschews constructions such as 'I found that . . .' in favour of 'it was found that . . .'. These are descriptions in a grammatical form that delete the description's producer. Another approach to producing *out-there-ness* involves constructing consensus and corroboration by presenting a description as shared across different producers, rather than being unique to one. Descriptions of this kind undercut attempts to discount them as the product of a particular person's stake or concerns. Detail and narrative work rather differently. They are involved in producing a version which is 'real' and vivid; they paint a scene as it might have been observed. One way to think of this is that they work to put the recipient of the description in the place of its producer. Indeed, one way of thinking about such descriptions is as providing an impression of remote sensing; they pull the recipient into the scene in the manner of a telescope viewer.

Steve Woolgar calls approaches to fact construction such as these *externalizing devices*. As he puts it, the 'externalizing device provides for the reading that the phenomenon described has an existence by virtue of actions beyond the realm of human agency' (1988b: 75). In effect, the description is of a thing (or action or whatever), and that thing exists, as described, without the describer's having any influence on it. Expectations about agency are moved from the producer of the factual account to the entity that is being constituted (see Figure 6.1). Let me emphasize strongly at this point that I am not assuming that people simply have agency and that they work to conceal it by various techniques. My concern with agency is as a participants' notion

which may be understood and reworked in a very wide range of different ways appropriate to particular settings (see Ashmore et al., 1994; Callon and Law, 1995).

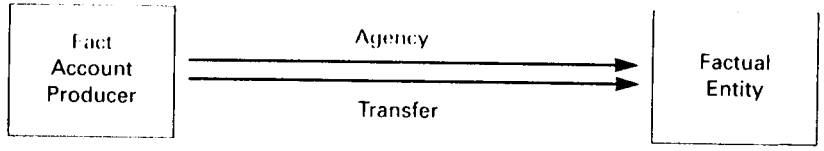

Figure 6.1 *Externalizing devices*

To illustrate this, let me turn Woolgar's idea of externalizing devices around and apply it to itself. In this text I am formulating [externalizing devices] as pre-existing objects. (The descriptive word *device* helps here, of course, as it is commonly used for physical gadgets that can be picked up, or put together.) As I describe this chapter as a *survey* of [externalizing devices], their treatment is made equivalent to doing a survey of the numbers and types of cars in a parking lot, say, or of the range of different uses of some farm land. It is dealing with pre-existing things that the surveyor just comes across. In these ways [externalizing devices] are being produced in this text as not something made up, or constituted, or argued for; not something which might be controversial, confused, a daft idea or simply non-existent; they are just there. They are, as it were, constructed as not being constructed. So, on with the device survey. . . .

Empiricist Discourse

The notion that there is a specific set of tropes, grammatical forms and argumentative styles that hang together comes originally from a sustained study of the discourse of a group of biochemists who were researching the way body cells store energy by Nigel Gilbert and Michael Mulkay (1984). They called this type of fact construction 'empiricist discourse' because many of its features are characteristic of traditional empiricism. Gilbert and Mulkay conducted, jointly, extended interviews with 34 members of this group. These interviews were conversational in the sense that they ranged across a set of topics in a fluid and informal manner. The main focus of study was transcripts of these interviews along with the scientists' research papers and letters.

Gilbert and Mulkay's work contrasts markedly with most of the studies of scientific knowledge discussed in Chapter 1. Unlike empirical relativists and social interest researchers, their aim was not to provide an account of what science is really like. For example, Gilbert and Mulkay did not try to decide

on the best model for how scientists select between different theories. Instead, they were concerned with how scientists constructed *accounts* of theory choice and, in particular, in the variations between such accounts. They noted, for example, that scientists typically offered one version of theory choice when describing their own theory selections, but a rather different one when criticizing as misguided choices the theory choices of competing scientists (Mulkay, 1991: ch. 10; Potter, 1984).

In effect, Gilbert and Mulkay's analysis extended the general emphasis on methodological relativism in sociology of scientific knowledge (SSK) to scientists' accounts of their actions and beliefs. Just as empirical relativists did not attempt to decide, say, which theory of neutrino production was correct (Pinch, 1986), discourse analysts of science did not attempt to judge which description that a scientist gave of theory choice was correct. They were not looking at accounts of choice in research papers, interviews or even scientific jokes, for their truth; the interest instead was in how that account was constructed and how it was used to manage specific interactional tasks.

The Empiricist Repertoire

The broadest level of analysis Gilbert and Mulkay carried out compared the way scientists described their actions, theories and experimental procedures in informal interviews with what they wrote in more formal contexts of research papers. They found systematic and widespread differences between the versions produced in these two settings, and they suggested that these differences are a consequence of scientists using two contrasting vocabularies or 'interpretative repertoires'. In this chapter the focus will be primarily on the empiricist repertoire.

Take the following examples. The first comes from the introduction to a research paper and the second from a methods section. Note that the technical details here are, well, technical – luckily it is not necessary to have a full understanding of them to get Gilbert and Mulkay's point.

> 1 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. $\rightarrow$ H$^+$/2e$^-$ ratio) has come mainly from one type of experiment.
>
> (Gilbert and Mulkay, 1984: 44)

> 2 Heavy beef heart mitochondria were prepared by the method of Wong and stored in liquid nitrogen. Well coupled mitochrondrial particles were prepared by a modification of the procedures of Madden. These particles were used to prepare inhibitor-protein-depleted particles by centrifuging under energised conditions according to the method of Gale.
>
> (Gilbert and Mulkay, 1984: 51)

Gilbert and Mulkay proposed that scientific papers have a coherent and distinctive set of linguistic and rhetorical features. These are clustered around three broad themes. First of all, papers draw on a grammatical form which minimizes the involvements or actions of the authors. The style is impersonal, using constructions such as 'the hypothesis proposed' or 'evidence has come mainly' which make no explicit mention of the authors' actions, interpretations and commitments. Second, data are treated as primary, both in the logical sense of forming the foundations of any theoretical ideas, and in the chronological sense of being identified before theory was developed from them. Indeed, verbs that might be expected to be applied to the actions of humans are often applied to data in this form of discourse: constructions such as 'these data suggest . . .' and 'the findings point to . . .' abound. Third, laboratory work is characterized in a strongly conventional manner as being constrained by rules which have a clear-cut and universal application. Methods sections construct a world of standard routines and analytic procedures ('centrifuging under energised conditions', 'the method of Gale').

These three features (grammatical impersonality, data primacy and universal procedural rules) are by no means restricted to 'hard' sciences. The biochemists studied by Gilbert and Mulkay seem to be doing science which is pretty hard when compared to social psychology, for example, but both disciplines share a widespread utilization of the empiricist repertoire, as a quick perusal of the mainstream journals will confirm.

Why does the formal writing of science use the empiricist repertoire? From the point of view of the smooth operation of science these sorts of constructions may have a positive consequence. Science is an institution where dispute is commonplace, and often a requirement of successful work. The pervasive use of the impersonal constructions of the empiricist repertoire may dampen this conflict down and reduce the possibility of the journals ending up full of personal attacks. Yet empiricist discourse is also bound up with the business of fact construction. The empiricist repertoire provides for descriptions of scientists' actions and beliefs which minimize the involvement of the scientist in constructing and interpreting what is studied. The scientist becomes passive, virtually a bystander, or evaporates altogether; while simultaneously the data take on a life of their own. They become rhetorically live actors, who can do suggesting, pointing, showing and implying. The empiricist repertoire is a standard device for constructing the out-there-ness of scientific phenomena.

