
On the Scientific Status of Interpretive Inquiry

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Abstract

Interpretive social science is well established institutionally at universities and research centres. It benefits from this institutional context in terms of prestige, credibility and grants. In comparison with non-interpretive disciplines however, its scientific status is questionable. What elements of it are really scientific and what elements are threats to this scientific character? This problem has been discussed in the past but unfortunately the discussion has gradually dried up without a successful resolution. In my thesis I am revitalising it.

I take a systematic rather than historical approach: instead of picking up the discussion where it has been abandoned, I begin with a working definition of science, and analyse to what extent interpretive inquiry meets the requirements of this definition. The structure of my thesis follows this definition in that what is discussed is the three substantial elements of it - theory, research method, and professional quality control. In relation to theory, I pose questions on a range of topics, such as whether interpretive social science is explanatory, and whether it generates new knowledge. In relation to method, I explore, amongst other things, whether qualitative method permits the production of valid and reliable findings. The discussion of professional quality control considers issues around the reporting of findings and the assessment of these findings by others.

I complement my analysis by considering three interpretive case studies, exploring both whether they produce theoretical knowledge and reflecting on their methodological strengths and weaknesses. Additionally, I explore the border between interpretive inquiry and non-fiction arts, such as literary reportage and documentary filmmaking, arguing that this border is more blurred than it may first appear.

Declaration:

The research contained within this thesis is my own work.

Filip Sosenko

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Chapter 1

Introduction

1.1 Problem definition

1.1.1 Interpretivism and social 'science'

The motivating basis of my dissertation is the belief that the concept of 'social science' has been being diluted for many decades, and that to revitalise discussion of the scientific or non-scientific character of the social sciences is not a superfluous project. On the contrary, it means addressing the central question for these disciplines.

The notion of 'science' acquired its meaning before the idea of studying *society* in a scientific manner was born. Social scientists cannot deny this fact and therefore should either accept it and fulfil the requirements for being granted the title of 'scientist', or reject these requirements and the whole concept of science. It is somewhat disingenuous to create a diversion by calling oneself a (social) scientist without actually doing any science. This last option means either illegitimate destruction of the original meaning of 'science', or having a parasitic relationship to it. Although I believe the meaning of 'science' is negotiable – like any other human activity, it has no clear-cut boundaries - it is not an 'open' concept into which one can shove anything one wishes. Gathering incommensurable things under this name is a violation of the concept's coherence, diluting its meaning to a state of near meaninglessness.

From the beginnings of Sociology thinkers have been aware of this issue and have taken pains to supplement substantial discourse ('what is society like?') with the meta-scientific one ('what is the status of our inquiry?'; 'in what sense is our inquiry scientific?'). But, as I have said, natural scientists had already defined science when

the social sciences began to emerge. Therefore the history of Sociology turned out to be the history of attempts at reconciling the requirements of 'scientificity' with studying a social world that seemed to have characteristics that were importantly different to those of the natural world. This tension tended to be relieved either by distorting the nature of the social world (by removing meaning and agency from it), or by revising the meaning of 'science'.

In relation to the first of these, Comte, Mill and Spencer's meta-scientific propositions remained underdeveloped as they could not resolve the tension between their wish that the new science use naturalistic methods, and their consciousness that they could not be the same as the methods used already in natural sciences. It took a few decades until Durkheim managed to state clearly what the sociological method should be like and, more importantly, appeared to have empirical success in *Suicide*. Since then, using quantitative measurement and statistical correlations has been the backbone of 'scientific' sociological research and explanation. In summary, this way of thinking about society and of doing research is not far away from the natural-scientific understanding of what 'science' should be like.

But as it turned out later, the positivistic program of sociology paid a price for its strategy of adjusting to the existing understanding of 'science'. This price was the distortion of the nature of the social world and the nature of human action, since statistical correlation of external indicators bypasses the actual consciousness of the actors, the meanings and intentions in their minds.

My dissertation is concerned with a second strategy, that is, to revise the meaning of 'science', which was commonly adopted by those thinkers generally known as 'interpretivists'. Even at the time of the naturalistic 'founding fathers' of Sociology, some thinkers began to focus on issues of meaning and agency. Importantly, they defined themselves as anti-sociologists (Szacki 1979: 316), which testifies to their doubts about their status. They were historically or pragmatically minded, and therefore aware of the distinctive character of the social world. Nowadays they are considered to be fully-fledged sociologists, or at least 'social scientists'.

This interpretivist approach had two sources. The first one was European (Dilthey, Rickert, Simmel, Tönnies, Weber, and later Schutz). It is not incidental that the names mentioned above are all German, for it was in Germany that Romanticism, the respect for historical studies and the idea of 'Geist' remained particularly strong. The second source was American pragmatism (James, Cooley, Mead, and later Blumer), which put stress on the fact that human behaviour is not the automatic response of the individual to stimuli or conditions but a communicative and interactive effort to address symbolically structured situations.

What is important for my project, however, is that in spite of their anti-naturalism these thinkers wished to see their enterprise not only as humanistic or historical, but also as social-scientific. They were aware of the tension arising from giving anti-naturalistic social science the name of 'science' (originating in the natural sciences, after all). But they wanted to be regarded as 'innovators' within the established scientific domain rather than 'secessionists'. Hence, they felt obliged to address the meta-scientific problem. Their attempts at relieving the tension, as I mentioned above, took the direction of extending or reshaping the meaning of 'science'. In particular, interpretive thinkers tried to legitimise their scientific status either by conceptualising their investigations of society and individual as social *theory* or by classifying their research *methods* as scientific.

However, by contrast with Durkheim's successors, there was no agreement on what this anti-positivistic method and theory should be like and what made their research different from 'lay' enquiry. It was postulated that 'interpretation', 'understanding' or 'verstehen' be the method, but it was disputable what was to be interpreted and in what way one should carry out the interpretation. At one point a provisional agreement was reached that in-depth interviews and participant observation are research *techniques* that allow for 'interpretation' and 'understanding', but still there was no agreement on what if anything made these techniques scientific. Symbolic Interactionism won the status of the major interpretive theory, but no discussion took place about its scientificity. It seems that this decision on the 'scientificity' of interpretivism was taken by *vox populi* rather than grounded in argument. So my impression is that over a century after the first interpretivists, *we are still waiting for a precise statement of why this anti-positivistic method and theory are scientific.*

The fact that these meta-scientific questions have not been satisfactorily addressed has consequences for the present day. Most contemporary anti-positivists seem not to care about it, perhaps assuming that what they are doing has been called *social science* for so long that there cannot be any doubt about its legitimacy. In other words, we have an elephant in the room that nobody is willing to notice. This situation is convenient for interpretivists, as their obligations are removed without losing the advantages of the 'social science' label.

This also has consequences for those who reject interpretivism as a *complete* account of social science, but accept it as *part* of their theories (Giddens, Archer, Bhaskar, Sztopka and others). They do not recognise the problem of having a potentially non-scientific component in their 'scientific' theories. This is even more significant as these theories are very popular nowadays.

However, ignoring the problem is not the only stance that is now adopted. Some interpretively-minded sociologists and anthropologists seem to have become reconciled to the hopelessness of justifying their scientific status, and think about it as follows: 'What we are doing is better characterised as 'social studies' or 'cultural research'. Why should it matter if we are not scientists?' Leaving the detailed scrutiny of this response for later, I would claim that such a stance should be met by some counter-questions: 'How would you then justify yourselves institutionally?', and 'Why are your findings better than anybody else's?'¹

1.1.2 The rejection of naturalism

I said earlier that the tension between scientificity and the non-natural character of the social world tended to be relieved by interpretivists by re-defining the sense of 'science'. But why does it seem difficult to combine 'science' with 'non-natural'?

At the most general level, the answer is as follows. Science can only be about something that is not changing or is changing in a patterned way. What is studied by

¹ Perhaps one of the major causes of this situation is the state administration's *need* for the scientific status of social research. Policy-makers emphasise that their schemes are 'evidence-based', and that this evidence is obtained in a (social-) 'scientific' way. What is at stake is obviously legitimisation. Hence it seems that social scientists are to some extent pushed into this situation by external forces.

the natural sciences - matter - is continuous, quantitative, meaningless, has no will, is always the same, and therefore is a perfect subject for scientific enquiry. People and societies are not matter, however: they are intentional, autonomous and historical. Social changes do not follow any rigid, pre-determined logic. Therefore it is difficult to produce causal explanations and 'objective' knowledge, and consequently to marry naturalistically defined science with the meaning- and agency-charged social world.

But why is it that people cannot be understood as mechanistically following rules? Lay wisdom has it that people are endowed with free will. But for some reason 'will' has not become a sociological concept. Social theorists have instead invoked three different arguments as the sources of the non-deterministic, agent-driven, non-mechanical character of the social world.

The first one is based on the fact that people follow ideas and values. But by their nature they cannot be followed if they are not understood by the agents themselves, which entails that no external determination is exercised over them. Additionally, let us notice that a new idea or value is something that has no analogue in the natural world, where the set of operating laws is definite and not open to change. In other words, in the case of ideas and values, 'you cannot know the ideas and values of tomorrow'. People's conduct can be driven tomorrow by meanings that do not exist today.

The second argument is that people act on the basis of their understanding of the situation, and this understanding may diverge very much from objectively existing conditions (which would be treated as variables and indicators by the positivist). Therefore what will happen is not determined externally by the system but contingently by real people. As William Thomas said in a lapidary way, 'If men define situations as real, they are real in their consequences' (Thomas & Thomas 1928: 572).

The third argument comes from George Herbert Mead, who pointed to the fact that society is a *communicative* achievement of *interacting* individuals, which entails that it is always tentative and open to unpredictable change. The outcome of human interaction is not determined, unlike the outcome of physical reaction. In natural

systems, change can be understood as a function of interacting elements, as their predictable and necessary outcome. Elements of matter interact, but do not communicate. In short, meanings and actions are negotiable between people.

1.1.3 Problems for interpretivism

Two specific problems related to the 'scientificity' of interpretivism drove me into undertaking this doctoral research. Firstly, there is the issue of the purpose and value of social scientific research, and hence of social researchers themselves, in the absence of a commitment to naturalism. Secondly, there is the question of whether 'non-fiction art' produces findings that are as good as those of interpretive social science, despite being accorded a much lower status.

Let us begin with the question of what social researchers are for and what the nature of the knowledge they acquire is. It was logically consistent in naturalism to believe that the researcher's role is to go beyond the knowledge of lay people, to correct it, to find new knowledge. But can this be sustained once the force of the anti-naturalist arguments is acknowledged?

Since anti-positivistic premises turn people from objects to agents, I believe the whole scientific methodology is questioned: the relationship at stake is not subject-object (the researcher studying an object), but subject-subject (person-person). This evokes the following, hardly asked, questions:

- 'why do we want to carry out *research* rather than to let people *represent* themselves?'
- 'what is unclear for subjects about themselves?'
- 'what is inaccessible for everyday people, but accessible for social scientists?'

I believe that for interpretivists, these questions of new knowledge and the role of the researcher boil down to the following question: how can we privilege the 'actor's point of view', as interpretivism demands, and then take this privilege back to the advantage of the social scientist? If the social scientist confines herself to describing the actor's perspective (meanings, motives), this knowledge is new only for her, but not for the subjects, and therefore there is no way of calling it 'science'. If, on the other hand, she challenges the actor's concepts and knowledge as inadequate, she

would contradict her own presumption that first-order conceptualisations constitute the carcass of the social world.

Curiosity about whether this dilemma can be satisfactorily resolved was one of the two main drives behind this dissertation, and one of its key messages is that only if this dilemma is answered in a sound way is one justified to grant interpretive social inquiry the title of 'science'.

The second drive for undertaking this research was the existence of numerous valuable works of 'non-fiction art', such as documentary films and literary reportage. It seemed to me that these are often at least as good as their academically produced counterparts, and indeed that the best of them arguably outshine even the most acclaimed contemporary works of interpretive social science in their interpretive insights and understanding. But this non-fictional art is not accorded the status of 'social science', and is widely regarded as inherently inferior to 'the real thing'. Is this just a matter of academic prejudice, or is there something distinctively 'scientific' about the procedures of 'interpretive' social *science*?

But clearly, if the project of this dissertation to explore the scientific status of interpretivism is to get under way, I need now to say more about the concept of 'science' itself. In the following section I shall try to arrive at a 'working definition' of this, though much more will be said in later chapters.

1.2 What is 'science'?

1.2.1 Strategies for defining 'science'

Characterizing 'science' is far from an easy task. The concept of science is so contested that there is no obvious way of determining its meaning. Further, we need to do this in a way that does not rule out in advance the possibility that there could be a social 'science'.² Perhaps we should simply turn to a reputable encyclopaedia

² In the Anglo-Saxon tradition there seems to be a sharper division between natural and social sciences, and therefore less inclination to talk about 'science' as such, than in the 'Continental' tradition. In practice, when the word is used in the former, it is usually assumed to mean natural sciences. I want to avoid the assumption that only investigation of the natural world can be 'scientific', and, in this sense, am closer to Continental usages.

definition? Unfortunately, this path will not lead us far. For example, *Encyclopaedia Britannica* contains a definition of 'science', but not of 'social science'. Shall we assume that the author of the 'science' entry intends it to apply to both? It seems not:

'Science, any of various intellectual activities concerned with the physical world and its phenomena and entailing unbiased observations and systematic experimentation. In general, a science involves a pursuit of knowledge covering general truths or the operations of fundamental laws'.

If looking at dictionary-type definitions is not a productive way, what alternatives do we have? Let us notice here that answering the question 'what is science' is difficult for the same reasons that make defining any social entity problematic; for social entities have a life of their own, are historically changing and are contestable. They partially are what people of a given time wish them to be, and partially they are not so. 'What is science' is in one group with questions such as 'what is family', 'what is democracy', and so on.

So one strategy for finding out what 'science' means would be to scrutinise how science is actually done, both in the past and in the present. For the purposes of this dissertation this would mean reviewing and analysing the various literatures and debates in the history and sociology of science (and also the 'sociology of scientific knowledge', nowadays understood as distinct from this: Potter 1996). But given the range and complexity of these debates, it is unlikely that a fully defensible characterisation of science would emerge from this, at least without writing a separate dissertation.

Similar problems face the use of another strategy for arriving at a definition of 'science', that is to define it philosophically, rather than socio-historically; we would then in effect be looking at how science *should* be done. But again, the chances of resolving the debates between inductivists and deductivists, Popper and Kuhn, and so on (Chalmers 1982), or of identifying key areas of agreement, are not good (without 'another dissertation').

So what I shall do instead is to try to identify some very basic and general features of 'science' which will provide a sufficient starting-point for my purposes here. These features will be refined and elaborated at later points in the dissertation. The

way I shall do this is to present a very schematic account of the origins of what we now recognize as 'science', beginning with ancient Greece and the emergence of philosophy.

1.2.2 The origins of 'modern science'

Generally, ancient philosophers had two aims: to eliminate the spiritual from the material and the social, and to look at things from a logical (rational) rather than practical point of view. (In everyday life we classify objects on the basis of their practical utility, not their objective characteristics). Because they wanted to be disengaged, disinterested in their pursuit of pure knowledge, they called themselves 'theatai' – 'observers' or 'contemplators'³. This does not however mean that they observed entities in the way modern scientists do, systematically and empirically. The way they went about discovering knowledge was instead by finding adequate meanings of the existing concepts, finding their real definitions, and through that eliminating incorrect knowledge⁴.

Both philosophy and science spring from those two aims, that is of breaking both with spiritual explanations and with lay knowledge. They are both founded on rationality and freedom from interests. The difference between them lies in their scope and method. Being preoccupied with the world in its entirety was metaphysical and consequently philosophical (concerning wisdom), whereas dealing with specific entities – what was physical – gave rise to science (knowledge, not wisdom) (Reale 1990). Philosophy is also method-free, so to speak, as it relies on the pure power of reason. Science relies on reason *and* systematic empirical research.

The following centuries were marked by the dominant position of theology, so until the Renaissance, proto-scientific thought hardly developed. Also, the 'professional debate' about results of enquiry was significantly constrained by the rudimentary

³ This word later gave birth to our present concept of 'theory'.

⁴ However, Aristotle put an emphasis on empirical sources of knowledge. In opposition to Plato, he thought that concepts are rooted in empirical reality, not outside it. Hence, to find the definitions of concepts we need to explore this reality. But it happened that Aristotle did not turn his empiricism into a well-developed methodology, so we cannot name his work 'scientific' in the modern sense of the word.

means of circulating ideas. The Renaissance, by contrast, was characterised by an interest in the earthly, empirical and functional in opposition to the eternal and spiritual. A number of technological inventions appeared. Experiments began to be used. Thinkers turned their attention away from concepts in favour of *facts*. If what counts are facts, then what matters is ways of knowing facts – *methodology*.

In ‘early modern’ times these elements were gradually developed and refined: firstly, in the form of the shift from classifying objects to finding regularities occurring between them (laws); secondly, in the pursuit of theoretical concepts that explain the occurrence of empirical regularities; thirdly, in the rise of experimental method, and finally in the growing facilitation of professional debate (widespread availability of books, first professional journals, academic courses).

The seminal figures of that time were Francis Bacon and Galileo. Bacon was the one responsible for defining science through scientific, empirical *method*, the primary function of which is to correctly establish the facts. He enhanced the significance of the experiment (against simple observation), worked out the idea of eliminative induction, and ‘combated’ the various ‘idols’ that deceive objective cognition. Galileo’s importance results from his substantial criticism of Aristotle’s concept of teleological causality; he proposed going without three out of the four original kinds of causes and leaving only one – ‘cause’ in the modern sense of the word. (By doing that, he consequently switched the aim of science from understanding *telos* to explaining causal chains). Galileo also incorporated mathematics into science. The meta-scientific ideas of Bacon and Galileo soon proved fruitful in the powerful scientific theory of Isaac Newton.

My view is that the paradigmatic understanding of ‘science’ is based on the ideas of these three figures. Ancient philosophers looked for knowledge consisting of correctly defined concepts, but they generally made their way in an intellectual, non-empirical manner. It was Bacon, Galileo and Newton who shaped the modern understanding of science as *theoretical* knowledge (involving systematically related technical concepts and laws) about empirical facts, achieved through the use of special *methods* (observation, experiment, measurement). Therefore this

paradigmatic conception of 'science' emphasises two key elements, theory and method.⁵

1.2.3 A working definition of 'science'

This modern conception of science was, of course, initially applied only to natural phenomena. Reflection on society was still carried out in a philosophical, non-empirical manner.⁶ It was not until the end of the 18th century that the first books containing empirical analyses of society were published (e.g. Thomas Malthus's *An Essay on the Principle of Population*, 1798).⁷ By the mid-nineteenth century a number of thinkers, such as Comte and Mill, called for an empirical science of society, directly modelled on the natural sciences

However, it must be stressed that this kind of naturalistic social science is not the only way in which what I have presented in the previous section as the essential features of scientificity – 'theory' and 'method' – need be understood.

Especially, we must not pre-judge the question of whether the specific methods of the natural sciences are the only ones that deserve to be called 'methods', and hence can produce what would nowadays be termed 'reliable and valid' results. Likewise, although *any* 'theory' must involve relatively systematic and technically defined 'knowledge' that corrects or goes beyond 'lay' knowledge, it need not display features specific to, for example, physics.

But also, there is one further element of 'science' which is worth singling out, although it is perhaps implicit in what has already been said, especially about 'method'. This is that there needs to be some way of ensuring that the methods appropriate to the research being conducted have in fact been carried out properly,

⁵ I think that we can describe the more recent methodological and epistemological disputes, particularly about induction, deduction, falsification and paradigms as *internal*, by which I mean that the disputants did not challenge this overall theoretical-methodological definition of 'science'.

⁶ A possible reason for this was that governors then did not feel the need for collecting empirical data about people they ruled – with the exception of intelligence and tax

⁷ These may have sprung from individual interest, but gradually governments recognised the importance of accurate empirical data for steering complex societies, and thus started commissioning social research.

and hence more generally that the claims made by researchers are open to critical evaluation by others. This might be called 'professional quality control'.

So we arrive at the following 'result' of this attempt to provide a working definition of 'science'. Its three basic elements are theory, empirical method and quality control, and through these it aims to produce knowledge that is new, reliable and withstands criticism. Hence I am going to call 'scientific', research that has three essential features: (a) the purpose of research is theoretical explanation of phenomena; (b) those explanations are grounded in reliable empirical data; and (c) the research is professionally assessed for its possible shortcomings. The central question for this dissertation, therefore, is whether interpretive social 'science' does or can have these features.

1.3 Research design

My working definition of 'science' implies that the examination of the scientific status of interpretivism logically takes the form of three questions:

- Can interpretive explanation be genuinely 'theoretical'?
- Is interpretive 'method' really a method?
- Can quality control really work in interpretive social science?

How should we go about trying to answer these questions? The approach that I shall take in this dissertation is very strongly, though not exclusively, focused on the *practice* of interpretivism. That is, instead of trying to answer these questions by engaging in 'theoretical', that is meta-scientific debates, such as those we find in the philosophy of social science, I shall examine what is actually done by interpretive social scientists: I shall look at the substantive studies they produce, the kinds of methods they use, the kinds of explanation they provide, and so on.

Of course, I will not simply be describing and analysing this 'practice', since I will also be using this to argue more generally about what is 'possible' in interpretive social science, in relation to the three questions identified above. Also, the kinds of issues I will explore about this 'interpretive practice' will be informed by more

'theoretical' arguments of the kind already introduced in this chapter, and which will be taken further in later ones.

In order to use this overall approach, we have to find ways of identifying 'interpretive practice'. One of the main ways I shall do this is to look in detail at a small number of specific examples of interpretive studies. I have selected three: a) Arlie Russell Hochschild's *The Time Bind* (2001), taught on university courses worldwide and frequently cited and discussed; b) Gerd Baumann's *Contesting Culture* (1996), to which similar points apply; and c) David Wagner's *Checkerboard Square. Culture and resistance in a homeless community* (1993), the winner of 1994 Charles Wright Mills Award.

As can be seen, rather than sampling randomly from numerous works, these 'case studies have been selected on the basis of their prominence, the generally high regard in which they are held, and so on; there would be little point in studying 'weak' examples. Indeed, if even 'good' examples of interpretive social science turned out to display certain weaknesses in respect of their 'scientificity', this would be quite telling for our answers to the general questions posed above.

Now these three studies would usually be described as cases of *qualitative* social research; is it reasonable then to present them as examples of 'interpretive' social science? Certainly there are some complicated issues about the relationship between 'interpretive' and 'qualitative' research, and I shall discuss these at various points in the dissertation, returning to them in the Conclusion. But for the moment I shall just say that what is now called 'qualitative research' is generally regarded as the main 'practical bearer' of the anti-naturalist, interpretive tradition.

Further, given the significance of 'methods', and of the closely related idea of 'quality control', for our working definition of 'science', it is especially useful to consider qualitative social science in discussing interpretivism, because of the widespread attention that is given to 'research methods' in qualitative social science. This means that, as well as looking at the case studies noted above, we can also look at what is said about these methods in standard textbooks about qualitative research methods, and then at whether their recommendations are put into practice.

For this purpose I have chosen five such textbooks: Lofland&Lofland's *Analysing Social Settings* (1971), Burgess' *In the Field* (1984), Flick's *An Introduction to Qualitative Research* (1998), Kirk&Miller's *Reliability and Validity in Qualitative Research* (1986), and Seale's *The Quality of Qualitative Research* (1999). I used two criteria to determine my choice of these: that a 'candidate' textbook is commonly used on academic research courses and that it had multiple editions. I also ensured that both older and recent textbooks were included.

There are two other aspects of this dissertation's own 'research design' that need to be explained here. The first is connected to a point I made earlier in this chapter, that one of the reasons for my interest in the scientific status of interpretivism is my belief that 'non-fiction arts' such as documentary films and literary reportage often seem to produce results that are just as good as those produced by social scientists 'in the academy', despite usually having a much lower status than these. So towards the end of the dissertation I have a chapter which discusses a range of examples of such 'non-academic' interpretation, to see how far they differ from their social scientific counterparts, especially in respect of 'methods' and 'quality control'.

Secondly, although much of the dissertation is concerned with the various kinds and examples of 'interpretivist practice' that I have been describing, there is an important task that has to be performed before this can begin. This is to arrive at a much fuller and more detailed understanding of 'interpretivism itself', as a conception of social 'science'; otherwise we will not be able to relate the examples of 'interpretive research' to the broader theoretical character of this approach.

So Part One of the dissertation is devoted to this 'preliminary' task. In Part Two, the focus is on 'theory' in interpretive social science, and in Part Three on 'method' and 'quality control'.

1.4 Chapter outline

Part I. Interpretive Social Science

In this first Part of the dissertation I carry out a necessary preliminary for its investigation of the scientific status of interpretivism, which is to outline how

interpretive social science has been conceived by its various proponents (and critics) and to arrive at a more systematic understanding of 'interpretation' and 'meaning'.

Chapter 2. Interpretivism and its critics

This chapter has two main purposes. The first is to explore the similarities and differences between the wide range of approaches to social science that are generally regarded as 'interpretive'. I shall do this first by dividing them into two broad groups, which for convenience can be labelled 'European', focusing mainly on concepts and culture, and 'American', where the focus is mainly on individual agency. I then go on to identify some important commonalities between these.

The second purpose is to review some of the main criticisms that have been made of interpretive social science, especially in the work of Bhaskar, Giddens and Habermas. I will argue that their objections are mainly against the 'substantive' adequacy of interpretivism, and say little about its scientificity.

Chapter 3. Towards a systematic view of 'interpretation'

In this chapter I take up the issue of 'commonalities' between different approaches to interpretive social science, and see if a more elaborate and systematic account can be given of 'interpretation' and 'meaning', one that would synthesise the various aspects of this that were introduced in the previous chapter. In particular, I try to show how the gap between 'culture' and 'agency' might be bridged. To do this I draw on what I believe is a major contribution made by Brian Fay in *Social Theory and Political Practice*, though I also propose a number of significant modifications to the scheme that he sets out. This then provides me with the necessary tools to discuss my main questions about the scientificity of interpretivism in the following two Parts.

Part II. Interpretation and Theory

In Part II I consider the first element of my working definition of science – that science has to contain *theoretical* explanation of the phenomena under study.

However, since 'theory' itself is a difficult concept, I examine this before looking at how 'theoretical' actual examples of interpretive social science are.

Chapter 4. Can interpretation be 'theoretical'?

In this chapter I start by trying to arrive at a 'working definition' of 'theory'. I do this by considering the conceptions of theory held by three important philosophers of science: Ernest Nagel, Marx Wartofsky, and Mary Hesse. These three have been chosen because they represent significantly different views about science; I then extract from them several features of 'theory' that they nonetheless broadly agree upon.

The working definition comprises five criteria. These are that theory (a) is general and abstract; (b) makes use of its own conceptual system; (c) is explanatory; (d) determines what is observable and testable, and (e) goes beyond subjects' lay knowledge. I then move on to discuss in a preliminary way whether 'interpretation' meets these five criteria, identifying various issues that are explored further in the following chapter, especially concerning the relationship between social scientific and lay concepts.

Chapter 5. Interpretive theory: three case studies

The results of the analysis in chapter 4 will be used here to explore in turn the three 'case studies' of substantive qualitative research (by Hochschild, Baumann and Wagner) noted in the 'research design' section above (1.3). In each case I start with an outline of the main claims advanced and the questions they are intended to answer. I make use of my modified version of Fay's schema to identify the various (kinds or levels) of interpretation provided in these studies, looking at a considerable number of specific examples, and examine the extent to which these meet one or more of the five criteria for 'theory' that have been identified.

Part III. Interpretation and Method

In Part Three I turn to the second component of my working definition of science, 'scientific method', and the closely related requirement of 'professional quality

control'. The extent to which interpretive social science does or can display these features is explored by examining in turn the nature of 'qualitative research methods, the methodological aspects of substantive qualitative studies, and how non-fiction arts compare with these.

Chapter 6. 'Methods' in qualitative research

In this chapter I consider what it is that, according to qualitative methodologists, makes such research scientific. I do this by examining what is said in the various textbooks on research methods noted in the previous section on 'research design' (1.3), taking account of what they say both about how research 'should' be conducted and about how it often actually is. The concepts of 'reliability' and 'validity' will be central to this discussion, and also the recommendations that are made about 'research reporting', which are important in respect of my third requirement for scientificity, quality control. I shall suggest that analysis of these methodological textbooks reveal a number of significant problems for the idea that qualitative research possesses genuinely scientific methods or can be made subject to adequate professional control.

Chapter 7. Interpretive methods: three case studies

In this chapter I return to my three case studies of qualitative research to explore the issues raised in the previous chapter. I shall consider how far each of them is conducted in conformity with standard methodological recommendations, and whether the ways in which they present their findings makes them open to critical assessment by their readers. I will argue that, despite the fact that these case studies are highly regarded pieces of research, there are many unsatisfactory features in terms of method and quality control, and that this – and the reasons why this is so - may cast some doubt on the possibility of meeting these requirements more generally.

Chapter 8. Interpretive social science and non-fiction arts

The purpose of this chapter is to see whether non-fiction arts deserve to be given a lower scientific status than interpretive social science, when judged in terms of their

use of 'scientific' methods and the possibilities for quality control. In order to do this, some preliminary discussion of the nature of the different genres to be considered is provided, that is of the literary genres of reportage, travelogue and essay, and of the documentary film. A number of examples of these different genres are then discussed, to see how far they fall short of the standards met by their academic social scientific counterparts, bearing in mind the findings of the previous two chapters, which do not altogether give these 'a clean bill of health'.

Chapter 9. Conclusion

The Conclusion to the dissertation has two main aims. The first is to bring together the main 'findings' of Parts Two and Three, and to examine their implications for the questions about the scientificity of interpretivism posed at the outset. The second is to take further the discussion of some of the issues raised by these. Particular attention will be given to the relationship between qualitative social research and interpretive social science, and to some of the elements to be found in the former which do not conform to an exclusively interpretive conception of social enquiry.

Part I

Interpretive Social Science

Chapter 2

Interpretivism and its Critics

2.1 The nature of interpretivism: two major traditions

A good starting point for discussing the nature of interpretivism is to observe that humans stand out from the worlds of animals and matter in two distinct though related ways: by creating and understanding concepts (ideas, values) and by controlling their actions in a reflective way. The two major traditions of interpretivism that will be discussed below chose to stress either the former or the latter element, culture (concepts) or agency. The first group believe that actions are driven by culturally shared meanings, while the second group give privilege to actors' individuality, creativity and assessment of the situational context. The aim is respectively to understand culture and to understand particular actors.

This distinction can be seen as involving two sets of interlinked elements. In the first, focused on 'culture', the elements are 'concepts', 'ideas', 'values', 'conventions', 'following the rule', 'implicit knowledge', 'taken for granted knowledge'. In the second, centred on 'action/actor', we have such elements as 'intention', 'control', 'agency', 'rationality', 'explicit knowledge', 'conceptualisation', 'choice', 'preference', 'opinion', and 'strategy'.

The difference between these two traditions could also be understood in terms of the following three pairs of oppositions: (a) people's conduct is driven by general ideas – people's conduct is driven by individual opinions; (b) meaning is socially embedded - meaning is created by people here and now; (c) what is worth researching most is 'macro' structures and major institutions such as religions, ideologies, worldviews – what is worth researching most are 'micro' elements such as particular actors and interactions.

These two types of interpretive thinking have distinct historical sources. One dates back to Rickert and Dilthey and hence can be called 'European', while the second dates back to Mead and other American social pragmatists and can be called 'American'. It needs to be emphasised that 'European' and 'American' are only rough labels here; as we will note throughout this chapter, there are exceptions and cross-cases that make this distinction only a rough and ready one - hence my use of quotation marks when using these names. Despite these qualifications, I believe the distinction is still of significant value for our purposes.

2.2 'European' interpretivism

'European' interpretivism is focused on the role ideas and values play in social life. The fact that the social world (in contrast to the natural world) is 'furnished' with meaningful objects, has sense for people, is the basis for the claim that human beings are not subordinated to external determination in the way things and animals are. They follow meanings rather than stimuli. The *sources* of this tradition are clearly philosophical. I shall begin by sketching some of the main philosophical ideas that historically constitute the sources of 'European' interpretivism. I will then move away from philosophy to identify and discuss the major strands of interpretivist social science within this tradition.

2.2.1 Philosophical sources

(i) Historicism (Wilhelm Dilthey) and neo-Kantianism (Heinrich Rickert)

These two schools of thought, both of which influenced sociology through the work of Max Weber, called into question the naturalistic assumption of the unity of the natural and social world. According to them the human world is specifically a world of culture, which is a universe of meanings, values and symbols. This domain is inevitably historical. Therefore naturalistic philosophy of science is of no use for the humanities.

Dilthey criticised positivism on the grounds that methods must be suited to the subject. Now, what method suits researching culture? Dilthey claims that since culture is internal to people, their conduct can not only be observed but also

understood. Hence the idea of *verstehen*. However, Dilthey in fact proposed two quite different kinds of understanding. The first one was psychological understanding: placing oneself in another person's position in order to grasp her experiences and mental processes. The second kind of understanding had cultural products as its subject. Its purpose was to 'locate the object under study within a definite whole – a language, a culture, a social system' (Szacki 1979: 328, see also Outhwaite 1975; chapter 3). Long after his death, Dilthey's idea of *verstehen* caused confusion amongst philosophers of social sciences, as some critics of *verstehen* identified it only with psychological understanding (e.g. Abel 1948).

Rickert also thought that the naturalist approach is one-sided and claimed that the difference between historical and natural sciences is methodological, not ontological. Following Windelband, Rickert saw the former as 'idiographic' and the latter as 'nomothetic'. 'Idiographic' means that causal laws cannot be formulated, but – significantly – it does not mean resignation from seeking causal explanations (Outhwaite 1975: 42). The fact that history is unrepeatable does not entail that phenomena happen without causes; it only means that causes are contingent, individual. The social scientist's task is to single out the actual cause from a spectrum of factors that are potentially causal. She needs to distinguish what is 'historically significant' from 'mere heterogeneity' (Szacki 1979: 331).

(ii) Edmund Husserl

Husserl was one of the last outstanding searchers for 'prime philosophy'. His conception may be described as a philosophy of consciousness, continuing the monological, Cartesian point of view. His starting point was the observation that people in their everyday life (*lebenswelt*) take the 'natural attitude', which is characterised by the 'obviousness' of objects and the world. People take reality for granted, perceive it as unproblematic. Hence the first step towards cognising the nature of things was to reject this obviousness. He called this operation 'transcendental reduction', or *epoche*. Only after carrying out *epoche* can one proceed to the second phase – 'eidetic reduction', the direct insight into the nature of things. From our perspective the most important aspect of Husserl's ideas was his analysis of taken for granted knowledge, as this element has been transplanted into the social sciences by Alfred Schutz.

(iii) Martin Heidegger, Hans-Georg Gadamer and hermeneutical philosophy

In his *Sein und Zeit* (1927) Heidegger granted privileged ontological status to understanding as the fundamental feature of human existence. Therefore philosophical anthropology reached a conclusion analogous to that of anti-naturalistic methodologists. Gadamer drew on Heidegger's ideas, but in *Wahrheit und Methode* (1960) introduced his original claim that reaching an agreement is the elementary feature of social life. Understanding the Other is a crucial step towards such agreement. Philosophical hermeneutics affected mainly critical sociologists rather than research practitioners. In a methodological manner, Anthony Giddens drew a conclusion identical to Heidegger's: *double hermeneutics* or *verstehen* is not first of all a method of social sciences but an ontological condition of the existence of society as such. Jurgen Habermas employed Gadamer's hermeneutics in his theory of communicative action. (See 2.5 below for further discussion of Giddens and Habermas).

(iv) Ludwig Wittgenstein

The post-empirical analytical philosophy of the later Wittgenstein also arrived at an anti-naturalistic standpoint that privileges everyday knowledge and language. The key Wittgensteinian concept is that of 'following a rule'. The speaking or acting individual follows certain culturally shared rules; thanks to these rules the action is intelligible to other people. What is important is that one learns the rules through practice, through training. This means that the ultimate account or mental representation of the rule does not exist, and consequently that one understands the rule when one is 'able to go on' in real-life situations. (This view is best summarised in his famous statement that 'what has to be done, cannot be said'). Meanings are born and modified in acting. There are no meanings 'as such', no prototypical meanings. The fallacy of metaphysics consists in pulling the meanings out of their real-life contexts and then analyzing them.

Wittgenstein's ideas affected social theorists but not practitioners. (Peter Winch is the most influenced by Wittgenstein the social thinker, but to a smaller extent also C. Taylor, A. Giddens and J. Habermas). Why was that so? His assumptions entail that although the researcher can become an insider familiar with the reality under study,

she cannot communicate her findings to the audience – the outsiders. If there is no ultimate mental representation of meaning, actions cannot be converted into words, and therefore there can be no representation, let alone scientific representation, of them.⁸

Having presented the main philosophical sources of interpretive sociology in the European tradition we can now move on to its main forms.

2.2.2 The major strands of ‘European’ interpretivism

(i) Max Weber and ‘interpretive explanation’

Before I set off to describe Weber’s views, it has to be stressed that there is a problematic discrepancy between his methodological writings and his empirical studies. It is particularly the latter that situate Weber in the ‘European’ strand. Weber’s *empirical research* was based on the assumption that ideas steer actions. He was focused on macrostructures (including ideological macrostructures such as religions) and historical processes rather than on individual actions. Unlike some later interpretive theorists, Weber did not think that the social world ‘belongs’ to social actors. Using contemporary jargon, Weber believed that society is an unanticipated but at the same time not systemically predetermined consequence of human actions.

Weber’s *social theory* however is centred on the combination of explanation and understanding. In the natural world, events occur because of causes. To explain those events, we need to know their causes. In the social world, events are also caused, but by human actions, which are intentional. Intentions are causes of human actions. Therefore, to explain human actions, we need to know the intentions behind them.

⁸ Additionally, this idea entails that since communication and mutual interpretation are ruled out, so is any comparison and criticism. Cultural ‘forms of life’ are not better or worse – they are just different. The major critic of this standpoint, Ernest Gellner, dubbed it ‘group solipsism’ (1973), as here it is not individuals but groups of people who cannot go beyond their own vision of the world. It is clear that were this true, the political consequences would be disastrous for everyday people.

What kinds of aim are there? By and large, these could be expressing emotions, following tradition, realising values or instrumentally rearranging one's environment. In the case of *affective* action, there is no logical relationship between behaviour and emotions: when in anger, one can throw a vase or start crying, or do something else. The link is incidental, and therefore is not comprehensible. In the case of *traditional* action, one is again not in control of means, because they are determined by tradition. But in the cases of *value-rational* and *purposive-rational* actions the actor rationally chooses means to the ends. What is crucial is that because this match of means and ends is rational, logical, it can be *understood*. This is the combination of explanation and understanding mentioned before: we explain rational human actions through understanding them.

What is the relation between understanding purposive-rational action, such as running a business, and interpreting ideas, such as the protestant ethic? Understanding the protestant ethic is necessary to comprehend the relationship between the means (running a business) and the aim (becoming rich, which is a sign of God's blessing). If we don't understand the protestant ethic, we won't know that there is a specific reason for trying to become rich: that material success is a sign that one is blessed by God and hence that one's soul is going to be saved after death. Weber preferred studying the content of ideas to researching what particular people did with them and understood by them. This differs significantly from other interpretive currents (apart from phenomenological sociology), which are directly empirical and call for first-hand contact with the people under study.⁹

(ii) Alfred Schutz and phenomenological sociology

Following Husserl, Schutz takes up the question of the conditions that must be fulfilled for the world to be recognized as objectively existing. He reached the conclusion that objectivity is closely connected to intersubjectivity; what is shared is seen as existing. If so, then *everyday knowledge*, shared by all members of the

⁹ Why was Weber not interested in asking subjects directly about their actions? We need to remember that Weber was a historian and therefore his method of 'ideal types' was suited to research the past, not the present. Weber, and any historian, could not interview his subjects or observe them. Hence he resorts to the only explanatory tool accessible to him, which is to the rational model of rational action. But the fact is that most social researchers study contemporary people. Therefore there is no need for them to use a method that is designed for other purposes and based on other assumptions. I believe that those who can have direct access to their subjects should not give up advantages of direct empirical research.

society, becomes the subject of sociology. Everyday knowledge is a stock of typifications and classifications. It gives people their frame of reference and orientation. Everyday knowledge is of a latent, taken for granted, non-discursive character and hence is the 'superior reality'. In more technical language, it is 'the unthematically given horizon within which participants in communication move in common when they refer thematically to something in the world' (Habermas 1984: 82). Consequently, the task of sociology is to thematise this intersubjective background knowledge (Habermas 1988: 109), conceptualising it, bringing it to the surface.¹⁰

It is important not to think that what Schutz means by 'shared knowledge' is only major cultural ideas and values. For in everyday conduct we employ a huge number of minute 'typifications'. For example, in our part of the world people share the knowledge that one does not flag down buses in between bus stops.

Apart from the 'stock of everyday knowledge' there is another element that makes society possible: this is the fact people 'reciprocate perspectives'. This means that everyone believes that other people share the same everyday knowledge with them. The coherence of society is sustained precisely because of this reciprocity, not because of external rules.

Following Weber, Schutz sees social science as aiming 'to form objective concepts and objectively verifiable theory of subjective meaning structures'¹¹. But whilst Weber opted for explanatory models of rational actions, Schutz wants to build models of typical actions, and not only rational ones. Schutz postulates that the social scientist should distinguish elements that are typical (i.e. that constitute the 'everyday stock of knowledge') from elements that are biographical. Then she tries to construct *homunculi*, models of the 'ideal' actor. It is 'typical' not because it is 'most common' but because it is devoid of biography, reduced to the carrier of the everyday stock of knowledge. It is not the logical but the typical that is explanatory¹².

¹⁰ But sociologists also belong to everyday life and everyday language. This is why sociology cannot separate itself from its subject domain in the way natural sciences can.

¹¹ Schutz 1963: 246. Note that Schutz explicitly claims here a theoretical character for interpretation.

¹² The other most seminal proponents of phenomenological sociology, Berger and Luckmann, did not elaborate on Weberian ideas on human action but focused on the nature of the 'everyday stock of knowledge'. They put forward a vision of society that combines

However, Schutz followed Husserl and Weber in being uninterested in first-hand empirical research.

(iii) Ethnomethodology (Harold Garfinkel)

Ethnomethodology is one of the currents that do not fit perfectly our rough classification 'European - American'. Its originators were American but they were influenced by Alfred Schutz; I believe that in its spirit it is closer to the European group. Ethnomethodology means research on everyday methods ('-methodology') of interpreting the sense of the situation commonly used by a certain society ('ethno-'). The influence of Alfred Schutz on this current consisted in them adopting his thesis about the fundamental role of everyday knowledge for the formation of social order. Ethnomethodologists believe that this order is produced locally and is constantly constructed and sustained.

The elementary principle is: 'defining the situation and creating it is the same'; therefore acting is identified here with interpreting. However, since the work of the apparatus of cognition consists in applying latent, subconscious 'procedures of interpretation', then the ethnomethodological model of action is of a psychologistic character, and there is no room for social and cultural factors in it. The procedures function below the cultural system, below the level of meaning [Czyzewski 1979, Bauman 1999]. But this means that this kind of social theory is not interpretive in our sense and therefore I am going to exclude ethnomethodology from my analysis.

(iv) Clifford Geertz and 'thick description'

Geertz, like the ethnomethodologists, is an American thinker whose approach is nonetheless closely related to 'European' interpretivism. An influential critic of functionalist, Levi-Straussian and materialist explanations of social life, he advocates interpretive social research in a fairly standard way:

constructivism (a conception explaining how the social fabric is created from scratch, when alien people meet) with the Durkheimian 'conscience collective' (how society becomes an objective reality through passing stock of knowledge to new generations – the process of socialisation/internalisation).

‘Interpretive explanation (...) trains its attention on what institutions, actions, images, utterances, events, customs, all the usual objects of social-scientific interest, mean to those whose institutions, actions, customs, and so on they are. As a result, it issues not in laws [...], but in [...] systematic unpackings of the conceptual world in which [subjects] live’;

‘Explanation comes to be regarded as a matter of connecting action to its sense rather than behavior to its determinants’ [Geertz 1983: 22, 34].

Geertz also notes the flaw of focusing research on rules or code of conduct prevailing in the community under study, as ‘code does not determine conduct’ (1973:18).¹³ The novelty of his position is the idea of ‘thick description’. It is not clear, however, what precisely he meant by characterising description as ‘thick’. On the one hand, Geertz promotes ‘thick description’ against ‘thin description’, that is against ‘external’, objective, behaviouristic descriptions of social phenomena. He uses an example that became famous: when someone is winking, a behaviouristic observer would describe it as blinking, completely missing the point. What is needed is interpretation, not observation.

On the other hand, Geertz counterposes ‘thick description’ to theory-building. Since meanings are local and dynamic, no time-indifferent and place-indifferent theoretical explanation can be built. So it seems that by ‘thick’ description Geertz means the description of meanings that comes from the subjects rather than from the researcher. However, I believe that actually by ‘thick’ Geertz means searching for some kind of ‘surplus’ of meaning, or ‘deeper’ meaning, and that it promises something more than just descriptions of meanings articulated by subjects. More about it will be said in the next section of this chapter¹⁴.

¹³ This theme however is most often associated with the work of Bourdieu.

¹⁴ Whether Geertz’s conception of interpretive enquiry accords it a ‘scientific’ status is open to some debate. Geertz himself rejects the theoretical, scientific pretences of anthropology and grants it the status of one of ‘humanities’ (adding that genres within the humanities are nowadays ‘blurred’). In this sense Geertz is not a defender of the scientific status of interpretive research. Indeed, denying ‘grand theory’ and concentrating on ‘local knowledge’ and local interpretations fits Lyotard’s definition of the postmodern condition – breaking with metanarratives. But Geertz should be distinguished from postmodern anthropologists, for a number of reasons: (a) he does not give up the classical epistemological distinction between the researcher and her subjects – he endows the former with cognitive authority; (b) he does not abandon realism – he believes that the subject of research exists independently from the researcher; (c) he is not primarily interested in politics, power, ethics, multivocality, colonialism – which are central to postmodern approach (Rabinow in Clifford 1986: 245); (d) for Geertz, the interpretation put forward by the anthropologist is the final and the only one, whereas for postmodernists a number of different interpretations are legitimate. Geertz also differs from poststructuralists (e.g. Foucault), who reject interpretivism. Meanings are seen there as dependent on political position and power – they are not autonomous, they are

(v) The Social Theory of 'Action Concepts' (Peter Winch, Charles Taylor).