Empiricist Discourse Beyond Science

One of the important questions for a project concerned with fact construction in general, rather than with construction only in science, is whether there are similar features to empiricist discourse outside of science. Are Gilbert and Mulkay documenting something specific to science? Or does empiricist discourse have a more widespread currency? To address these questions we need to look for other situations where this transfer of agency from author to phenomenon takes place.

First note, however, that even within science the empiricist repertoire only

appears in its fully fledged form in research papers and formal publications. Although Gilbert and Mulkay found a number of elements of the empiricist repertoire in their interviews, the grammatical impersonality – which is perhaps its most striking characteristic – was not widespread. The biochemists did not lace their interview talk with phrases such as 'it is believed that' or 'a study was conducted'. Such impersonal constructions might well have sounded peculiar in a situation where the speaker is physically and vividly present; although that is not to claim that such constructions will not appear when certain sorts of actions are performed or in certain sorts of face-to-face settings. For example, think of the formal speech associated with the British upper classes: 'one finds that the servants don't respect one as they used to'. However, Gilbert and Mulkay's analysis did not explore this level of specificity.

When considering whether the empiricist repertoire can be found in non-scientific situations two considerations are helpful. First, we can ask whether the different elements of the empiricist repertoire necessarily hang together outside of the domain of formal scientific writing. Second, we can ask whether some features of the empiricist repertoire mark out the institutional specificity of science. That is, do elements of the repertoire have a role in constituting what it is to do science (Drew and Heritage, 1992; Schegloff, 1991)?

Take the question of whether the empiricist repertoire is a coherent entity first. When Gilbert and Mulkay considered its role in scientific discourse, they did so in rather general terms, suggesting that it plays a central part in warranting scientific belief through giving primacy to the role of experimental findings and omitting references to personal or unique features of the scientists' beliefs or actions. As they put it, the 'great advantage of this form of account is that it makes the speaker's scientific conclusions appear entirely unproblematic and in need of no further support' (Gilbert and Mulkay, 1982: 400). There is a limitation in this style of argument, however, as Robin Wooffitt (1992) has pointed out. It tends to treat the empiricist repertoire as working both as a seamless whole, and at a level which is separable from individual instances of use. As we will see in a moment, it is possible to ask how specific elements of the repertoire are working in particular settings.

The second consideration is the question of the institutional specificity of science discourse. I will explore it by comparing Gilbert and Mulkay's work on science with a set of records of North American broadcast television and radio news. Transcripts of CNN, National Public Radio and so on are available on CD-ROM. This allows them to be searched for particular strings of words. This is a great help for doing large-scale searches for the prevalence of the sorts of constructions characteristic of the empiricist repertoire. A search through one year's output on CD-ROM came up with no examples of the sorts of present-tense impersonal formulations common in scientific writing: 'it is believed that', 'it is claimed that'. We can perhaps see why this is if we consider the difference between broadcast news and science with respect to the footing categories introduced in Chapter 5.

Scientific authors are in a complex and potentially tricky footing situation.

On the one hand, *they* have written the scientific articles which are presenting *their* research, *their* theories, *their* claims and so on. It is *they* who will be presented with the Nobel Prize, or perhaps accused of fraud; *they* are accountable for the contents of their papers as both principal and author. On the other hand, the empiricist repertoire is constructing the experimental data as an agent; *it* points in particular directions, *it* shows things, *it* leads to conclusions. Here the researcher maintains the role of author; but at the same time the data start to take on the role of principal. There is a potential tension, then, between these two tendencies, and impersonal constructions such as 'it is claimed that' may be a way of managing this tension. This is a construction which both implicates a responsible agent (the person or group who does the claiming) while avoiding a *direct* identification of the current writer or writers with that agent. The contrast will become clearer in the next section when I consider similar constructions in media contexts.

Constructions of Impersonality

News interview talk has a rather different pattern of footing from that in scientific papers. News interviewers are typically constructed as, and treated as, animators who are merely reporting the claims and views of others (Clayman, 1992; Heritage and Greatbatch, 1991). In general, broadcast news presenters do not get Pulitzer prizes and their everyday views and character can be at odds with their news persona – a theme played upon in the British comedy about a satellite news programme *Drop the Dead Donkey*, in which on-screen gravitas is contrasted with off-screen frivolity and slobbishness. Talk of this kind does not have the same requirement for impersonal present-tense constructions such as 'it is thought that'. And even where such constructions are used it is not likely that the news reader will be treated as the agent doing the believing. Past-tense empiricist constructions are much more common (see also Roeh and Nir, 1990).

Here is a specimen, with the empiricist construction highlighted. The context is the report of an attack on a 'tavern' in Cape Town prior to South Africa's first multi-ethnic election.

> 3 A caller has claimed responsibility on behalf of UPLAR, the military wing of the radical Pan Africanist Congress, but UPLAR's headquarters hasn't confirmed this. The weapons and tactics used are very similar to previous racially directed attacks. In July, five gunmen used grenades and [inaudible] rifles to attack a church service in a white suburb of Cape Town; 11 people were killed. That attack *was believed to be* the work of the UPLAR.
>
> (*National Public Radio*: 12 December 1993 – emphasis added)

This extract is part of a complex news story. However, it is not an unusual one; readers of this book will no doubt be familiar with news reporting of this kind. What I want to pick out particularly are the sorts of pragmatic considerations that occasion the empiricist construction 'was believed to be'.

The first thing to note is a similarity between news reports and scientific articles. Just as in science data are given meaning by theories, so news 'events'

are given meaning by interpreting them in frameworks of other events and embedding them in narratives (Tuchman, 1978). In this case, an attack on a tavern in South Africa, even with its details of death and drama, is only partial news. We expect stories to provide motive and background: who planted the bomb and why, and what is the response? With this story, the event has been linked to a previous attack, and this attack is linked to a political organization: the UPLAR. So one of the fact construction issues for this story to attend to is how these links are produced. The connection to the UPLAR is particularly interesting because it is not managed through citing evidence but through reporting a belief.

It is here that the report draws on an empiricist construction, for the belief is not attached to an agent or collective ('Kerry believed in discipline', 'Christians believed in God'). Instead it is free floating: 'that attack *was believed*'. In some ways it is rather odd to have a belief referenced without its 'owner'; we might think that it would be hard to recognize such a disembodied entity! However, this would be to try to understand beliefs within the rhetoric of cognitivism as objects with specific locations and therefore to miss the practical, interactional roles that such constructions serve (cf. Coulter, 1979).

One role for constructions of this kind is to avoid dealing with issues to do with the status of the claims made by news organizations. The official rhetoric of such organizations stresses both neutrality and an emphasis on reporting facts. As Gaye Tuchman (1978) documents in some detail, news organizations profess not to have beliefs and opinions; rather, they report facts, including facts about the beliefs and opinions of others.

This 'official story' about facts is *itself* a construction. For example, one of the common preoccupations of the television current-affairs film makers which we met over lunch in the last chapter was finding a set of informants for the film which would fit the pre-planned argument. The following extract displays this concern.

4 *Thompson*: So w- we know what he's going to do (.) in questions one to nine, we don't know what he's going to do in ten and eleven.
(1.8)
And that's the bits that we most (.) need him to be good on.
Chitty: Right.
(Tape 5: 2)

The point is that this careful procedure of selection and encouragement provides a finished film which appears to do the film maker's job of merely reporting on views rather than having views. The narrative appears to originate with the interviewees rather than the film makers.