Further argument supporting interpretivism comes from Peter Winch and Charles Taylor. Their point of departure is the observation that there is a substantial difference between 'doing' and 'happening', for example between 'voting' and 'blinking'. In order to carry out a social action such as voting we need to hold a concept of what we are going to do – before the very action. Unlike behavioural actions such as blinking, social actions cannot happen without the knowledge of actors. Knowledge - and hence concepts - is constitutive for social actions.¹⁵ This is essentially an anti-naturalistic argument: what people do is not determined mechanically from the outside by factors of which actors are unaware. Explanations of social actions cannot bypass actors' understanding of what they are doing. In other words, knowledge about the social world is internal to what happens in it.

2.3 'American' interpretivism

Now let us turn to the other major tradition, 'American' interpretivism (Mead, earlier James, Dewey, Cooley, Thomas). This puts emphasis on self-consciousness and interaction. The former means the possibility of making oneself the object of one's own thoughts. Animals are conscious, but only humans are self-conscious.¹⁶ Thanks to that man does not react automatically but *acts* reflectively. Self-consciousness is the mediator between the situation that is encountered and the action, the response to this situation. The issue of meaning is important as actors imagine themselves *in a situation* rather than in isolation. Situations are however constructed symbolically ('people act towards things on the basis of the meanings that these things hold for them'). Consequently, the 'American' approach focuses on actors' 'definitions of the situation'.

ideological tools, and therefore there is no point in interpreting them. Instead of that, one should inquire into the power game – who imposes meanings on whom. As a critic of any authority, Foucault stands against academia and realism; Geertz does not.

¹⁵ Winch claims that this idea is rooted in Wittgenstein's conception of 'following a rule' (mentioned above), but it is unclear to what extent this claim is correct, since Winch and Taylor argue that actions follow concepts, whereas Wittgenstein would probably say that actions follow *rules*. Winch seems to be saying that doing is based on meaning; for Wittgenstein meaning results from doing (from following a rule).

¹⁶ One may observe that man's self-consciousness was also seen as constitutive for human nature in German philosophical anthropology - Heidegger, Scheler, Plessner, Gehlen.

2.3.1 Philosophical sources

The philosophical source in this case is Social Pragmatism. This inherently American intellectual current was partly similar to the positivistic one and hence it may be difficult to see it as the source of this second main tradition of anti-naturalistic social science. In particular, the social pragmatists believed that studying social behaviour could be experimental. I think that this belief was however *signum temporis* and that actually the consequences of pragmatist assumptions are anti-behavioural.

Three assumptions are shared by social pragmatists: that people are agents rather than objects; that people become social beings through interaction with the environment; and that the concepts of 'process', 'communication' and 'self' resolve old dichotomies between individual and society, determinism and indeterminism, social norms and creativity, and so on.

The current was initiated by William James. Although he was more interested in psychology than sociology, he introduced an important concept, the 'social self', which consisted of two aspects: 'I' and 'Me'. The idea was that the person is both the cognising subject and the object of cognition (Szacki 1979: 406). It was John Dewey, however, who laid the key assumptions of social pragmatism. Firstly, he emphasised that human conduct is always a response to a situation rather than to an individual stimulus. Secondly, he altered the focus of analysis from cultural and social *consensus* to cultural and social *processes*. To him it was 'not society that shapes individuals but individuals who shape themselves, owing to which society exists' (Szacki 1979: 410). Thirdly, Dewey coined the concept of 'habit' as a solution to old dualisms of determinism and voluntarism. (Note the inspiration for Bourdieu's 'habitus').

Another predecessor of interactionist sociology, Charles Horton Cooley, is important as he put stress on the fact that the methodological consequence of social-pragmatic assumptions is the need for *understanding* individuals.

All of the above thinkers were 'armchair' researchers. The breakthrough came from William Thomas, who was a full-fledged empirical researcher as well as a theorist. Together with Florian Znaniecki he produced a study called *The Polish peasant in Europe and America*, which was a pioneering combination of social theory and empirical research. Additionally, Thomas popularised the concept of 'definition of the situation'.

Finally, and most importantly, George Herbert Mead took and significantly developed James' idea of 'self'. Thanks to the self, the actor perceives herself and therefore can shape the course of her conduct. To be precise, what happens is the interaction of the individualistic aspect of self ('I') with society, the internalised collectivistic aspect of self ('me'). Personality is born in the process of constant interaction between 'I' and 'me', between the organism and the environment.

Mead's concept of 'I' also connotes that the individual is creative. The individual and society do not exist outside social interaction. But Mead understands this kind of interaction more broadly than just two or more people communicating. In fact, the conversation of 'I' and 'Me' is also a social interaction. Hence, as Szacki puts it, 'a sociology based on Mead's assumptions would have to be humanist, interactionist, and indeterminist' (1979: 431). He continues: 'we can consider social pragmatism to have been the American variety (developed quite independently of the European ones) of humanistic Sociology' (1979: 434).

2.3.2 The major strands of 'American' interpretivism

(i) Herbert Blumer and symbolic interactionism.

In sociology, Mead's ideas are the basis of symbolic interactionism. They were popularised by Herbert Blumer in papers such as 'Sociological implications of the thought of GH Mead' (1966). Symbolic interactionism claims that the human world is a symbolic environment that is actively created, sustained and transformed by participants of social life. Four basic assumptions of this approach are: (a) social reality emerges from interaction; (b) people act on the basis of the meanings which objects have for them; (c) meanings come from interactions; (d) meanings are modified by new interpretations, made by people in current situations. [Halas 1998]

Society is a process in which, thanks to symbolic interactions, the actions of many individuals are coordinated. Hence, it is conceived more as collective action than social organization. Interaction is a part of *joint action*, which consists in adjusting separate lines of action by means of interpretation. (The participants of joint action mutually give hints concerning their lines of action).

From Mead, symbolic interactionism took an interest in the genesis of self. Consciousness is formed in the process of affecting others and communicating with them. The individual is an active agent ('I') and the object for itself ('Me'); the conversation of 'I' and 'Me' allows for constructing one's own course of conduct. It follows that the way one perceives oneself and one's environment, the way one acts, is determined symbolically by others, and not objectively by external determinants such as the number of TV sets in the household, skin colour, area of residence etc. Social organisation is the framework for actions but does not determine them; norms and rules are not the basis of social processes but social processes are the basis for the formation of norms and rules. The structures are the outcome of actions that maintain routines; their role is to structure the situation.

Symbolic interactionism rejects positivistic methodology. Since to conduct the research it is essential to take into consideration the actors' point of view and the situational aspect of action (conditions, venue, time of interaction), participant observation is the leading method. Thomas's concept of 'the definition of the situation', is the central category here. It is assumed that such definitions of the situation determine people's conduct, rather than systemic or structural pressure. Structures result from actions, and not the other way round.

Unlike Weber and Schutz, members of this school are extremely empirical; sometimes detailed description entirely dominates general conclusions. The strongest side of the school were sophisticated fieldwork monographs and the interest in the practical applicability of the research. (Microstructuralism and the interest in 'social engineering' – improving the situation of marginalised or disadvantaged groups – are characteristic features of American sociology).

(ii) Erving Goffman and the dramaturgical approach.

Goffman's work is counted on one side as belonging to interactionist sociology – its subject is face-to-face interactions and definitions of the situation – and on the other side to phenomenological sociology (the postulate of examining everyday life). His work is critical of macro-sociological determinism and of systemic visions ('system does not decide about individual behaviour'). According to Goffman one can talk about social reality only in the context of interaction; and the interaction is understood as a kind of game, where players aim to impose their definition of the situation over other players. The point is that through such imposition one wins control over the conduct of the others; the individual is naturally interested in this control. What counts is not *what* is done but *how* it is done. The behaviour is governed by the 'stage', not by the position in the social structure. The regularities in social life come from the fact that these 'performances' are repeated in the same places for the same people. (A social institution is just such a venue where a certain kind of performance takes place regularly).

The 'performances', to be more precise, are dramaturgical actions. What are the differences between this kind of action and the others? First of all, it refers not to a separate actor influencing the world objectively (teleologically – in the case of nature – or strategically – in the case of people) and not to an actor as a member of social group (this is the case of normatively driven action) but to the participants of interaction who are the audience for themselves. The actor evokes in the others a certain image of herself. She takes advantage here of the fact that only she has access to her subjectivity.

2.4 The 'common denominator' of interpretivist social science

Although there are important differences between – and indeed within – the two traditions of interpretivism that have been presented here, it is nonetheless possible to identify certain important commonalities. To start with, we can note that all interpretivists give privileged status to 'meaning' and to the 'actor's point of view'. To them, meanings and intentions require interpretation rather than measurement. Interpretivists reject any attempt to exclude 'meaning' from social explanation or, to degrading it to low rank, to epiphenomena of something more substantial. They also

believe that the social reality is built from interactions, not from latent structural relations.

Thus what interpretivists also have in common is their anti-naturalism and anti-sociologism.¹⁷ They object to 'mechanical' explanations and the alleged 'logic' of the social system. Meanings, values and symbols are *autonomous* from nature, on the one hand, and from systemic relations, on the other. While naturalists saw the material and social orders 'above' the level of meanings, interpretivists hold the opposite view. Meanings are not determined by the social or natural environment; they can stand in opposition to them. (This means that meanings can be used to question existing social structures, as in the examples of the French and Soviet revolutions).

Interpretivists believe that what is not inside actors' minds cannot causally influence their actions. As Blaikie puts it, 'Social reality has no independent existence outside the 'knowledge' of it held by the social actors who produce and reproduce it' (Blaikie 2000: 120). The main methodological problem is seen consequently as getting access to the subjects' minds.

The reason for this lies in the relationship between meaning (knowledge) and the world. This issue is relatively straightforward in the case of the physical world. The knowledge about the natural world is fully external to it: whether we are right or wrong about how matter works has no impact on the matter, cannot change it. This knowledge takes only the form of correct or incorrect opinions, so to say. In regards to the social world, it is more complicated. The knowledge held by people about other people and about social phenomena is not external to their actions and to these social phenomena.

As we have seen, this belief in the internality of knowledge to actions has taken two major forms. The first group of interpretivists we have described pointed to the fact that social practices are inseparable from their conceptualisation by acting individuals. For example, elections may take place only if people understand what

¹⁷ As Szacki wrote about interpretive sociology, 'attention was focused more on its criticism of other trends in sociology than on its positive program and its research practice' (1979: 338).

'voting' means. If concepts have to be understood by people for them to act in the first place, then no external determination is logically possible.

The second group of interpretivists emphasised that actors are individual agents who decide which direction their conduct is going to take. Their decisions again are based on their contingent 'definitions of the situations'. Such 'definitions', identically to cultural concepts and beliefs, are internal rather than external to conduct. This is testified by cases when 'definitions' are inadequate to reality.

Consider an example of a bank that went bankrupt even when it was sound, because its customers defined it as unsound and started withdrawing money, causing its bankruptcy. This kind of occurrence is not possible in the natural world: whether we are right or wrong about when a given volcano is going to erupt, it is going to erupt. This particular feature of the social world was concisely grasped by William Thomas, who said that 'if people define the situation as real, it is real in its consequences' (Thomas & Thomas 1928: 572)¹⁸. Thus both groups of interpretivists share the belief that since knowledge is internal to conduct, social actions are not determined by factors external to the actors, such as latent relationships and natural environment¹⁹.

In the next chapter I shall build on this account of the commonalities between these different approaches to interpretive social science, attempting to develop a more systematic framework for understanding 'interpretation' and 'meaning', which synthesises many of the elements presented here. But before doing so I shall consider the criticisms that have been made of interpretive science by a number of

¹⁸ This does not however mean that the actual consequences are always as subjects thought they would be. A group of people may believe that the end of the world is going to happen in 2007, but they may be wrong; or they may militarily confront another group believing they are more powerful, and be defeated. But these consequences result from their 'definitions of the situations'.

¹⁹ It needs to be said that 'mind', 'consciousness', 'individual behaviour' and 'interaction' are all in a sense a battlefield of different humanists and social scientists. In particular, interpretive sociologists and anthropologists compete with psychologists and philosophers. But whilst psychologists focus on 'emotions' and 'personality' and philosophers on 'free will' and 'morality', interpretivists found their niche in 'meaning'. Why interpretive sociologists bracket out emotions and personality is fairly self-explanatory; but why 'free will' did not become a sociological term is less obvious: after all, at the end of the day, interpretivists regard people as autonomous *agents*. (For example Giddens insists that 'one could have always acted otherwise' (1984)). It seems that the philosophical concept of 'free will' has infiltrated social theory behind the mask of a more technically sounding concept of 'agency'.

recent and influential social theorists. As will be seen, little if any attention is given by these to what is the central issue for my purposes, the scientific status of interpretive social science.

2.5 Critiques of interpretivism in social theory

2.5.1 Roy Bhaskar

In his best-known and widely acclaimed work *The possibility of naturalism* (1979), Bhaskar presented a detailed criticism of anti-naturalistic sociologies, which, accompanied by the rejection of positivism, led him to an attempted synthesis of the opposing philosophies in the social sciences. Since Bhaskar sympathises with Marx more than with any other social theorist (although he cannot be called a Marxist), it is not surprising that he finds the weakest point of *verstehende* sociologies in their *idealism*. Overlooking the material aspect of social life results in a very serious theoretical distortion: the possibility of non-idealistic causation is ruled out.

In reality, says Bhaskar, what happens is to a large extent conditioned or caused by factors external to individuals, which are mainly material, objective, causally powerful, organised into structures, and pre-exist any activity. Hence, 'speech requires language, making materials, actions conditions, agency resources, activity rules' [1979: 43]. These external factors not only constrain conduct, but also enable it (here Bhaskar supports the idea of the 'duality of structure'). People do not *create* their reality – as idealism implies – but *transform* it.

Because of this, not only do hermeneuticians tend to forget about what precedes actions, they also omit what follows them; therefore they cannot recognise and comprehend the phenomenon of unintended consequences. Bhaskar also accuses interpretivists of not asking where meanings come from, or in which way they become accepted. Overlooking these questions means missing the issues of power in social life, the importance of which cannot be over-estimated.

Bhaskar nonetheless accepts that the hermeneutical tradition is correct to claim that social reality is pre-interpreted for scientists, and that it is necessarily (but not

completely) built from actors' own concepts. Further, conceptual inquiry is the first and necessary step in social research, preceding empirical inquiry. However, for Bhaskar social *explanation* refers to social relations, and not to meanings. Sociology is about the production and reproduction of relations, mainly those between social positions and social practices. Social relationships are latent structures that condition observable phenomena (actions), and it is they that really endure in the social world, not groups or cultures as such. Hence research that is limited to understanding meanings is 'truistic' [1979: 164].

Bhaskar devotes many pages to a detailed critique of post-analytical (that is, rooted in the later work of Wittgenstein), anti-naturalistic sociological theory, of which Winch's is the best known example. The most serious of Bhaskar's objections is that Winch is not able to deal with conceptual conflict, interaction and change in any way; communication and argument are ruled out. Why is this so? Bhaskar observes that diversity in beliefs and wants is a condition of every communicative act, but the consequence of Winch's 'group solipsism' is that there is nothing to be said *within* a form of life. The relativism underlying the concept of 'forms of life' abolishes any need and possibility of rational discourse. The last but most paradoxical implication of Winch's standpoint is thus that 'The acquisition of hermeneutic knowledge, and *verstehen* generally, is either *unnecessary* – if the social scientist is *inside* the object life form – or *impossible* – if he is *outside* it' [1979: 190].

2.5.2 Anthony Giddens

Giddens's point of departure is the observation that in the history of social thought there have been two opposite currents: one focused on institutions or structures, tending to be positivistic and naturalistic, and the other, focused on action, which is anti-naturalistic and anti-positivistic. The main advantage of these latter 'theories of the mundane' (phenomenology, critical theory, Wittgensteinian philosophy) is that they take into consideration that people understand the conditions of their actions and act intentionally, and that people have their own reasons and motives for acting in this and not another way [1988: 59]. The main vice of such subjectivity-oriented conceptions is that they overlook long-term processes and macro-structural analysis [ibid 59]. In particular, they neglect unintended consequences of action and

unacknowledged conditions of action [1982: 28]. Additionally, they treat subjectivity as given, not as something that is to be explained [1982: 62].

Giddens heavily criticises the *verstehende* tradition for ruling out of social analysis the material aspect of life, and in effect the notions of power, domination and conflict. All social positions provide social actors with certain *resources* that enable certain types of activity and constrain others. Resources are inextricably connected with *power* : ‘power’ means having control over relevant resources. But social research that is limited to interpreting meanings completely misses out the role of resources. It simply ignores the questions ‘where do the rules come from?’ and ‘whose are the rules?’ Giddens also criticises those interpretivists who conflate the social meaning of action with actor’s real reasons for carrying it out. ‘Convention’ does not equal ‘intention’. Giddens points here to the fact that people are not ‘social puppets’ but are strategic beings endowed with a moral and political compass.

Only at one point does Giddens get close to suggesting that there is a problem with the scientific status of interpretivism. He does not explicitly ask ‘is interpretivism scientific or not?’, but rather ponders whether interpretivism is, as he puts it, *sociography* or *sociology*. He begins with the observation that social objectivists put strong emphasis on the corrigibility of common-sense beliefs (and therefore the enlightening role of the social sciences). By contrast, in underlining the fact that social practices are constituted by the ordinary, mutual knowledge of a society’s members, subjectivists (that is, interpretivists) put themselves in an awkward situation, for it is problematic in what sense the social sciences can then still be critical and not consist merely of descriptions. Giddens says: ‘The tasks of social science then seem precisely limited to ethnography [...] Such a paralysis of the critical will is as logically unsatisfactory as the untutored use of the revelatory model’ [1984: 336].²⁰ However, Giddens did not take his remark about ‘ethnography’ any further, but preferred to immediately launch into his own, ‘improved’, conception of sociology.

²⁰ The parenthesised missing section of this sentence identifies ethnography with ‘the hermeneutic endeavour of the ‘fusion of horizons’’ (Gadamer). But it seems to me that Giddens (like Bhaskar) underestimates the emancipatory potential and logic of Gadamer’s theory. For Gadamer the ‘fusion of horizons’ serves as a way of discovering one’s own false prejudices, which is undoubtedly emancipatory in character. [1975: 266, 320]

2.5.3 Jurgen Habermas

Habermas distinguishes three types of interpretive sociology: the phenomenological approach, the linguistic approach, and the hermeneutical approach. This sequence is not only chronological but in his view also reflects increasing correctness.

Beginning with *Schutz and the phenomenological approach*, Habermas' main objection is that although phenomenologists recognize the methodological requirement of communicative experience, they don't recognize that we can't evade it phenomenologically [1988: 112]. They try to reach other individuals' life-worlds through the experience of their own subjectivity. But because the sphere of intersubjectivity is in fact discontinuous, 'by this method we shall not encounter a single historically concrete life-world except that of the phenomenologists itself' [1988: 112]. One cannot evade real-world communication. Summing up, this approach lacks the empirical content.

Further, Schutz is bound by the limits of the philosophy of consciousness, which is monological rather than dialogical. Therefore he gets stuck after taking the correct first step, which is to recognise that the social scientist cannot take the position of the positivistic external observer, but must instead get 'inside people's minds'. His fault consists in believing that the social scientist may be a disinterested, objective interpreter [1984: 123]. But, Habermas argues, any interpretive experience necessarily requires bringing one's own 'world' into communication or interaction, and hence the only possible theory is a critical one, not an objective one.

Moving on to the *linguistic approach*, Habermas argues that through Wittgenstein's analysis of ordinary language, modern philosophy has left the unproductive territory of monological philosophy of consciousness and shifted towards a dialogical approach. However, it tends to sink in the marsh of idealism, which is clearly visible in the work of Winch, who claims that social relationships are of the same nature as the relationships between sentences, or that 'human beings act as they speak' [1988: 129].

Winch's main problem is the status of the language game used by social scientists. Wittgenstein had an answer to this: the language game that reflects upon other

language games is *therapeutic* – it is not theory but therapy. But Winch wants to defend the theoretical, not therapeutic, status of the social sciences. By doing so, he is logically forced to assume the possible existence of a neutral metalanguage, which we can use for describing particular language games. But – Habermas agrees with Wittgenstein – such a metalanguage cannot exist.

Winch commits the same mistake as Schutz when he assumes that the social researcher can interpret alien meaning without confronting it with her own native language (particularly reasons inherently present in it). Consequently, Winch relies as naively as Schutz on the possibility of an objective theory. But - because of the argument above – social theory must be critical; the researcher inevitably becomes entangled in the argument about *reasons* with the people she studies and communicates with. Winch was only able to ‘push’ the positivistic uninvolved observer to the position of the uninvolved, uncritical hermeneutician. He was not able to see that ‘uninvolved hermeneutician’ is an oxymoron.

Turning finally to the *hermeneutical approach*. Habermas says that Gadamer’s standpoint lacks the ‘objective context of social action’ [1988: 173-4]. Linguistic reality is constrained by two external factors: nature and the social relationships of power. The objective context of social action is therefore constituted by three factors: language, labour and domination. A second serious drawback is that Gadamer’s analysis of the role of prejudgements turns into the rehabilitation of prejudice as such [1988: 169]. Therefore he neglects the power of reflection to the advantage of tradition. The balance of authority and reason is shifted – and Gadamer unintentionally finds himself on the conservative side. Thus Gadamer’s position tends to identify understanding with agreeing (1984: 136). Additionally, Gadamer’s historical background makes him overlook the fact that in real life the hermeneutical relationship between subjects is critically reciprocal, not one-directional. Therefore it is not only us who can learn something from the other, but also the other who can learn something from us (1984: 134).

2.5.4 Critiques of interpretivism: substance v scientificity

These three critiques of interpretive social science by and large point to similar shortcomings: ignoring the significance of pre-existing resources and relations of

power for social actors, failing to attend to the unintended and structural consequences of social action, and so on. They share also the view that the preferable alternative to interpretivism is some form of 'critical' enquiry, though they differ in how they conceive of this: for Bhaskar, social science should be objective and emancipatory in the Marxist sense; for Giddens, it should be critical rather than objective, but critical in its *product* rather than its *process* – and hence emancipatory for the subjects; for Habermas too, social enquiry certainly must be 'critical', but not only in the product of research but also in its process (hence it is emancipatory for both researcher and subjects).

However, for my purposes the most significant common feature of these critiques of interpretive social science is that they focus almost exclusively on what might be called *substantive* defects of interpretivism, and correspondingly give little if any attention to questions about its *scientificity*. Admittedly, as we have seen, both Giddens and Bhaskar 'notice' that there is a problem with interpretivism's scientific status, but this is marginal to their main criticisms, and they do not develop their observations into a thorough analysis. By contrast, this dissertation is based on the view that this is not a peripheral but a central problem for interpretive social science.

This lack of serious concern with issues of scientificity in interpretive social science is all the more surprising, since the preferred alternatives to interpretivism supported by these theorists do not consist in 'returning to non-interpretive positivism', but instead attempt to incorporate an interpretive element. This is surely problematic for the scientific status of their own conceptions of social science, if the interpretive component cannot itself be established as scientific.

2.5.5 A note on postmodernist critiques of 'social science'

Postmodernist philosophers focus directly on 'scientificity' rather than on the substantial strengths and weaknesses of the social sciences, or of interpretivism in particular. They dismiss the notions of 'knowledge' and 'science' as such, mainly for being embroiled in structures of authority, power and oppression. From the postmodern point of view, there is no reality 'out there' waiting to be adequately grasped or mirrored in concepts. The world is discontinuous and immediate; it happens 'here and now', so any accounts should be local and contextual.

For the postmodernist, one cannot and should not escape one's particular position, which implies that all we can achieve is multivocality. Any attempt at constructing holistic, theoretical, macro-knowledge means the imposition of someone's image of the world upon others, and therefore political domination and oppression. 'Science' is just one of such attempts (amongst others such as religion, ideology etc.) and hence should be radically deconstructed.

My approach to the scientificity of interpretivism differs radically from that of postmodernists because I do not at all dismiss the notion of science. Instead, I try to identify what is scientific and what is not scientific in interpretivism. To the extent that I use postmodernists' ideas I do so for different purposes than they do. For example, postmodernists use their arguments about the researcher's authority to attack science as oppressive or serving the hidden political agenda of the researcher; by contrast I shall use it only to question the scientific reliability of interpretive research.

Chapter 3

Towards a Systematic View of 'Interpretation'

In the previous chapter I discussed the commonalities and differences between interpretive currents (see 2.4 above). Extracting the commonalities allowed us to highlight basic features of 'interpretivism'. Regarding the differences, I showed that from the historical point of view, interpretive social sciences have two distinct sources. The first of the two major groups was focused on humans as cultural beings; on this view, ideas, concepts and values are 'drivers' of conduct. The alternative position viewed humans as agents, and their conduct as grounded in their own understanding of the situation, in their preferences, interests, etc.

We also saw how different interpretivists tended to single out different elements as the central ones, such as 'action concepts', 'definitions of the situations', 'intentions', or 'taken for granted knowledge'. However, these elements are of course merged in real life and can be distinguished only analytically. Actors necessarily have to draw on taken for granted cultural conventions and meanings in order to address particular situations and reach their individual goals. There are no people who are not individual agents (holding opinions and definitions) and there are no agents who do not use taken for granted knowledge. Since these elements form a comprehensive whole rather than being in opposition, we could expect that the two traditions could be bridged in such a way that a systematic synthesis of 'interpretation' would be yielded.

In the following sections I am going to explore this possibility. I begin with the presentation of an already existing interesting attempt at systematising 'interpretation' and 'meaning', undertaken by Brian Fay in *Social Theory and Political Practice* (1975; chapter 4). This is followed by a series of critical remarks leading to a proposed modification of Fay's scheme. If this search for a synthesis were successful, we would achieve a systematic understanding of 'interpretation' and

'meaning' that would both be closer to real life and help us answering the key questions of this dissertation better than our earlier analysis of commonalities and historical traditions.

3.1 Fay's conception of interpretation and meaning

Fay's starting point is the analytical philosophy of Winch, who, as mentioned earlier, emphasised the existence of 'action concepts'. According to Fay, the philosophical rationale behind the need for 'understanding' is that social behaviour wouldn't be possible without actors' understanding of what they are doing. People do not automatically react to stimuli (or to intangible social forces) but choose one of the socially acknowledged ways of doing things. It is important that these social 'rules' or conventions have to be understood by the actors themselves. To give an example, one cannot vote if one is not familiar with the meaning of 'voting'. But the logical corollary of this is that 'voting' cannot be *explained* by the social researcher externally, as *behaviour*, but only *understood* internally, as a meaningful act. In this case however, understanding social action implies that it is also explained, but *not* as a behaviour.

Hence Fay sees interpretive social research as having actors' knowledge as its subject, but he also sees this kind of research as going beyond actors' knowledge. This does not happen through factual correction of lay knowledge, in the natural scientific manner, but through making implicit everyday meanings explicit. Conceptualising tacit knowledge makes it novel (in a particular sense) for the people in question.

This aim is pursued in a complex way. It begins with looking at what I shall call the 'highest' level of meaning - meanings explicitly held by particular actors ('higher-level' meanings), and gradually exploring lower levels of meaning, which means moving away from particular intentions and 'definitions' to implicit meanings that condition particular actions, make them possible to exist. Lower-level meanings are primary or fundamental to higher-level ones. For example, the meaning of 'private ownership' is a lower-level meaning in our world: since it is so obvious it is taken for granted and not directly conceptualised in everyday life. The key point is that somewhere around half-way through this process the researcher enters the area of

'tacit' knowledge, which means that in order to go deeper into meanings the researcher cannot rely on subjects' explicit conceptualisations, because what is most rudimentary 'goes without saying'. (The metaphor of 'air' is oft-called to picture how unaware we are of most obvious things).

We can spell out this process by identifying five distinct stages. The first step towards such understanding is grasping the intentions of the actor. It is not as simple as it sounds, since intentions have various levels of generality²¹. There are immediate intentions, concerning the very act, and further intentions, concerning the place of this act within actor's wider vision of the future. For example: 'why did he shoot the gun? – to kill the president; 'why did he kill the president?' – to sabotage the way the country is going. Discovering these further intentions is the second step in the process.

Once the immediate and further intentions are uncovered, the researcher proceeds to the third step, which consists in understanding social practices as such. For example, instead of asking 'why X voted for such-and-such party', the researcher focuses here on the practice of voting. What does it mean to 'vote' in this given society?

The next, fourth step aims at learning the *constitutive* meanings. These are assumptions underlying social practices, making them possible. For example, the meaning of 'buying', 'selling', etc. in our society is based on the concept of 'property'. If one does not understand the meaning of 'property', he or she is not going to understand the meaning of 'buying', 'selling' or 'stealing'. What is crucial from methodological point of view is that constitutive meanings are hardly accessible to the actors, because they do not get conceptualised or problematised in the course of (inter)action. (Although Fay does not mention this, it is obviously a difficult part of the research, as the researcher for a long time does not know what questions to ask. If you don't know about something, you are not going to ask about it).

The final step is to uncover relationships between constitutive meanings. This means exposing subjects' worldview: what is their idea of relationships between people (age, gender, sex, power, etc), their idea of social order and politics, their

²¹ Schutz has elaborated upon this point (see Bauman 1990).

idea of religion and dealing with the natural world. Which elements are more important than others? For example, the researcher may propose the following interpretation: 'For most members of the society under study, property is the second most desired thing, after love. It is more important than friends, honour, good nature, honesty, religion or health'.

Fay's distinction between higher- and lower-level meanings can be seen through the prism of the following four sets of oppositions, with the process of interpretation moving from the left side of the scheme (the 'why' of action, intentions) towards the right side of it (the taken for granted 'what' of action, culture):

Conceptualised – unconceptualised
Explicit – implicit
Conditioned – conditioning
Particular – general

The biggest advantage of Fay's framework is that it bridges the division between 'actions' and 'concepts'. He manages to show why and in what way the researcher who tries to explain an *action* has to go beyond it and explore *concepts* that are tacit, underlying. Interpreting such meanings is just as integral a part of the explanation as interpreting the explicit intentions held by the actor. 'Interpretation' means understanding both the 'what' and the 'why' of action, not just the latter or the former. In order to understand the part we need to understand the whole.

Fay's synthesis also allows us to see that those who focus solely on ideas and concepts (culture) overlook the existence and importance of individual opinions and intentions. They conflate conventions with intentions. And vice versa: those who focus on agency, tend to forget that actions are framed within culture, within taken for granted concepts. Fay avoids these two types of error. Nonetheless, his conception of interpretation and meaning is limited in some important respects.²² Below I make four critical remarks, which subsequently inform my attempt at modifying Fay's scheme.

²² I suspect that Fay's starting point – analytical philosophy – is responsible for these limitations.

3.2 Weaknesses of Fay's scheme

3.2.1 Agent's knowledge of intentions

Let us have a closer look at the first two steps of Fay's scheme. They are concerned with actors' intentions, and aim at explaining why a particular action has been undertaken. Similarly to Schutz, Fay claims that plans (intentions) have levels of generality. Every action is planned not only for a particular reason but also for a more general reason, as in the example of assassinating the president. Another example: someone works hard in order to get a good mark on their exam, but in further perspective this good mark is hoped to help this person get a good job, which in turn is supposed to help support his or her family.

This argument is sound, but it implicitly assumes that actors have clarity about their intentions. I believe this assumption is doubtful, since some actions are conceptualised by actors only at the very basic level – for example, 'I am going to buy some tools because I need them' – but deeper intentions may not be clear to actors, may not be conceptualised. However, an insightful interpreter can still try to access such intentions and conceptualise them.

For example, Hochschild writes:

'Along with the tools, perhaps John [a workaholic top manager] has tried to purchase the illusion of leisure they seemed to imply. (...) John's tools seemed to hold out the promise of another self, a self he would be "if only I had time."' (2001: 14)

Or another example, this time from Geertz:

'Balinese go to cockfights to find out what a man, usually composed, aloof, almost obsessively self-absorbed, a kind of moral autocosm, feels like when attacked, tormented, challenged, insulted, and driven in result to the extremes of fury, he has totally triumphed or been brought low' (cited in Crapanzano 1992: 73).

As we can see, these interpretations could not be made within Fay's scheme. They do not refer either to the 'most immediate' intentions, or to the 'further' intentions; nor do they refer to constitutive meanings or the like. Interestingly, such

interpretations *override* both rational explanations (such as ‘John needed the tools’ or ‘Balinese gamble to win money’) and explanations that bring overt emotions (such as ‘Balinese gamble because they like the thrill it gives’).

We may be interested in what brings researchers to produce such interpretations. Perhaps they have a good reason to doubt the actor’s explicit conceptualisation? If, for example, in Hochschild’s view John had absolutely no time for DIY, or if Geertz realised that in the long run nobody wins or loses money in cockfights. Or, perhaps, such interpretations are based on the researcher’s *own* experience? It is possible that Geertz, who took part in Balinese cockfights, projected his own ‘feeling of being attacked, tormented, challenged, insulted’ onto the Balinese players. Analogously, maybe Hochschild – a busy academic herself – buys things she could use ‘if only she had time’? (As we shall see, a major problem with such interpretations is that although they may only be researchers’ pure speculations, the readers typically have no means to challenge them).

3.2.2 Ignoring agents ‘opinions’

The second weakness of Fay’s scheme is that by focusing on ‘intentions’ and ‘concepts’, it does not really pay enough attention to the role of actors’ opinions or ‘definitions of the situation’. The latter are however of quite different nature than socially imposed and shared ‘concepts’. I believe that the distinction between ‘concepts’ and ‘opinions’ is important and useful as it helps us to grasp the difference between, for example, the meaning of (the concept of) ‘elections’ and the meaning of (the opinion) ‘the bank is not sound’.

Paying attention to ‘definitions’ has important consequences, since the main difference between opinions and ideas is that the former can be (factually) adequate or inadequate, whilst the latter can be (morally or politically) right or wrong. Opinions have as their subject the sphere of concrete, time- and space-embedded people, actions and social phenomena. As such, they can be corrected in a factual way (e.g. ‘the village has 77, not 67 households’, or ‘the bank is sound’). Ideas, by contrast, cannot be factually corrected or verified as they concern ideal rather than autonomously existing entities. They determine (define) what things are or should

be, rather than describing them. But as such, they can be subjected to moral and political dispute.

3.2.3 The absence of 'horizontal' interpretations

The third shortcoming of Fay's scheme is a particularly complicated and problematic one. As it has not been really written about, there is even no established conceptual means for discussing it.

A good starting point is to observe that social practices (be it family life, cockfights, capitalism, etc) have a two-fold character. On the one hand, they embody what people intend them to be: they have a certain purpose and are carried out in a certain way. But on the other hand, social practices have, so to speak, unintended features, side effects and consequences upon actors, as well as unintended influence on other social practices²³.

This theme is not new to the social sciences. Quite the opposite, it was explored probably from the very beginning of the discipline. Early sociologists regarded society as an entity that has its own life and logic independent of what people want it to be. Later on, sociologists turned their attention to 'self-fulfilling' and self-defeating prophecies' and 'the Matthew's effect' (Robert Merton is the most obvious name here). So-called 'dynamic' functionalism is, in fact, about the positive and negative consequences that social practices have upon different groups of actors (Sztompka 1986).

This kind of analysis has been historically associated with structural sociologists. But, interestingly, in qualitative research reports one often finds a number of insights into such side effects and consequences as well. Clifford Geertz is a good example of an interpretivist who often makes this kind of insight. For example, we can quote the following interpretations from 'Deep Play: Notes on Balinese Cockfight':

²³ We can imagine social life as some kind of a dialogue between actual features of social practices (hence, intended and unintended features), and people, who try to conceptualise, transform and design those features.

‘What the cockfight talks most forcibly about is status relationship, and what this says about them is that they are matters of life and death’ (Geertz 1973: 447);

‘Cockfight... provides a metasocial commentary upon the whole matter of assorting human beings into fixed hierarchical ranks’ (ibid 448);

‘[The cockfight] opens [the Balinese’s] subjectivity to himself’ ... ‘In the cockfight, the Balinese forms and discovers his temperament and his society’s temper at the same time’ (ibid 451).

These examples show that by ‘thick description’ Geertz actually means searching for some kind of ‘surplus’ of meaning or ‘deeper’ meaning, for something more than just descriptions of meanings articulated by subjects. At the same time, those meanings are not ‘constitutive’ in the way the meaning of ‘property’ is, but rather characterise social practices, are associated with them²⁴.

Should such insights be regarded as interpretations? At first sight this may seem doubtful, as they do not seem to concern meanings held by people, such as intentions, ideas, opinions or texts. But considered more carefully these consequences *are* consequences for what people aim at, think, value, feel and choose. In cockfights, the Balinese unintentionally forms and discovers his temperament. In cockfights, the Balinese is provided with a metasocial commentary upon his fixed social status. It is true that most often these effects are not conceptualised by those who are subjected to them. Most likely cockfights are not thought of as the ‘dramatisation of status hierarchy’ by the men who bet on cocks. But even when it is not conceptualised, it still has real effect on what meanings people attach to (and find in) practices and objects.

But if we regard such insights as interpretations, can we still use Fay’s scheme to determine their place in the overall process of understanding actions? Which level of

²⁴ Some thinkers take Geertz’s claims at face value and then criticise him for this divergence between his postulates and his empirical practice. Crapanzano writes: ‘Despite his hermeneutical pretensions, there is in fact in ‘Deep Play’ no understanding of the native from the native’s point of view’ (1992: 74). I believe this misses the point, since Geertz wants to interpret ‘deep’, tacit meanings that are not conceptualised by actors. Crapanzano’s criticism sounds very similar to what Fine&Martin wrote about Goffman: ‘To Goffman, the goal is to discover how the world is ‘subjectively experienced’ by the patient. To the reader, the book represents anything but. The essays represent how a ‘sane’ Goffman would himself experience a large mental hospital if he was incarcerated against his will. Readers learn precious little about how patients experience their own world or, at least, how they report this experience’ (Fine&Martin, in Seale 1999: 187). I think that the difference is that Goffman’s ‘overinterpretation’ does not result from his deliberate search for it, but from the fact that his research was conducted as covert participant observation. This limited his opportunity to ask subjects about their point of view and entailed potentially incorrect representation.

interpretation do they belong to? I find it highly problematic. Let us state first that Fay's steps 3-4-5 form a kind of pyramid of concepts. Step 3, understanding 'action concepts' – such as 'buying' – is followed by exploration of deeper, constitutive concepts, such as 'property', which in turn is followed by understanding of the relations between constitutive concepts, in our example between 'property' and, say, 'family', 'love', 'career', etc. Steps 3-4-5 are concerned with meanings that actors take for granted. Just because these meanings are taken for granted and shared, social interactions are possible.

However, it would seem that the unintended side effects and consequences mentioned above are not *constitutive* in Fay's sense. The Balinese finds cockfights telling him about the fixed social ranking every time he bets on cocks, but this meaning does not 'constitute' cockfights. Hence it seems that this kind of interpretation concerns *horizontally* rather than *vertically* related meanings: property is 'below' buying, but cockfight is not 'below' social status or temperament. Instead of 'deepening' interpretation we have 'broadening it out'.

This difference between the 'constitutiveness' and 'non-constitutiveness' of the meanings under study becomes even clearer if we imagine a situation where such meanings are made explicit to the actors. If the buyer suddenly said to the seller: 'You are exchanging your property for my property' then, apart from the seller's astonishment that what is taken for granted has been made explicit, there would be no objection or misunderstanding. If, however, during a Balinese cockfight someone said: 'All that is going on here is not about winning and losing money, it is about your social status', or: 'You guys are forming and discovering your temperament here', the participants would most likely have to consider these statements for a good long while before accepting or rejecting them.

I believe that what this means is that Fay's scheme is once again too restricted. Of all the meanings that are unconceptualised by members of society, many are 'taken for granted', 'shared' and 'constitutive', but not all of them. As the examples above showed, they can also accompany social institutions in a way that makes them very real but non-constitutive and non-shared (not taken for granted) at the same time.

There are, however, some problems with such 'horizontal' interpretations. I shall just mention them briefly, as addressing them fully would require a very advanced analysis. Firstly, when unintended features and side effects of social practices become recognised by actors, do not these effects in turn become *intentions*? For example, a young Balinese man who is merely beginning his cockfighting 'career' may realise that his initial intention – be it 'I want to win money' or 'I do it because everybody does it' – is not the prime one any longer, and that what attracts him to cockfights is precisely the 'dramatisation of status hierarchy' or 'feeling how it is to be tormented'. This possibility should not be ruled out. Unfortunately, most often interpreters do not indicate whether these meanings are or are not actors' intentions. Geertz is not an exception here.

Secondly, it is always possible that these meanings do not exist objectively but are fictional creations of the interpreter's mind. Let's notice that there cannot exist any direct proof for such interpretations, as we are talking about something that is not conceptualised/conscious. Of course the researcher can ask the actor if the interpretation is correct – but actors may not have clear insight into their own understanding.

To complete this discussion of 'horizontal' interpretation, we need to come back to what I said at the beginning of it, that unintended features of social practices have been widely studied by structuralists and functionalists. Would that mean that, say, Geertz and Merton have more in common than is usually thought? This is not the case, however, because whereas structuralists are interested in what *situation* is unintentionally entailed by a given social practice (e.g. that the crime rate went higher instead of lower), interpretivists are interested in the effect on people (e.g. that they discover their temperament when betting on cocks).

3.2.4 The restricted range of objects and contexts of interpretation

My last set of critical observations relates to the fact that Fay's scheme is focused exclusively on interpreting actions and concepts. But can't other kinds of entities also be subjected to interpretation? And might the need for interpretation differ between different ones? We don't want to miss something important here. To avoid taking for granted that actions and concepts are the natural and only objects of

interpretation, I propose to examine two interrelated questions: *what* is to be interpreted? And *when* do we need interpretation?

Regarding the former question, I believe that not one but four classes of entities embody meaning: actions, concepts, texts and artefacts.

Actions are the first class of entities that are meaningful, and therefore potentially require interpretation. There are two basic aspects of conduct: 'what' and 'why'. For example, 'chopping wood' is the 'what' of the action while 'to warm up the house' or 'to cook dinner' are the 'why' of it. As for the former, we need interpretation when a given action is culturally alien to us, whilst we do not need it when such an action is from our culture. We should note that this 'what' of action, however, is always a culturally shared concept.

The relationship between the 'what' and the 'why' of conduct, or in other words between convention and intention, has been a source of confusion within social theory, since some theorists overlooked the fact that the former does not determine the latter and claimed that it is the interpretation of 'what' that amounts to 'explanation of action'. In fact, the interpretation of actions necessarily includes the interpretation of culturally shared concepts – the 'what' of action - but also goes beyond that by focusing on actors' 'intended meanings'. The 'why' of the action – the actor's intention or reason behind it – always has to be interpreted as there is inevitably a margin of uncertainty about why this action was undertaken. Here there are two main possibilities.

Firstly, the action may be a *response* to a given situation; the actor's 'definition of the situation' is then crucial to understanding her conduct. 'Understanding' here does not mean grasping a culturally alien idea but understanding the logical link between actor's means and aims. The action is assumed to be a rational response to the situation²⁵. An example of the interpretation of 'definitions of the situations' can be found in Abu-Lughod's critically acclaimed work *Veiled Sentiments* (1999: 44n), an interpretive study of 'honour and poetry in *Awlad Ali*, a Bedouin society':

²⁵ As Benton and Craib (2001) observed, interpreting actions requires an assumption about the rationality of the actor.

'The Bedouins' sense of collective identity is crystallized in opposition to the Egyptians' [...] 'Egyptians are said to lack roots or nobility of origin'.
'Men variously described Egyptians as lacking in moral excellence, honor, sincerity and honesty, and generosity'.
'The Egyptians' lax enforcement of sexual segregation and the intimacy husbands and wives display in public are interpreted as signs of Egyptian men's weakness and the women's immorality'.

All these 'definitions' put together mean that Bedouins do not seek contacts with Egyptians. It should also be noted that unlike concepts and intentions, 'definitions of the situations' can be adequate or inadequate to reality. The interpreter may but does not have to find out if her subjects are right or wrong. Since their actions are guided by 'definitions' regardless the correctness, it is sufficient and enough to explain the action by pointing to the 'definitions'.

Secondly, the action may not be 'responsive' but rather may be 'performative', so to speak, by which I mean that it aims at introducing a new situation. Here interpretation is focused on the actor's intentions rather than 'definitions'. For example, Hochschild interpreted the motives behind displaying personal photographs in the office:

'For top managers like Bill Denton, having family photos meant: I take an enormous amount of time from my family for my work, and since I do this despite the fact I love my family, you can see how committed I am to work. For secretaries, having family photos often meant: I have another life. I may be subordinate but I express myself fully at home' (Hochschild 2001: 85-88)

Employing this distinction, which Fay does not make, we can see that his account of interpretation emphasises this second kind of intentions rather than the first. It should also be noted that intentions of this second kind are not adequate or inadequate to reality (which is why I chose to name them 'performative').

As we have seen, we need to interpret actions when they are culturally alien to us or when the intentions behind them are not revealed. Let us now consider the interpretation of *concepts*. When are these unclear? The most obvious – but not the only - situation is when we are dealing with the meaning of concepts culturally alien to us. Typically they are encoded in a foreign language, but they may also belong to sub-groups of our own society. Sometimes the same concept has a different meaning for different classes.

A good example of the interpretation of such meanings comes again from Lila Abu-Lughod's *Veiled Sentiments* (1999). In the chapter 'Asl: The Blood of Ancestry' (p. 41n) she puts forward the following interpretations, among others:

'Blood both links people to the past and binds them in the present. As a link to the past, through genealogy, blood is essential to the definition of cultural identity. Nobility of origin or ancestry (*asl*) is a point of great concern to Awlad Ali';

'Nobility of origin is believed to confer moral qualities and character. Bedouins value a constellation of qualities that could be captured by the umbrella phrase 'the honour code';

'The most highly prized Arab virtue is generosity, expressed primarily through the hospitality for which they are renowned';

'Fearlessness and courage are qualities considered natural in Bedouin men and women as concomitants of their nobility of origin'.

And in the following chapter, '*Garaba*: The Blood of Relationship' the author writes:

'The concept of blood is central to Bedouin identity in a second sense: through its ideological primacy in the present, as a means of determining social place and the links between people';

'The social world of the Awlad Ali is bifurcated into kin versus strangers/outside (s) (*garib* versus *gharib*), a distinction that shapes both sentiment and behavior. Bedouin kinship ideology is based on two fundamental propositions. First, all those related by blood share a substance that identifies them. [...] Second, because of this identification with each other, individuals who share blood feel close';

'The Bedouin vision of social relations is dominated by this ideology of natural, positive, and unbreakable bonds of blood between consanguines, particularly agnates, including putative or distant agnates, those related through common patrilineal descent as manifested by a shared eponymous ancestor'²⁶.