Now, coming back to our current example, if we assume that the same sorts of considerations apply with National Provincial Radio's report of the tavern attack, we can see that reporting the involvement of the UPLAR as something believed by the news reporters or presenters will be problematic. Officially at least, it is not their job to be having beliefs.

Another way for the newscasters to deal with the issue of belief about UPLAR involvement is to attribute it to some individuals or organizations. This is what Clayman's work on footing might lead us to expect. However, there are two potential problems here. First, some of the relevant parties might well be seen, by newscasters and listeners, as having a stake in claiming UPLAR involvement. For example, the South African police are an obvious source of information about violence of this kind, but were widely felt (!) to be discredited as a source. And the official white politicians and officials who could have been quoted might have been seen to be making inferences based on stereotypes or simply as wishing to smear the Pan African Congress in a period leading up to an election. Second, the news sources might be fragile or limited in a whole range of ways: news reports rarely carry claims attributed to 'what another reporter told me in a bar', or 'something I rather indistinctly overheard'.

In the light of these considerations, we can see the potential value of the construction 'was believed to be'. For it reports the belief, which is needed to build the general news narrative, while avoiding the potentially problematic attributions either to the news organization or to interested or limited parties in South Africa. Without wanting to labour it further, the point is that the empiricist construction 'was believed to be' can be understood as more than just an element in a general vocabulary used to warrant facts. Rather, it can be seen as a specific design solution to a range of fact construction and warranting problems. It is not the only solution, of course – it is easy to imagine the news story in Extract 3 constructed in different ways – but it is one neat way of managing the combined concerns of factuality and accountability.

Agency and Evidence

In addition to grammatical impersonality, a cardinal feature of the empiricist repertoire is its attribution of agency to experimental data: 'the results show', 'the data support' and so on. A further search through a recent broadcast news CD-ROM was able to find recurrent constructions of a similar nature such as 'the record shows', 'the facts lead' and 'the evidence shows'. Here are three examples with the 'facts show' construction emphasized:

5 *Facts show that* there's no increase in drug use related to [needle-exchange] programmes and there will likely be a decrease in HIV infection because of them. Anybody who looks at these facts as objectively as we have, I think, will come to the same conclusions.
(CNN: 30 September 1993)

6 [Governor of Florida responding to the death of a British tourist] I think *the facts show very clearly* that this year we've had much less crime against tourists than we had the year before, than we had the year before that. Every- any act is an act too many.
(CNN: 18 September 1993)

7 [Responding to claims that 'Gangsta Rap' is anti-social] And why should we tell, you know, young youth that, well, you should go to school, and [success] is

what's going to happen, when *facts show that* that's not what's going to happen.
(CNN: 27 January 1994)

I have reproduced three examples here because they illustrate some shared characteristics in the use of this construction. It is notable that they do not come from the newscaster's own discourse; that is, they are not from news reports or from the newscaster's own contribution to discussion. In addition, they are from settings where there is a controversial issue, and where the speaker is potentially in a weak or minority opinion. The doctor talking about the virtue of needle-exchange schemes in Extract 5 is presented in the context of official condemnation of the policy. In Extract 6 the Governor of Florida is responding to questions about the well-publicized death of a British tourist in the state. In Extract 7 the speaker is defending the values of his brand of music, 'gangster rap', in the face of critical points from callers to a phone-in programme.

I have already noted that in situations of conflict in both scientific and everyday settings people will provide increasingly technical support for positions and be increasingly concerned with giving a basis to their claims (Latour, 1987; Pomerantz, 1984b). This form of empiricist discourse can be understood as an extension of this process. The support is built up by constructing the facts, the record, the evidence, as having its own agency. Such constructions obscure the work of interpretation and construction done by the description's producer: 'the facts' are, first, not being constructed as facts and, second, their significance is not being generated by their producer, it is provided by the facts themselves. They do their own showing.

The general point here, then, is that the empiricist repertoire embodies features of fact construction that are found beyond scientific settings. The sociologist Seven Yearley (1985) has made a similar argument about other features of the empiricist repertoire and the social psychologist Nigel Edley (1993) has provided a fascinating analysis of its use in news reporting concerning British royalty. Particular elements of the repertoire that have more general currency are constructions of impersonality ('it was believed') and fact agency ('facts show'). Rather than see these constructions as embedded in a repertoire that is used as a whole and with general consequences, as Gilbert and Mulkay (1984) argue is the case with science, I suggest that they are better thought of as having discrete uses with respect to practices of fact construction involving warranting and accountability. It will be interesting to investigate such uses further as well as to go on to explore the epistemic use of these forms in everyday discourse. In the meantime, the empiricist repertoire can be considered as a set of resources that may be drawn on when externalizing facts by *divesting* agency from fact constructors and *investing* it in facts.

Consensus and Corroboration

Quite a lot of space was devoted to technical issues related to the empiricist repertoire. However, the transfer of explicitly formulated agency from the speaker to the facts by broadly grammatical means is only one of a number of externalizing devices. Constructions of consensus and corroboration are two further, and closely related, externalizing devices.

One way of transforming a description into a fact is to produce the assent of reliable witnesses. Note that this is quite different from the superficially similar argument that some belief is true or justified because it is endorsed by many people, or the claim that some activity is acceptable because many people do it (cf. Hilton et al., 1988). The crucial feature of this kind of everyday reasoning about facts is that consensus in reports provides *corroboration* of the factuality of a version – if one witness to a car accident claims the driver was going too fast they may be discounted; however, if most or all witnesses claim this they are likely to be more convincing.

There are potential problems with consensus warranting of this kind, however, which may require the use of a second form of everyday warranting. Although witnesses to an event may agree because they all saw the same thing, the agreement may be produced in other ways. For example, they may have cooked up a story together or, more innocently, interacting with one another may have resulted in a common but flawed understanding of the event. This places a premium on finding witnesses who have not communicated with one another, or who are independent because their versions cannot be cross-contaminated. Thus this second form of fact construction stresses the *independence* of the holders of the consensual view.

The question, then, is how can consensus and corroboration be constructed? How can they be deployed as resources for fact building? I will focus particularly on two studies that we have encountered already from Dorothy Smith and Robin Wooffitt. They address this question in some detail.

Smith and Henny-penny

We have already considered features of Smith's study of an account which establishes the facticity of a girl's mental illness which relate to issues of interest management. Smith also discussed the construction of the account's objectivity via the use of independent witnesses. Indeed, the narrative structure of Angela's account of K's decline cumulatively adds a series of witnesses to her mental health problem in a way that Smith equates with children's stories such as Henny-penny, where an increasingly large band of people go to tell the king that the sky is falling. Some extracts will give a feel of this. Note the way the descriptions of K's (supposedly) problem behaviour are produced as coming from each of the witnesses.

First there is Angela, the teller of the tale:

8 We would go to the beach or pool on a hot day, and I would sort of dip in and lie in the sun, while K insisted that she had to swim 30 lengths.
(Smith 1990: 18)

And then there is Angela and Trudi:

9 . . . a mutual friend, Trudi who was majoring in English, had looked over one of her essays, and told me afterward: She writes like a 12 year old – I think there is something wrong with her.
(Smith 1990: 18)

And then there is Angela, Trudi and Angela's mother:

10 At that time Angela's mother thought, well she misunderstood me. But later she noticed that K was unable to put on a teapot cover correctly, she would not reverse the position to make it fit, but would simply keep slamming it down on the pot.
(Smith 1990: 19)

The cumulative adding in goes on through the narrative with the addition of Betty and a woman friend of the family.