As one might expect, often words with identical meaning exist in two languages. Sometimes however a word does not have a direct counterpart in the other language. Its meaning may be wider or narrower than that of its counterpart, or there may not exist a word even slightly reminiscent of the meaning in question. In such cases interpretation is necessary²⁷. These remarks suggest that the interpretation of

²⁶ In Abu-Lughod's work we can clearly see the difference between concepts and 'definitions of the situations': the latter refer to particular historical objects, such as 'contemporary Egyptian people'.

²⁷ A brief comment needs to be made about 'translation'. We usually use the notion of 'translation' to describe the process of making the meaning of a foreign word clear. But are 'translation' and 'interpretation' the same thing? It does not seem to be the case. For two words can have a univocal meaning and have the same meaning in both languages. For

concepts should be understood as *mediation* between two parties. Translation may but does not have to incorporate this interpretive-mediatory element.

The second situation in which concepts are unclear does not concern whether we are talking about a foreign language or our own, but concerns the nature of the concepts themselves. Here I refer to the fact that some concepts have been said to be 'essentially contested'. In cases such as 'love', 'family' or 'democracy' it is not possible to create incontestable definitions. All attempts at pinning down the meaning are necessarily interpretative.

To sum up, the main difference between interpreting concepts and interpreting conduct is that in the former case there is only one question - 'what' is meant – while in the latter case there are two questions – 'what' and 'why'. We need to interpret concepts when part or all of their meaning is alien to us.

Concepts can also be unclear and therefore require interpretation when they belong to our language but the combination of them (a sentence or expression) does not make clear sense. Hence another kind of interpretation is needed in the case of meaningful messages, particularly *texts*. This is one kind of interpretation that is not included in Fay's scheme, which is centred on actions and 'action concepts'. Actions are about shaping the world, creating situations and changes. Meaningful messages, on the other hand, are mental/logical representations of the world and people, as well as evaluations of them²⁸.

Not all messages need interpretation however. If we ignore the case of unclear style, the more novel or complex the message of the text is, the more it requires interpretation. Additionally, as Gadamer (1975) observed, the message is difficult to understand if the author's 'tacit knowledge' (what is taken for granted, 'prejudgements') differs from that of the reader's. Gadamer claims as well that interpreting texts requires reconstruction of the question the author posed herself

example, English *car* and Polish *samochod* mean the same thing; there can be no other Polish word for English *car* than *samochod* and vice versa. This is why we would probably not describe this case of translation as 'interpretation'. Neither would we talk about 'interpretation' when a translated word has two easily distinguishable meanings; for example Polish *zamek* means either *zip* or *castle* in English.

²⁸ Someone can say that creating meaningful texts could be regarded as action. The difference however is that in the case of texts our prime interest is in the sense of the message rather than in the writer's intention behind creating this text.

(rather than her intentions). More generally, we need to interpret texts when an initial reading suggests several possible meanings.

Finally, there is a fourth class of meaningful entities, namely *artefacts*. These embody what their makers meant them to be and why they made them. Most often artefacts have a practical function, in which case the intention behind making them results from what they are and does not need to be separately interpreted. This however does not apply to artistic artefacts. Here, a third dimension is added: the meaning of a given object is not exhausted by 'what' it is (for example, a painting or a sculpture), nor by the artist's intention in producing it (say, to express her grief or to protest against something); the crucial meaning is the meaning of the message the artist embodied in the work. Similarly to the 'meaningful messages' (texts) discussed above, such meaning has to be interpreted.²⁹ To sum up, we may need to interpret artefacts when we don't know their function or when they carry an encoded message.³⁰

As I have noted already, Fay does not include texts or artefacts in his account of interpretation and meaning. At least in principle, this might imply that his overall framework requires radical revision, since it could perhaps be argued that this scheme cannot be used to understand the interpretation and meaning of these kinds of entities. However, I will not consider such an argument here, and will therefore not propose the modifications that it might imply. This is because the interpretation of texts and artefacts will not figure in the examples of interpretive research that I will be considering in later chapters. More generally, it could be argued that the interpretation of texts does not belong to mainstream social science, being used mainly in history and religious studies, and that the interpretation of artefacts is important primarily in ethnography.

²⁹ This brings us to the observation that some kinds of words and expressions are similar to artistic artefacts as their meaning is purposefully encoded. I have myths and poems in mind here. Interpretation in this case means decoding the message.

³⁰ The four kinds of entities that may require interpretation should perhaps be seen as analytical 'ideal types', since in real life they are often combined. Let us take for example 'the invoice'. Firstly, to issue an invoice is an action, secondly, the invoice is a material object (artefact), and thirdly, it contains a particular intelligible message. We may be interested in either of these aspects (why did she issue this invoice?; what is the invoice?; what does this particular invoice say?); nevertheless in real life each invoice embodies all of them.

3.3 Conclusion

The purpose of the analytical exercise undertaken in this chapter was to develop a robust and systematic (rather than historical) understanding of 'interpretation' and 'meaning' *before* trying to answer the key questions of this dissertation. So, in light of the criticisms of Fay that I have made above, what would this understanding be like?

I believe that in broad terms, Fay's scheme provides a good basis for the understanding of 'meaning' and 'interpretation'. I propose to modify it, however, in the following manner:

- (i) it should allow for the interpretation of intentions that are 'hidden' for the actor, as in the examples from Hochschild and Geertz;
- (ii) it should recognise the distinction between the meaning of concepts (ideas) and the meaning of opinions (definitions of the situation);
- (iii) it should distinguish between the interpretation of 'taken for granted', constitutive meanings, and the 'horizontal' interpretation of meanings that arise as side effects of social institutions (as in the example of cockfights).
- (iv) it should recognise the variety of reasons for which interpretation may be required, and how these may differ between different kinds of 'object' of interpretation.

Modified in these ways, I believe that Fay's framework provides a suitable tool for scrutinising interpretive social science with respect to its theoretical and methodological character.

Part II

Interpretation and Theory

Chapter 4

Can Interpretation be 'Theoretical'?

4.1 Introduction

Having identified 'theory' as one of three substantial elements of science, in the current chapter I am going to discuss in what ways interpretation is related to theory. The gut impression is negative: the two seem to be adversaries rather than congenial. If one felt uncomfortable with the question 'is interpretivism a kind of science?', one must feel even more awkward asking 'is interpretation a kind of theory?' Indeed social thinkers felt reluctant to ask this question. Perhaps it has been too difficult to bracket out the history of sociology and social anthropology, where structural-functionalism has been acclaimed as the most advanced (albeit eventually rejected) attempt at constructing a theory. But if it is said that science includes theory we must pose this question. Perhaps the gut impression is wrong?

But how exactly should we phrase this question? Should the subject of it be 'interpretivism', 'interpretation' or 'interpretive research'? Following that, what should constitute the object – 'a theory', 'theoretical', 'theoretical explanation', 'theoretical generalisation'? It seems that asking 'is interpretivism a theory?' would regard so-called 'grand theories', such as Symbolic Interactionism. An alternative to that would be to focus on particular interpretations rather than grand theories. Which option shall be given priority here? I would argue that the case for considering the status of particular interpretations is rather stronger. After all, grand theories like symbolic interactionism are meta-theories about the nature of the social world rather than specific theories. By contrast, particular interpretations are more like specific scientific theories in that they postulate something about the subject matter they are investigating. As such, this chapter will address the question: 'is interpretation theoretical?'

The chapter will have the following structure. I begin with the basic question of what 'theory' means according to leading philosophers of science. Once again, facing the situation of often-divergent opinions, to avoid intellectual paralysis I adopt the strategy of searching for working definitions. Then, in the main part of the chapter, I shall juxtapose 'interpretation' with the proposal defining features of 'theory'.

4.2 What is theory?

Before examining 'the interpretivist's problem with theory' we need clarification of what 'theory' actually is. This is however far from clear. From an etymological point of view, the term 'theoria' (a noun) was already used by the scholars of ancient Greece. It derives from *theorein*, which is built upon 'to theion' (the divine) and 'orao' (I see), i.e. 'contemplate the divine'. As 'divine' was then understood as harmony and order (or Logos) permeating the world, *theatai* meant 'observers' or 'contemplators' of order (Arendt 1981). The point was that philosophers were the ones who withhold practical attitude and practical interests in order to discover the true order of things. This idea is still at the basis of modern understanding of theory; the main difference is that nowadays theorists do not merely 'contemplate', but cooperate with empirical researchers, who collect empirical data and carry out experiments.

Unfortunately this is as far as this etymological hint takes us. So let us consider what philosophers of science think about theory. I have analysed four different conceptions of theory provided by Ernst Nagel, Marx Wartofsky, Mary Hesse and David Willer. Since Nagel's *The Structure of Science* (1961) is a popular and oft-quoted textbook, the choice of it was natural. But I wanted to take into account also other, different views. I chose these four thinkers in particular because their ideas form a continuum, from the realist standpoint of Nagel and Willer, through Wartofsky's constructivism to Hesse's 'metaphorical' approach.

Similarly to my approach in the section about 'science', I shall not attempt to solve old and complicated disputes here, but rather I will construct a broad, working definition of 'theory'. All criteria mentioned by these philosophers of science will be mentioned and discussed.

The criteria that these philosophers of science mention as necessary for a conception to be called a 'theory' fall under five categories.³¹ I have compacted them into groups on the basis of similitude:

I. Theory is general, formal, and precise

II. Theoretical concepts, such as laws, are meshed into a self-sustaining system

III. Theory is explanatory

IV. Theory is (indirectly) testable. Inferences (deductions, predictions) can be drawn from theory

V. Theory is new knowledge that corrects everyday knowledge. Theory consists of technical concepts which provide a new language for analysing the subject matter.

Let us now briefly have a closer look at them³².

³¹ Another possible theme might have addressed the relationship between theory and observation. However, the character of the relationship between 'theory' and 'observation' divides thinkers, whereas I am looking for shared themes. Wartofsky and Hesse argue that theory and observation are intrinsically linked:

'Theory defines what is observable' (Wartofsky 1968: 120)

'Observation and interpretation are inseparable' (Wartofsky 1968: 283)

'Observation' under the new theory becomes a quite different thing' (Wartofsky 1968: 283)

'The distinction between observational statements and theoretical statements is not logical but pragmatic' (Hesse 1966: 15)

'Descriptions are not independent of the theoretical language' (Hesse 1966: 164)

'There is only one language, the observational language' (Hesse 1966: 175)³¹

But others, for example Nagel, believe that observation and theory are separate and worry how to link them: 'Objects that are denoted by theoretical terms cannot be observed' (1961: 84) ... 'scientific concepts are linked to observable epiphenomena only through the rules of correspondence' (1961: 20). But the problem is that 'it is possible to introduce new rules of correspondence without changing the theory' (1961: 98).

Due to this disagreement, I am not going to consider the theory-observation relationship when discussing theoretical character of interpretivism in the forthcoming sections.

³² What our philosophers of science take for granted but what should be spelled out is that science is characterised also by the assumption of realism. As conceptualised originally by Aristotle, it pointed at conditions that are necessary for knowledge to exist and for the truth to exist. He claimed that real knowledge is possible only about something that cannot be different from what it is (Nagel 1961). For one can grasp, understand, explain what is

Criterion I. Theory is general, formal, and precise

These are quite obvious features of theory and therefore philosophers of science do not spend too much time discussing them. Just a few quotations:

‘Theoretical concepts may be articulated with a high degree of precision’
(Nagel 1961: 100)

‘The explanatory premises are more general than the explanatory statements’
(Nagel 1961: 45)

‘The uninterpreted formal system or the formalization of some domain will be called a theory’ (Wartofsky 1968: 144)³⁴

Criterion II. Theoretical concepts, such as laws, are meshed into a self-sustaining system

All philosophers of science agree that theory is always a system of explanatory laws rather than just one law. For example, the three main parts of the cell theory are³⁵:

changeable, various and concrete only with what is perennial, simple (elementary) and general. It follows that knowledge (philosophy, later science) has two deadly enemies: one is time and the second is context. Hence, science purposefully takes no account of the context and looks for what is time-unbound. The assumption of realism is also the necessary condition for the classic conception of truth as correspondence of concepts with reality.

In terms of science, the assumption of realism is necessary because the condition of science is the possibility of objectification. The object of research has to exist independently from the existence of the researcher. What cannot be objectified – what is vague, transitory, chaotically changing, constituted by the researcher or modified by her presence – cannot be subjected to scientific research. It entails the classic epistemological conception about the nature of the relationship between the researcher and her subjects. (Postmodern criticism of scientificity concentrates precisely on the claim that realism is an artificial construction of the researcher, and that objective study is not possible).

Against this background it seems that interpretive social science endorses the assumption of realism. Abandoning certain theories (structuralism, functionalism) does not mean abandoning realism. Both ‘definitions of the situation’ and ‘action concepts’ refer to something real, existing independently from the researcher. The researcher should take an objective stance, which means that she should not add anything from herself. This meets complications, however, when it comes to the relationship between the researcher and the subjects. The people in question are objects (of research) and subjects (persons, agents) at the same time.

³⁴ See also Willer 1967: 6, Wartofsky 1968: 124, 129, 133. This rules out the majority of common explanations. (Nagel (1961: 19) admits that everyday concepts can also be abstract and general. But they are not precise, and are often ambiguous).

³⁵ http://www.biology.arizona.edu/cell_bio/tutorials/cells/cells3.html

1. All life forms are made from one or more cells.
2. Cells only arise from pre-existing cells.
3. The cell is the smallest form of life.

It entails that the fundamental concepts of which a theory is composed are definable only through each other and not through other concepts coming from outside the theory. In other words, theory is understandable only within itself (Nagel 1961: 86; see also Wartofsky 1968: 144, 282, Nagel 1961: 89 and Willer 1967: 2).

Criterion III. Theory is explanatory

The previous two criteria do not give the impression that philosophers of science specifically privilege natural-scientific theories as models of what theory should be like. This is however different when it comes to 'explanation'. I will argue that this is a source of major trouble for those who are dealing with theoretical character of interpretive sciences.

But let us approach things in an orderly manner. To begin with, our philosophers of science hold that theory is explanatory rather than descriptive. It is assumed that in order to explain something that is particular we need to go one level 'down', below its description – we need to employ theoretical terms and laws. What is only classificatory, or describes objects in empirical rather than theoretical terms, is not explanatory (Hesse 1966: 171). Theory is not about features of existing objects; it is time- and space-independent.

The notion of 'description' is fairly straightforward, but the notion of 'explanation' is much less so. To state that explanation is something else than description is only the beginning of grasping the nature of the former. There is a very basic problem with the meaning of 'explanation'. Wartofsky states that 'the concept of explanation and the correlated concept of understanding present fundamental problems at the very basis of human knowledge' (1968: 240).

Wartofsky himself presents a basic, but thanks to that, clear, view of 'explanation'. He reaches the answer indirectly, through reflection on 'learning' and

'understanding'. He distinguishes two types of learning/understanding: 'how' and 'why'. In the former case, learning involves habit formation: for example finding that 'green apples cause stomach ache' leads to the habit of not eating green apples. Learning how involves understanding how one should proceed ('don't eat green apples!'). The other kind of learning involves 'the formation of concepts and the use of inference in some form' (Wartofsky 1968: 245). Differently to the previous case, this time one learns *why* one should proceed in a certain way. One understands the reason. Now, to Wartofsky *explaining* is just the kind of understanding which involves *giving reasons*.

Up till now it does not seem that philosophers of science are biased towards natural rather than social phenomena. But let us look more closely at Wartofsky's proposition. There are two specific dimensions there: the first one refers to *what* is explained and the second one refers to the *way* it is explained. It is claimed that theory explains *empirical laws*, and that theoretical explanations are *causal*. Let us write a bit more about each of them.

(i) Theory explains empirical laws

Nagel and Wartofsky claim that we need to distinguish between theoretical laws and empirical laws. Most importantly, it is the former that explains the latter (Wartofsky 1968: 276). Empirical laws have certain empirical content but do not concern directly sensory/observational data; rather, they point to a certain method of stating about observable features (Nagel 1961: 81,83). Unlike theoretical laws, empirical laws can be substantiated in an inductive way (ibid 84).

(ii) Theoretical explanations are causal

What does 'giving reasons' mean to Wartofsky and other philosophers of science? As it has already been mentioned above, it means bringing up a theoretical, general statement or law. This law is the reason for the empirical regularity to take place. The particular is explained by the general (Nagel 1961: 45, Wartofsky 1968: 267).

What, then, are the problems with this mainstream conception? Clearly, when philosophers of science discuss this criterion, they have the natural sciences in

mind. It is fairly obvious that what occurs in the natural world is sequences of phenomena or events, influencing one another, and that they fit the explanatory scheme 'initial conditions + law → effect'. But social phenomena, at least on the interpretive view, do not obviously fit this scheme. However, it may be possible to neutralise this naturalistic bias and retain the core of the criterion – that theory is explanatory – without sticking dogmatically to the content that is proposed – that theory explains empirical regularities through general statements. I will explore this possibility in more detail later on.

**Criterion IV. Inferences (deductions, predictions) can be drawn from theory.
Theory is (indirectly) testable**

Our four thinkers maintain that one of the constitutive features of 'theory' is that one can deduce or infer substantial³⁶ claims from it. There is no agreement, however, as to what exactly such claims are. The possible candidates are inferences, laws, observational statements and predictions. Thus

'Theory is a tool for inferring observational statements from other observational statements' (Nagel 1961: 122)

'Inferences may be carried out of theories' (Wartofsky 1968: 145)

'Theories are higher-level laws from which one can deduce other laws' (Wartofsky 1968: 276)

'Predictions can be made from theory' (Willer 1967: 2)

'[Theory] is not simply a representational device but also a predictive one as well' (Wartofsky 1968: 135)

One could logically expect that this has further important consequences regarding the testability of theories. If predictions or observational statements can be drawn from theories, then such empirical inferences constitute tests for theories. Willer, for example, states that theory 'must be testable' (1967: xix). At first sight, others seem to argue that theory cannot be tested. Surprisingly, this is taken by supporters of opposing assumptions: Wartofsky says that theory cannot be tested because it determines what is observable (1968: 120n), while Nagel claims that the untestability of theory results from the fact that theoretical terms are not connected to observational terms.

³⁶ By 'substantial' I mean 'non-definitional', as in Kant's distinction between synthetic and analytical statements.

However, this divergence can be regarded as only an apparent one. Wartofsky at one point qualifies his position by stating that theories 'are only *indirectly* testable' (1968: 276, italics mine). He thus seems to maintain only that one cannot simply and easily convert theoretical concepts into empirical ones. Nagel, in turn, admits that theoretical terms are *indirectly* connected with observational terms through 'rules of correspondence'. So one can say that for all these writers, theories are testable, even if only indirectly and always inconclusively.

Criterion V. Theory is new knowledge that corrects everyday knowledge. Theory consists of technical concepts which provide a new language for analysing the subject matter.

This last criterion is, together with the criterion of 'explanation', the most important one in my view. And similarly to 'explanation', this criterion is problematic because philosophers of science tend to privilege natural sciences as model sciences.

It goes almost without saying that correcting lay knowledge and everyday concepts is the aim of scientific theories. In Marx's telling words, 'All science would be superfluous if the outward appearances and essences of things directly coincided' (1966: 817)³⁷. But why is that? To find the answer, we need to look at the character of scientific enquiry in relation to everyday conduct. In everyday life, people tend to conceptualise nature from the practical angle – they pay attention to features that are useful or important (such as 'oak burns more efficiently than pine'), and use their human senses to find out about matter (something can be known as soft, blue, heavy etc.).

Natural sciences break with this practical attitude in favour of the objective one (finding objective rather than practical features of things, and objective regularities rather than only practical ones); they break with measuring things with human senses in favour of mechanical measurement; they break with seeing matter through the 'human' scale in favour of 'micro' and 'macro' scales. In order to create

³⁷ It is worth mentioning that magical and religious thought also presupposes the existence of a 'second', hidden level of essences, but they see it as spiritual in nature.

scientific knowledge we have to break with the 'human' point of view, with the 'human' scale. Nagel laconically states that theory is 'detached from features experienced in everyday life' (1961: 19). In other words, for natural scientists the relationship between concepts and the world is an external one: there is the world, and there are concepts that describe the world adequately or erroneously. Therefore the crucial notions here are those of 'truth', 'fact' and 'correctness' of knowledge.

This all has consequences for how philosophers of science view scientific language: scientists need new words for new, correct knowledge. Wartofsky claims that 'technical' appearance is a mark of the first stage in the 'career' of ideas. What is a 'technical' theory today is going to be everyday knowledge tomorrow, but we'll have another novel theory the day after. Consequently, what are technical concepts today are going to be everyday concepts tomorrow³⁸:

'The difference between observation and theoretical terms is a difference between an older, common, and more familiar theoretical framework (*especially that which is represented in common sense and in the ordinary use of language*) and a newer one achieved by criticism of the shortcomings of the older one'. (Wartofsky 1968: 283, emphasis mine)

Mary Hesse similarly claims that 'the essence of theoretical explanation is the introduction of a new vocabulary' (1966: 171)³⁹.

But can the same framework be used in relation to social rather than natural phenomena? Here social scientists differ in their opinions. *Naturalistic* social scientists try to employ the same philosophy in regards to the social world. They look at the social world from an objective rather than a practical angle; they try to find features and regularities that are constitutive and objectively important rather than useful for the people under study; they drop the 'human' scale in favour of micro and macro scales. They view actions as caused by external factors, such as social systems, and try to correct faulty lay knowledge.

³⁸ Note that Giddens 're-discovered' this idea many years after Wartofsky in the observation that scientific concepts can gradually become used in everyday language (1984: 284).

³⁹ Additionally, it is disputed whether theories and theoretical concepts have metaphorical or literal character. Mary Hesse and Max Black stand out here, claiming that all theories are metaphorical, and that it is the notion of univocity that is highly problematic.

Interpretive social sciences, by contrast, are based on the claim that the relationship between actors' knowledge and their conduct is very different to the one between knowledge of natural entities and those entities: it is internal, not external. Actions are not caused by external factors but by actors' own knowledge. For example, when people believe that a given bank is not sound, they begin to withdraw their money. This bank may in fact be very reliable, but it is the actors' own knowledge that caused their conduct, rather than the real state of things. Or a politician may believe in his nation's superiority over neighbours and begin a war with them. Because this link is internal, there can be no doubt about causality: actors' views cause their conduct. Therefore what precisely *must not* be done is to break with the 'human' scale, with subjects' point of view (with their 'definitions of the situation'). Doing otherwise leads to distorting the social reality and creating artificial objects and intentions.

But does this mean that interpretation does not correct actors' knowledge, that it only repeats what actors already know well? Could interpretation be regarded as science at all if this was the case? Two seminal social scientists expressed their concerns explicitly:

'Scientific emphasis on theory generated by researchers gets in the way of paying close attention to the theories that people use in everyday life' (Denzin 1978)

'The very idea of an interpretive theory is inconsistent' (Sperber 1985: 34)

I believe we do not need to give up at this point however. Here returning to Marx's position is helpful. When applied to the social world, saying that 'All science would be superfluous if the outward appearances and essences of things directly coincided' does not necessarily mean that actors' knowledge about their conduct is wrong: it potentially may mean that this knowledge does not *exhaust* what can be said about the conduct. I suggest here that science may *go beyond* appearances without writing them off. In this case scientific knowledge would be *novel for the subjects* without rejecting their lay knowledge. In the main body of this chapter I shall explore this possibility that interpretive research delivers knowledge that is novel for the subjects but which does not deny the autonomy of their conduct.

4.2.1 A working definition of theory

As noted earlier, I am going to avoid getting engaged in complex debates that are tangential to my main purpose, which consequently means that I am looking for agreements between thinkers. This is why criterion IV (inferences, testability) is going to be narrowed to the first element, as there is no agreement on whether inferences make theories testable or not. Also, since the nature of inferences is disputable, I am going to explore what, if anything, can be deduced or inferred from interpretations.

In Criterion I (general/precise/formal), the element of 'formality' is going to be dropped, as interpretive social-scientific theories (and particular interpretations) by their very nature cannot be expressed formally.

4.3 Can interpretation be regarded as theoretical?

I shall now analyse to what extent 'interpretation' and 'interpretivism' meet the five criteria of theory. Since I believe that some of the criteria are more interesting, more crucial, or more difficult to meet than others, they will be discussed at significantly greater length. I believe that the criteria of 'explanation' and 'correcting lay knowledge' are such. Before we proceed, however, it needs to be clarified that I shall employ the understanding of 'interpretation' that has been worked out in the previous chapter (a modification of Fay's scheme). I am also going to give actual examples of these kinds to facilitate the reading of these complicated issues.

4.3.1 Criterion I: Theory is general, formal, and precise

To what extent do particular interpretations meet these requirements? Let us quote two exemplary ones, the first referring to 'definitions of the situation' and the second to cultural ideas:

'The Egyptians' lax enforcement of sexual segregation and the intimacy husbands and wives display in public are interpreted [by Bedouins] as signs of Egyptian men's weakness and the women's immorality' (Abu-Lughod 1999: 44n)

'Blood both links people to the past and binds them in the present. As a link to the past, through genealogy, blood is essential to the definition of cultural identity. Nobility of origin or ancestry (*asl*) is a point of great concern to Awlad Ali'

To begin with, these interpretations are only as general as the community under study, but not wider: Such interpretations do not automatically hold in other communities. They do not hold universally across time and space, as do scientific laws.

They are consistent and fairly precise. They are, of course, only as precise as words can be, and not as precise as symbols or numbers; also, they are not precise in the sense that usually a number of similar words compete to be used in interpretation. This is different to the precision and clarity found in formal systems.

4.3.2 Criterion II: Theoretical concepts, such as laws, are meshed into a self-sustaining system

Particular interpretations often meet this condition. Interpretive research produces numerous interpretations rather than just one; these interpretations usually are related in a systemic manner.

4.3.3 Criterion III: Theory is explanatory

I wrote earlier that, in the view of philosophers of science, theory explains empirical regularities by pointing to the cause and relevant theoretical law in operation. I said that this view takes natural-scientific explanation for its model, and hence poses a problem for interpretivists. I suggested that other takes on 'explanation' are however also possible. In the current section I am discussing these possibilities.

(i) *What* is explained: can theories explain anything other than empirical laws/regularities?

Are 'empirical regularities' the content of the social world as well? In one sense, they are: one can argue that what happens in the social world is similar sequences of human actions. Positivist social scientists, in this naturalistic manner, treat social actions as effects of causes and conditions. They treat actions as empirical and

regular phenomena, resulting from the operation of hard social factors such as climate or income, or with social morphology, or, in the softest version, with the operation of social norms and social roles (Parsons). Positivistically minded social scientists are also interested in what is universal to societies and actors rather than in cultural and social diversity, similarly to natural sciences being focused on what is universal rather than on particular existing forms.

But it is only in this naturalistic sense that 'empirical regularities' are the content of the social world. For anti-naturalistic social scientists, this is a wrong path because the social world is substantially different to the natural one. Most importantly, as human actions are not determined reactions to hidden social stimuli, they cannot be treated in the natural-scientific manner, as empirical regularities. Additionally, cultural and social heterogeneity is something without analogy in the natural world, and is of prime importance rather than being an obstacle on the way to finding universal laws.

However, why not treat indeterminate actions and cultural idioms as legitimate objects of theory? Under what conditions would it be legitimate to do so? Presumably the condition would be that these objects are not clear, so that they seemed to require explanation. If everything is clear about them, there is no need for explanation in any sense of this term. This brings us to the following section.

(ii) *How* it is explained: is causal explanation the only kind?

Let us recapitulate the mainstream position here: the occurrence of empirical regularities is explained by calling a general statement, a theoretical law. Now, let us observe that these naturalistic notions of 'explanation' and 'cause' are only apparently straightforward. If we trace the 'prehistory' of science, we would find that it was dominated by an Aristotelian idea of actually four kinds of causes: the material cause, the formal cause, the efficient cause and the final (teleological) cause. Every phenomenon was seen as the outcome of the operation of all four kinds of cause. For example,

- Wood is what the table is *made out of*.
- Having four legs and a flat top is *what it is to be* a table.

- A carpenter is what *produces* a table.
- Eating on and writing on is what a table is *for*.⁴⁰

Importantly, the birth of science saw the reduction of these four kinds to just one: the efficient cause. (This operation was started by Bacon and finished by Hume). Ruling out teleological causes - acknowledging that natural events are caused exclusively by other natural events - allowed for the creation of knowledge that is certain, fixed, deductive, of the form 'it always happens like that'. Laws of nature are responsible for regularities.

However, ruling out the teleological cause also meant ruling out the maker's *intended meaning*. Since Bacon, what is caused - the result - is *necessary* but *not meaningful*, so to speak. By taking this step science effectively got rid of god, spirits and fate as causal factors – but, by the same token, rejected causal autonomy of human will and human reason.

This reduction worked well in natural sciences. But can it also work in the case of human societies? Here interpretive researchers stand firmly against this reduction and hence against this view of 'explanation' overall. However, they are not against explanation as such; they are against a naturalistic type of explanation, that is against 'external' (most often structural) explanations that do not take into account the consciousness of subjects or their 'definitions of the situations', but only variables. Such explanations treat consciousness and meanings as simple products, epiphenomena of external, objective situations/variables. In our current vocabulary this rejected kind of explanation was called 'causal', 'Humean', or 'it always happens like that'.

Now, could interpretation be regarded as explanatory in this situation? If so, in what sense? Instead of directly assessing this interpretive argument here, let us address it from the other way round: let's try to defend the naturalistic view.

In this view, something is done 'through' the individual rather than the individual autonomously doing something. For example, an act of crime is caused by some external sociological or psychological factor that affected the agent (e.g. 'X comes

⁴⁰ <http://faculty.washington.edu/smcohen/320/4causes.htm>

from a dysfunctional, violent family' ...'has always lived in a deprived area with high crime level'), rather than by the nature of this particular actor (e.g. 'X did it because he is a bad person'). Actions do not result from a void, so to speak – there is always some preceding factor involved.

It should be noted that this argument does not hold that causality (the operation of the preceding factor) always or even mostly happens beyond the consciousness of the actor. It does not deny that people often know what they are doing and why they are doing it. It holds, however, that being conscious of the preceding factor does not cancel its causal power: it is still this factor that causes behaviour, not the actor's consciousness. People are not autonomous from causal factors. *Telos* is the function of the cause. It is therefore perfectly legitimate to remove actor's consciousness or 'point of view' from the explanandum, to by-pass it. It means that we don't need the notion of teleological cause; the 'efficient cause' is all we need.

One could argue that 'meaning' is such a preceding factor: it triggers actions in the same manner as social structures/factors in the naturalistic view. On this view, it is perhaps possible to fit it into the 'efficient cause only' scheme, to explain human conduct in the manner of 'it always happens like that'. Since such explanation refers to the abstract rather than particular actor, it could be regarded as the equivalent of a general law of nature. It says that action X happened because of the meaning Y, not because Z was the actor in question. In the same manner, the volume of the water pushed up by my body equals the volume of my body not because it was me in particular who was immersed, but because of the law.

We can identify three kinds of cases in which this analysis might be applicable:

- Actions are caused by norms that are linked to values and institutions (e.g. Parsons' view). The link between meaning and action is logical. Explanation does not consist in referring to a general statement here, but rather in identifying the logical relationship between the meaning of the norm held by actor and her conduct. For example, someone goes to church every Sunday because it is socially 'normal' or 'expected' behaviour in her community. The 'efficient cause' here is the norm or value.

- Actions are rational responses to situations (as 'defined' by actors).
The link between meaning and action is rational. Explanation does not consist in referring to a general statement here, but rather in identifying the rational relationship between the meaning held by actor and her conduct. For example, if someone learns that her bank is not sound any longer, she rushes to withdraw her deposit. The 'efficient cause' here is the 'definition of the situation'.
- Actions are caused by broadly understood needs, such as the moral impulse, one's own interest, etc. Explanation does not consist in referring to a general statement here, but rather in identifying the logical relationship between the need of the actor and her conduct. An example from Hochschild: 'For secretaries, having family photos [in the office] often meant: I have another life. I may be subordinate but I express myself fully at home' (Hochschild 2001: 88). The 'efficient cause' here is the need, in this case the need to feel like a worthy, equal, not subordinate individual.

As we see, in all three cases it was actually possible to fit the explanation into the 'efficient cause only' scheme, effectively denying the autonomy of consciousness. Even if at first sight an action is fully directed to the future, as in the example of secretaries' photographs, we were able to do without the notion of 'teleological cause' by relating the action to the preceding cause.

However, there are important objections that could be made to this attempt to incorporate meanings into a framework of causal explanation. To begin with, empirical regularities in the social world are weak: it simply 'doesn't always happen like that'. Different people behave differently rather than in a homogenous way. This suggests two things.

Firstly, that *empirical regularities concern the individual to a much greater extent than they concern populations*. This is perhaps a fairly innovative view of an old problem, but I believe that it is high time to admit that there are far more empirical regularities in X's day-by-day conduct than in any group's conduct. Instead of saying 'it always happens like that', it's better to say 'she (or he) always does that'.

Secondly, the fact that different people behave differently rather than in a homogeneous way suggests that by-passing the nature of particular actors and their consciousness was a mistaken operation. When facing certain norms or situations, different people will take different actions.

In particular, we need to think about the following issues here: (a) peoples' skills, knowledge, creativity and resources vary. Some people are more rational, some are more emotional. For example, when learning that one's bank is not sound, there will always be some people who would not conclude that their money is in danger; (b) institutions do not always entail norms of behaviour, in which case actors do not have a clear indication about conduct; (c) even if there is a clear norm imposed upon the actor, she may prefer to act strategically rather than normatively; (d) situations often may be reasonably addressed in more than one way – the example of an unsound bank is quite unusual; (e) people have different personalities, preferences and values⁴¹.

All this means that we must take into consideration *who* is acting. The aim of behaviour cannot be reduced to its cause; it is not the function of a cause. We need to know the actor's point of view, her consciousness. The *telos* of action is partly independent of preceding factors, such as social norms. It is people who do things rather than things being done 'through' people. Summing up, on this view we cannot do without the Aristotelian 'teleological cause'.⁴²

4.3.3.1 Explanation by Familiarisation.

Actions do not exhaust the list of objects that need to be explained. In the social world, there are also differences in institutions. If a given institution is culturally alien

⁴¹ The classical sociological project deliberately left out differences between individual personalities .

⁴² In the light of this discussion I believe that rational explanation of conduct is a heuristic or methodological tool rather than an adequate account of reality. Building homunculi (models of rational actors) is the *starting* point in our research: it allows us to start asking questions to the subjects. But it is only the starting point; we need to research the 'actor's point of view' empirically. We cannot base our explanation on homunculi. Perhaps the same applies to normative explanations as well. There is a temptation to apply them to big populations, but as I said it is necessary to back them up with empirical research of subjects' 'teleological cause'. Not doing so may easily lead to creating sociological fictions (as Bourdieu has skilfully shown).

to me, I need an explanation of it. This is what social or cultural anthropology is about.

But here the notion of 'cause' is useless. I need an explanation of *what* rather than of *why*. Does this mean that explaining cultural differences is not really explaining, or instead that it is non-causal explaining? Stanislaw Ossowski comes to our aid here. In his work *On the Peculiarities of the Social Sciences* (1983) he noticed that there are two quite different meanings of 'explanation'. The first kind of explanation has Humean roots: to explain something means to identify a general statement that constitutes the logical reason for occurrence of whatever is being explained. In other words, this kind of explanation means stating that 'it always happens like that'. We are already familiar with this kind of explanation.

However, Ossowski claims, from the psychological point of view, relations between occurrences are not any clearer when a general statement is identified. This is where the second kind of explanation comes in: it consists of bringing down what is unknown to what is familiar. It happens in two situations: when an analogy is brought up and when personal experience is referred to. Therefore the particular (concrete) is explained through another particular (concrete). In other words, Ossowski draws our attention to the fact that *not all* explanation consists of giving reasons (answers to the question 'why').

Interestingly, this kind of explanation is also widely used in natural and social sciences. A number of their concepts come from vernacular, for example 'wave', 'power', or 'social role'. They also apply metaphors such as 'red dwarfs', 'black holes', 'iron cage', or 'symbolic market'. But most natural scientists do not want to admit that concepts that bring unknown to familiar are really explanatory; they see them as helpful but not necessary aids. A counterargument coming from Max Black and Mary Hesse (among others) holds that metaphors are central to our theoretical understanding of phenomena. I am going to leave this problem without further discussion, as it would require a very advanced and complicated analysis, which for practical reasons cannot be presented here.

4.3.3.2

To sum up this discussion of the mode of explanation: we have argued that mainstream philosophers of science support the view of 'explanation' that is biased towards natural rather than social entities. Sticking dogmatically to their view would mean that the problem of interpretive social sciences being theoretical is automatically out of question. But we argued that, firstly, it is also possible to explain other, social entities – human actions and cultural idioms – and secondly, that causal explanation is only one of three kinds of explanation, the other two being 'teleological' explanation (of actions) and 'alien through familiar' explanation (of culturally alien institutions and idioms).

4.3.4 Criterion IV: Inferences can be drawn from theory

Does interpretation allow for making some kinds of inferences? Four possibilities can be considered here. To begin with, if we are interpreting social and cultural *norms* we could try to predict that a certain norm-compliant conduct will occur. In fact this is what we do all the time in everyday life. Human conduct, however, is founded on more complex grounds than solely normative ones. The relation between meaning and action is far from straightforward: actors are guided by a number of elements (such as will, interests, rationality), and norms are only one of them. Because of that, normative predictions are only conditional and not really reliable.

Secondly, there is a possibility that from two norms we can *deduce* another norm of which we have not been aware. If the existence of such a norm is confirmed, we would have an argument for validating the interpretation of two original norms. This reasoning also applies to 'definitions of the situation'.

Following that, empirical inferences are possible when one of the elements of interpretation is normative and one is a 'definition of the situation'. For example, 'Honor is praised by Bedouins' + 'Egyptians do not care about honor' => 'Egyptians are not respected by Bedouins'. Although this may often work, it meets the predicament that in the social world two presumptions do not entail a single and definite corollary. In our example, it could as well be 'Bedouins hate Egyptians' or

'Egyptians are fought by Bedouins', or others. (It may vary from person to person – some Bedouins may have a preference for the former option while others for the latter one).

Finally, interpretation may not entail predictions, but it definitely may give rise to moral and political consequences. Understanding someone's conduct may influence ours, or may facilitate the communication between alien parties (and then subsequently it may lead to the agreement between parties who were in conflict over meanings and values). On this note, Fay wrote that 'New ways of living become real alternatives when one is able to see the sense of alternative life styles and different ways of looking at the world. At the least one's own assumptions are thrown into relief and therefore one becomes more fully self-conscious; at other times one may well come to redefine oneself and therefore to act differently' (Fay 1975: 81).

Could any of the four types of inferences mentioned above constitute the *test* of the validity of the original interpretation? Due to the complexity of human motivation mentioned above, actions cannot be regarded as tests of *normative* interpretations. If, for example, we observe Bedouins behaving disrespectfully towards Egyptians, it does not automatically mean that our interpretation ('Bedouins praise honor and consider Egyptians to be honorless') is correct. Bedouins may have another reason to disrespect Egyptians.

Carrying out a 'members' check' (i.e. asking people under study whether our interpretations are correct) is a rather unreliable test as people may feel inclined to 'idealise' their society and culture by endorsing only interpretations that are convenient for them, and by rejecting 'bad' interpretations. Also, 'members' check' may simply not be viable if an interpretation is expressed in a language alien to the people under study.

4.3.5 Criterion V: Theory is new knowledge that corrects everyday knowledge / Theory consists of technical concepts. Theory introduces a new language.

Naturalistic scientists take it for granted that their ultimate task is to correct lay knowledge about the world. To achieve this, they disengage from the practical attitude and rely on mechanical rather than human measurement. The corollary of this is that they disengage from everyday language in favour of purposefully coined concepts. But accepting the interpretive assumption about actors' subjectively-driven, meaning-based conduct puts us in an inconvenient situation, as it is problematic in what sense, if any, interpretation corrects their knowledge. I suggested earlier that it might be possible to *go beyond* subjects' knowledge without rejecting it. I shall now expand on this remark.

First we need to examine more closely this clash between the fundamental assumption of interpretive currents (holding that the actors' point of view is privileged over points of view imposed from the outside) and the fundamental assumption of science (holding that 'All science would be superfluous if the outward appearances and essences of things directly coincided', Marx 1966: 817). The question of consistency or clash between the two assumptions could be then expressed as follows: how can we favour 'the actor's point of view' and then take this privilege back to the advantage of the sociologist, who claims to gain new knowledge, the knowledge going beyond that of the subjects?

I suppose that what can help us with this complicated issue is an auxiliary, and even more basic, question: If actors are seen as agents and not subjects, why cannot they just represent themselves? Why not commission the research from the insiders? This would be the logical consequence of privileging 'the actor's point of view'! Why bother the researcher, who has to spend years trying to become familiar with the reality under study? Why should we be suspicious about subjects saying 'By saying (or doing) X we mean this and this'?

Let us explore these issues by considering three kinds of reasons for not just 'leaving it to the subjects'. The first are essentially *practical*. For example, it may simply be that people in question do not know our language – in which case they would not be able to represent themselves to us. The justification for research lies

therefore in its mediatory function: an alien community is brought closer to ours. (However, this means that the knowledge acquired is new only for us but not for the people under study).

Another practical reason for carrying out our own research is that people who we are interested in may not be bothered about representing themselves at all. Additionally, sending a researcher (who samples subjects) solves the problem of *who* exactly is going to represent the group – most often, the already existing political, economic or cultural leaders would not be regarded as a representative sample.

Secondly, there are methodological reasons. The claim here could be that the knowledge gained from interpretive research is not novel for the subjects, but is scientifically reliable. The most common case where we cannot relegate the cognitive task to the subjects is when they may be tempted to present an idealised, 'embellished' vision of themselves (what reality should be like rather than what it is like). A researcher who comes from the outside warrants relative objectivity⁴³.

⁴³ What happens when a group of people represent themselves? Most importantly, representation is inherently linked to the public sphere. In this sphere, what is aired are various 'issues' that are important for those who are representing themselves. When someone voices her issues voluntarily, it means that the issue concerns her and that she is 'interested', 'bothered' and that she cares about the result of this self-representation. But this means that she uses arguments in a rhetorical rather than an objective way. (It does not necessarily mean that she lies, but she may emphasise what is to her advantage and ignore what is not). She is not likely to be unbiased and self-critical. In short, people tend to 'embellish' themselves when self-representing. ('Embellishment' does not need to be deliberate – it can also be unconscious). Now it is obvious why the fundamental piece of advice given to researchers by methodological textbooks is 'treat what subjects say as data, not truth'.

In contrast with that, what happens when people are subjected to research? What they say or communicate to the researcher does not generally belong to the public sphere. They do not directly represent themselves. Hence the subjects are not in a situation where they can gain anything in political or other sense. The researcher is not going to be their advocate.

When interviewed, people are asked questions rather than actively voicing their concerns. Unlike in the situation of representation, they are not agents (in particular, they are not political agents). They are reduced to 'respondents'. Answering questions in an important sense is not an act. In comparison with the situation of voluntary voicing, interviewing people deprives them of agency.

This alone however does not guard against 'embellishment'. It may happen that the subjects will try to 'embellish' themselves because they feel uneasy about revealing something that is negative about them, or they may calculate that their testimonies will indirectly find the way to public sphere. This is where the research's task lies: to distinguish between truth and 'what is told to outsiders'. Because the qualitative researcher combines the advantages of

Additionally, there may not be a unified stance within the group in question, in which case whoever represented it (most likely, the most resourceful individuals), would effectively misrepresent the voiceless of their own group. The researcher once again makes use of her insider-outsider status (being within the group but not having a position in its structure), and has a chance of building a comprehensive image instead. She decides whom to include in the sample.

Finally, there are what might be named '*theoretical*' reasons, including the following:

(i) One argument says that people under study cannot simply represent themselves because culture predominantly has the character of 'tacit', unconceptualised, taken for granted knowledge. (The metaphor of 'air' is often evoked). This is why it is easier to conceptualise it from the outside than from the inside. We can distinguish two different versions of this. First, Giddens and Fay focus on the side of the subjects. They claim that when subjects are confronted with interpretations of their 'taken for granted' knowledge, they can reflect upon it and review it. The researcher's account of subjects' meanings pushes them to a critical revision of the meanings they hold. Unincidentally this reminds us of critical theory: the product of research has emancipatory potential, is critical in nature. In this sense the researcher *goes beyond* subjects' knowledge without breaching the assumption about the primacy of 'subjects' point of view'.

Second, Gadamer (and, under his inspiration, Habermas and Taylor) have arrived at a more sophisticated argument, which I will call 'critical interpretivism'. These three thinkers claim that those interpretivists who believe that they only describe subjects' 'taken for granted' knowledge (and then disclose it to the subjects) remain restricted by the classic dichotomies: 'description-explanation', 'subject – object', and so on. By doing so, they largely miss the point about the nature of the cognitive relationship between the researcher and her subjects. They hold the classic epistemological presumption that the cognitive relation is monological, the only difference being that the researcher understands rather than observes. The researcher is still seen as a neutral cognitive tool.

the insider with the advantages of the outsider, she is in a unique position to complete this task.

'Critical interpretivists' break with these dichotomies of 'description-explanation' and 'active subject – passive object', and situate 'interpretation' outside it. They claim that the cognitive relationship between the social researcher and her subjects is, in fact, dialogical. It is wrong to think that in the research process only one side, the subject, presents monologically her meanings, values and reasons to the investigator. Gadamer and others think that instead *two* reasons 'meet' in front of each other and actively try to communicate with each other.

The researcher tries to understand the subject through, and necessarily through, her own reason. But this is a critical activity. In order to understand alien meanings (idioms, values, institutions, etc), one necessarily needs to critically evaluate them at the same time, but this means active engagement of researcher's own values and rationale. One does not understand something if one is not able to critically assess it. The rationale of the person under study stands face to face with rationale of the one who takes pains to understand her⁴⁴. This means inverting upside down classical epistemology, which calls for the researcher to be a neutral cognitive tool.

What distinguishes this viewpoint from 'critical theory', as represented for example by Giddens, is that 'critical' refers here to the nature of the research *process* and not to its final *product*. For 'critical theorists', research is critical in product, so to speak, but not in process. Gadamer and Taylor, by contrast, put at least as much stress on the emancipation of the researcher from her prejudices, as critical theorists emphasise political emancipation of the people under study⁴⁵.