As Smith notes, the effect here is of each of these witnesses coming *independently* to inspect K's behaviour, and each *independently* coming to the conclusion that K is not well. The consensual judgement that K is mentally ill is given independent corroboration. Of course, it might be objected to Smith's analysis that she is merely picking out features of a perfectly straightforward descriptive text. What is wrong with the claim that this account is a simple description which colourlessly reports various people's independent discovery of K's mental illness?

The first response to this is that, hopefully, at this stage in this book it should be clear that description is anything but simple and straightforward. However, Smith goes further than making the sorts of general claim that descriptions are selective, and involve categorization, that we saw in earlier chapters. She suggests that the independence of the various witnesses is carefully managed by the organization of the narrative, which cuts across a number of other, rather downplayed elements. For example, there are parts of the account which suggest that Angela and Trudi and others are in fact good friends, and may have been in regular contact with one another. Such features could be used to build alternative narratives in which K is not actually mentally ill, but instead her mental illness is a shared fiction which develops among the group of friends in the course of conflicts and jealousies. Indeed, Smith puts some effort into building up the plausibility of this alternative version of events. The point here is that the consensus and corroboration are being worked up to have their effect.

Wooffitt and Active Voicing

It is worth repeating that Robin Wooffitt's study of accounts of the paranormal trades on the scepticism with which they are commonly greeted. Because of this scepticism such accounts are likely to be designed to resist undermining, and this is why they can be such a revealing topic for study when the concern is with fact construction. One of Wooffitt's observations is that when people produce accounts of extraordinary events they often include sections of quoted speech. These are commonly marked out as such by shifts in intonation although they are not always explicitly named as such ('he said . . .'). For reasons that will become clear, he calls this *active voicing*.

Wooffitt identifies a number of specific uses of active voicing in establishing the factuality of some claim. The first is through providing corroboration. Take the following extract which follows a story about a strange experience the speaker's husband had with a particular hut while living abroad.

11 And, well, what is even more fascinating about the story is,
that he's telling the experience to other people and they said
→ 'Oh, that wasn't too strange an experience'
because they had heard it before from this particular hut.
(Wooffitt, 1992: 158)

This general way of establishing objectivity works by showing that different people have had the same experience or seen the same thing. Here this is constructed by providing a quotation which supposedly comes from a group of witnesses. The active voicing here confirms that there was something present in the situation which could also be experienced by other people. Moreover, it is not just the speaker's judgements that other people had experienced something from the hut – we have their own words to prove it. This is the beauty of active voicing – it brings into being separate corroborating actors who, like ventriloquist's dummies, seem to have life, opinions and personality of their own.

This extract also illustrates why Wooffitt wants to call this active voicing. Here the quote is not claimed to come from an individual; it is what *they* said. Unless we are to imagine a chorus of synchronized speaking, this makes its status as an actual quote unlikely. Rather, it may be taken to be emblematic, as the *kind* of thing that people said, or what they *would* have said, or a shortened version that is true to the *gist* or *spirit*. This shows that it is actively worked up as the voice of speakers. Although this is clear in the case of Extract 11, Wooffitt proposes that analysts should make the assumption that *all* such quotes are actively voiced or, looked at the other way round, that the analyst should not assume that words presented as quotes actually are quotes. As he puts it, it is 'useful to begin with the assumption that the speakers are designing certain utterances to be heard *as if* they were said at the time (1992: 161). If we think back to the discussion of footing in the previous chapter, Wooffitt can be seen to be exploring the rhetorical deployment of the animator/origin distinction.

There is another point worth noting about the construction in this extract. The voicing is plural: '*they* said'. This makes it easy to hear it as reporting a *general* experience of a *range* of people. (Remember, this is delivered in the fast and fluid flow of a conversation: everyday interaction does not work in the slow-motion manner of transcript that can be inspected and re-read at leisure.) However, without any details we do not know that it was not something said by only a small number of people – perhaps just two – and we don't know if they were independent; were they asked about the hut on separate occasions, or could some people be merely, and politely, backing up a single

speaker? (*We* don't, of course, know if there were *any* speakers!) The point is that the non-specific plural avoids these troubling difficulties with the account. It allows the inferences that there are both wide consensus and independence; but the speaker has not actually or explicitly claimed that many people heard the sound, nor that they are independent. She is not accountable, then, for others' creative inferences.

The general point is that, although the account is making these inferences available, they are not explicitly stated, and are therefore deniable. This is a common feature of factual account construction. For example, if we look back to Extract 3 – the news report on a South African attack – we can see that the anonymous formulation 'it was believed that' allows the inference that the belief is widespread, which contributes to its fact constructional role, without being explicitly committed to the existence of more than one believer.

Wooffitt's work on active voicing shows the broader significance of footing and the way various features of footing may be constructed to service the task of fact building. It also shows that the 'seeing the same thing', which is basic to this kind of lay reasoning about consensus and corroboration, is not something simple; rather, it can be managed and worked up. In fact, one example which vividly illustrates this point is the sociological work on replication in science discussed in Chapter 1. Replication can be thought of as an institutionalized forum for independent witnessing. As Harry Collins and others have demonstrated, what counts as a proper or competent replication often becomes as contentious as the finding that is to be replicated. If Wooffitt had been a parapsychological sceptic rather than a conversation researcher he would have questioned participants about the number and status of the witnesses to the noise. The ventriloquized witnesses would themselves have become a focus for dispute and come to need their own procedures of fact management.

Detail and Narrative

Back in Chapter 3 I discussed post-structuralist work on realist discourse, particularly Barthes's study of the operations of Balzac's 'realist' short story in *S/Z*. One of the main roles of this work was to show up the limitations of simple ideas about realist discourse and, in particular, the idea that realist discourse derives its sense from a simple sequence of descriptions of objects and events. What Barthes was less interested in was the role of descriptive prose in establishing the factuality of a text. Issues of factuality do not immediately come to mind when dealing with literature, which is a paradigm fictional form – although it turns out to be quite difficult to make clear-cut distinctions between truth in literature and other realms (Whiteside and Issacharoff, 1987; Searle, 1975). Nevertheless, a number of literary theorists have been interested in the way particular literary effects are generated and, more specifically, in the way a vivid realistic world is created. One of the central concerns here is the role of detailed descriptions and their placement in narratives. For our purposes, what is interesting is how such effects can be generalized to other texts and to talk.

Detail and Focalization

The first thing to consider here is what is meant when talking about detail. Clearly, this is a contrastive category. What is detailed from one perspective might be gross and vague from another. The sort of detail given about the structure of a steel girder by a nuclear physicist is of quite a different order from the sorts of detail given by an engineer. Moreover, the term *detail* is often used with pejorative overtones for relatively unimportant things; there is the big picture and the details. However, I will use the notion of detail in its dictionary sense of 'treating something in its individual particulars' (*OED*). Specifically, I want to contrast descriptions which gloss general processes and categories with descriptions that capture the particulars of scenes or events as they might be seen by an observer.

Take, for example, the extracts from scientific papers reproduced earlier (1 and 2). They drew on general sorts of descriptions of the actions of scientists. Yet even in the procedure sections of scientific papers, which are often claimed to provide a description of the actions of experimenters, the descriptions are of *generic* practices ('mitochrondrial particles were prepared by a modification of the procedures of Madden'); that is, they are 'anybody's' descriptions. We are not told whether the test tubes were cracked, whether it was raining outside, or whether the stalls in the lab toilet had doors. Their point is to show that something standard or universal was done; any individual or unique features were unimportant, and therefore not described. Contrast this with an extract of counselling talk where Jimmy the 'jealous husband', whom we met briefly at the end of Chapter 4, is talking about an evening in a pub. This is just a small part of a long passage.

12 *Jimmy*: U:m (.) when these people came in. (.) >It was:< (.)
John and Caroline. (1.0) And then they had- (.)
this other fella Dave. "With them as well."
[...]
U:m. (1.2) He c- he came- (.) they all came in the pub anyway.
(1.0) Well (.) Connie sat beside (0.6) Caroline. And I sat
(further back). So you was (.) you was split between us.
They sat in- on the other side. (1.0) The only words Connie
spoke to me (1.0) for the rest of the evening (0.8) was (.)
get another drink. "Get another drink."
(DE-JF:C2:S1:10)

In contrast to the extracts from the scientific paper, what is striking about this description is that it is full of specific references. These are not formulations of generic features of going to pubs, although a proper understanding of the extract might draw on knowledge of such features. Rather, it is full of definite characters (Caroline, Dave), indexicals ('you was split between us'), and active voicing ('get another drink').

To tease out some of the fact constructional issues in descriptions of this kind I want to draw on a concept from narratology known as *focalization*. This was developed in the work of Gerard Genette (1980) and Mieke Bal

(1985). The simplest way to think about focalization is in terms of the *point of view* which a narrative presents. For example, in some narratives there is an omniscient, God-like narrator who can peer round the back of any of the characters and swoop between scenes and into their thoughts. Genette calls this, perhaps confusingly, *zero* focalization. In other narratives the narrator views scenes, but has no access to the thoughts or feelings of individual characters; this is *external* focalization. The discourse in the extract above more closely corresponds to what Genette calls *internal* focalization. That is, the narrative is constructed from the point of view of an individual character – it accesses that character's thoughts and feelings, but not those of other characters except through inference. For example, here is an extract from later in Jimmy's account of the pub evening:

13 *Jimmy*: Uh: I was (.) boiling at this stage and I was real angry
with Connie (.) And uh went up to bed 'n (.)
I lay on the bed. (0.7) °got into bed.° (0.6)
I- uh (.) could hear giggling ('n all that) downstairs
and then (.) the music changed (.) slow records.
(DE-JF:C2:S1:11)

Jimmy describes his own feelings as with a simple report which requires no inference. In contrast, this description allows the actions and feelings of others to be only indirectly inferred. He describes the sounds which are available to him from his bedroom. These reports of sounds are not meaningless behavioural particulars. They allow precise inferences to be made about the activities downstairs. Even without the rich contextual detailing that has gone on in the prior 80 or so lines of the account, I imagine that it is hard to resist the implication that something intimate and potentially sexual is being overheard. Even if we do not know the etymological association of giggle ('a lewd wanton woman; a giddy romping girl' – *OED*), giggling is not a characteristic of interaction between strangers or in formal situations; it signals a certain informality, shared jokes. The change to slow records is, of course, associated with a shift to close, intimate dancing such as might happen when people pair off at the end of a disco; or, from a slightly earlier era Chuck Berry sang 'but when the sun went down the rapid tempo of the music fell, c'est la vie say the old folks it goes to show you never can tell'.

The point, then, is that internal focalization is a narrative style which presents what goes on from what might loosely be called the perceptual field of an individual participant. In these extracts we can see it attending to both the position of the perceiver ('and I sat . . . so you was split between us', 'I lay on the bed') and what is perceived ('get a drink', the giggling). Such narratives allow the listener, or reader, to take on the position of the perceiver. They can understand things through the eyes or ears of the central character. In her discussion of focalization Bal indicates the link to issues of fact construction: 'If the focalizor coincides with a character, *that character will have a technical advantage over the other characters*. The reader watches with the character's eyes and will, in principle, *be inclined to accept* the vision presented by that character (Bal, 1985: 104 – emphasis added). Away from the purely literary arena, this kind of focalization goes with a special kind of category entitlement. The person is entitled to provide an authoritative description of a scene or event because he or she is a *witness*.

Witness as a Category Entitlement

As with other category entitlements, we can ask how the category entitlement of witness is built up and how it is undermined. What is its defensive and offensive rhetoric? One way in which the category 'witness' is established is to provide graphic, vivid descriptions. These are the sorts of descriptions that might be derived from a careful viewing of a scene, and they may have features that might seem hard to make up because of their specificity, perhaps, or their oddness. For example, direct quotation is the kind of thing that only a witness can properly report. It not only shows that the witness was present, but that they have powers of observation. Take 'get a drink' in Extract 12. It certainly does some business with respect to the moral identities of the parties to counselling, perhaps displaying Connie as insensitive or engrossed in her interaction with the other man. Yet it also displays Jimmy as a witness who is reporting an actual event rather than inventing or speculating, and, moreover, a witness who can report precise details (see also Juhila, 1995).

Another feature of establishing an identity as a witness involves establishing access to the witnessed scene. This both authorizes the presence of the witness and also aligns the reader or listener with the witness for the narrative. Paul Atkinson (1990) has explored this rather neatly in his study of the construction of social science ethnographies. He compared the introductory paragraph of a short story by Hemingway and a well-known ethnography about the life of cocktail waitresses. And he suggests that they bring the reader into the story in much the same way, by providing the sorts of external, as-perceived description that we saw in Jimmy's narrative above, and by building up a contrast between the inside and outside through apparently gratuitous references to the weather ('outside it was getting dark', 'outside a light rain gives softness to the night air of the city'). This kind of description, Atkinson suggests, 'furnishes the "guarantee" of an eyewitness report, couched in terms of the dispassionate observer, using the conventional style of the realist writer of fiction, or documentary reporter' (1990: 70). Jimmy's narrative does not have any references to the weather in it, but it does spend some time establishing why they went to the pub, who was there and where they were sitting.

One of the effective features of witnessing as an identity is that the witnesses' description is a report of the scene as perceived as opposed to being a broad formulation or interpretation of events. It provides the details that were seen or heard and allows the recipient of the description to make the inferences. The role of judging and evaluating is seemingly passed on to the recipient. For example, in Extract 13 Jimmy does not directly accuse Connie of being unfaithful to him – but his description is organized to make that

inference hard to resist. In this way the category witness works as an externalizing device.

For most of this chapter I have considered externalizing devices which draw attention *away* from the producer of the description, and therefore their potentially problematic stake and interest in events, either by using impersonal empiricist discourse or by emphasizing corroboration and consensus across observers. In the case of the category witness, however, the externalizing is done by working with an implied distinction between doing observation and evaluation: describing the facts and saying what they mean.

The importance of the everyday distinction between describing and inferring is shown in Dorothy Smith's study of the mental illness account, and the way it is organized to allow recipients to make up their own minds. Maria Wowk's (1984) study of a murder confession makes a similar point. In the example she studied, the assailant did not make a direct accusation that the victim was a prostitute rather than an 'innocent victim'. Such a direct claim might well have been treated as interested, and as an attempt to mitigate the crime. Instead, he constructs a description from which the category 'prostitute' can be inferred.

This lay distinction between witnessing and inferring may be what came into play in a study by Kim Scheppele (1994), who noted particular difficulties for women victims of rape and sexual abuse who revise their stories over a period of time. Such revisions are exploited by the prosecution, who trade on the assumption that initial versions are likely to be accurate, witnessed descriptions while later changes are likely to be distorted or motivated in some way. In contrast to this, Scheppele suggested that the standard expectations can be inverted in such cases because of the sorts of psychological strategies of denial and self-blame that victims of sexual violence engage in which involve an initial avoidance of the reality of abuse. On this topic, and its relation to fact construction more generally, see Michele Davies' (1995) study of fact construction in an autobiography concerning repressed memories of child sexual abuse. This considers both the issue of witness and memory, and the more general notion of legitimating knowledge via experience (cf. Kitzinger, 1994; Manzo 1993).