⁴⁴ It does not mean that the interpreter has to disagree with her subjects. But she has to have some rationale for accepting their meanings.

⁴⁵ Kevin Dwyer's *Moroccan Dialogues* (1980) is one example of 'critical interpretation' in action. This ethnography, not incidentally, is written in the form of dialogues between Dwyer and his main informant, Faqir. On the basis of his fieldwork Dwyer puts forward the claim that anthropology is not the quest for general knowledge, but is a dialogical experience of the researcher and her subject that changes both of them. Dwyer rejects the traditional epistemological view, in which 'both the researcher and the informant are deprived of their human dimension: the researcher is reduced to a more or less mechanical receiver, the informant to a relay' (1980: 258). He thinks that in the research process, both of them 'pursue their own Selves and try to expose it' (ibid 256). The result of the encounter of the Self and the Other is that both are re-shaped. 'Self and Other are engaging each other creatively, producing new phenomenon of Self and Other, the two sometimes challenging, sometimes accommodating each other' (ibid xviii). Dwyer has no hesitation to say about himself: 'how different, how changed the anthropologist had become at the end of the summer' (ibid xxii).

Summing up, critical interpretivists showed how it is possible to favour the 'actor's point of view' and to go beyond it.

(ii) The second 'theoretical' reason for carrying out research (and at the same time the second way in which new knowledge might be generated) concerns intentions rather than cultural 'action concepts'. It is claimed that intentions are often half-conceptualised or even inaccessible, hidden, for the actors. In such cases the actor cannot simply represent her intentions⁴⁶. For example:

'Balinese go to cockfights to find out what a man, usually composed, aloof, almost obsessively self-absorbed, a kind of moral autocosm, feels like when attacked, tormented, challenged, insulted, and driven in result to the extremes of fury, he has totally triumphed or been brought low' (Geertz in Crapanzano 1986: 73)

(The conscious or 'explicit' intention would be 'I bet on cocks because I want to win some money').

There is no doubt that such interpretations go beyond, or even correct, subjects' knowledge, but do they not collide with the basic interpretive assumption about privileging the actor's point of view? Indeed they seem to override and discard the actor's *explicit* point of view. But on the other hand, are such interpretations not still about what actors really *mean*, only subconsciously? Here we arrive at a very

Importantly, the Self, when challenged, is vulnerable. Thus Dwyer criticises the image of an anthropologist who maybe is vulnerable physically and emotionally in the field, but invulnerable in other senses. Instead, he claims that the researcher is vulnerable because she challenges her own reasons, values and meanings. The same goes for the Other.

In a manner reminding one of Gadamer, Dwyer perceives the encounter between the researcher and the informant as an encounter between cultures or societies: 'because all individuals carry and express their own society's concerns, the terms *Self* and *Other* must be understood in an extended sense, as embracing yet going beyond individuals, and standing also for the cultural and societal interests expressed in individual action' (ibid 255).

This experience of two cultures or societies meeting each other has no definite result: in the same way as there is no Archimedean point of beginning the encounter, there is no point of finish from which 'definitive' meaning, valid once and for all, can be established (ibid 281): 'differences between us [Dwyer and Faqir - FS] have not been surmounted but have been, instead, more deeply articulated and have generated differences on a new level' (ibid 285); 'We were continually creating a new experience, a mutual experience where much was now shared, but also where new differences had arisen and temporarily solidified' (ibid 215).

⁴⁶ But by the same token it means that the researcher cannot simply interview her subjects but needs instead to employ some other tool to explore those intentions.

complicated problem: can meaning be 'meant unconsciously', hidden from the actor? I am going to leave this question unanswered, as it takes us into territory that is not the direct concern of this thesis.

(iii) The third 'theoretical' reason for sending the researcher out into the field (and at the same time the third way in which new knowledge might be generated) comes to light when we think about the practice of interpretive research. It becomes clear that there exists a specific bias in the view of social theorists who support an interpretive approach: they emphasise the need for understanding unique meanings, typically alien cultural idioms, or 'definitions of the situation' specific for certain subjects. This assumes that there exists a certain well-defined, geo-temporal community (ethnic group, organisation) that holds the meanings in question.

But in practice, interpretive research is definitely not confined to such thinking. More often, the researcher *uses* interpretation to solve a certain research *problem* (such as the 'moral career' of patients of 'total' institutions). This means that the subject of research is *constructed* and concerns an *aggregate* (such as social class, a risk group, etc.), rather than a community. It is not possible for people under study to represent themselves, as they are not socially linked into a group. There has to be someone – the researcher – who leads and orchestrates the study. In attempting to answer such a problem, the researcher generates new knowledge. I shall discuss this point in further detail in Part III.

(iv) I would like now to propose the last, this time my own, theoretical reason for carrying out research. Let us begin by observing that natural sciences aim at correcting peoples' knowledge about the natural world, and positivistically oriented social sciences aim at correcting actors' knowledge about their own behaviour. We can see in what sense these two enterprises are akin. As we stated, interpretive social sciences are very different to positivistic social sciences here: they claim that since the link between acting and understanding action is internal rather than external, actors' knowledge about their own conduct cannot be corrected.

I do not question this. But I believe that the *ideas* and *opinions* that underlie this conduct are corrigible. It can be that a faulty 'definition of the situation' or a morally/politically harmful (unjust, inefficient, etc.) idea drives someone's conduct.

This faultiness or harmfulness can however be pointed at and criticised. In short, I am claiming that interpretivists cannot correct subjects' knowledge about their own conduct, but they can correct their knowledge about the external, social world.

By putting forward such claim I am taking a position significantly different to the classical one (Winch) and slightly different to Winch's critics' position (critical interpretivists). Since Winch's *The Idea of a Social Science*, the discussion on the corrigibility of actors' knowledge about their own conduct has been centred on 'action concepts'. He argued that 'action concepts' (social institutions), such as 'voting', are similar to objects in the material world: they are not right or wrong, true or false - they are just out there, furnishing the social world. Hence actions that are driven by such concepts are not corrigible: actors know what they are doing, or otherwise there would be no action at all.

This argument has met two kinds of criticism. Some, for example Habermas (1984: 96, footnote 37), pointed to the fact that convention does not equal intention. What underlies the action may be the actor's interest rather than the norm linked to a given institution. This observation is right, but is it significant for us? I think that Winch could accept it but still claim that interests are 'performative' in nature - cannot be right or wrong, correct or incorrect - and therefore the actor's knowledge about his own conduct is still incorrigible.

The second kind of criticism is more difficult for Winch to fight off. We are already familiar with it: it is the critical interpretivists' argument that action concepts / institutions can be criticised in a political or moral manner (rather than corrected factually). I basically agree with this argument. But I would like to add to it another one: namely that the social world is furnished not only with action concepts, but also with 'definitions of the situations' (opinions). Such 'definitions' drive actions, but they may be *factually* correct or incorrect, as in the example of apparently unsound bank.

Now, if we come back to the starting point of this section, we can see how new knowledge can be generated. The researcher can get to know both 'definitions of the situation' and the situation itself. So, to sum up: both kinds of criticism - the rhetorical criticism of institutions and the factual criticism of opinions - go beyond actors' knowledge, but they do not deny that this knowledge was the cause of their actions. Actors are agents, but they are not unerring.

4.3.5.1 Conclusion

The answers presented above provide us with the solution to the question of a potential clash of assumptions (privileging actors' point of view) and aims (correcting knowledge): what interpreting actually does is to go beyond subjects' knowledge through conceptualising 'tacit' knowledge, through conceptualising unconscious intentions, through rhetorical criticism of ideas, and through factual criticism of opinions. By doing so, it does not however write off subjects' self-understanding. Hence interpretation meets this criterion of 'theory'.

4.3.6 Interpretation and 'technical' concepts

Let us come back here to what was written in the previous section about natural sciences: in order to create scientific knowledge they attempt to find objective rather than practical features of things; they break with measuring things with human senses in favour of mechanical measurement; they break with seeing matter through the 'human' scale in favour of 'micro' and 'macro' scales.

The obvious consequence of this is that in the course of discovering new objective features and laws there is the need for new vocabulary to name them. Natural scientists search for a neutral language that belongs neither to the researcher nor to her subjects. Since the 'human' scale is abandoned in favour of 'micro' and 'macro' scales, these new concepts are purposefully divergent from lay ones⁴⁷.

Positivistic social sciences try to employ the same philosophy in regards to the social world - they drop the 'human' scale in favour of micro and macro scales. As a consequence, the researcher's concepts refer directly to subjects' actions, without the mediation of lay concepts. Interpretive social sciences however are based on the claim that the relationship of knowledge to the social world is internal, not external. Therefore what precisely must not be done is breaking with the 'human' scale, with subjects' point of view.

⁴⁷ Although some lay concepts leak into scientific vocabulary – e.g. 'wave' or 'power' – which testifies to the ever-present need for understanding alien through familiar.

What consequences does it have for concepts used by interpretatively-minded researchers? Could interpretation contain concepts unknown to the actors, or must the researcher use actors' concepts? But as this last option is in practice impossible, the researcher must be allowed to use her own concepts. What is the nature of the link between such first-order and second-order concepts then? And following that, could such second-order concepts be 'technical', or should they only be drawn from vernacular?

Even this short list of questions suggests that we are facing a very complicated subject matter here. I am only going to put forward a few reflections on this subject rather than a well-rounded solution to it. Firstly, I will present what has already been said about 'technical' concepts by interpretivists, focusing particularly on the arguments of Peter Winch and Herbert Blumer.⁴⁸ Following that, I will try to identify why these arguments have been unsatisfactory.

(i) Winch on technical concepts

In *The Idea of a Social Science* (1958) Winch attacked the positivistic method in Sociology. To his mind, discovering regularities through observation and formulating laws is a mistake resulting from misunderstanding the nature of the social reality. What is this nature, according to Winch? He relies on Wittgenstein's ideas from the period of *Philosophical Investigations*, and particularly on two pillars of that conception: the notions of 'following a rule' and 'forms of life'. Winch's innovation consists in applying these concepts to the social world as such, rather than to the sphere of language only (as Wittgenstein originally did). He presents his argument in four steps:

- (1) All human behaviour is governed by rules [1958: 52] (but 'rule' does not mean 'order') – thanks to that, meaning and understanding are possible
- (2) Rules are social things – you cannot follow a rule outside society; you acquire rules from society; there can't be a private rule
- (3) Societies are aggregates of rule-followers

⁴⁸ Charles Taylor is another interpretivist who supports *sciences* of man and who calls for specific language ('the language of perspicuous contrast') but nevertheless he does not think of it as of 'technical' or 'scientific' language. This is why he is excluded from the analysis.

- (4) A student of society can only understand rule-following behaviour if she knows the rule – that is, only when she knows what the members of this society know about their behaviour. Hence she must be an insider to this society ('sharing the form of life')

The consequence of this conception is to rule out positivistic science – for one cannot produce generalisations/comparisons, and each rule and culture has to be viewed as unique.

Although he was an anti-positivist and opposed generalisations and inductive method, Winch does not deny that social *science* is possible. In what way does he defend this scientific character? He claims that sociologists, economists, historians, political scientists should not end their research with unreflective understanding (i.e. recognising the agents' point of view) but can take pains to understand reflectively, to understand something more, and - if necessary – they can also create technical concepts to attain that aim. However, these second-level constructs must be intelligible to the agents themselves:

'Suppose that an observer, *O*, is offering the explanation for certain person's (*N*) action: then it should be noted that the force of *O*'s explanation rests on the fact that the concepts which appear in it must be grasped not merely by *O* and his hearers, but also *by N himself*'. (1958: 46)

For example:

'It is said of *N* that he voted Labour because he thought that a Labour government would be the most likely to preserve industrial peace. [...] *N* must have some idea of what it is 'to preserve industrial peace'. [...] If *N* does not grasp the concept of industrial peace it must be senseless to say that his reason for doing anything is a desire to see industrial peace promoted' (1958: 45-47).

This postulate of 'subject's endorsement' (or confirmation) results logically from what Winch calls the 'logical tie' between first-order and second-order concepts. The researcher has to understand the actor from her point of view, and confirmation by the subject is the sign that interpretation is correct.

What is most interesting for us is that Winch indirectly tried to defend the scientific status of interpretive research by directly defending the use of 'technical', scientific

concepts within the interpretive framework. However, I would argue that his attempt was conditioned by the dominant thinking of that time, the naturalistic one. This is why Winch could not resolve the tension between his view that science should go beyond lay knowledge (and lay concepts) and the interpretive assumption about the incorrigibility of the actors' point of view (which entails the need for 'members' check'). On the one hand he postulates that social scientists should keep using technical concepts, and on the other hand he requires that such concepts have to be comprehensible for the actors themselves (which is the warranty that their point of view has not been distorted or substituted).

It is therefore not clear what the second-order concepts are for: Winch *says nothing about the purpose of creating technical concepts*, and in particular how it is possible that the social scientist knows more – or better – about someone's conduct than the person in question. He says that unreflective understanding merely precedes scientific understanding (1958: 89), but he is completely unable to specify what this further enquiry should look like. Winch prefers to leave this dilemma to social researchers, and confines his efforts to outlining philosophical arguments against leaving out the 'actors' point of view'.

In more specific terms, Winch blends two divergent conceptions of language. The naturalistic paradigm sees scientific language as neutral, independent from whoever uses it. Scientific concepts are tools. The interpretive view holds it that meaning is never independent from the user: it comes from the user, in this case from both subjects and the researcher separately. Winch sometimes claims that 'the observer cannot simultaneously use external and internal concepts'... 'we have to learn concepts through which we understand other people' (1958: section 4.2), and other times he says that social scientists are allowed to describe actions and institutions in their own jargon (such as 'liquidity preference', *ibid* 89).

(ii) Herbert Blumer's perspective on sociological concepts

Blumer is another advocate of anti-positivistic sociology. He proposes genuinely qualitative exploration of the social world, treating every empirical case as unique, explanation through particular causes (with emphasis on actors' point of view and

understanding of the situation), and abandoning the search for universal, abstract laws. Yet he does not want the interpretive scrutiny to be limited to peoples' common-sense 'definitions', and argues for the proper use of scientific concepts.

In the paper 'What is wrong with social theory?' (1954) Blumer takes pains to point out the shortcomings in mainstream social theory and offers his own solution to this problem, consisting in a fundamental change in our understanding of scientific concepts. The major shortcoming he notices is the divorce of theory from the empirical world ('Theory became a world of its own, inside of which it feeds itself', (1954: 3). This problem with linking theory and empirical research has its roots in the fact that our scientific concepts are unpleasantly vague. Were they clear, says Blumer, our theoretical assertions could be closer to empirical test and correction. Having recognised that effective functioning of concepts is a matter of decisive importance, and that the ambiguous nature of concepts is the basic deficiency in social theory, the obvious question we face is "How to make our concepts clear and definite?"

For Blumer, the most serious attempt to answer this question consists in looking for operational definitions. But the main disadvantage of such a solution is that operational definitions lack theoretical possibilities. He does not totally discredit the chances for successfully resolving this difficulty, but claims that "there still remains what I am forced to recognise as the most important question of all, namely whether definitive concepts are suited to the study of our empirical social world". (Blumer is fully aware of how heretical that question is). He proposes the negative answer and develops his own perspective. The concepts of our discipline, he says, are fundamentally *sensitising*, not definitive, instruments. While definitive concepts refer precisely to what is common to a class of objects, sensitising concepts give us 'a general sense of reference and guidance' in approaching the empirical world. They lack specification of attributes and consequently one cannot build a clear definition of them.

Why are our concepts not definitive? It is not due to a shortage of scientific sophistication or immaturity of sociologists, but due to the very nature of the social world. Simply speaking, every object in this world (a person, group, institution and so on) has a unique character; it is not like in the natural world, where elements are

the same everywhere and every time. Therefore looking for distinctive features is as important as looking for common ones. These are sensitising concepts that allow us to find this mixture of unique and common in every empirical phenomenon, for they 'merely suggest directions along which to look', they do not restrict our attention only to certain features. But, Blumer says, the fact that sensitising concepts are not definitive does not imply that they must be vague. Indeed, they can be tested, improved and refined in the course of scientific scrutiny. "Relevant features of such [empirical] instances, which one finds not to be covered adequately by what the concept asserts and implies, become the means of revising the concept" (1954: 8).

Thus Blumer, like Winch, cannot escape the pressure of the naturalistic framework and ends up blending two conceptions of language and reality that are in tension. On the one hand, he remains bound by a monological conception of research, where theories are 'tested' and 'refined', and he believes in neutral language that would represent clearly the social world. On the other hand however, Blumer emphasises the uniqueness of actors and institutions.

This last observation is a plus to Blumer, but nevertheless his thinking remained confined here by the naturalistic dichotomy 'distinctive – common'. It may be suitable for the natural world, but it does not work for social scientists, let alone interpretivists. Ideas and social institutions do not form a system in the way matter does, and hence they are not classifiable along 'distinctions' and 'commonalities'. Following that, Blumer's ideal of 'clear' and 'adequate' language cannot be realised in the social sciences because institutions and ideas, unlike matter, do not have 'models', so to speak, that would be replicated or 'mirrored' in language. Institutions such as 'family' cannot be talked about in the same manner as natural phenomena. Blumer wants to eliminate 'noise' from social-scientific language – but he did not understand that as the cognitive process is a critical one, the 'noise' is an inextricable part of 'meaning'.

4.3.6.1 An alternative view of the use of technical concepts in interpretive inquiry

It is clear that the above two attempts at conceptualising the relation between interpretivism and technical concepts are not successful. I shall now propose an alternative way of approaching this problem, which builds on my earlier analysis.

In the previous section I discussed the problem of the tension between privileging the 'actor's point of view' and going beyond it. But in the case of interpretive research an analogous problem exists, not only when we think about subjects' and researcher's 'knowledge', but also when we think about their 'language'. How can we privilege actors' concepts and yet render them in theoretical, scientific concepts? Only if this tension is resolved we can endow interpretive research with scientific status.

Perhaps it is possible to transfer our reflections from one situation to another, from 'knowledge' to 'language'. There I suggested that the key to solving it is the auxiliary question 'why not let people simply represent themselves?', and identified three groups of reasons for sending the researcher out. So, let us now ask when it is not possible or not recommended to use actors' own concepts. Here again, there are practical and theoretical reasons for doing so.

In *practical* terms, there are at least two kinds of situation in which interpretivists cannot use subjects' concepts but need to employ their own. One is when the people in question use a language different to ours. Of course often there are identical or very close concepts in different languages, in which case we would not consider it to be a tension between subjects' concepts and researcher's own concepts. But sometimes people under study use an idiomatic meaning that has no analogue in our ethnic or class language. In this case there is a problem of diverging from the 'actor's point of view'. Another situation is when actors' conceptualisations presented to the researcher are too context-charged to be presented to the readers, who are unfamiliar with this context. Therefore the researcher has to either neutralise the context or contextualise the meaning, both of which require the use of her own concepts.⁴⁹

Turning now to *theoretical* reasons, there are a few kinds of cases to consider.

⁴⁹ In a certain important sense being faithful to the interpretive ideal of rendering subjects' concepts as they are *verbatim* would be nonsensical – or, rather, would be incomprehensible for the readers. The point is that the lack of researcher's 'mediation' would destroy the original meaning, because without the context of its occurrence this meaning is mutilated (Wittgenstein). It is not possible to quote an expression or to describe an action without commenting upon them – but this means the introduction of second-level concepts, concepts that do not belong to the subjects.

Firstly, when the interpreter explores unconscious or hidden intentions of actors, she naturally employs her own concepts as well. In this case there are no original concepts held by subjects, and hence there is no problem with the link between them and researcher's concepts.

Secondly, the researcher has to apply her own conceptualisation when she conducts a study of a 'problem' rather than of a community. For example, patients of total institutions do not think they have a 'mental career' – but Goffman found it a useful concept to summarise his research problem.

Thirdly, the researcher has to put forward her own concepts when the meaning in question is 'tacit', of 'lower-level' character. In the previous section I argued that the most advanced conception of interpretive understanding has been put forward by 'critical interpretivists' (Gadamer, Habermas, Taylor). What view of the researcher's concepts do they hold? There is little or no explicit discussion of this topic in the works of the above authors - Taylor wrote about the 'language of perspicuous contrast', but rather vaguely - so I will analyse it myself.

For 'critical interpretivists', the cognitive relationship between the social researcher and her subjects is dialogical rather than monological. It is not that the subject presents in a one-directional manner her meanings, values and reasons to the investigator, but rather that the two 'meet' and actively try to communicate with each other. The researcher tries to understand the subject's meanings necessarily through her own reason. This means that the researcher actively criticises what she is trying to understand.

But this logically entails that interpretive concepts are and have to be critical in nature. They incorporate the outcome of two sets of reasons – two meanings – confronting and criticising each other. Interpretive concepts are parallel to subjects' concepts, not one level above them. As they belong either to the researcher or to the subjects, they are not neutral. But this means that they are not 'technical' either, since 'technical' entails that something is independent from whoever uses it.

Summing up, critical interpretivism resolves the tension between privileging subjects' concepts, not writing them off, and going beyond them, using the researcher's own concepts.

The *fourth*, and last situation when it is necessary to employ researcher's own conceptualisation concerns my earlier proposition about distinguishing between the rhetorical criticism of subjects' *concepts* and the factual criticism of subjects' *opinions*. What has already been said about the tension between subjects' concepts and researcher's concepts refers to the situation when ideas/institutions are interpreted. I have suggested however that what can be criticised is not only concepts but also *opinions* ('definitions of the situation').

What does this imply for our discussion of the tension? As the criticism is factual rather than political/moral, it means that the researcher comes up with a new, revised sentence (statement) rather than with a revised concept. For example, saying that 'the bank was actually sound' does not change or criticise the meaning of 'bank' held by subjects.

In conclusion, I believe that the remarks above could be a starting point to a productive research project about the language of interpretive sociology. The foundation claim here is that interpretive concepts are critical (rhetorical) by nature, rather than 'technical' or 'neutral'. In the following chapter we will have an opportunity to find if we are on the right path, by examining actual cases of interpretive research.

Chapter 5

Interpretive Theory: Three Case Studies

5.1 Introduction

In the current chapter I want to put flesh to the body of the abstract analyses from the previous chapter by confronting them with three actual examples of interpretive research 'in action'. For logical reasons this exercise cannot constitute a test of accuracy of my arguments and classifications, but instead it will serve a number of functions. Firstly, and most obviously, it will help the understanding of abstract arguments by providing an illustration to them. Secondly, it will suggest to us which elements of our abstract analysis are overemphasised, which are underestimated, and which – potentially – have been overlooked. A look at practical works will help us see the previous chapter from the distance and by doing so to modify what needs to be modified. Next, it will enhance our understanding of qualitative research practice, about whether it can have a 'theoretical' character. Perhaps this character will turn out to be make-believe? If most prominent studies are faulty (theoretically & methodologically poor), it is telling about the state of qualitative research as such. Fourthly, it will help us establish whether the scheme outlined in chapter 3 constitutes a *working* tool for evaluating interpretive studies from a theoretical point of view (in particular, for exposing drawbacks of qualitative 'theories'). Finally, these three case studies are also supposed to show whether my modification of Fay's conceptual scheme works or not.

5.1.1 The criteria of selection

I have decided to choose prominent studies as a representative sample rather than sampling randomly from the whole body of numerous qualitative research studies. If I sampled randomly and picked three weak studies, it would not tell us much.

To reflect the current state of the art in qualitative research, all three studies are contemporary. Arlie Russell Hochschild's *The time bind: when work becomes home and home becomes work* has been critically acclaimed by both academic and non-academic reviewers. It was voted 'book of the year' by New York Times and is commonly found on university courses' reading lists. Gerd Baumann's *Contesting culture: discourses of identity in multi-ethnic London* has also attracted good reviews and wide student readership. David Wagner's *Checkerboard Square. Culture and resistance in a homeless community* won the Charles W. Mills Award in 1994.

A brief remark has to be made here. Namely, it is actually not obvious where exactly to look for examples of interpretive enquiry. This is the case because using qualitative research techniques does not mean that the researcher necessarily supports interpretive assumptions. Particularly in the case of structuralist ethnography it often happened that participant observation was used not for interpreting the meaningful content of subjects' point of view, but for finding hidden structural mechanisms through comparing what the subjects say with what they do. ('Treat what your subjects say as data, not as truth').

An example here would be Ernest Gellner's study of Moroccan Berbers and their institution of *igurramen* (1987: chapter 2). Gellner found that the key to understanding this institution is not what Berbers say it is (and they truly believe in it), but what functions it serves and how it is structurally reproduced. These latter issues looked differently in reality and in the Berbers' account of them, and therefore the subjects' 'point of view' was not privileged in the manner of interpretive social science.

Hence if we want to select genuinely interpretive studies, we need to apply an additional criterion to the totality of 'qualitative' studies. As we shall see, the three works mentioned above are not only qualitative but also comply with interpretive assumptions.

5.2 Hochschild: *The time bind*

5.2.1 General remarks about the book

Hochschild's book is an exploration of the worlds of work and home as experienced in everyday life in the era of corporate capitalism. Her initial research question was about family-friendly policies: she wanted to find out if they work, and as she expected the answer to be positive, she was planning to help this policy become common by disseminating her research findings and engaging critically in public debate. Hochschild is very knowledgeable about the realities of work-family balance in contemporary societies. As more mothers enter employment, and more employees work longer hours, the world of family life faces major damages. But how do people respond emotionally and organisationally to this situation? In terms of practical arrangements, they relegate traditional parental duties to third parties such as babysitters, childcare centres, summer camps, old people's homes, home nurses. They consume ready-to-eat meals to save time. They apply principles of Taylorism at home. The time at home becomes yet another shift, when one has to be well organised, quick and efficient. In terms of emotions, they are under huge stress: they simply have not enough time to relax, to maintain and repair relationships with loved ones.

It seemed logical to Hochschild therefore that employees of a family-friendly company would make good use of its new policy, to have more time home for their families. To her surprise, it turned out not to be the case. Part-time options, flexi-place and paternity leave were very unpopular. Flexi-time was used by a third of the workforce, but it didn't stop people working longer than scheduled or taking overtime.

Faced with this puzzle, Hochschild began hypothesising potential answers and looking for empirical data that would support or reject them. She chose to study qualitatively 'Amerco' (real name disguised), one of the top-200 US companies, renowned for being a leader in implementing family-friendly policies. Hochschild spent three summers researching it, mainly interviewing employees (130 respondents in total, across the whole hierarchy) but also observing meetings,

studying internal records and 'climate' surveys, as well as following six families dawn to dusk.

Hochschild selected four hypothetical answers to the question of preferring longer work to shorter, 'family-friendly' workdays. These were: that people work long hours because they need money; because they are afraid of being laid off; because their managers did not want them to work shorter hours; because they were ignorant of the policies. But it turned out that none of these answers was valid. Instead of that, in the course of her fieldwork Hochschild discovered that many employees did not want more time at home because they found home life even more stressful than work. Work offered them personal fulfilment, appreciation, friendships, community, and emotional support. Home, on the other hand, meant mostly rushing with tasks, rational planning and time management, arguments with spouses and children, and little appreciation. Therefore the worlds of home and work have become 'reversed'.

5.2.2 Theoretical frame

Nowhere in the text does Hochschild talk over the theoretical base of her research. Explicitly, she makes only minor remarks about her theoretical assumptions, such as 'I wanted to end up with a non-gendered book. In a way this whole family-friendly project is informally coded as a women's project. I wanted to de-gender it'. Therefore we need to carry out some detective work to expose this theoretical base.

The first thing that is distinct about Hochschild's theoretical approach is that 'Amerco' is treated as an example of a given phenomenon rather than a distinct case on its own. Although the method employed was ethnographical, Hochschild's aim clearly was to generate theory rather than to simply understand a unique meaning. There are strong arguments therefore to understand her work as a case study⁵⁰.

Secondly, Hochschild's theoretical approach is heterogeneous; there are three angles present in the book – interpretive, structural and critical - of which interpretive is the central one. Very briefly, Hochschild's ontological assumptions can be

⁵⁰ De Vaus claims that the case study methodology is 'fundamentally theoretical' (De Vaus 2001, chapter 15).

reconstructed in the following way: Private lives and actions are heavily conditioned (although not completely determined) by public (social) structures. Facing such pressures, individuals cope as well as they can, but on their own are rather helpless and destined to lose the battle with the structures. However, collectively we can reshape those structures and improve our lives. Let us have a closer look at these theoretical assumptions now.

Interpretive sociology. Several passages indicate that Hochschild's aim was to learn about transparent and latent meanings, motives, evaluations, particularly those concerning family life and professional life. For instance:

'What *is* a home? What *is* work? Imagine this book as an investigation into the ways we think about these two places and the states of mind we take to and from each of them' [2001: xxiv].

The above quotation reveals Hochschild's research presumptions as *interpretive* ones: she took pains to understand subjects' lives from their point of view, to grasp their 'definitions of the situation'.

If Hochschild stresses the role of social structures so much, why is she insisting on interpretive research? Basically, it is because she does not think that knowing structures means knowing *why* people choose *particular* responses to them. In our case, actors should rationally choose using family-friendly policies, to improve their home-management. But as they actually did not follow this path, it is necessary to diagnose why that happened. This however is possible only in a qualitative way, which is through paying attention to actors' points of view⁵¹.

Structural sociology. Hochschild's interpretive emphasis on the qualitative cognition of subjects' 'lifeworld' does not clash with the fact that the widest context of the work is a *structural* one. Although Hochschild does not use such concepts as 'system', 'structure', 'function' (NB without any detriment to the book), her work can be

⁵¹ Williams (1998) situates Hochschild within the 'interactionist' tradition that roots the experience and analysis of self (and emotions) in social interaction. But it is quite clear to me that Hochschild does not see interaction as the source of 'the time bind' and emotions that follow. Quite the contrary, it is structural macro-organisation that affects individual selves and interactions. Hochschild is not in the same camp as Goffman.

regarded as an application of the classical sociological assumption that individual lives are conditioned by social macrostructures. In this particular case the author is interested in the way the global capitalism of the late 20th century influences professional and family life of individuals, whether they want it or not. In this vision, individual agency is limited to navigating through conditions that exist independently from it.

The author does not avoid speaking about phenomena traditionally associated with 'hard' sociology, such as unanticipated consequences or objective relationships existing independently from subjects' 'definitions of the situations'. For example, when talking about odd-hours shifts at work (one day-shift followed by one night-shift, without the possibility of planning the weekly schedule in advance), she writes:

'While other people came home from school, ate dinner, and watched TV, the 3p.m. to 11p.m. shift was hard at work. While others slept, the 11p.m. to 7a.m. shift was wide awake. While others went home on Friday at 5p.m., many in the plant had just reached the middle of their workweek. Meanwhile, *such shift work put tremendous stress on friendships outside the factory.* [...] There was simply too little overlapping time (2001: 189, italics mine).

Or other examples:

'The more attached we are to the world of work, the more its deadlines, its cycles, its pauses and interruptions shape our lives and the more family time is forced to accommodate to the *pressures* of work' (2001: 45, emphasis mine).

'Capitalism and technological developments have long been gradually deskilling parents at home' (2001: 209)

In a certain sense, the book could be even treated as a 'Mertonian' study of a system that is functional for corporate capital and dysfunctional for individual psyches (dynamic functionalism; see Merton (1957) and Sztompka (1986)). Without employing the concept of the 'conflict between subsystems', Hochschild frequently points at phenomena of this kind:

'Such men in the middle [of the company's hierarchy] might seem poised to resist the process by which the worlds of home and work were being reversed, but they felt torn between the pressure to do more at home and a company-supported image of the serious player as a long-hours man' (131).

Having said that, Hochschild cannot be called a structuralist as she does not believe that structures determine peoples' conduct. What structures do is to produce tensions and resources – but it is up to people's creativity, rationality and emotions what action they will take. Indeed, Hochschild's book shows how it is possible to combine qualitative research with classic sociological assumption about the conditioning of actions and 'lifeworlds' by social macrosystems⁵². What matters here is that the author, although interested in structures, does not take the path of statistical correlations of operationalised variables - which would be natural for many structuralists - but uses quantitative data in a non-correlative way, to collect information about populations. There is no 'null hypothesis' in Hochschild's work⁵³. Associations are discovered qualitatively, not statistically.

Critical (engaged) sociology. Finally, Hochschild's research was guided by a mixture of descriptive and normative questions. As for the latter, she wanted to know whether work could be organised in such a way that we avoid penalising people for lives outside work, and we avoid penalising those they care for. Hochschild believes that the full exercise of agency in this case may occur only through collective emancipatory action, a social movement that would change the oppressive conditions (2001: chapter 16). She calls it 'the time movement', since its aim would be to regulate the maximum amount of hours worked weekly.

It is significant that throughout the book she often uses the word 'we', which indicates that those issues concern the reader as well. Readers' lives are subjected to the same conditions as Amerco employees' lives are: we are all time bound, and our family life (and ultimately happiness) is damaged by it. Thus Hochschild's research reminds one of C.W. Mills' call for sociological studies that are sensitive to the link between 'personal troubles' (or personal biography) and 'public issues'⁵⁴.

⁵² Here one can discern how the academic teaching of Sociology, in order to simplify the process and make students understand complex issues, binds 'qualitative' with meaning' and 'quantitative' with 'structure', and contrasts one to another.

⁵³ With minor exceptions. For example when she tested the hypothesis about rigid managers being the cause of why people do not use family-friendly policies, Hochschild assumed that 'rigidness' and 'flexibility' would not causally matter and then compared it with quantitative data.

⁵⁴ Williams (1998) even argues that Hochschild's book is a 'critique of human condition', but it seems to me that it would be more accurate to talk about a critique of a *social* condition. Human condition, as treated by grand *oeuvres* of literature and philosophy, refers to what is common to all people rather than society-specific.

5.2.3 What kinds of interpretation does Hochschild put forward?

To explore this question I shall employ the modified scheme of Brian Fay's. This means that I will take into account not only explicit intentions but also unconscious ones, as well as 'definitions of the situations' and non-constitutive concepts.

The Time Bind contains several interpretations of intentions. Most of them are known to actors themselves, but Hochschild also often puts forward interpretations of unconscious intentions, as in the following case:

'Along with the tools, perhaps John has tried to purchase the illusion of leisure they seemed to imply. (...) John's tools seemed to hold out the promise of another self, a self he would be "if only I had time." (2001: 14)

Hochschild also interprets several 'definitions of the situation' held by the subjects, particularly definitions concerning how they felt about their 'work' and 'home':

'To Linda, her home was not a place to relax. It was another workplace. [...] For Linda, home had become work and work had become home' (2001: 37-8).

Hochschild does not provide many interpretations of constitutive and non-constitutive concepts (ideas, values), but there are some cases of this, such as:

'Efficiency has become both a means to an end – more home time – and a way of life, an end in itself' (2001: 212)

'Many working parents [...] want not simply more time, but a *less alienating sense of time*. As one Amerco working mother put it, "I love my job, I love my family, and I don't want to move to the country. But I wish I could bring some of that ease of country living home, where relationships come first." In this alternative view, *time is to relationships what shelters are to families, not capital to be invested, but a habitat in which to live.*' (2001: 52, italics mine)

'Amerco working parents... like most Americans... thought they were free, but they didn't *feel* free' (2001: 243)⁵⁵.

⁵⁵ Hochschild also gives many insights into the nature of social phenomena that at first sight seem to be interpretations. (Particularly when they are mixed with other, real interpretations). For example:

'Capitalism and technological developments have long been gradually deskilling parents at home' (2001: 209)

'What the family used to produce – entertainment – it now consumes' (2001: 210)

5.2.4 Are Hochschild's interpretations theoretical?

Now I am going to analyse whether Hochschild's interpretations meet five criteria of 'theory' discussed in the previous chapter: that theory is general/abstract/precise, that it is independent and systematic, that it is explanatory, that it goes beyond subjects' knowledge, and that inferences can be drawn from it.

The first two of these can be dealt with very briefly. Hochschild's interpretive theory of the 'reversed worlds' of home and work is consistent and fairly precise. Since it keeps the necessary link with the actors' point of view, it is not abstract. Its generality is limited to the world of economy dominated by corporate capitalism and not yet counteracted by a collective effort to balance time spent working with time spent at home.

Further, Hochschild's theory is independent: it does not need the support of any other theory. It consists of two integrated elements – what is work, what is a home – which makes it a basic 'system'.

The third criterion, of theory being *explanatory*, requires much more extensive discussion. In the previous chapter I discussed in general terms whether interpretation can be explanatory. I argued there that although an interpretation is not a general law and does not deal with empirical regularities, it may still be legitimately considered to be explanatory. It explains human actions (understood as individual and indeterminate rather than as empirical regularities) and cultural idioms. It explains either by exploring the 'teleological cause' or by bringing down what is alien to what is familiar. Let us now analyse how Hochschild's interpretations fit into this picture.

First, I want to consider Hochschild's central interpretation which postulates 'the reversed worlds of home and work'. Is the central part of Hochschild's analysis (i.e. the interpretation of 'reversed worlds') explanatory? At first sight, it is the 'reversed

'While the mass media so often point to global competition as the major business story of the age, it is easy to miss the fact that corporate America's fiercest struggle has been with its local rival – the family' (2001: 203)

But at second sight it becomes obvious that such statements do not refer to what people implicitly or explicitly think or feel, but rather they point to structural relationships.

worlds' phenomenon that explains Amerco's employees conduct: people prefer to spend more time where it feels more homely (even if it is their workplace) and less time where it doesn't. But actually in Hochschild's view the causal nexus is more complex. Everything begins in a structural way: the changes in modern societies and modern capitalism allowed for the majority of women to undertake full-time jobs. This results in a serious time deficit regarding the management of family and home. Today parents need to carry out the 'second shift' after work. This causes extra tensions and consequently diminishes the attractiveness of home life. The fact that big companies nowadays 'engineer' special work culture for their employees is an additional factor. Companies play the role of a parent rather than a supervisor. They bring a lot of services next to the workplace: hairdressers, nurseries, dating agencies, gyms, etc. They try to make their employees love their workplace rather than be indifferent or hate it. This factor enhances the attractiveness of work.

Facing this combined relative unattractiveness of home and relative attractiveness of work, one could think that, rationally, people should try to spend more time at home to improve its management and ease stressed relationships there. In short, they should make a good use of family-friendly policies, particularly part-time and job sharing. But in reality people choose a psychologically easier option of escaping home in favour of working longer hours.

Hochschild makes use of 'teleological' explanation here. Let us notice that she could have left out their consciousness from the analysis and claimed that there must be some 'external' reason for them not using family-friendly policies (when rationally they should use them). But she preferred to study their actual 'point of view'.

Summing up, the 'reversed worlds' interpretation explains why workers take longer hours and do not use family-friendly policies. But on the other hand it is suggested that the phenomenon of 'reversed worlds' is caused itself by structural changes in American economy and society. Meanings guide actions but meanings are influenced by structures. Therefore Hochschild's interpretation of the 'reversed worlds' is explanatory but does not exhaust the explanation, does not constitute 100% of it.

Moving on to the minor interpretations put forward by Hochschild shows that most of them are explanatory in a 'teleological' way:

'Bill was a better father at work than he had been at home [...]. It was simply more satisfying being Dad here than anywhere else' (2001: 63)

Some of Hochschild's interpretations do not explain 'teleologically' but instead are 'mediatory' – they bring us into the 'lifeworld' of Hochschild's respondents:

'Many working parents [...] want not simply more time, but a less alienating sense of time. As one Amerco working mother put it, "I love my job, I love my family, and I don't want to move to the country. But I wish I could bring some of that ease of country living home, where relationships come first."

'Amerco working parents... like most Americans... thought they were free, but they didn't feel free' (2001: 243)

Interpretations such as the above explain to us 'what' Amerco parents are like rather than 'why' they act in a certain way.

Let us now move on to the fourth criterion, that *inferences can be drawn from theory*. I have mentioned that interpretation may entail moral and political inferences. In this sense, Hochschild's book contains a clear political message. She found out that family-friendly policies are not a good solution to the 'time bind', as employees cannot or do not want to work shorter hours. Hochschild believes that the oppressive conditions that create 'the time bind' could be changed only through a collective emancipatory action, through a social movement. Employers have to be forced to a 40-hour week.

Finally we can consider the fifth criterion, of *theory going beyond everyday knowledge*. In the previous chapter it has been asked if interpretive social sciences can correct subjects' knowledge without contradicting their own basic assumption. I suggested that it is possible for interpretation to go beyond subjects' knowledge through critically conceptualising 'tacit' knowledge, through conceptualising unconscious intentions, and through factual correction of opinions. Below I am going to look at Hochschild's interpretations from this perspective.

Does *The Time Bind* correct the subjects' knowledge? Hochschild does not say anything directly about how her findings are related to the subjects' own knowledge. Some of her claims suggest however that Hochschild is convinced that her findings go beyond the subjects' knowledge, as in the following sentence:

'Amerco working parents... *remained blind* to the enormous constraints they lived under' (2001: 243, emphasis mine).

In the following paragraphs I am going to discuss in sequence her central thesis and her minor interpretations.

Does Hochschild's interpretation of 'reversed worlds' go beyond subjects' knowledge about it? She suggests that the 'reversal' thesis is her original input to the social scientific knowledge. We need to distinguish here three aspects of this thesis. The first concerns the meaning of home and work. Do Amerco employees themselves conceptualise their situation as 'work has become home and home has become work'? Taking things literally, they do not. None of the interviewees conceptualises things exactly in this way, though some of them are close to it, for example Daniel:

'Driving me back to my bed-and-breakfast in the family van, Daniel shared his thoughts about how to escape the cycle his family seemed hopelessly trapped: "There are no easy fixes to the balance between home and work. Family teamwork is crucial. We need to transfer the idea of teams we have in sports and *production* to the *family*" (2001: 113, emphasis mine).

What Hochschild did with her interpretation was making explicit, conceptualising what was *felt* rather than thought of by the subjects. In this sense, she did not correct it but went beyond it, so to speak.

The second aspect to consider is causality: did Hochschild have special insight into causality? Does her thesis correct subjects' understanding of *why* they do what they do? Hochschild preliminarily distinguished between Amerco's managers and factory floor workers' reasons for not using family-friendly policies, but ultimately came to the conclusion that the 'reversal' of home and work concerns them equally, regardless of the character of job they were doing. This however does not find

confirmation in interview quotations. Managers conceptualised their reasons for working long hours as follows:

'We hire very good people with a *strong work ethic* to start with... People look around and see that. So then they work hard to *try to keep up*, and I don't think we can do anything about that...The environment is *very competitive*... *We impose [long hours] on ourselves*. We're our worst enemy' (2001: 56-7, emphasis mine).

Hence managers pointed to *competition* as the causal factor rather than to attractiveness of work and unattractiveness of home. Hochschild's thesis finds however *some* confirmation in factory floor workers' accounts; they recognised that the limited attractiveness of home was the cause of working long hours:

Michael: 'A lot of my coworkers come to work just *to get away from home*' (2001: 160, also 186, emphasis mine).

Mario: 'I work 50 percent for need, 25 percent for greed. A lot of it is greed. And 25 percent is *getting away from the house*' (2001: 179, emphasis mine).

Linda: 'So I take a lot of overtime. The more I *get out of the house*, the better I am' (2001: 38, emphasis mine).

Having said that, few factory floor workers she interviewed found their work attractive. Therefore it can be claimed that Hochschild got things wrong in the case of managers and did not bring anything new in the case of factory floor workers. Where Hochschild however went beyond people's knowledge regarding causality is in her argument that the tensions employees face have structural sources.

The third aspect concerns the scale of the phenomenon of 'reversal'. Here we find that Hochschild *corrected* subjects' knowledge. Everyday people whom she talked to did not realise that the 'reversal' is that common. Unlike Hochschild, they did not have access into other people's feelings about this problem. Therefore the main difference between the researcher and the subjects was that the former had insight into a number of cases.

Moving on from the central interpretation to the minor ones provided by Hochschild, most of these conceptualise subjects' intentions and the meanings held by them, as in the following examples:

'For top managers like Bill Denton, having family photos meant: I take an enormous amount of time from my family for my work, and since I do this despite the fact I love my family, you can see how committed I am to work. For secretaries, having family photos often meant: I have another life. I may be subordinate but I express myself full at home' (2001: 85-88)

'Efficiency has become both a means to an end – more home time – and a way of life, an end in itself' (2001: 212)

This kind of interpretation does not correct or write off what subjects mean, but rather conceptualises what is implicit or felt. As such, it goes beyond their knowledge about themselves.

The final issue that needs to be mentioned when discussing this general criterion is that of technical concepts. Generally speaking, Hochschild uses plenty of her own concepts, but they are of everyday rather than 'technical' character. As Hochschild conducted her research in a same-language community, in theory she could have kept the subjects' conceptualisations literal. But she had to use her own concepts when the subjects' knowledge was not explicit (for example, in the case of the reversed worlds of home and work) and in the case of unconscious intentions. The way she uses concepts does not breach with interpretive assumptions.

In conclusion, out of the criteria that can be met for interpretations to be theoretical, Hochschild's interpretations fulfil most. Her main problem is with 'going beyond subjects' knowledge'; the thesis of 'reversed worlds' correctly grasps how people felt about it, but it misjudges the causal role of these feelings: In the case of managers completely, in the case of factory floor workers partially.

5.3 Baumann: *Contesting culture*

5.3.1 General remarks about the book

Contesting Culture (1996) is the outcome of Baumann's qualitative exploration of Southall, a borough of 60,000 inhabitants in West London. The town is famously called 'the capital of South Asians in Britain' (1996: 41). Indeed, it is a home to a mixture of cultures and communities from this part of the world: the religious composition is Sikh 51%, Hindu 16%, Muslim 15%, Christian 8% (1996: 100).

Baumann however chose Southall not particularly because of South Asians but because he wanted to study how immigrants deal with multi-cultural complexity. Are they 'suspended between two cultures', as the dominant view holds, or do they 'reach across several cultures'? (1996: 1, 2). Baumann's hypothesis was that Southallians are an actual example of the inadequacy of what he calls the 'dominant discourse' about culture. This kind of discourse means seeing culture as a rigid, external entity, as well as conflating community, culture, and ethnic identity. Peoples' conduct is then reduced to the symptom of this equation (1996: 6). Following that, Baumann hypothesised that in reality Southall is rather the arena of the 'demotic discourse' – where the above equation is denied and replaced by the vision of cultural elements being negotiated and mixed ('Making culture, not having a culture'). Analogously, community building is believed to be a dynamic process.

During the course of his fieldwork Baumann found that actually Southallians engage in both kinds of discourses, depending on the context (1996: 31). He uncovered a significant discrepancy between the way Southallians *think* about their cultural background and the way they *use* culture as the key factor in community-building processes. In everyday life they are fully aware of cultural complexities within each culture and between different cultures, and are sensitive to 'shades' rather than holding black-and-white perspective. When it comes to political competition for resources however, they abandon this sensitivity in favour of reifying culture. Baumann summarises this finding by stating that Southallians possess 'dual discursive competence' (1996: 144).

5.3.2 Theoretical frame

Baumann does not explicitly say what social theory he supports. In general, the way he approached research is distinctively contemporary – firstly, the book focuses on social processes rather than states, and secondly, it is cross-disciplinary: there is an almost equal deal of attention paid to history, politics and economy as to culture. Although the method of fieldwork Baumann employs is mainly associated with ethnography, there is no point in arguing whether his work is anthropological or sociological. The work is a clear example that contemporary social science/research, following the increasing complexity and processuality of contemporary societies, becomes an amalgam of anthropology, sociology, history and political science. *Contesting Culture* covers numerous historical and political facts and interpretations of these facts by the subjects themselves.

But in order to learn about Baumann's particular theoretical assumptions we need to reconstruct them from the bits scattered across his book. Most fundamentally, he is concerned about avoiding the usage of pre-manufactured sociological categories, not imposing them on people under study. This is an inherently interpretive *credo*:

'By stereotyping informants as 'belonging to' or even 'speaking for' a pre-defined 'community', one runs the risk of tribalising people, instead of listening to them, and might end up studying communities of the researcher's own making.' (1996: 8)

'Social groups should be distinguished from social categories, whether these be called tribes or communities, races, generations, or castes.' (1996: 7)

These and other passages indicate that Baumann is a severe critic of both the dominant discourse and of classic structural sociology. Baumann points to the fact that in such accounts of the social world 'All agency seemed to be absent' (1996: 1), and he prefers to see people as active meaning-makers rather than passive meaning-recipients. His emphasis that the second generation of immigrants 'reach across several cultures' rather than being 'suspended between two cultures' (1996: 1, 2) is a good illustration here. All this suggests that to Baumann the social world is made by the subjects, not by pre-existing structures. It would probably be most apt to say that his work has strong parallels with Symbolic Interactionism, despite the fact that he does not use this term in the book. In line with his views, *Contesting*

Culture is relatively rich in accounts of concepts and 'definitions of the situations' held by the subjects.

On the other hand, however, Baumann warns against taking subjects' own conceptualisations unreflectively, at face value:

'All of these terms [such as tribes, communities, races, generations, or castes – F.S.] designated categories that Southallians might, or might not, use in one context or another. The task was to document their uses, rather than take the words *at face value* and then peddle them as self-evident analytical concepts' (1996: 7-8, italics mine, also similar statements on page 6).

What is Baumann's point here? He means that people in some situations can use concepts in a strategic way, to achieve certain effects, rather than to express what they actually understand by these concepts. (As will be shown soon, in particular he means that Southallians can use the notions of 'culture' and 'community' in a 'dominant' way, to gain resources in the competition between communities). If so, then such strategic usage should not be treated in the same way as everyday usage. Rather than treating it interpretatively, the researcher should discard the face meaning and should look for the underlying reasons to use such concepts strategically⁵⁷.

In other words, people are seen as creative, strategic beings: they are guided by meanings in everyday life and by interests in the public arena. In the latter case, they manipulate meanings. The problem with this claim is that Baumann is not clear about whether his subjects are aware of being strategic or not⁵⁸. It is important because this part of Baumann's assumptions potentially does not accord with interpretivism.

⁵⁷ All this reminds very strongly of the argument put forward by Ernest Gellner (1987: chapter 2). Gellner used there an example of the Berberian concept of 'baraka' to show that interpretation should not be applied when interests determine strategic conduct. In this case, a specific group of people were endowed with prestige and resources, allegedly thanks to God's will but in reality thanks to inheritance. She who tries to interpret the meaning of 'baraka' would be misled, as this meaning does not reveal social interests and social mechanisms behind it.

⁵⁸ For example, Bourdieu and Gellner are explicit that their subjects are not aware of acting strategically.

The diagnosis of Baumann's theoretical stance is even more complicated by the fact that sometimes his explanations mix the interpretation of intentions with the analysis of structural conditioning. On page 66, he is writing about community leaders:

'I have often marvelled at the painstaking and ceaseless toil that some community leaders are prepared to shoulder beside their full-time jobs. Given that competition and scarcity often spell failure rather than success at securing resources, why do they continue? The answer is threefold. Community leaders are often motivated by a strong moral sense of justice for, and service to, their claimed constituencies; they are able to function because the political establishment has co-opted them as representatives of their communities; and they gain from their efforts access to more desirable social networks and the respect or gratitude of those they have served'.

The first and the last elements of the explanation point to intentions, while the middle one refers to a structural condition. In Schutz's useful terminology, Baumann once points to in-order-to motives, and other times to because-of motives. More generally we can say that Baumann's book is theoretically heterogeneous, with interpretivism being the dominant perspective.

5.3.3 What kinds of interpretation does Baumann put forward?

Baumann interpreted about a number of Southallians' intentions, such as:

'Most adult Southallians profess a desire to move out and, in a euphemism for social advancement, move 'up the road' (1996: 43).

Some of those intentions are self-explanatory and do not need any further interpretation. But others are puzzling and pushed Baumann to explore them deeper. For example, Baumann found it difficult to understand why the desire to 'move up the road' was so common and strong (he found Southall not that bad a place to live after all):

'The widespread desire to move out of "tatty" Southall and "live up the road" may thus be interpreted in more ways than class advancement alone. It may, of course, promise an escape from the town's low rank in the pervasive class hierarchy among whole suburbs, conditioned in part at least by racist equations of class with colour of skin. It may be an escape, further, from neighbours deemed alien by whatever difference of cultural heritage is recognized by householders themselves; but it may also promise escape from the censure of those of the same categorized background. All three interpretations could easily be linked. Each could be seen as an escape from

being stereotyped: be it by life in an immigrant “ghetto”, by co-residence with subjectively alien minorities, or by the social control of ‘one’s own folks’ with whom one may share a heritage, but sometimes little more. (...) Such a link to an escape from being stereotyped is hypothetical; more tangible is the link between social control by one’s own network of kin and cultural community and the caution advised in ‘being friendly with the wrong crowd’, however one’s own network may define them’ (1996: 46).

Baumann often brings in subjects’ ‘definitions of the situations’, as in the following examples:

‘Adult Southallians regarded themselves as members of several communities as once’ (1996: 5).

‘Most Southallians are aware, of course, that there are Muslims who are ‘Black’ and Christians who are ‘Asian’; yet these are treated as classificatory anomalies, and most people are in most contexts content with a division into five cultures, each identified as a community: Sikh, Hindu, Muslim, Afro-Caribbean and white’ (1996: 72).

‘Southallians complain that their own town is ‘dirty’, ‘grotty’, or ‘tatty’ as other Londoners complain of the weather: as a matter of course’ (1996: 43)

‘Many Sikhs of the craft castes regard Jat farmers as little more than landless labourers, uneducated, crude, and rude, as well as unsuccessful in the British economy’ (1996: 111).

‘Unlike the public arena of the Broadway, the sidestreets are considered a private space, the pavements almost part of the residents’ property’ (1996: 45)

Although Baumann’s work is richer in interpretations of ‘definitions’ than of concepts, we can still find some, for example:

‘Southallians share neither the urban English convention that neighbours are nobody’s business, nor the rural conventions of good neighbourliness with anyone, no matter who they are’ (1996: 45)

‘Making one’s life meant ranging across [several communities and cultures]’ (1996: 5)

Last but not least it has to be asked whether Baumann’s central argument is also interpretive in nature. There are two components of this argument: (1) what ‘culture’ and ‘community’ mean to Southallians in everyday life (but not in the public competition for resources), and (2) why people engage in the dominant discourse.

Both components are interpretive: the former interprets meaning and the latter interprets intentions.

5.3.4 Are Baumann's interpretations theoretical?

Although Baumann claims theoretical character of his findings ('The ethnographic data presented in this book are arranged in the form of an argument that might be called theoretical', 1996: 1), it is not explicitly discussed what he means by 'theoretical'. I am therefore going to analyse whether his argument meets five requirements of 'theory' discussed earlier.

Again, the first two criteria can be dealt with very briefly. As for the '*theory is general, abstract, precise and consistent*' one, Baumann's theory is general in the sense that it suggests (although implicitly) that the same phenomenon as in Southall occurs in other multi-cultural communities. I suppose it can be called 'abstract' only partially, since it transforms actions into two abstract concepts of 'demotic' and 'dominant' discourse. It is consistent.

Regarding the second criterion, '*theoretical concepts are meshed / theory is self-sustaining, independent / theory is a system*', it needs to be admitted that five central concepts employed by Baumann – i.e. culture, community, demotic discourse, dominant discourse and dual discursive competence – are essentially interlinked. Additionally, the last three are definable through each other.

Let us now move on to criterion III: '*theory is explanatory*'. Is Baumann's central interpretation explanatory? His main argument combines both 'what' and 'why' questions. Initially, he describes how demotic and dominant discourses are intertwined, and what Southallians understand by 'culture' and 'community'. Subsequently, Baumann explains why the dominant discourse takes place.

As for the demotic discourse, he does not say explicitly why people engage it. We can only guess that most likely they do so because it reflects what reality is really like, i.e. cultures and communities do cross and mix. In short, Baumann only gives description here; there is no causal explanation. But in fact there is some other (non-causal) form of explanation in his description: he brings alien down to familiar when

he says that Southallians 'reach across' rather than 'are suspended' between many cultures (1996: 2).

But if the dominant discourse distorts reality, why do people engage it? According to Baumann, when people compete for resources in the public arena, nuances get wiped off. They become the victim of strategic calculation:

'The least tribalist way to explain why Southallians [engage the dominant discourse] still seems to me to lie in what other Southallians do. If one community finds that playing the dominant game of reified cultures will gain it resources and respect, any other community would be foolish to opt out of the dominant rules. There is, usually, little point in engaging the demotic discourse when facing resource competition predicated on the dominant one.' (1996: 193)

Therefore Baumann's argument is explanatory in a 'teleological' way.

Moving on, are Baumann's minor interpretations explanatory? Some of his minor interpretations explain through bringing what is alien down to what is familiar. For instance, he writes that 'those who can realistically consider moving out [of Southall] voice [the taintiness of Southall] more readily than those who cannot', and subsequently explains it by the fact that 'few people can be expected publicly to put down what personally they cannot escape' (1996: 44). Here explanation has been achieved by calling an emotion familiar to all readers. Other times he explains actions through interpreting 'teleological causes', as in the example of community leaders:

'Community leaders are often motivated by a strong moral sense of justice for, and service to, their claimed constituencies; (...) and they gain from their efforts access to more desirable social networks and the respect or gratitude of those they have served' (1996: 66).

As for the fourth criterion, *'inferences can be drawn from theory'*, Baumann's insights into the background of the demotic and dominant discourses let us predict the shape of future actions in Southall (and, generalising, in other multi-cultural loci). Additionally – and perhaps more importantly – Baumann's findings entail consequences of potential use for policy makers. Namely they suggest that on the public arena, the 'dominant discourse' claims for resources should be treated with

reservation (particularly by decision-makers 'above' Southall). Such claims are likely to be distorting the reality of cultures and communities in question. Taking them for granted may lead to depriving of resources some people who cannot engage the dominant discourse. For example white Britons in Southall cannot engage in it without being accused of racism.

Let us now consider the final criterion, *'theory goes beyond subjects' knowledge'*. Within Baumann's central thesis, I am going to distinguish between his interpretation of concepts and his interpretation of intentions.

Let me begin with the interpretation of concepts and consider how subjects understand the meaning of 'culture' and 'community'. Baumann's subjects were aware that cultures and communities cross. He admits that right in the first phase of the research his respondents showed deep understanding of cross-cultural nuances and showed competence in evaluating complex multi-cultural situations (1996: 3-5): they were 'relativist in discussing culture and community' and 'regarded themselves as members of several communities at once, each with its own culture'. They knew that 'making one's life meant ranging across many cultures' rather than 'being suspended between them'. This was so obvious to them that they were shocked finding that Baumann and other academics did not know that.

But on the other hand it seems that they were not aware of using the dominant discourse intermittently with the demotic one. They did not see that the notions of 'culture' and 'community' they employ have different meanings in these two contexts.

Summing up, Baumann's finding that Southallians engage in both discourses would be interesting to them. To some extent he went beyond his subjects' knowledge.

Next, we can consider the issue of whether Baumann's causal interpretations go beyond Southallians knowledge? He gives us very few clues to answer this question. We need to split this question into two: why they engage in the demotic discourse and why they engage in the dominant one. As for the former, Baumann sees it as natural that people treat 'culture' and 'community' in relative terms. To

him, the reality *is* like that: cultures and communities cross, and this is reflected in peoples' 'demotic' views. Since they are also aware of this, Baumann did not add anything to their knowledge.

The issue of the dominant discourse is more complicated. Let us remind ourselves of Baumann's interpretation of their intentions:

'The least tribalist way to explain why Southallians [engage in the dominant discourse], still seems to me to lie in what other Southallians do. If one community finds that playing the dominant game of reified cultures will gain it resources and respect, any other community would be foolish to opt out of the dominant rules. There is, usually, little point in engaging the demotic discourse when facing resource competition predicated on the dominant one.' (1996: 193)

This explanation however does not tell us to what extent this motivation is conceptualised by the actors, and to what extent it is 'hidden' or unconscious. Usually when it comes to strategic behaviour people are aware of their motivation. But on the other hand only two respondents⁵⁹ confirmed it in interviews:

"It can thus be Afro-Caribbeans themselves who deny the legitimacy of South Asians claiming a "Black' identity; the claim can be viewed, not as a sign of solidarity, but as an opportunist ploy in the pursuit of public resources. The point is made most clearly by a Rastafarian feminist activist: 'Asians call themselves 'black' or 'coloured' when it suits them... I don't mind them calling themselves 'coloured', but they do it for political reasons'" (Yabsley 1990:66 cited in Baumann 1996: 170).

Another respondent stated that 'Asians make use of the term 'Black' when it suits them' (1996: 171).

Certainly these examples show that at least for some Southallians Baumann has not discovered anything new. There is a possibility however that several inhabitants did not think about this at all, in which case Baumann did go beyond their knowledge⁶⁰.

⁵⁹ To make things even worse, those were not Baumann's respondents but somebody else's!

⁶⁰ From the methodological point of view it is surprising that Baumann did not ask them directly about their motivation: 'why do you sometimes talk about cultures and communities as if they were flexible and sometimes as if they were rigid?', or: 'do you engage in the dominant discourse because it helps defeating other communities in the competition on resources?'. Baumann does not justify this omission at all. Perhaps he was worried that their answer would strip his insights of the status of 'discovery'.

Baumann's research also goes beyond subjects' knowledge in another sense. By drawing a comprehensive map of all communities and cultures in Southall, he transcended what individual Southallians know about their hometown. The key feature of good fieldwork, such as Baumann's and Hochschild's, is that the researcher is able to gain comprehensive knowledge of all essential segments/positions of the studied community, while the subjects themselves know only their own group/position very well.

To complete the consideration of Baumann's work in the light of this criterion, let us consider whether Baumann develops technical concepts. The language of the book could be said to be very academic, highly abstract, but there are not many typically social-scientific concepts such as 'social structure', 'social positions', etc. By far the most common situation when the reader has the impression of 'technicality' is when Baumann abstracts from his subjects' lay conceptualisations, which are necessarily bound by particularity and are activity-oriented rather than reflection-oriented. I have mentioned this point in the chapter on theory: because the reader is not familiar with particular context of action or utterance, she needs it to be either removed (abstracted) or made explicit.

For instance, the concept of 'imbalance of resources' (1996: 161) abstracts what the subjects perhaps conceptualise as: 'The Sikh community has relatively far more members in the City Board and powerful businessmen than we have, [so they can make things happen as it suits them, they think they own the town]'. Similarly, when the author is discussing "the widespread desire to move out of 'tatty' Southall and 'live up the road'", he is changing the latter expression (belonging to the subjects) into a technical concept: 'Geographical mobility is perceived by vast numbers of Southallians as a sign of *upward social mobility*' [1996: 46, italics mine].

It is here that we can clearly see the difference in the approach of the subjects and the researcher: the former are in the 'everyday life world', which is characterised by practical approach, particular activity, contextualisation and 'tacit' knowledge. The researcher, on the other hand, is in the 'scientific world' with its theoretical approach (abstract thinking, replacing actions with concepts, replacing proper names with general names). It is why the reader gets the impression that *Contesting Culture* is a *scientific* book:

'To deal with this 'minority' position among communities, three strategies can be discerned. A political option open to Irish Southallians is to seek recognition as an ethnic minority or ethnic community in accordance with the dominant discourse of ethno-cultural communities. A second option, open to all white Southallians, is a cognitive strategy. It consists in cultivating a shared minority consciousness, and it is achieved most easily by stressing white Southallians' exclusion from public services distributed by ethnic targeting. A third option consists in a pragmatic strategy, and is the one that best avoids subjective alienation: it involves the creative forging of affinities and alliances across community boundaries' (1996: 134-135).

It is not difficult to imagine specific individuals in the described position ('minority position') seeking the way out of this situation by means of various actions and strategies. There is no doubt that their thinking is bound by particularity and context, i.e. they think about specific groups, actions, and effects. Most likely they did not draw up a list of strategies like the author, or give their strategies specific names ('political', 'cognitive', 'pragmatic'.) These differences mean that to the reader the paragraph sounds scientific.

Two further examples are:

'As structural positions are thought, by Southallians, to show less differentiation, the equation of community and culture is reformulated to be more inclusive' (1996: 191)

'Community boundaries reflect perceived structural positions in the post-migration environment' (1996: 190).

What is important is that this kind of technical concept, whilst working for the reader, does not write off or compete with lay concepts on which they are based. However, as we will now see, the same is not true of all of his central theoretical concepts, namely 'demotic discourse', 'dominant discourse', 'dual discursive competence', 'culture', and 'community'.

The notions of 'demotic/dominant discourse' and 'dual discursive competence' grasp in an intellectual manner what is *praxis* for Southallians. They transform actions into concepts. Therefore these three concepts, although sounding very technical, actually also do not write off subjects' point of view. They are technical but do not violate interpretive assumptions.

The notions of 'culture' and 'community', on the contrary, sound everyday and refer to something that the subjects conceptualise, but in fact are used by Baumann in a technical and non-interpretive way. Baumann understands them differently to his subjects, as can be seen in the following sentence: 'Yet the process of culture-making is more often called 'developing the community', 'community building', or 'changing the community' (1996: 196). Why is he talking about culture-making and not community-making then? Following that, Baumann understands 'community' in terms of people sharing ethnicity and culture (1996: 134), whereas his subjects understand it as a local group of people 'living and working together' (1996: 95).

Again, that he understands 'culture' differently to his subjects is visible when he talks about 'a shared Southall culture'. It is quite obvious that what he takes for 'culture' – socio-economic situation, feelings about Southall and actions⁶¹ in the public arena – would be understood as elements of *community* by Southallians themselves. Since there is divergence between the way Southallians and Baumann understand these two concepts, we need to take into consideration the possibility that the (technical) way Baumann uses the concepts of 'culture' and 'community' does not comply with interpretive assumptions.

In conclusion, it seems that most criteria are met. Two issues are problematic however. Firstly, it is questionable to what extent Baumann's explanation of why people engage the dominant discourse goes beyond their own conceptualisation of it. Secondly, two of his own concepts seem not to comply with interpretive assumptions.

⁶¹ 'What Southall culture the ethnographer can discover at first sight is a **culture** of people intent upon moving out and up and yet tied to the place by common economic problems, *community* bonds, and the recourse to *community* claims in the competitive public arena' (1996: 71). (Subjects' conceptualisations are italicised, Baumann's concept is bold.)

5.4 Wagner: *Checkerboard Square*

5.4.1 General remarks about the work

David Wagner's book is a qualitative exploration of a homeless community. Wagner conducted his research between 1990 and 1991 in a medium-sized city (population 270,000) in the Northeast of America. An old colonial centre with factories and mills, in the 1980s the city's socio-economical character was transformed by the decline of industry and influx of high-technology, banking and real estate businesses. Consequently, a number of affluent upper-middle class people moved to the city, while working class people were forced to change their relatively well-paid industrial jobs for low paid service jobs. Rising property prices meant that many of them could no longer afford to pay rent. This and the fact that the federal administration of the time cut social benefits resulted in a significant increase in the number of homeless people in the town.

Why one more book on homelessness, we may ask? For Wagner, the reason was that this issue is haunted by myths, created by politicians, media people, and social scientists alike. These myths result from 'the absence of the homeless' voices within both public debate about poverty and the social science literature' (1993: 3). Thus the author's idea was to study the 'lifeworlds' of the homeless, to conduct qualitative research in order to obtain first-hand accounts and through that, to discard myths. ('Statistical research may only add up to confusion', 1993: 3).

It needs to be clarified here that the myth Wagner particularly has in mind concerns not the causes of poverty and homelessness, which he admits are structural, but the culture that 'develops *once people become homeless*' (1993: 43). He questions 'the dominant portrayal of the homeless as vulnerable and dependent people worthy perhaps of sympathy but judged to be socially disorganised, disaffiliated, and disempowered' (1993: 3).

Contrary to the dominant view, Wagner's central finding was the existence of a 'culture of resistance'. He found that in reality homeless people he studied created culture that was centred on 'maximising income while retaining some autonomy' (1993: 79, 83). Thus on the one hand they were very resourceful in the sense of

finding ways to secure enough resources to make a living, and on the other hand they tried to maintain dignity and control over their lives. The former was achieved to significant extent through efficient networking, developing alternative forms of social organisation (informal primary groups based on age, sex, addiction, or other similarities), and strong norms of reciprocity and solidarity (1993: 148). Also, people were very strategic in getting what they wanted from social services. Apart from that, when necessary, the homeless people were able to take collective action to improve their situation.

Maintaining dignity and control over one's life took mainly two forms: firstly, the form of refusing to be oppressed and controlled by bureaucratic institutions ('assistance usually carried the price of humiliation and subordination', 1993: 179), by authoritarian bosses, and so on (1993: 84, 99). Hence many homeless people preferred to sleep rough than undergo 'degradation ceremonies' or similar humiliating and objectifying treatment (1993: 99). Secondly, people resisted low-paid wage labour, particularly in the service economy (1993: 79). A strong norm was held that industrial work is superior to service work (1993: 83). The 'culture of resistance' was also manifested by refusing the social norm of being in a long-term relationship. The people Wagner studied tended to avoid it and preferred short-term, if any, relationships (1993: 65).

It has to be mentioned that although Wagner's aim was to understand street people, he did not try to become an insider (in the sense of sharing everyday physical experiences). He did not sleep rough or join his subjects in rummaging the trash to understand what their life was like. His focus was on their culture.

5.4.2 Theoretical frame

When analysing Wagner's theoretical framework we find the same mix of interpretive, structural and critical elements present in Hochschild's research. That Wagner's work has a strong *interpretive* dimension – gives privilege to the actors' point of view - finds confirmation in statements such as that the methodology employed has to examine 'subjective interpretations based on [peoples'] own experience' (1993: 44), or that the discourse on the poor is characterised by 'the absence of the homeless voices'. In short, these are cultural meanings rather than

structural variables that are the key to comprehend the homeless persons' conduct and 'lifeworld'.

To clarify how this approach works, I will give an example. At one point Wagner challenges the puzzle that some homeless individuals prefer living on the streets to being accommodated in shelters. This apparently irrational or bizarre behaviour ceased to be so when he learned the subjects' account of this matter:

"by getting into a shelter ... the subject risked exposure to a variety of controls, including having children taken away, being institutionalised, or being imprisoned. Agnes [one of the subjects] remarks:

'You're in an extreme catch-22 situation. I mean if you talk to them [city welfare workers, shelter attendants] and they find you're living on the streets under a bridge and you have children, you'll end up at [department of child welfare] and have your kids taken away. If you say you're distraught, well, you may end up in a [mental institution]. ... I learned the hard way, don't say anything! And then, of course, they think I am a weirdo because when I go to the shelter I say nothing.'

Agnes's comments were not idiosyncratic or paranoid. Many subjects did fear losing their freedom, their children, or their social benefits by presenting themselves for assistance and being identified by workers as irresponsible, as substance abusers, as mentally ill, as criminal, or as unfit mothers." (1993: 100)

Structural Sociology and Realism. Yet the work is not a mere collection of such 'definitions of the situations'. In the same manner as Hochschild, Wagner aims at getting to know both the 'definitions' and the situations themselves. Situations may exist independently from their accounts, or the accounts may be inadequate, or 'definitions' may vary. This means that Wagner is a sociological realist. For example, he puts forward the following structural claim:

'In part the factors limiting any contact with the families of origin were *structural* because poverty and its attendant humiliation create a lack of reciprocity in relationships' (1993: 58, emphasis mine).

Regarding the causes of homelessness, poverty and substance abuse, Wagner has built a comprehensive image: 'The stress of working-class life suggests a complex interaction of social, economic and cultural factors as leading to alcoholism, homelessness, and downward mobility' (1993: 57). This means that structures do

not operate 'mechanically': every situation is different, every actor is different. No causal factor can be singled out as *the* cause:

'No simple causative relationship can be determined from the fact that a street person drinks alcohol or is diagnosed as mentally ill. Thousands of residents of North City are alcoholics, for example, and they are not and probably never will become homeless. Thousands of people are labelled mentally ill, and most will never become homeless. This alone should give pause to a tendency to overdetermine the causes of homelessness. As has been pointed out by several critics, social scientists are unable to distinguish cause and effect; that is, once homeless, many people start to drink, and many begin to exhibit signs of depression and even bizarre behaviour rather than having become homeless because of these behaviours' (1993: 149).

Although Wagner is concerned with structural causes, he also gives a *paramount role to agency*. The subjects' actions and 'definitions of situation' are not treated as a straightforward product of systemic causes, but as equally important as objective factors in constituting the social reality. This reality is believed to be the outcome of the interaction between objective factors, their 'definitions', and actions (strategies) that follows. People *creatively* adapt to the conditions they have not created. In other words, subjects do not have control over objective conditions and factors, but at the same time this objective reality does not determine straightforwardly their actions and 'definitions'. There is no internal 'logic' of the system:

'These strategies of survival were hardly random but were structured by the exigencies of the job market and the complex rules of social benefits, as well as subjects' subjective understandings' (1993: 83).

Each case should be studied individually. Different people 'define' things differently, have different resources, skills and characters, and therefore react differently in similar situations:

'A street person may drink heavily, for example, and not be a "Street Drunk" [i.e. a member of the bums' subculture]. The street person, although he or she drinks may not be drawn to associate with this subculture because he or she does not share other norms and values of that group or may be the wrong age or gender. Others who may be labelled mentally ill have social ties with the subculture organised around mental illness because, for a variety of reasons, they feel more comfortable spending time with other groups and do not identify with such label' (1993: 149).

Summing up, Wagner employs two kinds of analysis in his work: when he studies subjects' actions and intentions, he applies interpretivism (exploration of 'in-order-to motives', as Schutz would say); whereas when he explores causes of the subjects' situation, he applies structuralism ('because-of motives', Schutz). Therefore he distinguishes clearly 'definitions' from 'situations'.

Finally, we can see a *critical* dimension in Wagner's work. An important feature of his book – in fact, the one that won him the C. Wright Mills Award – is his engagement in the social policy discourse regarding the poor. On top of the research conducted in a value-free manner, Wagner does fight misconceptions about the homeless, points to mechanisms of oppression, and recommends practical solutions and policies. His main argument here is that the society's dominant beliefs fail the very poor (1993: 3). The beliefs Wagner particularly has in mind are work and family ethic. In general, the poor cannot or do not have incentives to comply with this ethic, but as a consequence they are perceived negatively and are punished by the system: 'work and family ethic dominate the treatment of the poor by the state' (1993: 14).

Regarding the work ethic, Wagner observes that usually the homeless are regarded as 'lazy', because (in the common view) they prefer begging to working, and therefore they do not deserve assistance. But actually, a significant percentage of homeless people do work at any time. However, since this work is often temporary, off-books or illegal, it seems that street people do not work. Also, the poor are aware that lowest wages do not surpass the government's poverty line, that upward mobility is realistically non-existent in case of lowest paying jobs, and that such jobs are not fulfilling at all. And even if they would like to take such a job, they would find it practically difficult to get through the job interview stage. In other words, the most common stance - 'get a job' – is hypocritical and separated from the reality.

The dominant family ethic is another problem. The homeless tend to resist forming legitimised long-term partnerships, most often having escaped from dysfunctional families themselves. But then they are denied benefits because they are single (1993: 14, 16). Hence one of Wagner's recommendations is that the social policy should not be informed by these two dominant ethics. As long as it is, the situation

of the poor cannot be effectively altered: 'The local relief system tends to contribute to the problem of poverty' (1993: 104).

An equally important recommendation considers another assumption underlying assistance to the homeless. It is namely assumed that assistance may only be effective if it is given to an individual (1993: 19). It is thought that in order to improve her situation the homeless person needs to separate from the broader homeless community (1993: 179). Social networks and subcultures are seen by service providers and policy-makers as barriers to personal changes. Wagner blames the ever-present American norm of individualism for this assumption. He points to the fact that the homeless prefer solidarity with others to assistance that requires breaking the bonds with their group. Wagner suggests that instead of trying to neutralise existing strong social bonds between the homeless it would be a much better idea to capitalise on them. But this would mean the break with individualistic approach in assistance:

'The strong ties within the street community suggest collective approaches to poverty that build on existing social networks to assist poor people in obtaining housing and other benefits collectively' (1993: 19).

Finally, Wagner criticises the dominant political stances on homelessness. He rejects the conservative view of street people as lazy and immoral as having no confirmation on empirical grounds, but he also rejects the liberal view of the homeless as 'vulnerable victims holding no moral responsibility for their condition' (1993: 4). Wagner claims that such 'politics of compassion' firstly overlooks that radical changes would be necessary to eliminate poverty and homelessness (1993: 5), and secondly it does not really refrain from enforcement and control of poor people. The option of therapy may be preferred to imprisonment, but 'when poor people do not do what they should, liberals are as likely as conservatives to enforce social control'. Not surprisingly, street people themselves are well aware that the choice they are given is between "help" and repression.

5.4.3 What kinds of interpretation does Wagner put forward?

Wagner's work provides plenty of 'definitions of the situations'. For example:

'A major complaint was that most shelters were religiously affiliated and mandated prayer and religious study:

Tiny: The [name] shelter? They just want to brainwash you into their religion.

Jonathan: [The] Captain [at Salvation Army] and me didn't get along. All these rules, all this good stuff... he tried to control my life.' (1993: 101)

'Most subjects recognised that low-paying service jobs would never allow them to escape poverty' (1993: 82);

'Subjects felt their families were hypocritical in their moralistic attitudes about work, appearance, and family life' (1993: 60)

'The enforced work requirements were rejected as "make work" and a "sham" by most' (1993: 107).

Wagner also offers insights into the homeless' culture and beliefs:

'The street people portrayed in this book want housing and income. They are either overtly or instinctively radical on the issue of redistribution. Even more than most Americans, however, they are hostile toward bureaucratic control and resist complying with state edicts. They question government benevolence that enforces behavioural codes before they can receive shelter, that would force them to work for a welfare check, and that forces them to surrender their children if they are judged unfit parents' (1993: 18).

'However, rather than condemn all social services, most subjects wanted certain services. The types of services they wanted, however, were those with no behavioural controls and those in which services were provided as an entitlement. Finally, subjects most appreciated settings that were small communities in which they and their friends were able to maintain a strong amount of control and have social interaction' (1993: 116).

'The Street Drunks can be distinguished from the other subcultures by their extremely strong self-concept of independence' (1993: 154).

'A strong norm existed that blue-collar, industrial work was superior to service work' (1993: 83).

5.4.4 Are Wagner's interpretations theoretical?

As in the cases of Hochschild and Baumann, I shall discuss the first two criteria only briefly. Regarding the first one, 'theory is general, abstract, precise and consistent', it can be claimed that Wagner's interpretation is general, since the 'culture of resistance' seems to be an intrinsic feature of homeless communities rather than a contingent characteristic of the particular community he studied. As in the case of two previous empirical studies, it is not abstract. It is fairly consistent.

As for the second criterion, 'theoretical concepts are meshed / theory is self-sustaining, independent / theory is a system', Wagner's theory is independent and systematic. It concerns not one aspect of the homeless culture but all of them.

Let us now move on to the third criterion, '*theory is explanatory*'. Wagner's main interpretation says that in response to the conditions they are in, homeless people develop 'the culture of resistance', the main features of which are the resistance to form traditional families and the resistance to take up low-paid service jobs. This interpretation, first of all, explains to us *what* the 'lifeworld' of the homeless is like; and secondly, by pointing at emotions such as 'saving dignity' it explains *why* they behave in the way they do.

Some minor interpretations explain subjects' conduct by pointing to a reason. For example, Wagner's statement that 'Subjects felt their families were hypocritical in their moralistic attitudes about work, appearance, and family life' (1993: 60) explains why they did not feel like sustaining close bonds with their homed relatives. Or, exploring incomprehensible and disturbing cases when subjects frequently avoided taking low-income service jobs, Wagner found the following beliefs that explained it:

'...One-sided authority relations strongly influenced the subjects to avoid service jobs' (1993: 84).

'service or white-collar work was not perceived as 'real' work by many subjects but was indicative of softness and a failure to be strong and manly' (1993: 84).

Let us now focus on the fourth criterion, '*inferences may be drawn from theory*'. To some extent, on the basis of Wagner's interpretations we can predict future conduct of his subjects (and, if we generalise, of other homeless people). But even more importantly we can draw implications for social policy; in fact, this is what Wagner explicitly attempted in his book. Additionally, Wagner's insights are likely to influence the readers' minds and consequently their actions. For example, they have changed my mind, which tended to support the 'get a job' approach. Now I do not think in this way when I see homeless people. Fay could not be more right when he observed

that '(...) when one is able to see the sense of alternative life styles and different ways of looking at the world (...) at the least one's own assumptions are thrown into relief and therefore one becomes more fully self-conscious; at other times one may well come to redefine oneself and therefore to act differently' (Fay 1975: 81).

Regarding the last, fifth, criterion - 'theory goes beyond subjects' knowledge - I wrote earlier that there exists a substantial tension between the most fundamental assumption of interpretivism ('privilege the actors' point of view') and the most substantial assumption of science ('go beyond what is already known'). I suggested that the way to address it is to ask an auxiliary question, 'why do we want to carry out research in the first place?' My argument was that apart from 'practical' and 'methodological' reasons for research, there is also a 'theoretical' justification of it. In particular I meant that the qualitative researcher becomes familiar with the knowledge that is taken for granted and unconceptualised. By making it explicit the researcher in a sense goes beyond her subjects' knowledge.

Wagner's answer to this question could be reconstructed in the following way: in our capitalist society, the poor are practically voiceless. There is no substantial public discussion on the poor that would include them. The media by their nature present only sensational material, always preferring stories of an attention-begging protagonist to comprehensive, broad, sociological, contextualised picture. Social scientists in turn failed to give the poor a voice as they tend to research the problems with questionnaires and do not employ longitudinal studies.

To Wagner, it would be best if the poor had a voice of their own strong enough to be heard by society and policy makers. But realistically this is not going to happen through public media. In this situation, what can be done is the social-scientific research conducted in a methodologically different way to what has been done in the past. It would have to be ethnographic (open-structured), longitudinal, value- and prejudice-free. In this way, the voice of the poor would be given a platform that is charged with some authority. As such, it would have a chance to be heard.

Hence Wagner's answer to the question 'why do we want to carry out research in the first place?' is very interesting as it goes beyond the three main reasons brought

forward by myself. Since the poor are practically denied the opportunity to speak for themselves, I believe this reason can be classified as (another kind of) 'practical' one. (Another because the first practical reason is that our audience may not understand the language of the subjects).

It should be at least briefly said how Wagner's answer relate to the 'theoretical' reason for carrying out research discussed earlier. (This reason is to conceptualise what is taken for granted). It seems to me that although Wagner sporadically writes that his subjects' understanding of their circumstances (such as how capitalist economy really works) 'remained at the level of a gut feeling' (1993: 80), I do not think that his main reason for undertaking the research was to uncover what the subjects take for granted – rather, Wagner mainly wanted to *transmit* certain data to certain audiences.

Take for example the following account of 'the Myth of Laziness':

'If, however, middle-class and official stereotypes about what constitutes work are ignored for the moment, the reality in Checkerboard Square was that most subjects *did* work. The popular ascription of 'laziness' among the poor can only be upheld by ignoring the dominant underground economy of nonwage labour, as well as other productive activities such as parenting, participating in social service activities, and scavenging in order to survive. At any given time, it appeared that only about a sixth of our subjects were working full time at wage labor. However, at least another half of the subjects had significant earnings off the books in a wide variety of pursuits. (...) A large number of subjects were attached to areas of employment that were casual and sporadic, marked by surges of heavy labor alternating with periods of unemployment. (...) Many income-generating strategies fit the term 'shadow work'. (...) A large number of men admitted to having sold drugs, and some admitted to theft. (...) Questions about the morality of these people activities aside, none of these people could be characterised as lazy. Some, like Herb and Louise, were so busy with work that they rarely appeared on the streets. Subjects like Sydney or Seth working for landlords, or Alicyea working as a live-in babysitter in exchange for room and board, put in twelve-hour days' (1993: 77-78).

Now, this observation is nothing new to the subjects. I believe the following remark of Anthony Giddens suits Wagner's study perfectly: 'In very many instances the 'findings' of sociologists are such only to those not in the context of activity of the actors studied. (...) The social sciences necessarily draw upon a great deal that is already known to the members of the societies they investigate...' (1984: 284, 354); 'The social scientist is a communicator, introducing frames of meaning associated

with certain contexts of social life to those in others' (1984: 285). (Having said that, it is to no detriment to Wagner's work).

Going down this track even further, we can say that descriptions such as the above one bring closer underrepresented, underprivileged segments of society to (mostly) middle-class, privileged readers. This in turn reminds of what Richard Rorty said:

'...we shall be able to see the social sciences as (...) interpreting other people to us, and thus enlarging and deepening our sense of community. We shall see the anthropologists and historians as having made it possible for us – us educated, leisured, policy-makers of the West – to see exotic specimens of humanity as also 'one of us', the sociologists as having done the same for the poor (and various sorts of nearby outsiders), the psychologists having done the same for the eccentric and the insane. This is not all that the social sciences have done, but it is perhaps the most important thing' (Rorty 1982: 203).

In some places however, Wagner formulates comparisons across social classes, which in an important sense go beyond the understandings of both subjects (immersed in their segment of the society) and the readers (the privileged). To give an example:

'As agonising as it is, role shedding seems understandable for street people whose daily lives are characterised by uncertainty and often hunger and danger. It is a functional, if tragic, adaptation to the harshness of street life. But it is also not an alien or an unnatural decision to disavow long-term relationships or having children, as is becoming increasingly evident in the late-twentieth-century United States. In fact, Ruth, Lorraine, and Agnes have much in common with some of their middle-class feminist peers. Although they articulate their motivations somewhat differently, their decision to not have families resembles the same coming to terms with their own separate needs and with distancing themselves from the need for a male companion as is true for middle-class women. The homeless, particularly women, are caught in a no-win situation: They, like middle-class women, may be deemed unsuitable and not real women without children and husbands: but when they do have children, many service providers, as well as the public, complain about their propagating with no means of support' (Wagner 1993: 62).

The difference between simply revealing something to other classes (as in the first example) and the last paragraph is that in the former case subjects could not learn anything about themselves from the researcher's account, whereas in the latter case

both subjects and middle-class readers could potentially see themselves (and the society) in a new light, and re-evaluate or rethink their life situation.

Additionally, Wagner managed to transcend his subjects' knowledge in yet two other senses: firstly, he built a more comprehensive image of the whole 'Checkerboard' community than any of the insiders had, and secondly, he had a better overview of causal factors determining various phenomena. As for the former case, Wagner could achieve this precisely because he came from the outside. It is difficult to imagine anyone from the very community being allowed to explore all its different segments freely. Also, Wagner did not have the 'taken for granted' knowledge, which allowed him for an unprejudiced look. Thus, he was able to get to know a variety of 'definitions of the situations' while particular subjects tended to hold one and be ignorant of others.

Regarding causality, the subjects tended to single out one factor as the cause, rather than a complex of factors. For example, Alicyea stated: 'Screwed up families... is a big cause of homelessness' (1993: 58). Wagner was able not only to get to know different perspectives on causality, but also to have a relatively objective judgement of them, as he was not charged by experiences that his subjects gone through (in this example, coming from a dysfunctional family).

Moving on to the issue of technical concepts, what kinds of concepts does Wagner use in his study? His language should be described as 'intellectual' rather than 'technical'. The theoretical framework endorsed by the author means that there are not an awful lot of concepts traditionally associated with sociology, such as 'social structure', 'social norm' or 'social role'. Sporadically Wagner uses subjects' concepts and phrases. They appear in quotation marks. Most of the time however, he employs his own concepts. Now, how do they fit the scheme I put forward in the chapter on theory? They fit most closely with two reasons that I proposed: making tacit knowledge explicit and getting rid of particular context.

Beginning with making tacit knowledge explicit, Wagner often conceptualises what his subjects take for granted. The central notion of 'the culture of resistance' is a good example. Some other example would include:

'Within the framework of their own culture, *maximising income while retaining some autonomy*, as well as some hope for the future, involved a subcultural norm of resistance to low-paid wage labour, particularly in the service economy' (1993: 79, emphasis mine);

'Subjects' insistence on remaining on the streets and avoiding their relatives was often *an exercise in self-preservation*' (1993: 60, emphasis mine);

'Lower-class people do exercise some compensatory forms of *control over their lives* by refusing certain jobs and authority arrangements, by developing alternative forms of income, and by aching out relief' (1993: 93, emphasis mine).

Moving on to getting rid of a particular context, the author frequently substitutes general concepts for particular names or meanings. For example, when talking about the subjects' families' feeling that their homeless relatives should be put in mental hospitals, shelters, drug rehabilitation homes, foster care, etc. (respectively to the nature of each case), Wagner writes: 'Families saw *institutionalisation* as the only solution to their homeless relatives' problems' [1993: 60, italics mine]. Now, the concept of 'institutionalisation' did not belong to the subjects studied, but is the researcher's generalisation of what each of them particularly meant.

Similarly, when Wagner writes that 'Subjects face extensive *social control* over their behavior' (1993: 101, emphasis mine), he replaces a number of particular ways in which social control can be exercised with one general name. The same can be said about the statement '*One sided authority relations* strongly influenced the subjects to avoid [service] jobs' (1993: 83, emphasis mine), in which the emphasised part replaced what subjects most likely conceptualised as 'arrogant bosses' or 'being treated as a piece of furniture'.

But there is also a further reason in Wagner's work for using concepts not coming from the very subjects, which was not mentioned in the previous chapter on theory, so let me elaborate a little here. Several times Wagner uses his own concepts in the situation when he 'translates' what is non-conceptual *praxis* for the subjects into the concepts of an unengaged observer. For example, when referring to subjects' behaviour and feelings, Wagner often uses words such as 'frustrated', 'realistic', 'hostile', 'humiliation'. These words most often do not refer to subjects' self-referential concepts (e.g. 'we are frustrated'), but to the character or meaning of their

feelings, actions and utterances. Such concepts are omnipresent in everyday life; all of us use them several times a day.

The analytical view of this is as follows. The researcher takes a cognitive attitude; therefore she is after (and perceives) phenomena and relationships. Her subjects, on the contrary, are immersed in *praxis* – challenging particular situations and planning relevant actions. This difference in categorising the world entails that the subjects may not be interested in what interests the researcher, and in consequence they may not hold any relevant ‘definition of the situation’. Moreover, peoples’ conduct is usually informed by a mixture of emotions, evaluations and knowledge, which means that there may be no clearly conceptualised ‘definitions’ underlying it. The researcher tries to break this mixture down into distinct elements.

For example, Wagner explores the issue of love relationships and forming marriages amongst homeless people. He discovered that

‘For about half of the subjects interviewed, the effects of poverty and street life, and their own critical penetrations of relationships and family, led to an avoidance of long-term love interests’ (1993: 62),

‘Those street people who did form partnerships appeared to develop a range of caring relationships characterised by loose, permeable bonds. They were not established by marriage, were not usually monogamous, and were often episodic’ (1993: 63),

‘The self-conscious resistance to marriage among these street people may or may not be deviant but rather simply reflect the absence of the societal pressure and social control over relationships that exist in other social classes’ (1993: 65).

Now, each subject knows their own story, but presumably they do not explore the causes in the way Wagner did, nor do they purposefully try to build the comprehensive image by interviewing numerous subjects. And the observations Wagner provides – such as ‘loose, permeable bonds’ – are probably not consciously conceptualised as such since such observations are usually conspicuous when the observer is from a different class. All this amounts to the fact that in most cases the researcher’s interest constitutes – *constructs* - her object of study. The subjects themselves most likely do not try to formulate such general knowledge, as their interest focuses on particular cases. Therefore what we find in an interpretive social-science book is constructed differently to what is in subjects’ minds.

Another example of this could be Wagner's description of one subculture within the homeless community:

'The Politicos are primarily baby-boomer age... they had some resources advantages over many other street people... the subculture develops from several different sources: the hippie, countercultural movement, the Vietnam veteran experience, and the mental health movement' (1993: 150, 164).

This is an example of building the general image of a group, the image that outsiders need but insiders do not – and that is why they may not conceptualise it in precise terms ('we are this, this and this... our group develops from such and such sources...').

Or:

'[X, Y, Z] had extensive *social networks* of friends' (1993: 63, emphasis mine).

In everyday language one would say 'had many friends'. Probably the author used this technical concept to emphasise the flow of resources between individuals and X, Y, Z's access to them; the everyday expression, on the other hand, connotes socialising and good time.

Summarising this section, the general impression of Wagner's 'theory' is positive, apart from the 'going beyond' criterion. To a large extent, he did not add anything new to the subjects' own knowledge (in the sense of conceptualising 'tacit' concepts, 'definitions' and beliefs). However, he transcended their knowledge in the sense of building a comprehensive image of the whole community.

5.5 Conclusion

What conclusions can be drawn from the current chapter? Firstly, our scheme developed in the precedent chapter proved to be useful: Hochschild and others put forward interpretations of unconscious intentions, of 'definitions of the situations', and of 'non-constitutive' concepts. Therefore it was worthwhile to spend time modifying Fay's scheme.