Undermining Detail and Turning to Vagueness

Although the detailed description of particulars can be used to build up a category entitlement as a witness, it can be countered in a variety of ways. Indeed, any established approach to fact construction may be expected to have its established counters. The use of offensive rhetorics stimulates the development of defensive rhetorics and vice versa. One feature of detail is that it can be inspected for contradictions and confusions or provide material that can be reworked into a different kind of narrative entirely. For example, the fact that Smith can re-read Angela's account of K's mental illness and rebuild the supposedly independent witnesses as a connected group, who may have their own interests in painting K as ill, depends on her reworking of the provided detail. Although the rich detail allows the reader to 'make up her own mind', it also allows her to intervene in the story actively and invert its moral force, leaving K a victim of persecution rather than a psychiatric case.

This procedure of unravelling details has a distinguished past. James Herrick studied the rhetorical approach used by eighteenth-century Deists when criticizing Christian accounts of miracles. He suggested that the principal form of attack was a form of ridicule, which worked through a close reading of Christian texts searching for elements that were problematic, unclear or 'laughable'. These became the focus of an intensive, often exaggerated discussion, which typically removed the details from their original context. For example, Herrick quotes an attack which concentrates on the phrase 'flow of blood' in a description of Jesus healing a bleeding woman.

> Neither one nor the other of the Evangelists signify of what degree her haemorrhage was, nor from what part of her body it proceeded, nor how often or seldom she was addicted to it. It might be, for aught we know, only a little bleeding of the nose, that now and then she was subject to: Or it might be an obnoxiousness to an evacuation of blood by siege of urine: Or it was, not improbably, of the menstrous kind. Any of these might be the case of this woman for what's written; and I don't find any of our divines have determined of what sort it was. But a great miracle is wrought, they think, in her cure, without knowing the disease. (Cited in Herrick, 1989: 322–3)

The approach, then, was to work with potentially minor problems or details, and build them up so that they raised questions with the whole account. From a rather different perspective Malcolm Ashmore (1993) has shown how a combination of narrative detail and ridicule can be used to undermine an established scientific theory.

Let us now return to Jimmy's narrative of the traumatic evening in the pub. We can see the process of undermining at work in Connie's immediate response. (Remember, this is a counselling session, and both Connie and the counsellor are present to hear Jimmy's narrative.) Connie's immediate reply emphasizes the flexibility in the production of versions of events, and how they can be produced to show different things (cf. Simons, 1989).

14 *Connie*: I'd just like to say: when I te:ll that sa:me story (.)
that same story sounds very very different.
(DE-JF:C2:S1:11)

And she goes on to rework details of Jimmy's account; for example, recharacterizing an event that Jimmy has presented as her pulling her skirt up to display her legs to a boy, Dave.

15 *Connie*: My skirt probably went up to about there.
((*Jimmy gives a sharp intake of breath.*))
Maybe a bit shorter. It was done for no- I never looked at
that particular bloke when I did it it was my friend commented
Oh you're showing o:ff a lot o' leg tonight.
(DE-JF:C2:S1:11)

Detail of this kind, then, is a double-edged rhetorical weapon. It can be used to build up a category entitlement as witness, as well as do a range of specific actions, but it can also be reworked, undermined and ridiculed. In effect, the same story can be retold to sound different. For this reason, factual accounts may be constructed using vague or global formulations. Global formulations may be an important element in the armoury of defensive rhetoric.

One example of the rhetorical use of vagueness is provided by Paul Drew and Elizabeth Holt's (1989) study of the use of 'idiomatic expressions'; that is, clichéd or proverbial expressions such as 'it takes two to tango' or 'between a rock and a hard place'. They suggest that such expressions are not sprinkled randomly through conversations. Rather, they tend to crop up at specific junctures. For instance, one common situation in which they appear is where someone is complaining about something to a friend or relative, and that person is withholding support or agreement. This is illustrated in the following extract, in which Ilene is complaining about the actions of a company:

16 *Ilene*: .hhh We've checked now on all the papers 'e has an' Moss'n Comp'ny said they were sent through the post we have had n:nothing from Moss'n Comp'ny through the post.
(0.3)
Ilene: Anyway. (.) That's th- uh you know you can't (.) argue ih it's like (.) uh: [m
Shirley: [Well
(.)
Ilene: banging yer head against a brick wa:ll
(Drew and Holt, 1989: 508)

If Shirley had been supporting Ilene's complaint we would have expected her to express that support at various points in this interaction. However, her only interjection here is 'well', which, as we saw in Chapter 3, is typically an indication that some dispreferred or disaffiliative action is likely to be produced (Levinson, 1983; Nofsinger, 1991; Shiffrin, 1987). It is at this point that Ilene produces the idiomatic expression 'banging yer head against a brick wall'. Drew and Holt suggest that such expressions have two roles. First, they tend to terminate or round off the sequence, and possibly change topic (cf. Drew and Holt, forthcoming). Second, because of their figurative or formulaic quality they are robust. In other words, they are not easy to challenge with specific facts or information. That means they are suited to situations where there is conflict, or at least lack of support. The vagueness here is not a weakness; it is a virtue. Banging yer head against a brick wall can be 'right', or descriptive, in all sorts of situations and in all sorts of ways. It takes work to undermine.

Vague or broad glosses do not have to be idiomatic, of course. For example, I spent some time discussing Connie's version of the evening in the pub at the end of Chapter 4. She formulated it as 'an episode, with a bloke, in a pub, y'know? And me having a few drinks and messin'.' This description does not have the internal focalization of Jimmy's narrative. It has no narrative point of view, it reports no individual perceptions or emotions; instead it provides a broad categorization of the event. The innocence of messin' contrasts with, and works against, what is implied by the giggles and slow music from downstairs in Extract 13. This example, then, shows a clash of two rather different fact constructional practices, each with its own strengths and shortcomings.

Hayden White, Oliver North and Narrative Warranting

The notion of narrative has become increasingly prominent in the social sciences in the past few years. It has been proposed as a central organizing concept for psychology (Bruner, 1990; Gergen, 1994; Polkinghorne, 1988; Sarbin, 1986) and is seen as central in ethnography (Atkinson, 1990; Clifford and Marcus, 1986), and other areas. As we saw in Chapter 3, narrative is treated as epistemologically central in some postmodernist arguments, particularly those of Jean-François Lyotard concerning the breakdown of grand narratives of legitimation. All of this has led to narrative being used in a range of often rather loose senses. Sometimes it becomes difficult to see what is *not* narrative. Nevertheless, there are important strands of work here related to fact construction, and the thinker who has probably done most to address them is the history theorist Hayden White.

White is an ambitious and subtle theorist; yet, the central theme in his work can be easily stated. His argument is that it is a mistake to see history making as the collection of facts about the past. Rather, history making is a combination of fact finding and producing narratives that give those facts sense. It is about producing coherence as well as correspondence. As he put it:

> Every history must meet standards of coherence no less than correspondence if it is to pass as a plausible account of 'the way things really were'. [. . .] A mere list of confirmable singular existential statements does not add up to an account of reality if there is not some coherence, logical or aesthetic, connecting them to one another. (White, 1978: 122)

The argument, then, is that plausible, believable accounts of the past are produced by placing facts within a narrative. White's (1973) major work was an attempt to characterize the major historians of the nineteenth century according to the style of narrative they favoured (romantic, tragic, comic, satirical), their basic modes of argument (contextualist, mechanistic) and the ideological implications that are drawn from their histories (radical, conservative). The narrative forms of history were treated as lying underneath the surface of historical texts as a 'deep structure'.