Secondly, the general impression is that in actual research practice, interpretation meets the majority of criteria of 'theory'. Some patterns could be observed. The main strength and yet at the same time the main weakness of interpretivism is to be found in two criteria – the one referring to explanation and the one concerning 'going beyond' subjects' knowledge. Here I mean that often interpretations that are explanatory do not transcend subjects' knowledge, or vice versa: the ones that are not explanatory do go beyond everyday knowledge of people under study.

Another strong pattern is that the first two criteria – 'theory is general / abstract / consistent ...' and 'theory is a system / is independent ...' are difficult to apply. It is not clear how to determine whether a given interpretation fulfils these criteria or not. Interpretation looks positive when it comes to the criterion of 'inferences' however. Practically in all three case studies the researchers drew political or moral consequences. Readers could also take their own stance.

Importantly, the reading of our three case studies suggested certain shortcomings of the scheme we developed in the chapter on theory. In particular, in Wagner's study we encountered a 'practical' reason for undertaking research; this time it was that his subjects had no possibility to voice out their point of view. Therefore the researcher in a way acted as a relatively objective 'transmitter' of their voice. Also in Wagner's study we recognised another reason for using concepts different to the subjects' ones: we pointed to the fact that often the researcher, who takes the 'cognitive' attitude, conceptualises what is *praxis* (unconceptualised acting) for her subjects.

Part III

Interpretation and Method

In Part III I am looking at the second component of my working definition of science, that is at the 'scientific method'. As I argued in Chapter 1, method is an essential component of science. But is interpretive method really scientific? In what sense is it a method?

We should start by saying that social scientists do not usually use the name 'interpretive method'; instead, they talk about 'qualitative method'. Shall we assume that this qualitative method is what we mean here by 'interpretive method'? By and large, the answer is affirmative; we need however to be aware of one philosophical problem concerning the relationship between interpretivism and qualitative research. Namely, carrying out qualitative research does not necessarily mean that one supports interpretive assumptions. Qualitative methods are not used exclusively by researchers supporting interpretive theories such as symbolic interactionism. In practice, those who hold structural or functional assumptions also sometimes apply qualitative methods. Why is that? In this latter case, one pays attention to subjects' meanings only for exploratory (descriptive) reasons; the real purpose is finding out about the hidden functioning of institutions and structures by comparing what is said with what is done. Malinowski's study of Trobrianders (1922) and Gellner's study of Berbers (1987: chapter 2) exemplify this kind of relationship between non-interpretive social theory and qualitative research. Meanings are treated not as ultimate drivers of conduct but rather as resources or tools in the hands of groups that have divergent interests and conflicts. Insight into meanings is necessary to have insight into social mechanisms⁶². This problem is going to be discussed in more length in the conclusion of the thesis.

But having said that qualitative does not necessarily mean interpretive, we need however to state that interpretive research *has* to be qualitative. Therefore if one, like us, wants to find examples of actual interpretive research, one has to first look at examples of qualitative research, and then leave out those cases where researchers do not hold interpretive assumptions. In this section of my thesis I am going to look at such pieces of research.

⁶² Note that this kind of structuralism (focused on conflict and interests) is very different to structuralism that looks for relationships between objective independent and dependent variables. The latter applies quantitative rather than qualitative methods.

Let us come back now to our main question: is qualitative method really scientific? Some methodologists have no doubts about this issue: 'qualitative research has – with difficulties but successfully – achieved its place amongst the sciences' (Flick 1998: 246). There has been however significant criticism of this idea from within interpretive social sciences. Particularly two kinds of objections have been put forward:

Firstly, there has been some criticism about the *political justification* of qualitative research. In a way reminding us of the criticism of quantitative researcher – that she is a social engineer, seeing her subjects as objects that can be instrumentally treated, disregarding their wish about what should be done to them – the qualitative researcher has been attacked for being politically incorrect. This type of internal critique came from postmodern anthropologists. They accused traditional anthropological researchers of being politically oppressive, both in the fact that they claim cognitive authority and in the fact that they still embody the figure of the colonialist even if officially there are no colonies. Postmodern thinkers advocate research that is politically sensitive rather than scientific.

The second kind of criticism directly concerned the 'scientificity' of qualitative research rather than its justification. It came from researchers who found that carrying out interpretive fieldwork is everything but applying an objective research tool (e.g. Malinowski's *Diary in the strict sense of term*). On the contrary, they claimed that fieldwork is a very subjective, personal experience, and so is its outcome (Burgess 1984: 143). Research design most often stays on paper, as the reality under study requires constant flexibility and changing the course of the study. Attempts at codifying the rules of qualitative research virtually failed. The reliability and validity of qualitative research was put under question.

Of those two kinds of internal critique, the second one plays a more important role for me as it puts into question the 'scientificity' qualitative research without saying that scientific research is wrong. The 'postmodern' view does not propose better research techniques or alternative ways to achieve reliability – it discards them as such, together with the notions of realism and realistic representation.

Another reason for questioning the scientific nature of interpretive enquiry lies in the fact that sometimes brilliant and valid insights are produced by people who are not regarded as social scientists, such as non-fiction artists (documentary filmmakers and literary reportage writers in particular). Does it mean that one need not be trained in qualitative research to apply it? If it were the case, it would suggest that granting scientific status to qualitative research is too gracious. To answer this question we must consider whether interpretive method really contains elements that distinguish it from other, non-scientific undertakings.

Facing these three problems, I believe that we have a good reason to not take Flick's optimistic verdict for granted, but instead to scrutinise in what way those methodologists who specialise in qualitative research discard the above doubts. Importantly, however, I believe that we should again not take *their* arguments for granted but instead we should have a look if their suggestions are realised in practice, by qualitative researchers.

The above approach determines the structure of my investigation in Part III. I begin with Chapter 6 that looks at what methodologists think about the way qualitative research should be carried out, and at their opinion on how these principles are realised in practice. Subsequently, in chapter seven, I compare it with the way it is applied in three pieces of actual qualitative research (Hochschild's, Baumann's and Wagner's). Lastly, in chapter eight, I examine the differences between the research methods used by interpretive researchers and the methods used by non-fiction artists, lay people, and journalists. These three chapters together will provide us with a map or signposts helping us to determine the scientific status of qualitative methods.

Chapter 6

'Methods' in Qualitative Research

In this chapter I present the outcome of my studies of methodological literature. I chose five textbooks: Lofland&Lofland's *Analysing Social Settings* (1971), Burgess' *In the Field* (1984), Flick's *An Introduction to Qualitative Research* (1998), Kirk&Miller's *Reliability and Validity in Qualitative Research* (1986), and Seale's *The Quality of Qualitative Research* (1999). I used two criteria to determine my choice: that a 'candidate' textbook is commonly used on academic research courses and that it had multiple editions. I also ensured that older and recent textbooks are included.

So far we have been using the term 'research method' quite loosely. The five research textbooks listed above distinguish the following components of research: research design, data collection, data analysis, research report, followed by professional quality control.

We can see that the first three phases are decisive for obtaining quality findings, whereas the last two are decisive for communicating those findings and putting them under scrutiny of a scientific community. These elements are essentially interlinked. The quality of reporting depends on the quality of research: if there is a mistake or negligence during the fieldwork, it cannot be corrected or made up for 'on paper' (although it can be concealed). Quality control in turn depends on the quality of reporting. Good reporting allows for good quality control.

For the purpose of my analysis, this division is a natural one. Therefore in this chapter there are two main sections:

6.1. Problems with the quality of research: how to achieve scientific (valid and reliable) findings?

6.2. Problems with the quality of scientific research reporting. Dilemmas of peer reviewing.

Within each section I am analysing what elements, according to qualitative methodologists, make qualitative research scientific, what elements are a threat to its 'scientificity', what procedures can be deployed to neutralise such 'unscientific' elements, and finally I am asking if such procedures are employed in research practice.

6.1 Problems of validity and reliability in qualitative research

There are three elements or steps of scientific research method. These are research design, data collection, and data analysis.

6.1.1 Research design

Although qualitative methodologists claim that research design is a necessary element of the research process, they do not really view it as an element that secures 'scientificity' of study. But why is that? Taking things logically, research design should be granted scientific character: after all, it makes the researcher follow a certain plan, which means that no important element gets missed out, that research is carried out in the proper order, in short, that findings are generated systematically. Most importantly, the researcher should put special effort into properly matching the method to the subject of research (Lofland 1971: 11, Flick 1998: 279) and into sampling. It is assumed that correctly carried out sampling secures the representativeness, and hence generalisability, of findings⁶³. Against that, lay enquiry does not follow a careful plan, is often unfocused and is therefore unsystematic.

But the reason methodologists do not give research design scientific 'points' is that in reality no preset, rigid design is possible to follow. Two methodologists say: 'Naturalistic research is first and foremost emergent' (Lofland 1971: 19); 'Research design will be continually modified and developed by the researcher throughout the

⁶³ Internal representativeness means that the findings hold true for the setting in question, while external representativeness means they are true in other similar settings as well.

project' (Burgess 1984: 5). Agar similarly writes, 'You can't specify the questions you're going to ask when you move into the community; you don't know how to ask questions yet. You can't define a sample; you don't know what the range of social types is and which ones are relevant to the topics you're interested in' (in Burgess 1984: 34). Matching the method and research techniques to the subject depends on the situation – cannot be structured or codified.

Flexibility is hence the researcher's most characteristic feature (Lofland 1971: 19, Burgess 1984: 143), and circularity is the research design's core feature (Flick 1998: 42). Circularity means here that the researcher revises her research design throughout the research process; she comes back to earlier stages every now and then rather than proceed linearly as in quantitative research.

This flexibility and circularity of research put a real threat of arbitrariness on the side of the investigator. Burgess observes: '[flexibility] leads to work of this kind being branded as subjective, impressionistic, idiosyncratic and biased' (1984: 143, also Flick 1998: 42). Three tools have been proposed to neutralise the potential arbitrariness resulting from the flexibility. *Auditing* is supposed to protect the researcher from too much discretion and from missing an important element of research. Before the proper data collection, running a *pilot study* helps to correct research design errors. At the stage of reporting, the researcher is asked for a detailed description of research procedures, and particularly for justifications of research design decisions (*methodological reflexivity*). This makes her think about the justifications throughout the research process. But as we will see in the forthcoming sections, in practice only the second tool mentioned above is commonly employed.

On top of the problem with lack of clear guidelines, *sampling* meets some extra problems. Firstly, qualitative sampling takes its strength from its theoretical character ('saturation' of findings) rather than from statistical calculations. There is no certainty however that when the researcher thinks her findings are 'saturated', they really are. Secondly, generalisation is problematic because people are creative agents, and may always act differently and interpret differently. This is most obvious in the case of generalising to other settings, but also in the case of the original research setting – 'tomorrow' our respondents can develop a slightly different way of

acting or change meanings they hold. Speaking metaphorically, meanings 'do not like' being moved to other settings or to other times.

But apart from these two 'theoretical' obstacles, the 'scientificity' of sampling faces even more difficulties when it comes to practice. Burgess, who - out of our five methodologists - discusses research practice at most length, admits that sampling decisions are often intuitive or opportunistic:

'There is some unintentional and unprincipled sampling involved' (Burgess 1984: 58);

'...Countless field studies where the willingness of members of the institution to cooperate with the researcher, convenience and ease of access influence the choice of the location' (ibid 59);

'However, a judgement or accidental sample is the most familiar form of sampling' (ibid 213).

6.1.2 Data collection

Let us now move on to the stage of data collection. Similarly to the case of research design, methodologists do not attach too much 'scientific' importance to specific methods of data collection. It may be surprising, since the common view has it that there are old and often used techniques of data collection (such as participant observation, in-depth interviewing) available to the researcher but not to lay, untrained people. Here we will focus on two substantial aspects of data collection: validity and reliability. 'Scientific' data collection is supposed to bring findings that are valid and reliable⁶⁴. I will discuss them in this order.

⁶⁴ The methodological textbooks studied here give an impression that the 'reliability' and 'validity' of research were seen as the basis of its quality and assessment until the beginning of the 1980's, but in the past two decades this approach lost its impact. This happened to some extent due to the influence of critical theory on mainstream theory (it should be the lay communities under study who assess the research, not a community of social scientists; Seale 1999: 9), but the main damage has been done by postmodern criticism of classical epistemology (accent on multivocality; ibid 13). Nowadays, one can roughly discern three approaches (ibid 219): 1) radical postmodernism, claiming that there can be no criteria of assessment and consistently that science is 'an outdated mode of thought' (ibid 166); this approach is popular only in selected research centers. 2) moderate postmodernism, claiming that old criteria should be replaced by new criteria such as 'credibility' or 'transferability' (Flick 1998: 226, 228n, 235, Seale 1999: 43); this is a popular if not dominant approach. 3) defensive realism – sticking to the old criteria despite the recognition of their imperfections, but seeing them less as verification tools and more as resources for deep exploration of the subject (Seale).

6.1.2.1 The validity of findings

Textbooks define validity as following: 'Validity is the extent to which a measurement procedure gives the correct answer. Valid is a weak synonym for "true"' (Kirk & Miller 1986: 19). A distinction is often made between internal validity, which concerns the extent to which propositions are supported in a study of a particular setting; and external validity, which concerns the extent to which propositions are likely to hold true in other settings, to be generalised.

What makes research findings valid, and hence contributes to their scientificity? At the stage of research design, the element most responsible for securing validity was matching correctly research methods and techniques to the subject of research. At the stage of data collection there are three such elements. The first two are obvious for fieldworkers: keeping detailed fieldnotes (Seale 1999: 220) and carrying out fieldwork for a long period of time (Kirk and Miller 1986: 32). Fieldnotes protect from forgetting data (Burgess 1984: 166) and grant these data the status of 'evidence', whereas 'the length of time spent in the field is a guarantee of the validity of fieldwork evidence, because multiple points of view are adopted by the observer over a period of time, so that the phenomenon is described from many different angles' (Seale 1999: 55).

The third element responsible for the validity of collected data is less obvious. One methodologist underlines that it is *interpretive epistemology* that warrants validity. Lofland (1971: 11) claims that such epistemology comprises two assumptions: 1) face-to-face interaction is the fullest condition of participating in the mind of another human being, 2) you must participate in the mind of another human being ('take the role of the other') in order to acquire social knowledge. Summing up, getting to know the people under study brings fewer problems with validity than indirect perception.

Kirk and Miller stress that it is the bundle of these features that provide validity: '...the sensitive, intelligent fieldworker armed with a good theoretical orientation and good rapport over a long period of time is the best [validity] check we can make' (1986: 32).

Since the 'moderate' standpoint has not delivered any coherent and advanced argument yet, I am going to apply old understanding of science as 'validity and reliability'.

What are the dangers to achieving valid results? Similarly to the case of research design, methodologists admit that none of the qualitative methods and techniques is or could be codified, and neither could be the proper way of 'being a researcher'. Pre-set objectivity of research simply does not exist: 'No 'rules' can be given about how to record, code, index, analyse and report observations that are made in field settings' (Burgess 1984: 183, also Flick 1998: 146, Seale 1999: 7); 'Analyses of research practice have demonstrated that a large part of the ideals of objectivity formulated in advance cannot be fulfilled' (Flick 1998: 3). This fundamentally distinguishes interpretive studies from quantitative ones. Hence the process of data collection is not really viewed as suitable to be critically assessed by peer reviewers (Burgess 1984: 31, 107, Flick 1998: 54, 57).

But the inherent flexibility of the researcher entails that the distinction between method and art becomes hazy (Flick 1998: 184). As one of the critics wrote about Geertz: 'Geertz is developing as a philosopher, not as a methodologist. His anthropology is an art, not a science. To a very large extent therefore his work does not provide a model for other anthropologists or sociologists of lesser talent to follow since he proceeds from an intuitive grasp of what is important [...]' (Colson 1975: 637). This dilemma refers not only to conducting research but also to assessing it. One of the methodologists admits: 'If the findings and procedures of scientific research are mainly judged according to their presentation and to the stylistic and other qualities of the report or article, the border between science and (fine) literature becomes blurred' (Flick 1998: 244).

But not only the border between scientist and *artist* becomes vague, it is also the difference between scientist and *lay person* that is problematic. Because research techniques are not codified, researchers cannot really be trained. It could then be argued that what matters most in obtaining rich data – apart from the amount of research experience - are *interpersonal skills*. This latter element however cannot be trained: there are naturally better and worse researchers. So it may be that a lay person with good interpersonal skills will acquire more valuable knowledge than a scientific researcher with poor interpersonal skills.

Regarding the issue of fieldnotes and evidence, it is problematic how to support those interpretations that deal with 'tacit' meanings, as there is no explicit, tangible first-order utterance or behaviour. The requirement of recorded evidence may unjustly undermine the adequacy of a good interpretation.

6.1.2.2 Enhancing the quality of findings: remedies for low validity.

It can however be argued that scientific researchers have in their arsenal specific 'remedy' tools that help them collect data that are more reliable and valid than the data of lay researchers. All the methodologists devote significant space to these tools. I am going to discuss them below in detail.

The authors mention three ways of enhancing the validity of research. *Triangulation*, or simply 'diversification', is the first one (Burgess 1984: 144). Methodologists mention four types of triangulation: of data, investigator, theory, and methods. They mean, in this order, diversification of sources and kinds of data, carrying out research with more than one investigator, approaching the investigation from different theoretical perspectives, and mixing research methods (e.g. qualitative and quantitative). The triangulation of methods is most popular.

Triangulation as such encounters two kinds of criticism. First, postmodern researchers notice that it incorrectly assumes that there exists one, objective reality (see Seale 1999: 53) that is 'fixed' with different research tools. On this note, the 'multiple investigators' triangulation is an interesting case (Burgess 1984: 158, Seale 1999: 155n, Flick 1998: 226). All authors recommend this tool when applicable, but none of them sees that it cannot help with interpretive dilemmas. Of course one interpretation can be achieved after discussions between research team members, but it does not mean that this interpretation is adequate or final. In fact, it would be much more fruitful to reveal to the readers those different standpoints from which one was distilled. If the postmodern argument is right, multiple researchers do not enhance validity in interpretive studies.

Another problem with triangulation is logical: triangulation cannot escape from the dilemma of induction. No matter how many tools we use or how many checks we perform, we never can be certain that our results are correct.

The second way of enhancing validity is to *search for negative instances* (Seale 1999: 38, 73n). It is claimed that a thesis whose scope has been limited after finding examples that qualify it is more credible than a thesis that has not been subjected to such search. Again, I believe this to be a good idea for qualitative studies of social processes or phenomena, but it may not be a particularly useful principle in the case of interpretive dilemmas.

Member check is the last procedure of enhancing validity (Seale 1999: 43, 60). It basically means that after completing her study, the researcher presents her interpretations to the subjects and asks them whether they are correct or incorrect. However, there is one big problem with this procedure, which unfortunately but characteristically is not discussed by methodologists (although it is noticed in Flick 1998: 225). I mean that member check logically presupposes that the research is not trying to generate new knowledge. For members presented with something new about them can reject it even if it is adequate, or can acknowledge it and review their meanings (actions, values), which means that research results have a critical (rhetorical) nature rather than an objective one. Seale (1999: 182) and Flick give telling examples. Additionally, member check cannot be applied in the case of transcultural translation or mediation. By definition such mediation entails the use of concepts that in turn are alien for the subjects.

6.1.2.3 The reliability of findings

Textbooks define reliability as following: 'Reliability is the extent to which a measurement procedure yields the same answer however and whoever it is carried out' (Kirk & Miller 1986: 19). Internal reliability concerns the replicability of a particular study: would other researchers studying the same setting generate the same findings? External reliability concerns the replicability of entire studies: would other researchers studying similar settings generate the same findings? (Seale 1999: 42).

What makes findings reliable? Earlier we said that findings are valid when they reflect reality adequately. The key factor in obtaining valid results is to match research methods and techniques appropriately to the subject of study. Even if this has been done correctly however, the researcher may fail to apply the chosen

methods and techniques properly. This is where the notion of reliability is helpful: findings are reliable if any other researcher (who applied procedures correctly) would obtain the same results as the original ones. In other words, findings are reliable when a successful replication takes place.

Replication of studies, a very common procedure in natural sciences, is however not common in the social sciences. (Reasons for that are going to be discussed later on in the thesis). In this situation, the assessment of reliability is usually based on a careful analysis of procedures carried out by the original researcher. This analysis constitutes a kind of 'imaginary replication': the critic puts herself in the shoes of the researcher and decides if she would carry out the research in the same or in a different way. Reliability therefore refers to the extent the researcher is accountable. If the researcher can show strong reasons for carrying out research procedures in the way she did, her findings are reliable. Consequently, methodologists claim that it is most of all research description that matters. If it is detailed and generally of high quality, then findings are accountable and reliable.

Note that validity does not automatically imply reliability, and vice versa. Research procedures may be chosen correctly but carried out in a bad way. And analogically, findings may be reliable but not valid. This happens when a badly matched procedure, for example a structured questionnaire instead of an in-depth interview, was carried out correctly. 'The reliability of the observations does not entail theoretical validity' (Kirk and Miller 1986: 26).

What are the dangers to the reliability of findings? Most obviously, only a fraction of what happens in the field becomes fieldnotes, and even less of it makes its way to the final research report, which limits the possibility of assessing whether research procedures were carried out correctly. Also, the researcher may use detailed descriptions for building authority and in consequence for covering up the lack of genuine research findings.

6.1.2.4 Enhancing the quality of findings: remedies for low reliability

Drawing on 'grounded theory', Flick proposes to 'check the reliability of interpretation by testing it concretely against other passages in the same text or against other texts' (Flick 1998: 221). Further, Lincoln and Guba proposed the procedure of

'auditing' (Flick 1998: 229, 276, Seale 1999: 43). Generally speaking, the idea is to use independent research consultants in the course of the research rather than afterwards. The researcher tells them about her decisions, problems and achievements, and auditing consultants take care that the researcher 'does not make her life too easy', by demanding relevant actions. Unfortunately, this procedure is not popular amongst practitioners.

Some methodologists try to enhance reliability of findings by codifying research techniques, regardless of the awareness that qualitative research requires flexibility rather than strict code of conduct. For example, Burgess in his *In the Field* takes pain to provide the readers with some comprehensive lists of 'things to do' when carrying out various phases of qualitative research. For example, he writes about general principles concerning research access:

'First, the access should not merely be negotiated with those who occupy the highest positions in a social situation but with individuals at different levels so as to avoid misunderstandings. Furthermore, in situations where different groups are involved it is essential to negotiate with all the parties so as to avoid accusations of bias and to prevent the research report being considered partisan. Secondly, it is important to develop an account of the research that is plausible to those involved. In turn, it is important to indicate to individuals any major changes in the direction of the research. Thirdly, attempts should be made to present an accurate account of the research design although this may not always be possible and compromise may be essential. Fourthly, researchers need to monitor their own activities not only to understand the research process but to deepen their own understanding of the relationship between research question and analyses, for data are derived and shaped in all these initial encounters' (Burgess 1984: 51).

Apart from the tension between flexibility and codifiability expressed earlier, two remarks should be made here. Firstly, it seems to me that such rules originate more from practical experience than from methodological reflection. As such, they are probably employed also by other experienced, but non-academic researchers (literary reporters, documentary filmmakers etc). Secondly, such lists perhaps should be treated as 'checklists' to be used after research (to find out whether any important element has been omitted) rather than before it (as research guidelines).

6.1.3 A note on an overlooked element: social positioning & trust

I believe we may try to look for 'scientificity' of qualitative research also in another aspect of data collection.

Methodologists notice that the researcher should aim at constructing a comprehensive image. Burgess refers to Van Mennem: '[he] considers that sociologists need to establish relationships with a number of informants as it is doubtful whether sufficient individuals are acquainted with all aspects of a cultural setting' (Burgess 1984: 75). Methodologists however take it for granted that the researcher has access to various sub-structures within the group in question. But the 'social positioning' of the researcher is important from our point of view, for it distinguishes the scientific researcher from both mere insiders and lay enquirers.

What methodologists do not take any notice of is the fact that the scientific researcher is in a peculiar but rather fortunate situation: since the people under study are aware that her purpose – the research – is legitimate, and since as an outsider she is not embroiled in this particular structure, she is likely to be given research access to the wide gamut of positions/roles and to be provided with information and opinions restricted for others (other insiders or illegitimate outsiders). Summing up, thanks to her position of 'the outsider inside the group' she is able to gain a comprehensive image of this group.

What matters here is that she is actually privileged in comparison to the insiders, because they are positioned in the local social structure and hence usually have no straightforward access to people holding very different positions/roles (typically, executives have little access to manual workers and vice versa). She is also privileged in comparison to lay researchers, who have no legitimate reason for enquiry and hence would not be given research access to different 'compartments' of the group in question.

Having said that, we need to acknowledge that in one particular aspect her legitimised status of scientific researcher is to her disadvantage in comparison with the lay enquirer. Namely, subjects often tend to treat the scientific researcher as a 'spy', and keep important information from her. The lay researcher, by contrast, is

not perceived as any threat, and therefore given more trust. Therefore the findings of the lay researcher may be paradoxically more valid than those of the scientific researcher.

6.1.4 Data analysis

Data analysis is seen as the core of qualitative research (Flick 1998: 178) and the core aspect of assessment. But it is surprising that this core element is relatively underdeveloped. The general impression is that methodologists are much better at discussing particular issues than at drawing a clear general picture. Two issues within data analysis are focused upon: generalisability/representativeness and coding. Let us have a closer examination of them.

6.1.4.1 Generalising the findings: the issue of representativeness

a) problems with generalising due to the character of qualitative sampling

Sampling decisions are supposed to aim either at the representativeness of cases (the 'width' of research) or at the relevance of cases (the 'depth' of research) (Flick 1998: 70). Flick claims that the latter is more typical for qualitative research (ibid. 66). Unlike in quantitative research, the qualitative researcher cannot apply random sampling and mathematical principles of generalisation.

The problem with such 'theoretical' sampling – sampling cases until the study is 'saturated' – is that the researcher may pick 'wrong', unrepresentative individuals, and terminate the research before all important people are included. In this case however, we encounter the problem with assessing such decisions. How can relevance be measured? Can someone not familiar with a certain field criticise the researcher's decision?

b) problems with generalising due to the nature of meaning

The methodologists overlook the fact that generalising findings to a wider setting presents a logical difficulty. I mean here that although interpretivism assumes that

there are no fixed meanings in the social world, researchers who claim to be influenced by it – such as grounded theorists – aim to produce abstract typologies, which in turn assume that identical meanings and actions (re)appear in different locations.

6.1.4.2 The problematic relationship between interpretation and coding.

The term 'interpretation' appears surprisingly seldom in methodological textbooks. The authors prefer to talk about practicalities – i.e. coding rather than interpreting. If the term appears, it is treated as unproblematic. None of the textbooks addresses the issue of 'meaning', in particular of 'lower-level' and 'higher-level' meanings (Fay) (one of them states that 'one of the key jobs of the social analyst is to articulate latent meanings' (Lofland 1971: 74), but does not expand on this). It seems that 'interpretation' is not understood as intercultural 'translation' or mediation. The problem with interpretation is seen as the problem of the adequacy of data (misinterpretation/misrepresentation due to researcher's errors, or bias; Lofland 1971: 44, Seale 1999: 56) rather than the problem of meaning (due to the complex nature of phenomena or cultural differences). Methodologists believe that good or true interpretation is 'natural', whereas bad or false interpretation results from bias. It is also admitted that the grounded theory focuses on phenomena, not meanings (Flick 1998: 181, 183). Flick (ibid 221) claims that researchers can receive training in interpretation, but he does not give any clue as to what this training may look like.

Coding and categorising are seen as quite straightforward, practical activities. There are no rules for producing codes though (Burgess 1984: 183). Methodologists do not want to admit that they are not sure how interpretation relates to coding. Flick opens his chapter entitled 'Coding and categorising' with the statement: 'the interpretation of data is at the core of qualitative research' (1998: 176), which suggests that interpreting happens at the stage of coding. But then he says that coding means abstracting (decontextualising – F.S.): 'concepts or codes are formulated as closely as possible to the text (of fieldnotes – FS) and later more and more abstractly' (ibid 176). If so, then interpretation cannot be identified with coding, as interpretation does not entail abstraction.

Seale says that 'coding is an attempt to fix meaning' (1999: 154), but then does not mention 'interpretation' at all. Later on he says that 'coding is in fact a method for "showing" [large] bodies of data' (ibid 156), which means that codes do not interpret meaning but are signposts for meanings. He gives an example: '...one can carry forward a concept such as 'covering up' to demonstrate its relationship to another concept such as 'justifying inaction', but one cannot constantly carry forward a description such as 'When I walk, I walk as normally as possible' and demonstrate its relationship to another description such as 'My husband doesn't really understand' (Seale 1999: 88). It is quite obvious that the matter here is decontextualisation, but Seale does not mention it. Also it is clear in this case that the choice of codes is not straightforward, and that they are interpretive.

But just as 'interpretation' is replaced by 'coding', 'coding' subsequently becomes 'filing'. Lofland says that 'the maintenance of a filing system is actually a physical manifestation of a more abstract process: that of building codes, or coding' (1971: 132), and then devotes the whole chapter to the nuances of filing.

Summing up, what coding does is to abstract findings within the case under study, removing particularities and finding a common core. This is a different operation to the one of generalising mentioned above. However, similarly to that latter case, there are logical difficulties here as well: abstracting findings may potentially distort the original meaning in a crucial way.

6.2 Problems with presenting and assessing the quality of research findings

In this section I shall discuss 'scientific research reporting' and 'scientific quality control'. The reason these two elements are discussed together is that conscientious research reporting is necessary for quality control to be productive.

Before moving on to the main discussion a remark is worth making. Research writing, seen as a fairly unproblematic and 'un-philosophical' issue in the past, has been the subject of massive concern on the part of modern methodologists. Why is that? This shift has happened under the influence of postmodern argumentation.

This argumentation emphasises two linked things: first, that writing itself is *part* of research (and a really important part) rather than *following* research; and second, that writing in a sense *constructs* the world under study rather than mirrors it (Flick 1998: 12, 30, 146, 169, Seale 1999: 150). The world represented in the report is always to some extent the researcher's construct. This is a very interesting argument that however cannot be taken further here due to lack of space.

6.2.1 Theory and practice of scientific research reporting

There are two analytical dimensions, so to speak, of research reporting. The first one concerns the subject of study and focuses on providing data about it. The second dimension refers to the research and the researcher and focuses on providing description of research stages, tools used, and information about the researcher. It aims at showing how data was collected and analysed. It is the quality of the latter dimension that determines the reliability of the first dimension. I will discuss them in this order, but naturally I will devote much more space to the second dimension.

6.2.1.1 Reporting evidence

Presenting evidence is a less complicated issue than describing research itself. There are only two problems that are of importance here. The first one has already been mentioned in the section on 'data collection': I wrote there that it is difficult to provide direct evidence for interpretations of 'tacit' meanings. Logically, it should be expected that the researcher be explicit about such cases and employ the mode 'I have chosen this interpretation over other ones because...'. This is however very rare in practice.

The second problem concerns whether the whole body of data/fieldnotes should be revealed (Seale 1999: 156). There is no agreement here. Those who think that it should, believe that 'Many questions can be addressed by referring to the fieldnotes that are provided in the final research report' (Burgess 1984: 213). But there are three problems here: a practical one – how to enclose sometimes five thousand pages of fieldnotes in the research report; a readership one – who would like to read

all those fieldnotes; and a logical one, expressed by Seale: 'The requirement that all data is shown to the reader is not only impractical but, I believe, misguided' (1999: 156). Seale thinks that data itself can be summarised. Data makes sense for the reader only when it is ordered, edited.

6.2.1.2 Reporting research

Let us consider the theory and practice of 'telling how the study was done'. Probably the most common feature of all textbooks is the stress on revealing to the readers how the research was done (in terms of methodological decisions), not only what the results are (Seale 1999: X). Since there are hardly any principles of research, carrying out fieldwork is far from obvious; providing justifications is therefore crucial. All major decisions need to be justified and the author should be self-reflexive. That comprises of showing one's values, assumptions, theoretical positions⁶⁵, research design, methods and techniques applied, sampling decisions, problems encountered, failures experienced, as well as showing one's fieldnotes. All that is supposed to enhance the reliability of studies:

'The research report with its presentation of and reflection on the methodological proceedings, with all its narratives about access to and the activities in the field, with its documentations of various materials, with its transcribed observations and conversations, interpretations and theoretical inferences is the only basis for answering the question of the quality of the investigation.' (Luders 1995: 325 in Flick 1999: 243)

'Field researchers need to defend their actions by discussing the principles by which they select some situations, events and people but reject others while working in the field' (Burgess 1984: 53)

'Methodological awareness involves a commitment to showing as much as possible to the audience of research studies about the procedures and evidence that have led to particular conclusions' (Seale 1999: X)

'Values, assumptions and theoretical perspectives must be shown to the readers' (Seale 1999: 154)

'Reliability depends essentially on explicitly described observational procedures' (Kirk&Miller 1986: 41)

'If no observation can be free from theory, the best that can be done is to make explicit whatever theory has been used' (Seale 1999: 163)

⁶⁵ 'Theory' is however understood very loosely: 'Yet it is clear that the word 'theory' is here standing for many things, such as values, prejudices or subconscious desires of the researcher, many of which are by definition not available for explanation by the person who has been influenced by them' (Seale 1999: 163). Additionally, it is not explained what the relationship between the theory held before the research and the theory formulated after the research is. (It is claimed that research should lead to the formulation of a theory, but also that it is theoretically informed from the beginning).

The bundle of elements listed above can be seen to fall under three categories - theoretical, methodological and epistemological - all of which in turn can be described as 'reflexivity'⁶⁶.

The researcher should be reflexive about her particular research decisions; let us name it *methodological* reflexivity. In such a case, the audience provided with information on the methodological side of the research is in a good position to assess whether the findings are reliable or not (particularly whether they are representative for wider social circumstances). Therefore this allows them to control the quality of research.

Secondly, the researcher should be reflexive about her *theoretical* assumptions, letting the audience 'see' them in the findings, and imagine the findings seen from other theoretical perspectives. Additionally, spelling out theoretical assumptions guards the researcher from holding contradictory ones.

Finally, *epistemological* reflexivity is supposed to neutralise researcher's bias. The classic way to deal with researcher's bias was simply to call on the researcher to stay unbiased, and to separate observational/evidence statements from her interpretations. When it became obvious however that the positivistic ideal of the researcher as a neutral tool is impossible to sustain in qualitative research, methodologists took pains to deal with it.

Some of them took postmodern positions, claiming that 'bias' is a term from the objectivist's dictionary and that all we can achieve is multivocality. 'Modern realists' decided to accept the fact that researcher's subjectivity is an ineradicable and substantial element of production of knowledge (Flick 1998: 6) - she is not a neutral tool - but they did not want to give up realism. Their way to go about it is to call on

⁶⁶ In the course of time the concept has acquired a number of different meanings, to the point when it is no longer possible to use it straightforwardly, without qualifications. One of the most common understandings of it has been popularised by Anthony Giddens. He pointed to the fact that everyday people pick up ideas and theories produced by social scientists about those everyday people, and they in turn take this knowledge into account in their conduct. But obviously this meaning of reflexivity is not the one I am interested here. Instead, I would like to focus on the reflexivity of the researcher.

the researcher to be aware and explicit about her bias, so that later on this bias can be recognised and eradicated. In this way the objective reality can be distilled from the mixture of subjectivity and objectivity.

The researcher may provide the audience with some insight into her personal, biographical and social characteristics. This can be very hard for the researcher as it is difficult for her to see where she is biased and where she is not, when her perception may be influenced by unconceptualised assumptions and when not. As Seale noticed, 'once "assumptions" are clear to those who hold them, they are presumably no longer "assumed"... There seem to be inevitable limits to the possibilities for reflexive accounting' (1999: 164).

But it is frankly admitted that unfortunately in most cases researchers do not comply with those requirements: the *practice* of scientific reporting diverges from theory significantly. According to methodologists, qualitative researchers often fail in the substantial task of showing procedures. The list of complaints is fairly devastating:

'Sociologists who engage in field research seldom discuss the ways in which they analyse their data' (Burgess 1984: 177)

'However, it is relatively rare for researchers to indicate how theories are developed and generated within their studies or to indicate the procedures that are adopted' (Burgess 1984: 160)

'However, it is rare to find a systematic discussion of the principles of selection that are used in a particular study' (Burgess 1984: 53)

'Rarely has any researcher or student actually seen another's field notes' (Kirk&Miller 1986: 52)

'Reliability in replication studies requires a complete specification of background assumptions and field procedures, information which many researchers, in practice, do not provide' (Seale 1999: 141)

'Peculiarly, despite considerable professional and philosophical concern about error and bias in naturalistic studies, those topics rarely arise in connection with accomplished works. They appear rarely, even, as unpublished allegations along the grapevine of professional social science' (Lofland 1971: 50)

'Often researchers are not concerned about whether a site is 'typical' or 'representative' (Burgess 1984: 59)

'There is a considerable difference between the interviews that are discussed in standard methodology texts and the practice of interviewing' (Burgess 1984: 101)

' "Confessions" published after main work usually concern methodology but not how the research was actually conducted' (Burgess 1984: 209).

'Telling how the study was done' may also be jeopardised or be only apparent if it takes the form of 'confession' rather than 'reflexivity'. 'Confession' does not serve discussing field decisions – although it may seem to do so - but 'is a strategy for gaining authority rather than giving it away, and involves no departure from realist assumptions' (Seale 1999: 161).

I believe the inherent opposition between the aims of establishing the researcher's authority and self-criticism is the main reason why the practice of 'telling how the study was done' is so divergent from the 'theory' of it.

I want to finish this section by pointing to the optimism of methodologists as to the self-criticism of researchers and their eagerness for exposing their research to peer criticism. The methodologists are by and large optimistic about the honesty of researchers concerning their fieldwork decisions and their self-critical attitude in general. It is striking that they retain their optimism in spite of noticing that the reality may not be that great:

'What typically goes into describing how the study was done are 'the second worst' things that happened' (Lofland 1971: 149)

'We delude ourselves if we expect naturalistic researchers actually to 'tell all' in print' (Lofland 1971: 149)

'Conventional reporting covers something up' (Seale 1999: 166)

'Semiotic means are sensitive to cheating' (Flick 1998: 245)

'The failure of qualitative research is discussed much too seldom' (Flick 1998: 280)

'Peculiarly, despite considerable professional and philosophical concern about error and bias in naturalistic studies, those topics rarely arise in connection with accomplished works. They appear rarely, even, as unpublished allegations along the grapevine of professional social science' (Lofland 1971: 50).

None of them however is happy to draw consequences from those insights. Quite the opposite! Lofland defends researchers by stating that 'Perhaps constant general worry about potential error and bias protects the naturalistic researcher from their actual occurrence' (1971: 50). To me, this has quite significant consequences for the scientific status of qualitative research. If we suspend optimism for a moment, it would seem that all those good methodological recommendations do not guarantee anything if researchers manipulate them and sweep errors under the carpet. That may also cast some light on the fact that there are not very many methodological

considerations in research reports, and that many good methodological ideas end their life on paper.

6.2.2 Theory and practice of scientific quality control

All authors agree about the need for a research community existing as a key audience for social researchers concerned about the quality of their efforts (Seale 1999: 30). 'Scientific' quality control is meant to ensure the growth of general knowledge through criticism. There are two ways of controlling quality: the main form of it is *peer review*, which means finding weaknesses in other researchers' reports. Another possibility of verifying quality of findings is to run one's own *replication of research*.

6.2.2.1 Replication of studies

Replication of research, the most effective way of controlling quality in natural sciences, in practice proved to be very difficult in the case of social sciences: 'Replication of studies is rarely seen in qualitative social science' (Seale 1999: 41). And when such attempts have been undertaken, 'The history of replications has been a disappointing one. Replications have generally revealed discrepancies between the first and second study' (Seale 1999: 142)⁶⁷. This is disappointing not only because findings generally do not get confirmed, but also because exposing such discrepancies, surprisingly and unfortunately, has not led to a productive critical discussion. Seale (1999: 145) gives a well-known example of a criticism of Whyte's *Street Corner Society* by W. Boelen. She revisited Whyte's research site forty years later and, on the basis of her interviews with the same people who Whyte lived with during his fieldwork, accused him of exaggerating the poverty they lived in. In particular, Whyte wrote that they had no bathtub, and re-interviewed respondents said that in fact they had a bathtub. Whyte responded to Boelen's criticism by restating that there was no bathtub in the house. Now, if verifying simple facts can cause such trouble, how about sublime interpretations?

⁶⁷ NB this suggests that the problem of 'cumulativeness', dropped rather than resolved some time ago, seems not to stop haunting qualitative methodology.

Why has the possibility of verification in qualitative research been overestimated? To begin with, replication meets substantial epistemological problems: the social reality is never frozen in time, and social research always changes its subjects at least slightly. On top of that, poor reporting of research methodology means that critics do not know exactly what procedures were originally applied, which in turn means that their replication may diverge from the original. Good research reporting is necessary for replication to be a real replication, rather than just another, similar but not identical, study.

6.2.2.2 The peer review process

Methodologists do not discuss the realities of quality control. Actually, detailed peer critique is infrequent in qualitative social science. Usually it is confined to book reviews in journals, which are superficial due to very limited length.

In some cases critics focus on methodological shortcomings, but nevertheless the works in question remain influential and relied on by other researchers despite meeting severe criticism. (As happened for example with Goffman's and Mead's studies). In other cases, researchers who did similar fieldwork come up with criticism of data ('My data denies Smith's findings', see an example in. Baumann 1996: 88) rather than the criticism of methodology applied.

But what are the main hindrances here? What makes peer reviewing in interpretive social science difficult? One could suspect that the difficulties with 'how to research effectively' and 'how to report effectively' entail difficulties with controlling quality of research. This is indeed the case: problems are inherited. Firstly, methodological problems with research – no agreement on how exactly to carry out good research – entail no agreement on what criteria are to be used in assessment. Seale (1999: 32) quotes Clifford: 'The criteria for judging a good account have never been settled and are changing' (Clifford 1986: 9). Secondly, this is in turn amplified by practical deficiencies of research reporting. As I mentioned earlier, good research reporting is necessary for quality control to be productive. Poor reporting often means poor grounds for research quality assessment.

But, on top of that, several further issues are relevant:

Mainstream publishers are not keen on methodological chapters. That means that even if the researcher is methodologically conscientious, her critics have little chance to learn about methodological details of her fieldwork, for they would get removed from the published report.

Researchers naturally tend to convince the readers that the research was carried out professionally rather than reveal their errors. This means covering up research decisions that are intuitive or opportunistic. At the end of the day, nobody wants to fail his or her work. This does not make the critic's life easy.

Some of the unspoken norms of social-scientific community may play a very significant although underestimated role here. Particularly, one of the methodologists admitted that 'To raise questions about the reliability of another's observations is taboo, as though it were an accusation of incompetence, bias, or dishonesty' (Kirk&Miller 1986: 52).

There is no agreement on whether the whole body of data/fieldnotes should be revealed (Seale 1999: 156). Those who think that it should believe that 'Many questions can be addressed by referring to the fieldnotes that are provided in the final research report' (Burgess 1984: 213). But there is a practical problem here – how to enclose sometimes five thousand pages of fieldnotes in the research report? And which publisher would not mind it?

And finally, *quality control may be effectively undermined by the tendency on the part of the researcher to shower the readers with minute details.* Kirk and Miller state that 'When discussing the validity checks of social research, it is useful to remember that a careful description of what is done generally tends to suggest an obsessive preoccupation with detail on the part of the researcher (1986: 20). In effect, 'The evidence within the studies themselves is overlooked... there is need for detective work' (Burgess 1984: 209).

6.2.3 Conclusion

In assessing the scientificity of interpretive research in terms of its methodology, we have seen that there are a number of factors testifying 'for' and 'against' granting it scientific status. Although methodologists are not united in their view how this kind of research should be carried out, they have produced some ideas how to neutralise those elements that are 'unscientific'. Due to the nature of interpretation it is doubtful if such neutralisation can be fully successful, but even partial success should be seen positively.

But even when researchers do apply those 'repair' tools, the gain is in most cases lost at the stage of research reporting. After research itself, this stage is another crucial moment on the way to achieving 'scientificity'. For various reasons however, researchers-turned-authors usually fail in meeting the standards of scientific research reporting. In consequence, the stage of quality control is undermined before it even starts. In summary, the 'scientificity' of interpretive research is undermined more by the practice of research than by 'theory' of it.

Chapter 7

Interpretive Methods: Three Case Studies

In this chapter I am going to apply our discussion on qualitative methodology to three actual case studies. These are going to be the same titles as earlier in this thesis, which are Hochschild's *The Time Bind*, Baumann's *Contesting Culture* and Wagner's *Checkerboard Square*.

As we are predictably not in a position to replicate these studies, the only option of quality control available to us is peer review based on research reports. This is why the structure of my investigation follows from the previous chapter, 'How to present research findings effectively'. There are two main sections, one on reporting evidence and one on reporting the research process. The latter in turn is divided into research design, data collection and data analysis.

The focus of my presentation here will be the reliability and validity of findings. Let us recall the definitions:

'Reliability is the extent to which a measurement procedure yields the same answer however and whoever it is carried out' (Kirk & Miller 1986: 19).

'Validity is the extent to which a measurement procedure gives the correct answer. Valid is a weak synonym for "true"' (Kirk & Miller 1986: 19).

Let us also remember how validity and reliability relate to reporting evidence and reporting research. Good *reporting of evidence* provides the means to assess the validity of findings. *Research reporting* provides the means to assess both reliability and validity of findings: the detailed account of *what* procedures were carried out⁶⁸ enhances or weakens the validity of findings, while reporting *how* procedures were applied enhances or weakens the reliability. In this way we, the critics, can assess whether the right procedures were chosen (with respect to the particular subject),

⁶⁸ Most importantly – providing justification for not carrying out alternative procedures.

and if they were the right ones, we can state that the findings are valid. Showing the correct reasoning (data analysis) also influences the validity of findings.

Following that, if the procedures were carried out correctly (most often it means laboriously and carefully), the findings are reliable. Analogously, if findings result from badly chosen procedures, they are not valid, in which case the issue of their reliability is secondary if not irrelevant.

Finally, it needs to be emphasised that as quality controllers we have no choice but to presume that what is missing in a research report was also missing in actual research.

7.1 Hochschild: *The Time Bind*

Hochschild's book is the outcome of qualitative research conducted by her in 'Amerco' (real name disguised). She spent three summers researching the company, mainly interviewing its employees (130 respondents in total, across the whole hierarchy) but also observing meetings, studying internal records and 'climate' surveys, as well as following six families dawn-dusk.