The analogy with the Chomskyan linguistics that was dominant at the time is very clear. In Chomsky's work utterances were taken to follow grammatical forms as a consequence of an underlying 'deep structure' which is 'hard wired' in to human beings. Both Chomsky and White characterize these deep structures as part of the psychological endowment of members of a culture.

> The historian shares with his audience *general notions* of the forms that significant human situations must take by virtue of his participation in the specific process of

> sense making which identify him as a member of one cultural endowment rather than another.[. . .] The original strangeness, mystery, or exoticism of the events is dispelled and they take on a familiar aspect, not in their details, but in their functions as elements of a familiar kind of configuration.[. . .] They are familiarized, not only because the reader now has more *information* about the events, but also because he has been shown how the data conform to an *icon* of a comprehensible finished process, a plot structure with which he is familiar as a part of his cultural endowment. (White, 1978: 86)

White is writing more about understanding than fact construction, but the argument extends easily enough. An account of the past comes to seem factual as it draws on a narrative form that is part of the reader's cultural competence. She reads the history and experiences it as factual, because it conforms to her narrative expectations. It seems 'right', 'well formed', 'coherent'.

This argument has some plausibility to it. Or, as White might say, it provides an initially credible narrative of the workings of historical understanding. There is even some experimental research that broadly supports it (Bennet and Feldman, 1981). However, it also has some potential problems. Apart from the now well-established difficulties with the cognitivist deep structural notions about understanding (what is the status of this deep structure?; how does it work?), White has developed the argument rather more in the abstract than through detailed analyses of particular examples. Specific studies have attempted to apply this general idea about narrative to sets of texts. For example, Moya Ann Ball (1991) explored the narrative construction of 'the Gulf of Tonkin incident', arguing that it was framed as a specific and familiar narrative to justify increased US involvement in the Vietnam War. And John Sorenson (1991) studied media constructions of famine in the Horn of Africa suggesting that a familiar ideological parable was reproduced in reports. Yet, neither the presence of a single, coherent narrative, nor its role in fact construction is well established in these and similar studies.

Another difficulty with White's account is that it seems to depend on a distinction between the historical facts and the narratives they are woven into. I write 'seems' here because at times White offers a more constitutive account which has 'the facts' being constituted by the narrative rather than being pre-existing objects which are subsequently organized into narratives. Here is White at one of his more constitutive moments: 'tropics is the process by which all discourse constitutes the objects which it pretends only to describe realistically and to analyze objectively' (1978: 2). Any one of a range of the theoretical perspectives discussed in the first three chapters of this book could be used to raise searching questions with the facts/narratives distinction.

A further twist to arguments about historical facts and narrative interpretations is given by Michael Lynch and David Bogen in a study of Oliver North and his testimony to the Iran-Contra hearings. They were particularly interested in the policy of generating 'plausible deniability', which was the objective of a range of practices in the US intelligence community. The point of this policy was to be able to conduct covert operations in such a way that

the sediment of documents and official recordings left by the operation would allow it to be officially denied and, moreover, that the denial would be plausible because it would mesh with the records. For example, Admiral Poindexter cited the policy of plausible deniability as the rationale for taking full responsibility for diverting money from covert arms sales to Iran to the Contras; by taking responsibility he protected the President. Lynch and Bogen claim that 'in his testimony before the committee, he asserted that he specifically withheld the authorizing documents from the President in order to give Reagan "deniability" in case the diversion should become public' (Lynch and Bogen, 1996: ms. 8; see also Bogen and Lynch, 1989).

Lynch and Bogen use the Iran-Contra hearings to make a point about interpreting historical records. They suggest that the way historical records were practically reconstructed in the hearings provides a broader lesson about history and interpretation.

> Over the course of these hearings it became clear that the historical archive was itself a product of organizational work – of collecting, assembling and deleting files, retrieving documents or shredding them, coding and recoding messages, and the like. This circumstance suggests, in turn, the following as a general, rather diabolical property of the historical imagination: that it not only involves interpretations of evidence, but that the evidence itself is suffused with the workings of an historical sensibility. (1996)

Rather than there being neutral, historical facts which historians organize into narratives, here the picture is of the documents that supposedly record historical facts being generated and selected precisely to sustain particular (fictional) narratives. Indeed, for Lynch and Bogen there is something fundamentally postmodern about North's approach to history. In working closely and practically with historical materials he blurs distinctions between the straightforwardly factual and the artfully fictional, and between the literal and the ironic. He has moved – for quite different purposes – into the kind of historical terrain populated by David Byrne and his 'history' of Virgil Texas in *True Stories*.

Lynch and Bogen draw heavily on ethnomethodological thinking, and their study of the Iran-Contra hearings fits closely in with the tradition of work I discussed in Chapter 2 on organizational practices of fact making. Their caution about the historical archive being designed to fit particular narrative reconstructions parallels Max Atkinson's (1978) argument about the embedding of theories of suicide in the operation of coroners' courts and their construction of suicide statistics.

General Narratives and Specific Descriptions

The discussion above leaves rather tarnished the originally simple picture of factual accounts being warranted by placing them in a set of fundamental or familiar narrative forms. However, it does not show that narrative is irrelevant to issues of fact construction; far from it. One of the difficulties is with the different senses in which the notion of narrative is used. It is useful to contrast

narrative in the sense of broad literary forms or genres (comedy, detective story and so on) with the idea that versions of events can be produced using different kinds of narrative conventions (such as zero versus internal focalization) and in ways that relate to issues of motive and character. Producing narratives involves choices also of where to start and where to finish, what to include and what to leave out, what to put next to what, and so on.

This raises a range of general issues concerning the importance of the control and management of versions. For example, one of the most significant features of Smith's study of Angela's narrative construction of K's mental illness is that K is not present to intervene in the account, to provide alternative versions, to add other things that happened which provide a new context to events, or to simply deny that some of the claimed events took place. Studies of interaction in official hearings and courtrooms emphasize the importance of controlling where an answer starts and stops and what counts as a complete answer (Molotch and Boden, 1985). For example, in another study of Oliver North's testimony in the Iran-Contra hearing, Tim Halkowski (1992) showed how the Committee counsel effectively managed the witness's version by treating some of his answers as incomplete and in need of elaboration, and cutting off some attempts at elaboration.

Anita Pomerantz (1988/89) provides another example of the narrative management of versions in a study of a news report of President George Bush's involvement with smuggling aid to the Contras using laundered drug money. (The Contras may have been politically poisonous, but they have inadvertently been a boon to social research on fact construction!) The report details a minor Bush associate's CIA involvement using 'documentary evidence' and then shows a clip of this person denying any connection to the CIA. Pomerantz argues that the audience is primed to be sceptical by the report's factual assertion of the man's CIA involvement – and the immediately subsequent denial becomes further evidence of guilt. The effect of this report is to show up the man as not only part of the covert CIA operation but also a barefaced liar. Part of the effectiveness here is that the news team are in full control of their material, so they can juxtapose the contrasting versions as well as assembling a display of evidence in support of one of them. The point may seem obvious, but this is an important facet of fact construction which is well worth explicating.

To end this discussion of narrative and fact construction, let us return to Connie and Jimmy and their disputed evening in the pub. One of the important points to remember is that, like Balzac's clock on the Elysée-Bourbon, it is not the empirical particulars that are important. In this case, the point of the narrative is what it shows about the nature of the parties, and particularly their moral identities. Is Connie a hopeless flirt who would drive any partner to distraction? Or is Jimmy painfully and pathologically jealous and likely to blow up any incident out of all proportion? Jimmy's elaborate narrative, with its inexorable build of leg displaying, wilful ignoring, insensitive drink scrounging, and finally intimate slow dancing with another man, builds a powerful warrant for the version that has Connie as a hopeless flirt or worse.

In terms of counselling, Jimmy is trying to single her out as the one with the problem (for a fuller account, see Edwards, 1995).

The point I wish to emphasize is that Jimmy's work of identifying Connie in this way is a narrative construction. Yet it is not the sort of basic narrative genre discussed by Hayden White and others (see Gergen, 1994). It is not particularly comic nor is it especially tragic, despite a description of an abortive attempt at suicide towards its end. Rather, it is an organized and cumulative set of descriptions which focalize Jimmy and make his construction of events believable and understandable. Put another way, I am suggesting here that its status as a narrative of a particular genre is not, in itself, crucial for either the fact constructional role of this discourse or its specific action orientation. Instead, I suggest that 'narrative' should be thought of as a rather loose preliminary category that usefully collects together a range of disparate but important discursive phenomena.

Truth Is Stranger than Fiction

The well-known cliché has it that truth is stranger than fiction. However, the sorts of issues raised in this chapter suggest that there is no neat separation between the tropes of fact and fiction. Often, the resources for building vivid and plausible fictions are precisely the same resources that are used for building credible facts. This raises many interesting questions about relations between literary representations and representational practices in realms such as courtrooms and everyday talk. Is one parasitic upon the other, for example? Are the sorts of historical changes in conceptions of the real in literary texts documented by Erich Auerbach (1957) related to changes in other institutional forms, or perhaps to changing conceptions of self? These are hard questions to address, and up to now there has been very little work on the poetic and narratological features of everyday talk that could be used to even start to make sensible comparisons with literary studies (although Harvey Sacks, 1992, makes a range of rich and suggestive observations on this topic). For example, a more systematic study of the kinds of focalization that occur in everyday talk and news interview talk could be particularly revealing. Jimmy's story was internally focalized; in what sort of situations are narratives with zero focalization using an omniscient, God-like point of view used?

Despite some limitations in Hayden White's account of narrative, his emphasis on the dual importance of the correspondence and coherence of historical accounts has a wider currency. It is not difficult, for example, to find cases where 'getting things wrong' leads to an account being treated as *more* rather than less plausible; that is, where coherence, in some broad sense, triumphs over correspondence. In some recent cases of wrongful imprisonment in Britain the 'uncanny' similarity of different police versions of what happened in the cells was used as evidence that they were carefully rehearsed confabulations. The argument was that 'real testimony' has contradictions

and confusions; all the officers describing events in the same way was 'too good to be true', much more likely to be the rehearsed outcome of conspiracy than spontaneous personal remembering (cf. Chancellor Lawson's discounting of consensual newspaper reports – pages 116–18 above).

A newspaper article about the poet Philip Larkin's meeting with Prime Minister Thatcher illustrates a more complex use of the same idea:

> He had been introduced to her once before, at a reception in Downing Street in 1980, and liked to tell the story that as she welcomed him she said: 'Oh, Dr Larkin, I am great admirer of your poems.' 'Quote me a line, then,' [. . .] Larkin says that Mrs Thatcher misquoted the line: 'Her mind was full of knives' 'I took that as a great compliment' [. . .]. '*I thought if it weren't spontaneous, she'd have got it right*. I also thought she might think a mind full of knives rather along her own lines, not that I don't kiss the ground she treads.' (*Independent on Sunday*, 3 July, 1994 emphasis added)

Note the way that Larkin is reported to have used Mrs Thatcher's failure to get the quote precisely right as an indication that she actually knew the poem, but was having to recover it from memory. That is, her mistake was treated as evidence of genuine recall rather than the sort of too pat correctness which would go with a recent briefing (cf. Lynch and Bogen, 1996; Edwards and Potter, 1992).

This raises the theme of lay notions of memory and their relation to fact construction. For detailed descriptions to work as mere rememberings, and thus as externalizing devices, an image of memory as a neutral storage space from which memories can be dumped is most effective. The facts are loaded into memory via eyes and ears and dumped back at a later time untouched by interest, expectation or desire. The conversation analytic account of the way talk is constructed undermines this image. Rather than being passively dumped, it suggests that versions are highly patterned in their detail for the performance of action. Take Jimmy's narrative, for instance. At a superficial hearing it might seem like a simple record of the events of the evening: a dump. Yet, as we start to examine the detail that is placed in this narrative we can see that it is highly selective and carefully organized. What is in the narrative is there for its role in building a moral identity for himself and Connie.

I started the chapter with a discussion of Gilbert and Mulkay's notion of an empiricist repertoire. In their original conception, the empiricist repertoire is an integrated vocabulary of terms, explanatory moves, and metaphors, all used with a consistent grammatical style such that the author's involvement in the research paper is minimized and the data themselves are given maximum power over their own interpretation. The empiricist repertoire, then, is a systematic accounting procedure for externalizing. I considered the question of how general the empiricist repertoire is by examining empiricist tropes in news broadcasts. In this setting they do not cohere together into a full repertoire but are embedded in a range of other kinds of discourse where they perform specific tasks such as making assertions without being explicit about their sources or generality, or emphasizing the credibility of claims. This opens up a potentially fruitful avenue of study which would consider the use of empiricist tropes in a whole range of non-scientific settings.

The next theme in the chapter was consensus and corroboration. The centrality of corroboration in the appraisal of factuality is well established, and embedded in institutional procedures for factual evaluation such as the use of multiple witnesses in court cases and the emphasis on replication in discussions of scientific research methods. The emphasis here was on the way consensus and corroboration can be worked up in particular settings. There are a wide range of ways in which corroboration can be built; some more explicit; some less so. I concentrated on Smith's study of the construction of independent witnessing in her paper on K's putative illness and Wooffitt's analysis of the use of active voicing to ventriloquize a range of corroborating personages. Manufacturing consensus and corroboration is potentially a particularly strong form of externalizing as it shares responsibility for the factual account with other agents.

The final part of the paper focused on detail and narrative. Detail works on a number of levels. At its simplest, the provision of detail can offer a vivid representation of a scene or event; one which will perhaps be seen as unlikely to have been invented. Detail may be organized into an internally focalized narrative to present events from the point of view of a participant and thereby to build a special category entitlement of the speaker as a witness. Moreover, detail could be organized in ways that mirror literary techniques for drawing the listener or reader into the narrative. The rest of this section concentrated on some of the issues raised in relating narratives to fact construction and some of the vulnerabilities which arise when providing rich detail in accounts.

So where are we? At this point in the book we have finished our exploration of what I have called the epistemological orientation of factual accounts; the ways in which they are built as credible and factual. Given that an account has been established as factual by the appropriate management of interests and entitlements, combined with relevant externalizing techniques, how are such accounts designed to perform particular actions? Are there regular procedures for doing common actions? In the next chapter I want to move on to the complicated but fascinating topic of the action orientation of factual accounts.