The style of the book is unusual for a social-scientific work. Hochschild makes the reading attractive by skilful application of literary style and conventions, particularly by telling 'stories'. Stories are easy to understand and appropriate as readers can empathise with principal characters and compare themselves to them. But they may, intentionally or not, fulfil a rhetorical role: it is easy to turn the reader's attention from weaknesses of research or from stories that are less straightforward.

Hochschild also makes the reading attractive by presenting her fieldwork as a detective story. It is intriguing that this trick seems to be very catchy in the commercial sense, which has been confirmed by the publishing success of not only *The Time Bind* but also of Putnam's *Bowling Alone*.

7.1.1 Reporting evidence

The Time Bind is very rich in quotations from interviews. These quotations serve as evidence and complement Hochschild's abstract, decontextualised statements. A lot of evidence provided is descriptive statistical data – which is useful but also partly turns the reader's attention away from the deficiencies of qualitative evidence. These deficiencies include a failure to take counter-evidence seriously, a failure to justify the scope of her claims, and straightforward problems of data presentation. I will consider them in turn.

Interestingly, some of Hochschild's evidence turns out to be actually *counter-evidence*, but she does not feel embarrassed by this fact. For example, at one point Hochschild claims that 'When I asked employees whether they worked long hours because they were afraid of getting on a layoff list, virtually everyone said no' (2001: 29). One of her principal characters, Becky, stated however that 'You always get the

sense that you can be replaced. The company doesn't say that to us. They don't have to' (2001: 154). This seems to be an anomaly that Hochschild should at least take seriously. Similarly, there is a lot of evidence throughout the book suggesting that competition is the dominant cause of working long hours amongst managers and that money is the analogous cause amongst shop-floor workers. Hochschild however persistently forces the claim that it is the 'reversal' of home and work and the engineered company culture that is to be blamed for it.

Hochschild also often fails to present evidence to justify the scope of her claims. For example, the following statements are supported by evidence for a certain group but Hochschild freely generalises them to the whole society: 'Work-family balance model... was a reality for a small minority at Amerco, and probably a larger minority nationwide' (2001: 203). Hochschild does not say why this latter should be the case. Taking it commonsensically, she is right - but social sciences' aim is precisely to question commonsensical statements. On the same page, she writes: 'families that fall into the reversal model in which home is work and work is home have been on the increase over the past thirty years'. Does she have evidence for this? Why thirty years and not twenty or forty? Another example: 'Work... in recent decades has largely competed with the family, and won' (2001: 203). Even accepting Hochschild's argument, this is right only for those who work long hours.

At a few crucial points Hochschild fails with clear, straightforward data presentation – just when it is most needed for the reader to critically assess the evidence. For example, in the chapter where she discusses work-family balance, Hochschild quotes the results of her survey: 'When asked, "Overall, how well do you feel you can balance the demands of your work and family?", only 9 percent said "very well."' (2001: 199-200). This is definitely suspicious – we would like first of all to know how many people responded 'well' and 'not too bad'! Was it 20 percent or 70 percent? Why did Hochschild not reveal this – maybe the data did not support her thesis?

A similar example can be found on page 200. Hochschild wanted to find evidence supporting her thesis that nowadays work often feels like home. 'I asked this question: "Is it sometimes true that work feels like home should feel?" Twenty-five percent answered "very often" or "quite often", and 33 percent answered "occasionally". Only 37 percent answered "very rarely." It looks very convincing and

reliable, but notice that the remaining 5% is missing! What actually happened to it? Maybe 5% of respondents did not answer the question. This would mean however that Hochschild's measurement scale was dodgy: 'very often, often, occasionally, very rarely' – obviously, what is missing is 'rarely'. Hochschild is too professional a social scientist to make such a mistake. It seems therefore that the category 'rarely' was there, and that 5% respondents ticked it. But why did Hochschild aggregate categories 'very often' and 'often', while not doing so with 'rarely' and 'very rarely'? I believe she did so because it would look less convincing. Saying 'Only 37 percent...' already looks weak as 37% is a lot, but 'Only 42 percent...' looks even more ridiculous. Also, why did she aggregate 'very often' and 'quite often' in the first place? Maybe only 2% answered 'very often' and 23% said 'quite often'? These gaps in presentation of crucial data weaken the validity of Hochschild's work.

As if Hochschild was not sure she had provided enough strong evidence that employees felt at work like at home, a few paragraphs later she says: 'To the question "Where do you feel the most relaxed?" only a slight majority in the survey, 51 percent, said "home." Two things are problematic with this example: does it mean that 49% answered they are most relaxed 'at work'? Or maybe there was (at least there should be) the third option ("equally at home and work") left to respondents, and 25% ticked it? It is impossible to know it from Hochschild's report. Secondly, the wording of the question is bad: does the fact that one feels '*most* relaxed' at work entail that he or she feels stressed at home? Maybe they feel '*just* relaxed'? (Notice how frequently Hochschild uses the phrase 'Only (x) percent...', putting rhetorical pressure on the reader to convince him or her of the argument.)

Having said that, it must be conceded that even when she is a bit manipulative with interpreting evidence to her advantage, Hochschild's presentation of evidence is straightforward enough to allow for criticising her research, as has just happened for example in the previous paragraphs. It is possible to criticise the validity of Hochschild's insights on the basis of evidence she provides the reader with. It requires 'detective work' though.

7.1.2 Reporting research

Hochschild's research shows acutely the flexibility and circularity of qualitative study. Research design, data collection and data analysis are heavily intertwined, and form the pattern "design – collection – analysis – design – collection – analysis – design...". The author's own understanding (and presentation) of her research as a kind of 'detective investigation' is not inappropriate – Sherlock Holmes used to follow exactly the same pattern in his enquires.

For example, Hochschild began her research with a very rudimentary design: she only had a broad research question, she knew what organisation she was going to study, and held some theoretical assumptions. Then she went off to the field. There she quickly learned that very few people were using family-friendly policies, and after analysing that she changed her design (new, more precise research questions and new sampling within Amerco).

For analytical reasons however, below I shall discuss Hochschild's research design, data collection and data analysis separately.

7.1.2.1 Research design

Of all the elements of research design, Hochschild pays more attention to discussing her research questions and sampling than to revealing her assumptions or justifying the choice of methods.

The logic of Hochschild's research was not the same throughout the study. At the beginning, she wanted to *verify* her hypotheses, but later when this proved to be a blind alley she started approaching the problem in an *exploratory* manner, with an open mind, which led her to the discovery of the phenomenon of 'reversed' worlds of home and work.

Hochschild's initial *research question* - if, and how, family-friendly policies work - had to be abandoned immediately after beginning her fieldwork, as it turned out that these policies are unpopular. It was replaced then by a series of hypotheses explaining why this was so. The most original of them was the one exploring the reversal of home and work.

As I wrote in the chapter on theory, Hochschild does not reveal her *theoretical assumptions*. Judging by the way she collected and analysed her data however, we came to the conclusion that her main approach was interpretive.

Let us now consider various aspects of Hochschild's *sampling*:

(i) Case sampling. Hochschild admits that it was not her who picked Amerco as research object, it was Amerco management who approached her with this offer (xviii). At the beginning therefore we have opportunistic sampling. Hochschild gives a retrospective justification that Amerco was the best possible place to study family-friendly policies as it was ranked as one of leading companies in this field (2001: xvii).

(ii) Interviewee sampling. Hochschild interviewed 130 people across the company, top to bottom. She does not give details of the selection procedure, or of the representativeness of those sampled against the whole company (I mean here the relative number of sampled executives, managers, administratives, shop-floor workers). It is not said why she interviewed this number of people, not less or more. She does not provide us with the theoretical justification for conducting such comprehensive research:

'What was it in the lives of the families themselves, I asked, that made them complicit in the creation of their own time binds? To find answers, *I knew* I would have to explore work and family life from the top of the Amerco hierarchy to the bottom.' (2001: 52, italics mine).

As can be seen in the above quotation, Hochschild's sampling was driven more by intuition or experience. Using a well-known social-scientific category from 'grounded theory', we need to guess that she kept sampling until her research findings became 'theoretically saturated'. Additionally, we do not know how long the interviews were.

(iii) Non-participant observation sampling. Hochschild shadowed six families dawn-to-dusk. It is not said for how long however. Again, we can only commonsensically guess that she did it until patterns of routines become established and every new day of observation did not bring any new findings.

(iv) Sampling of 'stories'. Hochschild does not say why she decided to choose certain characters as principal characters of her book. In some cases she sampled subjects who were in a non-typical social situation (e.g. Becky, a single mother of two with no support from ex-husband, p.150n), but did not explain why she picked them.

(v) Sampling for the post-fieldwork survey. Hochschild wanted to know whether her findings could be justifiably generalised into the wider population. She carried out a survey outside Amerco. The population she targeted was however very specific: only middle and upper-middle class managers (parents of children in paid childcare centres). First of all, it was narrower than her original Amerco sample (where she sampled 'from factory floor to executive suite'), and therefore it constituted supportive evidence only for this smaller group. But more importantly, Amerco's employees themselves were not typical for the entire nation: they were either managers working for a corporation that was big enough to create a 'culture' for them, or manual factory workers. None of these groups is significant in size within the US economy, which in terms of labour is small-business and services dominated. Meanwhile, Hochschild triumphantly announced that 'the results of the survey confirm that much of what we have seen [at Amerco] is in fact happening across the nation' (2001: 201).

Hochschild's problem was that her original sampling (Amerco) was well-suited only to her initial research question (how family-friendly policies work). Once she discovered the phenomenon of 'reversed' worlds of home and work, she should have chosen another, broader sample for grounding her thesis. What she did was exactly the contrary – she chose a narrower sample.

7.1.2.2 Data collection

The report of data collection should provide the reader with the description of the manner in which research techniques planned in research design have been carried out. The logic is as follows: in research design, the researcher tells us *what* procedures she is going to employ and *why*. This lets us assess the validity of her findings. In data collection, she tells us *how* she implemented her plan. This in turn allows us to assess the reliability of her findings.

In Hochschild's case however this logic did not work in precisely this way, as her initial research design was very sketchy. She designed her research procedures in the course of data collection rather than before it. Therefore we learn what procedures were carried out and how they were carried out at the same moment rather than in two chronological stages.

By and large, Hochschild is conscientious about revealing the 'what' and 'how' of the research procedures leading to her findings. This effectively allows us to evaluate the validity and reliability of particular findings. For example, when she reports that her hypothesis explaining that people worked longer hours because they needed money turned out to be false, as 'those employees who earned more were less interested in part-time work than those who earned less' (2001: 28), the critic may argue that in this particular case applying statistical correlation as the research tool was not the best choice or even was a wrong choice. The latter would mean that the wrong procedure was carried out correctly - hence, the finding would be reliable but not valid.

Similarly, when at the end of her research Hochschild carried out an external test of her findings from Amerco (the test is described in detail), the critic may argue that she properly carried out a wrong procedure, since it is logically invalid to generalise her thesis into a wider population on the basis of a non-random sample. Again, this would mean that the result of Hochschild's test is reliable but not valid.

Sometimes Hochschild's report allows for a reverse kind of criticism: that she carried out proper procedures in an improper way. For example, at the beginning of the research she tended to ask her respondents closed questions, such as 'Are you working your 60-hour week because you're afraid of being laid off?'. It can be argued that interviewing subjects was the right technique, but that the questions should be asked in an open way, in this case 'Why are you working 60 hours per week?'. (The answer 'yes' to the first, closed question is less valid than if subjects answered the open question 'I am working long hours because I am afraid of being laid off'. Closed questions are suggestive, and additionally the positive answer may disguise that the subjects also have other reasons for working long hours).

Sometimes however she does not reveal her research procedures fully. An example of a missing 'how' of procedures can be found on page 32. Hochschild argues there that Amy Truett's explanation of why people do not apply for family-friendly schemes does not find empirical confirmation. She claims that progressive managers receive roughly the same amount of requests as resistant managers. But how did she operationalise 'progressive managers'? Did she rely on workers' opinions about which manager is progressive? It should be explicitly revealed.

In extreme cases, what is missing is both the 'what' and the 'how' of procedures, as in the following example. In the central chapter on 'reversal' of home and work she writes that 'Overall, this 'reversal' was a predominant pattern in about a fifth of Amerco families' (2001: 45), but does not say in which way she obtained this data (what sampling, scale and criteria of measurement she applied).

Finally, what is missing most often is the justification of the choice of procedures (the 'why' of procedures). Hochschild applied a wide range of research techniques: mainly interviews, but she also observed meetings, observed six families in an overt but non-participant manner, and accessed the company's internal 'climate' surveys. She does not discuss however why she used a particular technique rather than a different one. In particular, Hochschild does not justify why sometimes she opted for interviews and sometimes for participant observation, and why sometimes she asked her respondents open questions and sometimes closed ones.

But most importantly, Hochschild does not discuss why she decided to use *interpretive* data to answer some questions and *quantitative* data to answer others. It may be of no significance for the average reader, but from the point of view of philosophy of the social sciences it is important.

For example, she singled out a number of potential factors explaining why Amerco employees did not use family-friendly policies. Two of them were 'need of money' and 'layoff insecurity'. Regarding the former, she found that this explanation did not fit the facts: at the company she studied, the richer the employees were, the less interested they were in time at home. The poorer they were, the more they were interested. Therefore she offers statistical correlation as the evidence supporting the answer. When she discusses the other hypothesis however, she abandons this kind

of data in favour of qualitative one: 'Another explanation I tried out was that with all the downsizing going on, people are working very hard so that they don't get on the layoff list. But when I asked people, "Are you working your 60-hour week because you're afraid of being laid off?", they said, "No. I'm doing it because I love my work."' (as Hochschild herself quoted in an interview, 1997). Due to her writing gift the reading of Hochschild's report is fluent, but it is also very easy to take things for granted: why did she use statistical correlation in the first case rather than directly asking people, "Are you working your 60-hour week because you need money?" (which is what she did in the second case).

Let us now consider two key (from the point of view of a peer reviewer) aspects of Hochschild's data collection: 'getting along' and 'research position'. I have decided to leave out those elements that are less interesting, such as period of study, getting in, or research tools.

Getting along. The strength of the rapport established by the researcher with interviewees is one of the major determinants of the validity of findings. This is even more crucial in Hochschild's case as her research concerned people's emotions that are partly very private or intimate. Is it easy to honestly tell an anonymous interviewer that one is happier at work than at home? Or that one's family life is in a shambles, that one does not feel loved at home? Intuitively, we would think that to obtain frank answers to such sensitive questions the researcher has to be deeply trusted by respondents. On the other hand, we can imagine some respondents finding it easier to open up in front of a *stranger* than someone who is a friend, as the former guarantees anonymity.

This is of prime importance, but Hochschild neither takes a stance on this problem nor says anything about her relationship with the people under study. We can however infer that she preferred the route of winning trust of her respondents instead of purposefully staying emotionally distant. Using a literary style, she suggests that she did win the trust of some of her subjects, particularly those whom she followed dusk-dawn and who later became principal characters of the book:

'I found myself watching a small child creep into her mother's bed at dawn for an extra cuddle and snooze. Many times children approached me to locate a missing

button on a shirt or - more hopelessly for me - to play Super Mario Brothers on the Nintendo set, while a busy parent cooked dinner' (2001: 9).

But there are also some indications that Hochschild did not develop close enough rapport with most of the subjects to ask questions about deep personal feelings or to do a 'members' check'. This effectively decreases the reliability of her findings. Most importantly, several of her respondents were met only once and interviewed for a short period of time. About interviewing Bill, a top manager: 'Inside his office, he motions me to a chair, leans forward in his, and says, "I've set aside an hour for you" (2001: 55). Obviously, no close rapport can be forged over one hour. Therefore Hochschild often had to rely on her intuition or empathy. About Bill: 'Focused on performances [of his children] and emergencies [taking them to the doctors] - the best times and the worst, he said - he knew little about those times when his children were offstage, unable to get started on something, discouraged, or confused' (2001: 66). Now, the second part of the sentence is Hochschild's own insight - she did not hear it from her interviewee. She simply did not have enough of his trust.

Here is another fragment which unintentionally testifies to a superficial rapport: 'During several night shifts at an Amerco factory, tired workers patiently talked with me over coffee in the breakroom. One *even* took me to a local bar to meet her friends and relatives' (2001: 9, emphasis mine). This would not impress William Foote Whyte. But the clearest example in which Hochschild willy-nilly admits she did not win the confidence of her interviewees is this:

'Another young male manager who *had won the confidence of* a group of top executives "*leaked*" *this account* of a conversation about family-friendly policies: "The older guys had a meeting. They were asking themselves, "What's happening? Why are we being challenged?" They think that they're being criticised by the women. The way they managed their lives and the way they were brought up is being challenged. That is a major threat, and they won't tolerate it' (2001: 71, emphasis mine).

Now, in this particular case Hochschild managed to learn about her subjects from a second-hand account. But the question remains of how much other important data she missed when no-one 'leaked'.

The position of the researcher. Matching adequately the research position to the subject of study is not a research technique *per se*, but is at least as important as the right choice of technique. It can be said that different positions can be taken *within* each technique, and the choice is far from automatic. Examples of research positions are overt interviewer, covert observer, overt participant, overt observer etc., but also field researcher (research assistant) v research principal, independent researcher v collaborating with someone (e.g. the management – Hochschild's case). In short, *what* positioning is taken influences the validity of findings, *how* the right positioning is achieved and sustained influences the reliability.

Hochschild took an overt research position. Her respondents most likely knew that she was employed by Amerco's directors, which could influence their attitude to the disadvantage of the study. (Researcher as a 'spy'). Hochschild does not justify her choice of research position, or alternative positions. Are the readers in a position to evaluate if this position was right anyway? I suppose it is possible.

Firstly, could she conduct a covert study? This would mean that she had to get a job at Amerco to become one of its employees (a covert participant). Or, alternatively, she could have taken up another job in the town where Amerco was located and try to befriend people working for Amerco (a covert non-participant). Would that give her any *advantage* though? Potentially Hochschild 'an employee' could find out herself if what she called the 'engineered company culture' actually *was* there. She could also tell her manager that she wants to go part-time because she needs to take care of her ageing parents, and in this way check if managers are the 'bottleneck' of family-friendly policies. Taking a covert position would also endow Hochschild with more trust of the respondents. It does not mean that going overt automatically has no chance to be trusted, but it means that she has to put much more effort into winning it. (As I have already argued, there are signs that Hochschild did not do the maximum of this work).

But the *disadvantages* of going covert in her situation outweigh the advantages. Most basically, going covert was not a realistic option: it is likely that Hochschild did not have the skills to become an employee, or there were no vacancies, and she had other commitments (another job). Secondly, she would not be able to do the dusk-down shadowing of families if she was a covert researcher. She needed a legitimate reason for doing that. Thirdly, even if she had managed to become an employee and discovered that the company's 'managed culture' is a myth or that her manager was a 'bottleneck', all that would be just individual tests, and as such they could not be treated as any conclusive evidence.

But most importantly, the covert researcher loses the advantages of being the 'outsider inside'. Being overt means that the researcher is able to get access to the variety of positions and sub-groups of the structure under study. Such comprehensiveness is just the feature that is most striking in Hochschild's research strategy. She decided to study all of the essential segments of 'Amerco' – literally 'From Executive Suite to Factory Floor' – to find out answers to the research questions she posed. As I said earlier, Hochschild does not provide us with the theoretical justification for conducting such a comprehensive research, but is driven more by intuition or experience. ('I knew I would have to explore work and family life from the top of the Amerco hierarchy to the bottom', 2001: 52). Such theoretical argument does however exist. I believe that gaining knowledge of a certain fragment of social reality (be it a group, an organisation, an environment) that would go beyond the knowledge of the very subjects (and that therefore could justly lay claim to being called 'scientific' knowledge) requires the freedom of exploring the 'field', and that freedom is what the people in question are not granted. This is because they are bound by their positions/roles and by internal group relations.

The freedom mentioned above can only be the privilege of someone coming from the 'outside', who is granted the cognitive access to all crucial segments of the reality in question. To my mind, overt social researchers are privileged precisely in this way⁶⁹. It will sound banal, but no one in Amerco could have carried out Hochschild's research: just as somebody from the 'factory floor' does not possess cognitive access to managers from the 'executive suite', the latter also are not able

⁶⁹ And they may be the only ones. In the following chapter I will discuss the case of non-scientists, e.g. reporters.

to objectively learn about their employees, who must self-censor what they say to their bosses.

Also, there is another reason that makes the position of the overt researcher a privileged one over the covert researcher. What I mean is that the latter is anchored in the internal group structure of positions and roles, which inevitably entails having a certain perspective on the organisation under study, the result of which is that the knowledge about the group is distorted in a specific manner. Writing about Bill, a top manager responsible for the implementation of family-friendly policy in Amerco, Hochschild states: 'But two things kept Bill from acting on his understanding of the problem: [...] he lived in a social bubble among men who also worked very long hours, had (house)wives at home, and assumed the normality of this arrangement.' (2001: 62). Had Hochschild become an employee herself, she would have acquired the perspective of that particular position.

Summing up, I am saying that although Hochschild did not provide sufficient justification for it, she was positioned correctly for her research. This strengthens the validity of her findings.

7.1.2.3 Data Analysis: theorising the findings.

Hochschild does not use the term 'data analysis', and neither does she separate it as a phase of research. Therefore I am going to discuss it on the basis of what I take to be Hochschild's data analysis. In general, she is careful about showing her reasoning, particularly in chapter 3, where she discusses her explanatory hypotheses.

What does Hochschild do with her data? At the beginning of the research she took pains to collect evidence supporting or rejecting her research hypotheses. The data was not interpreted at this stage, it served the evidentiary, verificatory purposes. Later in the course of research Hochschild ran out of hypotheses and changed the strategy: she began 'listening' to the data out there. Approaching the problem with an open mind rather than pre-existing hypotheses proved fruitful as it led her to the discovery of the phenomenon of 'reversed' worlds of home and work. Therefore it is safe to say that she *interpreted* what she was told and what she saw.

But the main part of her data analysis was about *decontextualising* or *abstracting* this interpreted data. I mean that Hochschild took pains to extract certain similarities between people under study while leaving out what was particular, contextual. I believe that in this sense Hochschild tried to construct a *theory*. Finally, at the end of her data analysis Hochschild made an attempt at *generalising* her findings into the wider population. Here again, she was looking for evidence or verification.

Potential errors in Hochschild's data analysis. One of the problems with the 'reversal' thesis is that we do not know the scale of this phenomenon. Hochschild takes pains to convince readers that her finding is both common within Amerco and outside it. I will discuss it in this order.

(i) The problem with illegitimate internal validation.

I believe that the weakest aspect of the book is the explanation of *why* the subjects do not employ family-friendly policies. The author pushes the thesis that it is a result of reversing the essence of home and work. In Hochschild's interpretation, the causal nexus is: attractiveness of work + unattractiveness of home => long working hours/taking overtime => family crisis => further unattractiveness of home (vicious circle) (2001: 198). Hochschild arrived at her main finding after interviewing 130 employees across Amerco hierarchy. We do not have the basis for questioning the correctness of this sampling. Should we then admit that the 'reversal' thesis is internally valid?

I believe that this would be too sympathetic to Hochschild. Her sampling was correct, but her analysis of evidence was somewhat arbitrary. In particular, Hochschild treated her subjects' own understanding of why they work long hours in an arbitrary manner. If we look at their exemplary answer to the question "Are you working your 60-hour week because you're afraid of being laid off?" - "No. I'm doing it because I love my work" - we can see that Hochschild took the first part of it ('No') as truth, as a serious answer, while the second part ('I do it because I love my work') is ignored, is not treated as true or *serious* explanation. Had she treated it seriously, it would be seen as a causal factor in the overall theory. Also, she would not have put forward other hypothetical explanations such as 'ignorance of family-friendly

policies'. Hochschild does not however say why she sometimes relies on first-hand accounts (takes them as true) and sometimes does not take them into consideration (as if they were inadequate).

If we take a closer look at what her subjects said about their motivation for working long hours, we will see that the 'reversal' of home and work was not the only case, nor even the most frequent one. There are two different stories here, of managers and of shop-floor workers, but Hochschild ignored that they are different. As for the former, she unjustifiably ignored the 'competition between employees' as a factor explaining why people work long hours. It does not appear on her list of potential answers.

Interestingly, this element appears numerous times in her interviews with managers (2001: 74, 98, 100, 107, 129). The managers explained this as follows, e.g. Bill: 'We hire very good people with a strong work ethic to start with... People look around and see that. So then they work hard to try to keep up, and I don't think we can do anything about that...The environment is very competitive... We impose [long hours] on ourselves. We're our worst enemy' (2001: 56-7). The major causal nexus in the case of managers goes like this, in my mind: competition at work => long hours => family crisis => unattractiveness of home => attractiveness of work.

Other motivations for working long hours were reported by shop-floor workers, mainly money, but also the 'devil eye' (2001: 108) and 'long hour machismo' (ibid 128), but Hochschild eventually did not include them in explanatory scheme. For example Mario said: 'I work 50 percent for need, 25 percent for greed. A lot of it is greed. And 25 percent is getting away from the house' (2001: 179).

Summing up, Hochschild's data analysis seems to be incorrect with respect to the internal validation. Additionally, her evidence suggests that the 'reversal' of home and work is felt more by women than men, as men tend to help less at home (2001: 38). Hochschild says she wanted to write a non-gendered book, but gender *does* matter. Another point is that perhaps home has *always* been work! Can Hochschild prove that the situation was different in the past? Of course most women did not have jobs outside the house then - but they had more children on average than today. I believe parents have always been busy at home.

(ii) The problem with illegitimate external generalisation.

Is Hochschild's main finding valid externally? She decided to carry out a survey outside Amerco to generalise this finding into the wider society. There are numerous predicaments with this decision. I have already mentioned some of them in the section on sampling, particularly that the population she targeted was more specific than Amerco's, instead of being less specific. But apart from this, there is a logical issue here: even a significant number of positive tests is not sufficient for final validation, let alone a single test. Therefore Hochschild's data analysis was too simple and too uncritical here.

7.2 Baumann: *Contesting culture*

Baumann devoted the first nine pages of his book to the description of the research process. The following notes are primarily based on this part, but also on other methodological remarks scattered across the book.

7.2.1 Reporting evidence

Usually Baumann supports his claims with good evidence, in the form of verbatim quotations, as in the following example:

‘Yet while Irish culture is recognized as a heritage, an Irish local community can hardly be said to exist. Even the key organiser of Irish events admitted to McGarry: ‘Our social life here in Southall has gone dead, you see, because we haven’t got – the community that’s interested in the social life we do put on. – We put on dances, we’ve done everything we can, but it doesn’t seem to take. – It’s just you haven’t got the community’ (Baumann 1996: 92).

Or, to support his claim that culture is conceptualised in the ‘dominant discourse’ manner by Southall children and youth, he provides the reader with several questionnaire excerpts, such as ‘My culture is religion. follow the rules.’, or ‘Culture = This means religion or some sort of club, cast, different from another’ (1996: 101, misspelling original).

On a few occasions, evidence is provided but is not too compelling. For example, the author writes:

‘Unlike the public arena of the Broadway, the sidestreets are considered a private space, the pavements almost part of the residents’ property. (...) In mine, ten seconds’ walk from the Broadway throng, a neighbour and I, working on my car, were passed by two young men, busily engaged in a noisy conversation. One of them used a common expletive. In an instant, my neighbour turned, raised himself, and hollered: *Watch your language, boys!*’ (1996: 45).

This situation can however be interpreted in other ways than Baumann’s. It does not necessarily mean that Southallians consider sidestreets to be their private space. For instance, perhaps South Asians do not like others to swear in public.

I have not found any examples of *counter-instances* in the book. There are however at least a few examples where Baumann tries to explain someone's conduct but does not provide *any evidence* for that – and therefore we do not know whether he is only guessing or whether he got those data from in-depth interviews. An example from page 66:

'I have often marvelled at the painstaking and ceaseless toil that some community leaders are prepared to shoulder beside their full-time jobs. Given the competition and scarcity often spell failure rather than success at securing resources, why do they continue? The answer is threefold. Community leaders are often motivated by a strong moral sense of justice for, and service to, their claimed constituencies; they are able to function because the political establishment has co-opted them as representatives of their communities; and they gain from their effort access to more desirable social networks and the respect or gratitude of those they have served' (1996: 66).

If these insights are based on community leaders' own accounts, why did Baumann not state it explicitly? But if these are only guesses, it is puzzling why Baumann did not directly ask his subjects or at least carry out a 'member check' of his interpretations. Maybe he did not have enough trust from the community leaders to hope for sincere answers?

Even if the lack of trust had been the reason behind this particular problem, it definitely was not in the case of finding Southallians' motives behind their desire to 'move up the road'. Yet again Baumann does not discuss where he got the evidence from:

'The widespread desire to move out of 'tatty' Southall and 'live up the road' may thus be interpreted in more ways than class advancement alone. It may, of course, promise an escape from the town's low rank in the pervasive class hierarchy among whole suburbs, conditioned in part at least by racist equations of class with colour of skin. It may be an escape, further, from neighbours deemed alien by whatever difference of cultural heritage is recognised by householders themselves; but it may also promise escape from the often strict censure of those of the same categorised background. All three interpretations could easily be linked. Each could be seen as an escape from being stereotyped: be it by life in an immigrant 'ghetto', by co-residence with subjectively alien minorities, or by the social control of 'one's own folks' with whom one may share a heritage, but sometimes little more. It is possible, indeed, to surmise that the oft-desired privacy afforded by 'moving up the road' is valued as a kind of household autonomy that life in the central wards of Southall seldom affords' (1996: 46).

On this note, it can even be argued that Baumann does not have the evidence for his central interpretation! He dwells upon the causes of the persistence of the dominant discourse in Southall, and gives the following explanation:

‘The least tribalist way to explain why Southallians [engage the dominant discourse], still seems to me to lie in what other Southallians do. If one community finds that playing the dominant game of reified cultures will gain it resources and respect, any other community would be foolish to opt out of the dominant rules. There is, usually, little point in engaging the demotic discourse when facing resource competition predicated on the dominant one.’ (1996: 193)

Unfortunately, Baumann does not justify why he preferred to guess the reasons for engaging the dominant discourse rather than enquiring his subjects directly⁷⁰. Perhaps he had a valid reason for doing things this way: maybe he thought that his subjects act without deeper reflection on their motives. But since he does not give us a clue, we are forced to take into consideration a possibility unsympathetic to Baumann - that his respondents did not trust him enough to be honest about something that was to their disadvantage, and hence he was worried that they would try to ‘whiten’ their motives.

But cases of weak support or no support are only one problem with the validity of Baumann’s claims. What is perhaps even more troubling is the extent to which he uses second-hand evidence (i.e. data from someone else’s research on Southall): in some chapters second-hand quotations and examples dominate Baumann’s own fieldwork evidence! For example, in the section ‘Whites and three strategies in the absence of community’ (1996: 134n), on ten pages of text there are two quotations from a local newspaper, ten quotations from or references to ‘Hawkes 1990’, nine quotations from or references to ‘McGarry 1990’, and no citations from Baumann’s own research. This is a serious blow to the validity of his claims – as the readers are not in the position to evaluate the quality of the source data.

⁷⁰ If we apply our insight from the chapter on theory here, we would say that the above explanation is only a homunculus of Southallians, a heuristic device. For some reason Baumann did not go beyond that.

7.2.2 Reporting research

7.2.2.1 Research design

The context of beginning. Baumann claims that personal motives were behind his involvement and interest in the research (1996: 1). In particular, having himself been an immigrant in the UK, he experienced personally the shallowness of the 'dominant discourse'. Baumann does not reflect however on the way this experience could have influenced his research findings.

Research Questions. The overarching question was 'What is the experience and use of culture like in multi-ethnic locations?' (1996: 31). The main sub-questions were: does the equation 'culture-community-ethnicity' - formulated in the political context - find confirmation in the actual world? What is the role of context and contingency in peoples' views of culture and community? Are immigrants suspended between two cultures or reaching across them? Which two cultures are we talking about?

Phases of research. The research took place in three stages. Phase one focused on young Southallians (the second generations of immigrants). The following stage targeted adults (the first generation of immigrants). In phase three Baumann and his assistants carried out a quantitative check up of qualitative insights previously gathered (to confirm or contradict the results).

He is conscientious in providing the readers with justifications for arranging the two first phases of research in this particular way. The main weakness of Baumann's research strategy is his rationale behind carrying out quantitative 'verification' of qualitative data. Quantitative research can only help generalising qualitative findings but cannot 'confirm or contradict' them.

Sampling. Here we can consider case sampling and sampling of informants:

(i) Case sampling

Baumann says the following about his decision to study Southall: 'It was Adam Kuper who, knowing my intention to 'work among (other) immigrants' pointed me to

Southall and suggested that I move there. I might have settled for something less complicated and quicker without him' (1996: xi). In this paragraph we are told, on the one hand, that the sampling was opportunistic as it was not Baumann himself who decided that Southall is a good place to study his research problem, but on the other hand that the case was sampled in a justified way ('The location was suggested by Kuper, an anthropological expert' and 'Southall is not an easy place to study').

(ii) Sampling of informants.

As it was mentioned before, Baumann justified his decision to study young Southallians in its first stage and adults in the second stage. When it comes to the way he sampled particular individuals, Bauman is however less overt. He only says that he took up auxiliary teaching in a Southall school (1996: 4), which lets us guess that he met his teenage informants through this school. Later on he used the 'snowball': 'Young Southallians invited me to meet their parents, neighbours invited me to weddings and birthday parties, and I pursued the adult contacts I had made in community centres and temples, churches and schools, tandoori shops and pubs' (1996: 4). We do not know how many informants Baumann had in total.

7.2.2.2 Data collection

Period of study. Baumann lived six years in the centre of Southall (1996: 2), which in ethnographical terms translates into very high validity of his insights.

Getting In. Baumann had no problem with gatekeepers as it was fine to just rent a house in Southall. Having said that, he must have met with a lot of suspicion from his neighbours as he was a white, non-working class foreigner. He does not say how he dealt with that or what impact it could have on his data.

Research techniques. Baumann primarily used participant observation with the emphasis on informal interviews. His secondary source of data was a questionnaire he carried out as a joint project with another researcher, who was studying uses and views of the media in Southall. From a technical point of view this was not a good idea - respondents could be potentially confused regarding what they were being asked about, plus the questionnaire was far too long for young respondents - ninety-odd detailed questions (1996: 7).

Research procedures are usually well-described, which enhances the reliability⁷¹. For example, Baumann writes that 'Our combined questionnaire made a point of asking locally relevant questions in local terms, language, and style. It was piloted several times, and in the end we managed to administer it to some 350 young Southallians. This bulky survey of ninety-odd detailed questions (...)' (1996: 7).

Data recording is not discussed explicitly. Only at one point in the book do we learn about it, when Baumann cites a conversation in a pub, and says: 'Hurrying to the toilet to take my notes, I wondered if (...)' (1996: 65). There is no indication if he tape-recorded interviews.

Tools for enhancing validity. Baumann addressed the issue of validity by using two kinds of triangulation. Firstly, he employed *multiple methods* – qualitative and quantitative. Secondly, the data collection phase was carried out by *multiple independent investigators* - Baumann himself and his research assistants (trained students). These kinds of triangulation enhanced the validity of the findings.

Regarding the 'search for negative cases', Baumann does not say if he actively tried to find examples denying his thesis. Rather, he writes that 'I did find a few people who said *I am a Muslim and nothing else, I am a Christian and have no other community, or I am an African from the Caribbean, but as African as the people born in Africa*'. I have tried not to discount their positions in the body of the book, and have mentioned them wherever the ethnographic context allowed it' (1996: 5). In my view however, these voices effectively get lost.

The position of the researcher. Baumann took the position of overt participant-observer. I suppose the element of participation was intended to bring knowledge of local issues and to achieve the subjects' trust. There was not too much 'observation' involved as Baumann did not research any rituals or collective actions.

⁷¹ With the exception of cases when he is using an ironic tone when discussing procedures. For example: 'One of our joint madneses, a qualitatively conceived mass survey among young Southallians, came near to spelling disaster: 350 young people volunteered 5 million bytes of data' (1996: xii). It is difficult to guess whether he is critical about this procedure or actually proud of it.

Getting along. Baumann does not state precisely what level of trust he managed to achieve with his informants. He convinces the readers that he was trusted in an indirect way, by establishing ethnographic authority. To endow himself with such authority, Baumann applies the favourite literary trick of ethnographic researchers:

‘Fieldwork and the curiosity for local knowledge began to imprint their own stamp on my daily routines. Gradually, the “Railway Tavern” turned into a living-room away from home, and the grotty living-room of my house into a place where Narinder and Balbin, Joshua, Sukhbir, and Syd would drop in to have an illicit cigarette after school or a drink after work’ (1996: 2).

Research problems. Baumann reports two mistakes committed during the research; one technical and one practical. On page 151 there is a table, underneath which we can read that ‘an error in manual keying-in of the data [has been made] which we have not been able to correct.’ The error is however of next to no importance. The practical error is mentioned on page 4: ‘my lessons in Punjabi and Urdu had borne little fruit’, but Baumann immediately justifies why it was not important: ‘most adults mastered English with a fluency I would never achieve in any of the ‘community languages’, and I therefore changed my language strategy to learning extensive glossaries of vernacular key words that had no easy equivalent in English’. He does not mention any problems with interpretation.

7.2.2.3 Data analysis

Baumann’s data analysis consists to a large extent in *describing* cultural fabric and community processes in Southall. When possible, his descriptions are *abstract*, that is decontextualised (freed from particular, contingent elements). A good example of an abstract descriptive claim is ‘Southallians possess dual discursive competence’ (1996: 144). But his argument is also theoretical in the sense that it *explains* why Southallians sometimes engage the dominant discourse.

7.3 Wagner: *Checkerboard Square*

This book does not contain an awful lot of information about the methodological side of the research - five pages in total. I do not think this is due to the author's low methodological awareness. I suppose that it is due to the fact that the book is a published work targeting a relatively wide audience rather than a PhD thesis fated to non-existence on libraries' book-shelves.

7.3.1 Reporting evidence

Generally Wagner provides the reader with abundant evidence. He usually brings up particular cases and names of people who were interviewed, as in the following fragment:

'Subjects [in their childhood] were abused by natural parents, siblings, mother's boyfriends, uncles, aunts, cousins, and foster and adoptive parents. Judge, like many other subjects, told the interviewer horrifying stories. Judge was beaten so badly as an infant that he developed a permanent spinal and lung problem called Hylien's membrane, which nearly cost him life. Ron was one of several subjects who was locked in a cellar and abandoned for weeks, in his case when he was 4 years old. Roy was set on fire by his father during an argument when he was 8 years old. Most of the subjects who were abused ended in the child welfare system by their teen years, usually suffering additional abuse in foster homes' (1993: 47).

Wagner often brings quotations from interviews and conversations:

'Negative experiences with family and with adult relationships influenced subjects' decisions on 'travelling light' - not only with a few possessions but also without a partner to weigh one down. Such orientations appeared among street people of varying ages:

Ruth, age 23: My problem really was I always wanted to be with somebody. ... [Now] I want to be by myself. ... Look what happened to me with men. ... I don't trust anyone to be with me.

Lorraine, age 40: I got married 3 weeks after meeting my husband. Now I can give you 100 reasons not to get married... but I see [relationships of the past] as extremely bad for me.

Tiny, age 62: Remarriage? Wow. ... No, I have a lot of other things to worry about, like just surviving and getting enough to eat. ... I'm long beyond marriage material ... this I'm sure [of]' (1993: 62)

Sporadically, Wagner is rather impressionistic or vague when he says, for example, 'I often felt that...' (1993: 84). Also, some of his interpretations are unavoidably personal, as in the following example:

'Several family members of study subjects I interviewed generally offered *frustrated* and *hostile* perspectives of their kin [emphasis mine - FS]. Louie, the 40-year-old son of Wally - a 77-year-old homeless - told me: *We [the family] did everything we could to help him [Wally]. He just didn't want our help. [We] even got him this room once, but he didn't like it, and when he found out he lost his food stamps, he was furious. He was just no good*'. (1993: 46)

I am not sure whether on the basis of this quotation one can endorse Wagner's interpretation that families were 'frustrated' and 'hostile'. Probably he could read these feelings from interviewees' body language and tone more than from their words.

Finally, Wagner's main thesis about the 'culture of resistance' is well supported.

7.3.2 Reporting research

7.3.2.1 Research design

Research Questions. Wagner explicitly states what questions drove his research (about subjects' lives, how they became homeless, how long they were homeless, how they coped with everyday difficulties, etc). He was well aware of what he wanted to study and what was outside his interest. As for the latter, Wagner particularly did not see the point in sharing experiences with his subjects, in the sense that he did not sleep under the bridge or eat what they ate.

Phases of research. Wagner's study consisted of two phases. First, he prepared the list of 65 individuals who had been homeless for the previous three years, and then conducted a set of interviews (in 1990). After completing this phase of research, he realised that interviews – although being the main method used - were not sufficient: 'It had become obvious that the first phase of research had resulted in many contacts and insights but that individual interviews were limited in what they could reveal about group interaction, about the nature of conversation and of truths that

were known among the whole community, and about trends within the community that personal accounts might deny or obscure' (1993: 37). Therefore he and his assistants set off to carry out participant observation.

Summing up, Wagner's research design was flexible, not pre-determined - which is not surprising in the case of a qualitative study - but he gives a good justification for his decisions.

Sampling. There are two issues here:

Sampling of subjects. The research began, as I mentioned, with the preparation of the list of subjects. To establish who had been homeless over the period of at least a few years, Wagner used the record of participants of homeless people's city protest in 1987. (Almost all of the homeless people of this town took part in the protest). Out of 110 individuals, three years later he managed to find 65. Therefore, his sample should be seen as internally representative - more than half of the cohort was subjected to study⁷².

Wagner also mentions two key informants, with whom 'extended conversations were invaluable' (1993: 38). He does not however say in detail who these informants were, if they were typical of the population, or in what way were they more knowledgeable than the rest of the community.

Case sampling. Wagner does not justify his choice of 'North City' for the location of the study. He is aware that its homeless population differed from the national 'average' in that Black and Hispanic people were underrepresented. Hence he admits that the issue of *external* representativeness (generalisability) of his study is open (1993: 38-9). It is surprising that despite that he says that 'there is every reason to believe that samples including more Afro-Americans or Latinos would likely share more rather than less of the culture of resistance discussed in the book' (1993: 39).

Research assumptions (epistemological reflexivity). Wagner's epistemological reflexivity (i.e. a reflexivity about the role researcher's social and biographical

⁷² We do not know, however, if all of those individuals were valuable informants - the fact that some interviews lasted only 45 minutes suggests that some of them did not bring anything to the study.

background and assumptions may influence research results) is centred on the issue of value-freedom. The author is very careful about separating the data from his moral views. He devoted a specific section at the end of the book to practical suggestions, which he overtly says are influenced by his personal political views, but throughout the book he stays neutral. Wagner also says that 'Our data cannot disprove either conservative or liberal positions [on homelessness – F.S.] because they relate essentially to a moral-religious level of meaning' (1993: 91).

7.3.2.2 Data collection

Duration of study. It is confusing how long the research actually lasted: once Wagner mentions 'between the fall of 1990 and the end of summer of 1991' (1993: 37), but later he talks about '2 years spent observing people' (ibid 38). This duration endows his findings with average validity.

Getting In. Wagner does not say anything detailed about getting access to the community. He writes: "Katherine" and "Mitch" served as the primary contacts with the homeless people of "North City" in 1990 and helped me gain initial access to the people of Checkerboard Square' (1993: xi).

Data recording. It is a bit confusing what happened with data recording in the first phase. Wagner writes that 'field notes were not systematically recorded in phase one', but what field notes does he have in mind? Does he mean interview records or rather notes of circumstances in which interviews took place? Proper fieldnotes were developed in phase two, however.

Research techniques. Wagner satisfactorily justifies the choice of qualitative research as the general framework. He also justifies participant observation as the proper technique for the second phase of research (1993: 37, cited above). He is therefore good at informing the reader about the 'what' and the 'why' of procedures. He neglects however the description of research procedures, the 'how' of them. All we know is that he conducted in-depth interviews, lasting between 45min and 5 hours (1993: 36), participant observation (limited to day-time interaction), and that he employed research assistants ('multiple investigators' kind of triangulation). Particular fieldwork decisions and arrangements are not talked over in detail.

Only sporadically does Wagner reveal details of his research procedures, as in the following example:

'Since we asked about [family] abuse only in a general way ("Were there problems in your family when you were growing up?"), I can only assume that more subjects may have been abused than the more than half who described severe physical and sexual abuse'. (1993: 47, emphasis mine)

Wagner does not mention searching for negative cases or carrying out a 'members' check'.

The position of the researcher. Wagner took the position of overt interviewer and overt participant observer. He does not discuss alternatives. It seems that for practical reasons alone he could not take the covert position. Additionally, thanks to this overt-ness he had access to all subcultures within the homeless community.

Getting along. The issue of subjects' trust, so central to the assessment of validity of qualitative research, is not properly discussed by the author. The research report does not really allow for the examination of the depth of rapport established by Wagner (and by his assistants) with his informants. He only writes that "Katherine" and "Mitch" [primary contacts] enabled members of the research team to gain the trust of street people' (1993: xi).

Research problems. The reader's impression is that the author did not encounter problems in his research, particularly problems with interpretation. Three possibilities arise: either Wagner's research was really unproblematic, or he disguised research problems, or the non-chronological structure of the report is to be blamed for this impression. When research results are structured thematically and not chronologically, i.e. as the fieldwork unfolded, fitting decision-making into the report may be awkward.

7.3.2.3 Data analysis

Wagner does not use the term 'data analysis'; and neither does he distinguish it as a separate phase of research. What Wagner mostly did with his data was de-contextualising what subjects told him and what he witnessed. For example he

renders their activities as 'exercise in self-preservation', 'resistance to dominant social norms', or maximising income while retaining some autonomy'. His research ultimately does not go beyond subjects' knowledge about themselves, but it approximates the group's daily life to other audiences very well.

7.4 Conclusion

Let us reflect, then, on the extent to which methodological scientificity is achieved in these three studies.

On the issue of reporting evidence, all of the writers put forward a good quantity of evidence to back up their claims. However, there were specific weaknesses to be found. Hochschild did not take counter-evidence seriously enough, and also did not present enough evidence to back up the scope of her claims. Baumann sometimes does not report where his evidence for certain claims comes from, and also, at times, relies on second-hand evidence. Wagner was generally strong, although occasionally his interpretations were impressionistic or vague. Overall, we might say that evidence was in good supply, despite some limitations. This places the studies considered part of the way towards methodological scientificity.

On the other main issue, reporting research, the picture is a little more complex, partly because of the range of elements that fall into this category. Starting with Hochschild, there were certainly issues with her sampling, especially with her shift from Amerco to other companies. In terms of justifying which procedures were chosen, Hochschild's work was deficient. Hochschild also did not extensively discuss the research position chosen, although I argued that her ultimate decision was justified. Finally, I argued that internal validity was questionable, given the evidence she presented.

Baumann's main strength is unquestionably the exceptional length of his study (six years). His sampling and research position were correct. Research procedures are usually well described. Baumann also employed triangulation of investigators. What is doubtful about his research procedures is the level of trust from his subjects.

Finally, Wagner's study exhibits good within-case sampling but suffers from insufficient case sampling. Hence, the representativeness of his research is not too compelling. To his advantage, research procedures are usually well justified. What is disappointing is the lack of detailed description of how the procedures were carried out. Also, we do not know how much he was trusted by the subjects.

Overall, reporting research is less satisfactory than reporting evidence. None of the researchers managed to score really high points here. There seem to be no pattern concerning difficulties – no single aspect of research can be pointed at as causing particular problems to all researchers.

Chapter 8

Interpretive Social Science and Non-fiction Arts

Theoretical and practical problems with the ‘scientificity’ of interpretive research, discussed in the preceding two chapters, unavoidably raise the question about the differences between them and other human activities that are not regarded as scientific. As we have seen, scientificity frequently fails in *practice* – in which case we should not grant ‘scientific’ findings more value than findings achieved in other ways. Secondly, a lack of codification of research procedures suggests that non-scientific researchers perhaps carry out exactly the same actions as their scientific counter-partners but in a ‘natural’, intuitive way.

These doubts seem to find confirmation in the fact that non-scientific researchers of the social world, such as documentary filmmakers, literary reporters and other non-fiction artists, often produce brilliant results. Does this mean that there is no real difference in value between interpretive social research and insights produced by non-fiction artists, such as documentary filmmakers and literary reportage writers? To compare their value, we need to compare their research methods. Are they different to social-scientific methods? Are artistic findings less valid and less reliable than social-scientific interpretive findings? These are the main questions of the current chapter.

8.1 Comparing ‘non-fiction art’ and the interpretive social sciences

8.1.1 ‘Non-fiction arts’ defined against other enterprises

Non-fiction arts, as the name speaks for itself, belong to two categories at the same time. On the one hand, non-fiction arts share with fictional arts the essential feature of having the *artistic character*. Whether we think it is right or not, works of non-

fiction arts are assessed first and foremost by their artistic value, rather than by how accurately they represent the world.

On the other hand, they belong to a group of enterprises that are concerned with the enquiry into, and the representation of, the *actual* social world. As such, they are different from *fictional* arts⁷³ and similar to at least four other areas where the actual social world is the subject of enquiry and representation: social sciences; journalism; business/economy; and government/politics. Following Bill Nichols (2001: 39), I will call these domains 'discourses of sobriety'. This name is useful as it separates them from fictional art, poetry and other imaginary representations of actual and fictional worlds. This set is not homogenous by any means. There are several connections, similarities and differences between each discourse:

Social sciences, business and government all use social-scientific research methods. They do it for different purposes though: social sciences seek general knowledge, while business and government are always interested in knowledge that could be *applied* to current situations (in the form of actions and policies). They actively shape the world. Application is only secondary in the social sciences. Further, government/politics often actively *take sides* in public discussions on contested values, actions and choices. Social sciences, at least most of the time, do not contain argumentation of moral character.

But how about journalism? In a manner similar to business/economy and government/politics, journalism is focused on current social issues and problems. The enforced conciseness of the message implies, however, that these issues are not described, explained and discussed in length and complexity. Additionally, the character of the journalistic job and the lack of quality control mean that usually research is not as thorough as in other cases discussed here. The main difference between journalism and social sciences lies therefore along methodological lines (the reliability of findings in particular). Like all other discourses journalism can be engaged, but (similarly to social sciences) does not directly act in the manner of

⁷³ Having said that, it needs to be emphasised that some fictional works of art attempt to lay claims about the actual world by using fictional characters and events. (See for example Schrader's *Blue Collar* or Kieslowski's *Short film about killing*, or books such as Böll's *The Clown*). Also, it happens that authors of such works spend a lot of time and effort researching the actual world. I believe this area highly deserves to be researched and written about. It goes beyond the scope of my thesis though.

business or government. On the other hand, journalism often takes up moral standpoints, similarly to government/politics and unlike social sciences.

Finally, how does non-fiction art relate to other discourses? It is probably the most diverse one of all of them: it can concern current issues or past issues, public problems or private life, it can be a search of knowledge for itself or of applicable knowledge. It can be as thorough as social-scientific research or less thorough, similar to journalistic investigation. Now, where it is an unengaged study of everyday life of ordinary people *from their point of view*, non-fiction art seems to be very close to interpretive social sciences (much closer than other 'discourses of sobriety'). Hence the difference between the two is most problematic and worth being studied.

I am going to begin with section 8.1 about features that define the character of non-fiction art, but that do not concern reliability and validity of findings. Having completed the definitional task, in section 8.2 I will subsequently discuss research methods used by non-fiction artists and conventions used in 'research reporting'; this discussion will help us find an answer to the question of inferiority of artistic findings against social-scientific ones.

8.1.2 'Non-fiction arts': the sub-genres

What genres belong to the class of 'non-fiction arts'? Roughly, we can talk about written media and visual media. In the former, we have genres such as literary reportage, travelogue, and essay. Visual media include documentary photography and documentary film. These names sound rather straightforward. But actually anyone who writes about non-fiction arts may find herself in the situation when she is saying 'This is a piece of literary reportage', and someone else is objecting 'No, this is a piece of anthropology' or an 'essay'. She faces a core definitional problem and needs to find a way out of it.

This sounds all too familiar to social scientists. What is 'sociology'? What is the difference between sociology and anthropology? Such questions have direct analogues in non-fiction art: what is 'literary reportage'? What is the difference between literary reportage, travelogue and essay? What is the difference between documentary film and feature film?

All attempts at defining these enterprises have basically failed. Short definitions are too inclusive or too exclusive at the same time. For example a claim that the aim of sociology is to correct and develop concepts used by the actors themselves (Giddens) excludes some research that would be called 'sociological', but it applies well to many documentary films and literary reportage writings. Similarly, to say that 'The aesthetic of representation is what distinguishes social sciences from non-fiction art' leaves out numerous important differences and underestimates the high aesthetic values of some social-scientific books.

The failure of the definitional effort indicates that the enterprises we are trying to define have no single substantial features. It only confirms that social sciences and non-fiction arts, and perhaps most of other human enterprises, are characterised by their fluid nature (they change in time), by internal differentiation and by divergence of 'theory' and 'practice' (practitioners do things their own way and are often innovative).

But does this mean that we have to give up our attempts and admit that there is no real difference between interpretive social sciences and non-fiction art? I believe this would be wrong. Even if sometimes we dispute which name to use, in most cases we do not hesitate whether a given piece of work is literary reportage, a sociological research report etc. This suggests that there exists 'family resemblance' rather than core characteristics of these genres.

Now, how to explore this family resemblance? We need to look at actual works and try to distil those features that occur often. We face an awkward predicament (*petitio principii* in fact), however: if we select a work to look at, it means that we have already classified it as an exemplar of a certain class. But this is exactly what we are not allowed to do before we know what the criteria of classification are!

There is no good way out of this circle. I believe that the least bad solution is to apply the *institutional criterion*, according to which we are entitled to qualify a given work as e.g. 'reportage' when it is described as 'reportage' in reviews or when its author is dubbed 'the king of reportage.' It is similar for social sciences, i.e. an

ethnography is something called so by a legitimate creator/institution⁷⁴. Once we have selected some works as representative, we are able to look for 'families' of features characteristic for each genre. The institutional criterion has therefore been used in this dissertation.

In the following two sub-sections I shall discuss the character of literary genres (such as literary reportage and travelogue) and documentary filmmaking.

8.1.3 Literary non-fiction genres

Within the literary non-fiction domain, there are three genres that are of particular interest for us: literary reportage, travelogue, and essay. Here I am going to discuss, firstly, those features that are common to them; secondly, the differences between them; and finally, I am going to compare these literary genres with interpretive research reports. Specifically, I focus on the activities of the author, her position in the book, the style she uses, etc.

8.1.3.1 Non-fiction literary genres in their similarities

Two features are shared by non-fiction genres. First of all, literary reporters, travelogue writers and essayists *travel* to a certain place or places with the aim of learning and experiencing *first-hand*. Such a journey, importantly, is anything but tourism: it has research as its aim. In Kapuscinski's words:

'As for me, the most precious journey is the reporter's one (...), which aims at bettering one's knowledge of the world, of history, of changes taking place, and then disseminating this knowledge. Such a journey demands concentration and attention, but thanks to them I can better understand the world and mechanisms in it'. (Kapuscinski 2003: 12)

'In a reporter's journey there is no room for tourism whatsoever. This journey demands hard work and huge theoretical preparations. Learning about the region one is travelling to. Such a journey does not know any relaxation' (ibid 13)

⁷⁴ Problems however remain even with this criterion. What if there is no institutional agreement, as happens with some 'not-so-documentary' films? Or if somebody called 'the king of reportage' (Kapuscinski) not only dissociates himself from another author described as 'the founding father of reportage' (E.E. Kisch), but sporadically calls his studies 'ethnographical, anthropological'?

Secondly, they are interested in *everyday life* of ordinary people. In particular, literary reportage (and documentary films), unlike most fictional literature/films, strives to connect the ordinary point of view with broader social phenomena. Interpretive social science does the same. Hence we can say that non-fiction art is characterised by the 'sociological imagination', that is by "...the capacity to range from the most impersonal and remote transformations to the most intimate features of the human self - and to see the relations between the two" (Mills 1959: 14)... "The sociological imagination enables its possessor to understand the larger historical scene in terms of its meaning for the inner life and the external career of a variety of individuals" (ibid 12)⁷⁵.

This feature should not be obscured by the fact that literary reporters usually go where something important is happening (social changes, conflicts, etc.), as they are still interested in the way ordinary people cause, experience, and suffer from the consequences of important events. Kapuscinski says: 'More than in revolution I am interested in what happened before revolution; more than in frontline – what is going on behind it; more than in war – what is going to happen after the war' (2003: 117). This very much likens her to the social scientist and distinguishes her from the press journalist⁷⁶.

Ordinary people are the object of the literary reporter's interest not only because important events have impact on their lives (which is often left out by press journalism), and not only because ordinary people's view of events may differ from the view of 'key' actors, but also because literary reporters see ordinary people (in

⁷⁵ Contrary to that, literature represents by and large the protagonist's mental changes, resulting from her interactions with other people and from random events having impact upon her life. ('The true novelists are creators of characters' – 'The Art of Literature', *Encyclopaedia Britannica* p.127).

⁷⁶ In newspapers there is only enough room for describing events, but not for the comprehensive picture or analysis: '...all of a sudden we see people dying, some tank burning – but we don't know what's happening around' (Kapuscinski 2003: 110). Kapuscinski writes the following about his work for a press agency and his motivation to choose literary reportage: 'On the one hand, travelling from one country to another, from one continent to another – I was discovering a rich, fascinating world that the day before I had not been aware of or even I had not had an inkling of, and on the other hand – the means of communication I had, which is press telegram, inevitably was such a superficial and shallow shortcut that all the richness, otherness and completeness of this other world was lost. Now, it is just this feeling of insufficiency, flaw and banality of the press agency journalism that pushed me to start writing books. Each of them is some second volume of a certain entity, of which the first volume is my telegrams from Asia, Africa and Latin America, stored somewhere in the Polish Press Agency archives' (2003: 30).

particular their culture and mentality) as the indirect but very much real *cause* of important events. Kapuscinski again:

'We can describe one more coup d'etat, rebellion, one more spectacular event, but it all repeats itself and explains nothing; we should go deeper instead, to the causes, and the causes are of a cultural nature' (2003: 117).

8.1.3.2 Internal differences between non-fiction literary genres

Literary reporters and essayists want to research a concrete subject. The elements of their works are bound by the logic of the exploration of this subject. The subject is more important than the author; often the latter is completely hidden. Travelogue writers, by contrast, do not have a subject of research as such. They learn and experience as their journey unfolds. They are open to what the journey brings. This is why the author is most often present in the events described; she is central. Travelogue is animated by what happened to the traveller, literary reportage and essay are perpetuated by the exploration of the subject. Two examples, one of literary reportage and one of travelogue:

'The Georgian likes his day of work and wants it to be closed with a cheerful accent. To let some ceremonial moment endow that day with value and preserve it in memory. No day will be repeated twice and that is why each of them should be separately celebrated, treated as an event. So the Georgian gives himself fully to this rite. He cleanses himself, finds his completeness. The feast is nothing like gulp, feed, gobble. One doesn't come here to black out, to get smashed. The Georgian despises heavy drinking, hates drinking for the quantity's sake. The table is only a pretext, tasty and full of wine, but nothing more than a pretext.
An opportunity to celebrate life.'

(Ryszard Kapuscinski, *Invitation to Georgia*, 1990a: 19)

'Travnik, Bosnia, 4 July:

This morning, brilliant sunshine, heat; I went up to the hills to draw. Marguerites, young wheat, serene shadows. Coming down, met a peasant on a pony. He got down and rolled me a cigarette, which we smoked squatting beside the path. With my smattering of Serbian I gathered that he was taking bread home, that he had spent about a thousand dinars on finding a girl with sturdy arms and big breasts, that he had five children and three cows, and that one should watch out for thunder, which had killed seven people the year before'

(Nicholas Bouvier, *The Way of The World*, 1992: 15).

There is one important feature that distinguishes the essay from literary reportage and from travelogue: namely, essayists prefer to write about people and phenomena in a general manner, while literary reporters and travelogue authors most often write about particular people and particular situations. Two examples of the essay:

'The North Americans are credulous and we [Mexicans] are believers; they love fairy tales and detective stories and we love myths and legends. [...] We get drunk in order to confess; they get drunk in order to forget. They are optimists and we are nihilists [...]. We are suspicious and they are trusting. We are sorrowful and sarcastic and they are happy and full of jokes. North Americans want to understand and we want to contemplate. They are activists and we are quietists; we enjoy our wounds and they enjoy their inventions. They believe in hygiene, health, work and contentment, but perhaps they have never experienced true joy'

(Octavio Paz, *The Labyrinth of Solitude*, 1962)

'America is neither dream nor reality. It is a hyperreality. It is a hyperreality because it is a utopia which has behaved from the very beginning as though it were already achieved. Everything here is real and pragmatic, and yet it is all the stuff of dreams too. It may be that the truth of America can only be seen by a European, since he alone will discover here the perfect simulacrum – that of the immanence and material transcription of all values. The Americans, for their part, have no sense of simulation. They are themselves simulation in its most developed state, but they have no language in which to describe it, since they themselves are the model'.

(Jean Baudrillard, *America*, 1988)

8.1.3.3 Non-fiction literary art compared with interpretive social sciences

Let us begin with noting a few similarities and differences that do not require advanced discussion. Having done that, I am going to focus at more length on the less obvious comparative features. (Note that here I am not considering differences in research methods; this is going to be undertaken later in the chapter).

Earlier on I said that non-fiction literary artists travel to learn and experience first hand, and that they are interested in the lives of ordinary people. The same can be said about interpretive social scientists. One minor difference in this respect is that

literary artists usually travel to numerous places, while interpretive researchers tend to have chosen a single setting of enquiry⁷⁷.

A difference between interpretive researchers and literary reporters is that usually the former are insensitive to unique, individual, historical events. They could do their fieldwork one year earlier or later⁷⁸. Literary reporters, as I said, usually travel to where something important is happening. Having said that, both of them seek to capture what is recurring, everyday, and social. What also likens non-fiction literary artists to interpretive researchers is that usually they study current phenomena and living people, rather than the past.

A characteristic feature of social sciences and all other 'discourses of sobriety', including non-fiction arts, is that the described facts do not simply represent the reality but rather represent the author's train of thought. Editing is unavoidable because the social reality cannot be straightforwardly represented: it necessarily requires interpretation and argumentation. In Kapuscinski's words, the author 'explores the problem, filters it through her own personality, polishes up' (2003: 42). Fiction films and novels, by contrast, are concerned with developing the plot rather than with developing the subject.

Let us now consider three features of non-fiction arts that require a longer discussion:

Exposing the general through the particular

In interpretive social sciences, claims are always general (e.g. 'Balinese cockfights have such-and-such features' rather than 'a particular Balinese cockfight...'). Particular events and actions are only introduced after the general statement, as the evidence supporting it. For example, Gerd Baumann writes the following in his ethnography of Southall:

⁷⁷ This is getting complicated these days as more research is done on virtual communities. But such communities can likewise be the subject of non-fiction art.

⁷⁸ An exception that proves the rule is Renato Rosaldo's work on the Ilongots (1980) or Veena Das' anthropological research on trauma.

'Unlike the public arena of the Broadway, the sidestreets are considered a private space, the pavements almost part of the residents' property. (...) In mine, ten seconds' walk from the Broadway throng, a neighbour and I, working on my car, were passed by two young men, busily engaged in a noisy conversation. One of them used a common expletive. In an instant, my neighbour turned, raised himself, and hollered: *Watch your language, boys!*' (Baumann 1996: 45).

Non-fiction artists, with the already mentioned exception of essayists, adopt the opposite strategy: they show the universal through the particular, they use 'the power of the detail', so to speak. They render individual events (actions, decisions) in such a way that they portray something general for a certain group, nation or mankind. They make use of the fact that the reader/viewer has the ability to make generalisations from a unique element. To quote a few non-fiction artists and critics:

'To achieve great expression, one must choose one subject and organise the image (story) around it' (Kapusinski 1990: 96)

'If you make a film about the post office, make a film about one letter' (B. Wright)

'We can understand larger cultural qualities by understanding individual behaviour' (Bill Nichols)

'We [reporters] always found some secret 'back door', and on the example of the personal fortunes of an individual or a place we tried to smuggle in more universal truths' (Kapusinski 2003: 67).

For example, Bouvier writes about his stay in Tabriz: 'It happened during our absence that some mysterious broom swept our apartment or some invisible hands placed a bowl of hot soup on our table' (Bouvier 1992: 116). This sentence is infinitely more attractive stylistically and rhetorically than 'Tabrizians are friendly and hospitable.' Another actual example comes from a piece of literary reportage on Caucasians:

'Our host had a little hand bell, which he used to communicate with his family. Ding, dong - and his wife was approaching quickly. Using gestures he showed her an empty dish or decanter. The wife disappeared in to the kitchen and after a few seconds came back, adding roast-meat and pouring wine. Ding, dong, ding, dong - the youngest son appeared. "Cigarettes!" - commanded his father and a short while later he was opening a new pack. The feasters, all of them men, were clearly impressed. "Caucasian *macho*" - I was thinking, observing Fejzudin. That evening the bell was busy and the wife couldn't keep pace topping up the wine. Eventually the guests left; I stayed overnight. I was woken by thirst. I dragged myself to the kitchen and saw the 'macho' bustling about pots. He had already done the dishes, and was just about to start peeling potatoes. His wife and sons were fast asleep.' (Gorecki 2002: 39, translation mine)

Exposing generals through particulars works not only on the level of sentences or paragraphs, as in the examples from Bouvier and Gorecki, but also on the level of the whole work:

'It is my ambition to write a book that would carry a universal message. I wrote *Shah of Shahs* not to describe the Iranian revolution. I was interested in the event, the phenomenon consisting in the fact that an old, traditional culture and civilisation opposed the attempted imposition of an alien cultural model, a progressive model' (Kapuscinski 2003: 80).

'I am not merely interested in describing Haile Selasie's or Shah's courts, I pick this aspect of court that can be found in other situations' (ibid 81).

'...to me, an element of history serves to describe a model, a syndrome.' (ibid 73)

What is most important is the writer's (or filmmaker's) ability to choose a detail that encompasses a universal meaning. When she lacks this ability, the work turns into an uninteresting chronicle of everything and anything. Kapuscinski says:

'When I was preparing to write *Imperium* [a book about the Soviet Union - F.S.] I read many pieces of reportage about the USSR. I was disappointed by them. There is no mental effort in them, no reflection. There is only description of events - the waiter was dirty, the car broke down. Yet writing about facts cannot be equal to unreflectiveness; it is only sound knowledge that enables us to choose a fact and endow it with universal meaning.' (2003: 94)

Humanistic exploration of the subject vs. its systematisation

Non-fiction artists and interpretive social scientists share an important feature: they write about people, for people. The argument I put forward below is that the former put this feature in the centre of their efforts, while interpretivists mostly suppress it.

Why is it important that they both write *about* people? Because this humanistic content means that the reader can relate it to her life and actions. It is a true peculiarity of the social world: whatever is said about another human being, whether this person is very distant socially, culturally or geographically from me or very close to me in these respects, a comparative link is established between me and this person (or group). Nothing of this sort happens when matter is the subject.

Why is it important that they both write *for* people? Artistic works treat their readers as full persons, having their own lives and being capable of acting. They aim at enriching this person as a person. The link between the artist and the reader is humanistic. Whatever is written, is written for, and only for, the reader. The presence of the reader is substantial.

By contrast, the link between the interpretive (and, for that matter, also non-interpretive) social scientist and the reader is, at least in the classical view, not humanistic but professional. The reader is not treated as a person but as a fellow critic. The critic is a person in a specific role. The writer does not want to enrich the critic but strives to convince her of the argument.

Having said that, more and more contemporary social scientists advocate a combination of this traditional, professional view and the humanistic one. There are also researchers who give priority to the humanistic relation over the professional relation. In other words, interpretive social scientists feel torn about what constitutes the general frame of their research: whether it is the general growth of knowledge or engagement with the reader. If it is the former, then the choice of research subject has to be justified in professional currency. The research report is supposed to be, first of all, valuable for the discipline rather than engaging for the reader.

In non-fiction art, by contrast, the overall context is determined by the *audience's interest* rather than by the growth of general knowledge: people read and watch what they find interesting and stimulating. Therefore artists choose subjects that the audience may relate to. This is realised in two ways. Firstly, artists take voice in discussions on issues that are contested, and actively shape attitudes by taking sides. Kapuscinski says the following about the reporter's 'message':

'Genuine journalism [he means both news and literary journalism – FS] is intentional, that is aims at something and takes pains to make some change (2003: 134). (...) I write to convey a message, and I see my profession as some form of mission. (...) I think that people who have opportunity to travel are somehow burdened with responsibility: the responsibility of showing that other people have their feelings and needs, that we must get to know and understand them, and those who have learned about them should translate and transmit this knowledge (ibid 98). (...) My work is of trans-cultural nature, and I see my reporter's mission in attempting to overcome stereotypes' (ibid 63).

Traditionally, shaping attitudes has not been the aim of the social sciences. Perhaps it is still the dominant approach nowadays. But the tension between treating the reader as a fellow professional or as a person has been present in the social sciences for a long time, just to mention Howard Becker's classic 1967 paper 'Whose side are we on?'. Two of our case studies, Hochschild and Wagner, also exemplify the view that what is right or contested is not of only secondary importance for social sciences.

Secondly, non-fiction artists provide us with what I call 'humanistic exploration' of lives, cultures and societies. What do I mean by that? As some humanists rightly observed, studying alien cultures and societies grants the researcher a 'mirror' of her own culture. This 'mirror' allows us to look in a new, different way at ourselves and our lives - to imagine our lives led in a different way, to imagine ourselves involved in different matters, values and emotions. What is obvious for me, taken for granted (the metaphor of 'air' is often evoked), needs the Other to be conceptualised. In Bouvier's words: 'One has to be at a distance [from one's own milieu] to distinguish its contours' (Bouvier 1992: 27).

Artistic works such as film, literature or poetry make intentional use of this 'mirror'. They are, in the broadest sense, attempts at approximating the experience of being someone else than one is, of being the Other. Flaubert's words capture this concisely: 'I wish I could live all lives' (in Bourdieu 1992: 205). The point of non-fiction art is also precisely this: to broaden one's knowledge about one's own life, culture and society by looking at other lives, cultures and societies. Enriching the audience as persons is preferred to broadening general knowledge. Kapuscinski's remark is symptomatic here:

'The Emperor [a book about Haile Selassie's Ethiopia – F.S.] is very popular in Switzerland: people recognise similar, hierarchical behaviour in large corporations or institutions there. Now, this is what interests me' (2003: 81).

The following fragments of non-fiction literature are examples of the 'mirror' that stimulate the reader to think critically about her own society, culture and life:

'The number of people here [in New York] who think alone, sing alone, and eat and talk alone in the streets is mind-boggling. And yet they don't add up.

Quite the reverse. They subtract from each other and their resemblance to one another is uncertain' (Baudrillard 1988: 15).

'The Georgian likes his day of work and wants it to be closed with a cheerful accent. To let some ceremonial moment endow that day with value and preserve it in memory. No day will be repeated twice and that is why each of them should be separately celebrated, treated as an event. So the Georgian gives himself fully to this rite. He cleanses himself, finds his completeness. The feast is nothing like gulp, feed, gobble. One doesn't come here to black out, to get smashed. The Georgian despises heavy drinking, hates drinking for the quantity's sake. The table is only a pretext, tasty and full of wine, but nothing more than a pretext. An opportunity to celebrate life'.

(Kapusinski 1990a: 19, translation mine).

In these examples the 'mirror' is used, so to speak, implicitly: there is no direct, explicit reference to the reader's culture or life. It is left to the reader to carry out the comparison. Sometimes, however, non-fiction writers use the 'mirror' in a direct way, as in this fragment of Octavio Paz's essay on America and Mexico:

'The North Americans are credulous and we [Mexicans] are believers; they love fairy tales and detective stories and we love myths and legends. [...] We get drunk in order to confess; they get drunk in order to forget. They are optimists and we are nihilists [...]. We are suspicious and they are trusting. We are sorrowful and sarcastic and they are happy and full of jokes. North Americans want to understand and we want to contemplate. They are activists and we are quietists; we enjoy our wounds and they enjoy their inventions. They believe in hygiene, health, work and contentment, but perhaps they have never experienced true joy'
(Octavio Paz, *The Labyrinth of Solitude*)

Interestingly, this fragment provides an explicit 'mirror' for Mexican and American readers, but only an implicit one for others: it stimulates me to think about my own, Polish, mentality.

Now, how does this 'mirror' feature relate to interpretive social sciences? Very often they study peoples who are culturally or socially different to us. Hence, the potential of them being a 'mirror' for the reader is inherently present in this kind of research. Brian Fay clearly sees this potential of interpretive social sciences:

'New ways of living become real alternatives when one is able to see the sense of alternative life styles and different ways of looking at the world. At the

least one's own assumptions are thrown into relief and therefore one becomes more fully self-conscious; at other times one may well come to redefine oneself and therefore to act differently' (Fay 1975: 81).

Paradoxically however, when interpretive research is pursued and understood as *science*, this potential is suppressed, and the 'mirror' effect occurs only as a side effect rather than as the aim. The researcher is expected to study the subject systematically, thoroughly, rather than to choose aspects that are interesting for her and the readers as cultural and social beings. Compare the following fragment of scientifically understood interpretive research with the fragments of non-fiction literature introduced earlier (Baudrillard, Paz, Kapuscinski):

'In fact, *wayugo*, the name of that creeper species, is also used as a general term for canoe magic. (...) For, as in all other magic, there are several types of *wayugo* spells. The ritual is always practically the same: five coils of the creeper are, on the previous day, placed on a large wooden dish and chanted over in the owner's hut by himself. (...) Next day they are brought to the beach ceremonially on the wooden plate. In one of the *wayugo* systems, there is an additional rite, in which the *toliwaga* (canoe owner) takes a piece of the creeper, inserts it into one of the holes pierced in the rim of the dug-out for the lashing, and pulling it to and fro, recites once more the spell' (Malinowski 1922: 137).

It should be quite clear that this account results from a systematic study and has value for the discipline of anthropology, but is not significant for the lay reader in the way the other fragments are.

Horizontal interpretations

In the early chapters of this thesis I mentioned three major kinds of interpretation: the interpretation of intentions (including unconscious intentions), the interpretation of shared, constitutive concepts, and the interpretation of what I called 'horizontal meanings'. What distinguishes such 'horizontal meanings' from shared, constitutive meanings, is that they are not shared and not constitutive for actions. They are also not conceptualised. Nevertheless such meanings may have influence on peoples' feelings, evaluations and actions.

Because of their nature, 'horizontal meanings' are more easily detected through the comparison of cultures, from the perspective of the outsider, rather than from the

inside. Now, this is what I believe non-fiction art is best at and offers in abundance. Take the following insights:

'But the conception Americans have of the museum is much wider than our own. To them, everything is worthy of protection, embalming, restoration. Everything can have a second birth, the eternal birth of the simulacrum. Not only are the Americans missionaries, they are also Anabaptists: having missed out on the original baptism, they dream of baptising everything a second time and only accord value to this later sacrament which is, as we know, a repeat performance of the first, but its repetition *as something more real*' (Baudrillard 1988: 41)

'The gun stands for the hope that there exists that which is other than oneself. The gun is our last defence against isolation within the travelling sphere. The gun is our mediator with the world and therefore our saviour. The gun saves us from the fear that all life is within us. (...)

Savages do not have guns. This is the effective meaning of savagery, which we may define as enslavement to space, as one speaks obversely of the explorer's mastery of space. The relation of master and savage is a spatial relation. The African highland is flat, the approach of the savage across space continuous. From the fringes of the horizon he approaches, growing to manhood beneath my eyes until he reaches the verge of that precarious zone in which, invulnerable to his weapons, I command his life. On the far side he is nothing to me and I probably nothing to him. On the near side mutual fear will drive us to our little comedies of man and man, prospector and guide, benefactor and beneficiary, victim and assassin, teacher and pupil, father and child' (Coetzee 1974: 84, 86).

Having said that, I do not claim that non-fiction art avoids interpreting intentions and shared knowledge.

8.1.4 Documentary film

The definitional problem appears yet another time when it comes to documentary filmmaking. As Mark Cousins stated, documentary film is the most flexible genre of all film genres (1998: xi). It can deal with present events or past events, everyday life or extraordinary events. The subject can be individual or collective. The form can be realistic or experimental.

In this section I am going to briefly discuss differences between documentary films and feature films⁷⁹. Due to a lack of space and the complexity of the issue I am not going to bring up the comparison between documentary films and documentary photography, although numerous features of the latter make it a fascinating object of study for a philosopher of social science. I shall then compare documentary films and interpretive social sciences.

8.1.4.1 Documentary film and fiction film

What is the difference between the two? Intuitively, the answer would be that documentary films are 'true/real stories, without actors'. Basically, we believe that we are watching something true/real rather than fictional when we believe that it would have happened without the camera's presence anyway. Things seem to be not staged, not scripted. There are four reasons however why this definition is a rough-edged one:

Firstly, we can be deceived about the 'real-ness' of the events filmed. The impression of authenticity is achieved mostly through the application of a special *form*, consisting of elements such as the handheld camera, no artificial lighting, poor sound quality, etc. The presence of these elements suggest that the filmmaker did not have time to prepare for filming, that what we are watching did not happen in a film studio. But this implies that a fictional story can purposefully be shot using the form of documentary, as if it was not prepared (for example *Blair Witch Project* or *Man Bites Dog*). This genre is called 'mocumentary' and can be very sophisticated.

Following that, there is a problem with the 'no actors' criterion. For example, real characters can be asked to re-enact an event from the past, in which case they acquire traits of actors. Or actors can stage a true story, in which case the border between reality and fiction is blurred again. Also, the documentary filmmaker can 'plant' an actor into a real-life situation without informing other people involved in this

⁷⁹ In this chapter I exclude factual TV programmes, of the 'Discovery Channel' type, from the analysis. How are they different from documentary films that I focus on? They are easily distinguishable by the fact that the narration is led by explicit commentary rather than visually. TV factual programmes are directed to wide audiences, and as such they have to be undemanding. It is easier to listen than to attentively watch sequences of pictures. The basic principle of good documentary filmmaking is that the commentary cannot duplicate the image, but must back it up where it is impossible to say something visually.

situation – in which case the train of events would be slightly altered but not ‘staged’ as such.

Thirdly, there is a problem with the ‘as if the camera was not there’ criterion. An unscripted event or action can happen precisely due to the camera’s presence, and would not have happened otherwise. Joris Ivens’s *Misère au Borinage* (1934), is a very telling example. The filmmaker wanted to make a historical documentary about a 1920’s miners’ strike in a Belgian coal town, in which the police brutally pacified the socialist-sympathetic crowd. Hence he employed extras who staged the strike. But it happened that real miners spontaneously joined the extras in big numbers, and the strike was again pacified by the police. Was it not ‘real’, ‘true’ just because it was catalysed?

Finally, one distinct difference is that documentaries, unlike feature films, are about issues rather than stories. This means that images are linked logically, not spatially or chronologically (although of course they can be so). Through juxtaposing discontinuous threads, it is suggested to the viewer that a logical link exists between them. In Bill Nichols’ words:

‘We can assume that what is achieved by continuity editing⁸⁰ in fiction is achieved by history in documentary film: things share relationships in time and space not because of the editing but because of their actual, historical linkages. Editing in documentary often seeks to demonstrate these linkages’ (2001: 28),

‘Places and things may appear and disappear as they are brought forward in support of the film’s point of view or perspective. A logic of implication bridges these leaps from one person or place to another’ (ibid 28-29),

‘The logic organising a documentary film supports an underlying argument, assertion, or claim about the historical world that gives this genre its sense of particularity’ (ibid 27).

In Rudolf Arnheim’s words: ‘In the documentary film, editing is to ensure that the viewer will think, not just watch’ (in: Karabasz 1999a: 117). E. Lindgren similarly said that ‘Editing reflects the thinking process’ (in: Reisz 1968: 213).

Also, ethical dilemmas are not a concern for fictional art, while they are very much so for documentary filmmaking and non-fiction literature. (And, for that matter, for

⁸⁰ ‘Continuity editing’ means that the elements of time and space are continuous, e.g. somebody first wakes up and then has a shower, rather than the other way round.

interpretive social science). It refers both to the stage of research and to the stage of creating the representation.

8.1.4.2 Documentary films and interpretive social sciences

Most of the remarks made in the earlier section where I compared non-fiction literature with interpretive social sciences equally apply to documentary filmmaking. Most documentaries attempt to capture first-hand images of ordinary people. They apply the method of talking about the general through showing the particular⁸¹, they provide the viewers with the 'mirror' by showing the Other, and they encounter ethical dilemmas when studying and representing people. Documentaries however have one substantial feature that literature - both artistic and scientific - does not have. I am going to devote this section to discussing it.

Documentary film differs from interpretive social sciences and non-fiction literature since it uses *images*, not *words*, as its means of expression. What is an image as compared with words? Two features are substantial. Firstly, the image, unlike the word, renders immediate *complexity*: numerous elements, such as motion, space, objects, sounds, emotions, colours, the number of people, the flow of time, gestures, etc., are interlinked. The image is therefore holistic and synthetic, whereas the word is analytic. Secondly, the word is delayed against its referent, and hence gives us the possibility of negation. The image does not – because it appears spontaneous, immediate, always a record of the present. The word can be inadequate – it may seem that the image cannot. The word can lie; the image by its nature carries *the impression of truth*.

Do these differences mean that using motion pictures is more suitable for reporting qualitative research than using written reports? Two supporting arguments follow the two features listed above.

⁸¹ NB Geertz said that 'a documentary filmmaker and an anthropologist face the task of showing the general in the particular. They always face concrete situations, particular people' (1973: 22). Geertz missed here the fact that anthropologists formulate general statements explicitly, and use particulars only as evidence.

Firstly, since the image reflects the complexity of reality and is a spontaneous record of the present, it seems to be the primary way of rendering the character of *action*. Movement, appearance and interaction seem to be particularly designed to be filmed. Action takes time and space, and therefore suits film (motion pictures) best. Also, since as viewers we perceive the present and not the past, the outcome of the action is open-ended, similarly to the real life. Documentary film therefore renders action 'as it happens', with all 'meandering' and re-evaluations (as was rightly pointed at by Alfred Schutz).

Similarly, life and culture are rendered in the 'unconceptual' way – as something internally experienced by people. Film gives a taste of this experience to the viewer; it is able to render praxis as praxis and not as semantic meanings. Interestingly, this embodies one of Wittgenstein's most famous theses: 'what has to be done, cannot be said'. The visual has advantage over the word also in another respect: by showing the subject's face, the viewers are given a chance to perceive his or her emotional states. Similarly, showing a face is a huge source of information about the subject's social environment. The character of living conditions, the character of people one associates with, the character of one's life experiences shape her face to a surprisingly high extent (Karabasz 1999a: 67)⁸².

There is hence an argument that documentary film is more suitable for qualitative research, for 'mediating between ways of life' than the analytic (because word-laden) interpretive sociology and anthropology. To this a further argument can be added, based on the second feature of the image mentioned above – that the image carries the impression of truth. This means that the visual evidence feels stronger and more reliable than the written account. The viewer sees what the filmmaker saw; in this sense the mediation of the filmmaker and the camera appears to be lesser than the mediation of the researcher and words. Words can be easily manipulated; images much less so.

There are however two strong, interlinked arguments denying this higher suitability of motion images over words for 'mediating ways of life'. Again, they are logically linked to our two 'supporting' arguments. With respect to the first, I wrote that the

⁸² Apart from that, showing the face is the best means to emotionally bond the viewer with the protagonist.

documentary film offers the taste of experiencing the lives of people filmed. The viewer has the impression of participating in the space, time, emotions and actions of these people. But how *true* is this impression? To what extent can the viewer really empathise with the protagonists, penetrate their actual experiences? The answer lies in the fact that the viewer ultimately cannot be located inside the original context, cannot share it or participate in it, cannot interact with the protagonists. The image only *pretends* to be delivering the original experience to the viewer. It cannot however replace praxis or social action: viewing is not acting. 'What has to be done, cannot be filmed'. This 'deceitful' feature of the image has been recognised by Susan Sontag⁸³. She has noticed that the photographic image is not real but surreal: although it approximates the experience/reality depicted, this experience always differs more or less from the original experience.

The second argument against privileging the visual medium over words concerns the fact that the image carries the *impression* of truth, and that it *feels* like stronger and more reliable evidence than the written account. The image *seems* to be a glassless window onto another reality. The argument against it is that this impression is wrong. Why is that? The impression of being exposed to truth or reality derives from a peculiar feature of the framed picture: the viewer unreflectively believes that she sees exactly what the filmmaker saw, and she also believes that the reality is exhausted by what is pictured. This is ultimately the trick – or, as some prefer, magic – of the cinema. But both beliefs are false. The viewer actually views *less* than the filmmaker originally saw, and what is pictured actually constitutes only a fragment of reality. This suggests that the film is not a window onto reality but merely a *representation* of this reality. The filmmaker chooses – and cannot escape this choice – the form of representation and the content of representation. Let us discuss them in turn.

Firstly, all visual representations have formal elements such as light/contrast, framing, B&W/colour, angle of view, depth of field etc. These elements influence impressions and evaluations of the viewer⁸⁴. But the reality filmed does not tell the filmmaker which formal elements to use: she has to make this decision on her own,

⁸³ in her book *On Photography* (1979). Her writings regard photography, but they are equally relevant to motion pictures.

⁸⁴ For example, soft lighting renders the character as gentle, peaceful or untroubled, while sharp, contrasty light makes her seem to be experiencing an internal conflict.

she has to interpret the reality and match the form to it. Formal decisions cannot be evaded. In short, the filmmaker steers the impressions of the viewer.

Secondly, the filmmaker cannot escape the choice of the content. Film pretends that its content naturally mirrors reality, it pretends that it is the unmediated, uninterpreted reality; the information rather than a message. In fact, the filmmaker has to select what to say about the reality, what argument to put forward – but then she foils this fact, she presents her argument as the reality itself. This selection takes place both at the stage of filming and editing.

During the shooting process, the filmmaker decides first of all *where* to direct her camera and *when* to film. Therefore she decides what is important and who is important. Different decisions lead to different views of the reality. During the editing process, the filmmaker decides what interpretation of the reality she wants to give. Differently arranged scenes would make different sense. Similarly to the issue of form, there is no ‘natural’ or neutral choice here, there is no escape from making one’s own interpretation. The social reality is always contestable, not obvious – different opinions and evaluations are possible. The filmmaker’s job is to foil this fact by skilfully presenting her interpretation of the reality as the reality itself. The image lends authority to the filmmaker, becomes the evidence supporting her argument. The visual footage is organised so as to give best support to the director’s argument⁸⁵.

These two arguments suggest that we have no reason to trust the visual medium more than the written one. The mediation of the filmmaker and the camera is *not* lesser than the mediation of the researcher and words. Having said that, there is no reason for disqualifying film altogether. These two arguments mean that we need to use slightly different critical tools when watching a film than when reading a book.

To sum up this sub-section: I have presented one group of arguments suggesting that due to the image’s complexity and realism, motion pictures are a better medium for qualitative exploration of other cultures and societies, and another group of arguments suggesting that rather than mirroring reality, the film is unavoidably only

⁸⁵ The fact that the film is not information but a message is easy to miss, particularly when there is no explicit commentary. Both editing and filming techniques are transparent – they conceal that they are tools of interpretation and argumentation.

a representation of it. Now I would like to say that actually these two arguments are extremes between which the nature of the film lies. The image has a very specific nature: it is something in between the experience and the account, or to be more precise, something in between a first-hand experience and a second-hand account. But it is neither one nor the other. It has features of both. It is not a representation in the way a painting or a book is, because it is confined by the reality in front of the camera – in an important sense, ‘the lens never lies’. But it is also not a window onto reality, not a replication of it but an account of it.

8.2 Scientific v non-scientific methods of research

Having discussed the nature of non-fiction art, we are now ready to pose the main question of this chapter: what are the methods used by non-fiction artists? Are they actually different to methods used by interpretive social scientists? Are artistic findings less valid and reliable? I am now going to present research methods used by artists, and subsequently I will juxtapose them with interpretive research methods.

8.2.1 What research methods are used by non-fiction artists?

It is not easy to identify methods that non-fiction artists employ. Methodological issues are almost never talked about in the works themselves (as irrelevant for the non-academic reader). Only in rare cases of published autobiographies, teaching materials or Q&A with artists, can we learn about the methodological aspects of the research process. Ryszard Kapuscinski’s ‘Autoportret Reportera’ [*Reporter’s Self-Portrait*] is an example of a ‘professional’ biography, while Kazimierz Karabasz’s ‘Rozmowa o dokumencie’ [*A Talk About The Documentary Film*] is a student handbook of documentary filmmaking. I am going to draw on both of them.

8.2.1.1 Documentary filmmakers’ methods

Documentary filmmakers, by and large, take two approaches. Some of them strive to make films that *explore* certain groups or individuals. Such films seek to picture the protagonists’ characteristic features, their everyday life, their experiences and

worldviews. To the viewer, the subjects are interesting as unique human and cultural beings. If we look for the analogy in anthropology, it would be finding an interesting tribe or cultural meaning. The following two documentary films are examples of this 'exploratory' approach:

'Exit' (dir. F. Melgar, Switzerland 2005, 75')

This film is a portrait of a Swiss-based society called 'Exit'. It is a group of people who carry out 'assisted suicide' with persons who are incurable and in huge pain. The film observes the members of 'Exit' in their activities, it being long conversations with the incurable and the very act of providing the poison. There are also several interview excerpts. There is no commentary. The film portrays the members of 'Exit' as people who are very well aware of what they are doing rather than fanatics. In particular, they know that you either help the incurable or refuse it; there is no third option such as 'I'm not sure so I won't do anything'. There is no escape from the moral responsibility. The film's message is very humanistic.

'Dark days' (dir. M. Singer, USA 2000, 80')

An exploration of the life of homeless people who live in the tunnels of underground trains in New York. The subjects are filmed during their daily activities – trawling through the rubbish, selling items that have been found, selling drugs, drinking, maintaining their card-box 'houses', washing, etc. Several interview excerpts are interwoven, giving us a picture of people who have often been traumatised in the past, who are deskilled, but who also have needs and emotions of any other homed person.

The second approach is very different: instead of exploring the subject, the filmmaker *illustrates her argument or concept*. Such films do not represent a certain group or individual in their uniqueness, but illustrate a certain social phenomenon. They often point at causal relationships. These 'conceptual' films have a synthetic construction – locations and people are linked logically rather than spatio-temporarily; the main characters are not outlined in-depth. Here are two examples:

'Jak zyc' [Recipe for Life] (dir. M. Lozinski, Poland 1977, 85')

The film is a documentation of a summer camp for young married couples, members of the Polish Socialistic Youth Association, an annex of the ruling party. The camp's

organisers run a competition (with an award) for the model family. Some couples take the competition seriously, but most of them contest it and prefer to enjoy the holidays. The jury struggles to encourage people and penalises the stubborn ones. The film brilliantly portrays the futility of social engineering, particularly of centrally planned social life (in the former communist block). It also shows how an ideological system colonised many people's mentality, in this case making them conformist and opportunist.

'Avenge but one of my two eyes' (dir. A. Mograbi, Israel 2005, 104')

A film about the Palestinian-Israeli conflict, taking up the issue of suicide bombings. The director juxtaposes two, apparently unrelated, threads. The first one is about Jews celebrating an anniversary of Samson's suicide attack on the Romans in the 2nd century BC, when Israel was occupied by them. People interviewed by Mograbi say that had they needed to fight for independence, they would prefer to die in a suicide attack on the enemy, like Samson did, rather than to surrender. The second thread of the film is a portrayal of Palestinians being literally surrounded by Israelis. The Israeli people in the film are not aware of the fact that they occupy Palestine in the same manner as the Romans occupied Israel, and that it is inconsistent to blame Palestinian suicide bombers and at the same time to give oneself the right to do it.

By and large, the choice of *research method* is determined by the filmmaker's intention to make an 'exploratory' film or a 'conceptual' film. If she wants to make the former one, then it means spending a significant amount of time with the subjects (up to half a year at one go⁸⁶ or returning over many years⁸⁷). This very much reminds one of participant observation and unstructured interviews.

If a 'conceptual' film is being made, the filming is preceded by the search for a real case picturing the director's argument. (Hence it is something without analogy in the social sciences). Once it is done, the filmmaker proceeds to film, since there is no need to get to know the protagonist in a great depth. 'Conceptual' films often require less lengthy research than 'exploratory' films, but the research is often more

⁸⁶ E.g. Slesicki's portrait of a group of Gypsy-Travelers, 'Zanim opadna liscie'.

⁸⁷ E.g. 'Balseros', 'Spellbound'.

comprehensive: the filmmaker reaches many members of the community in question. This allows for the 'multiplication of voices' in the film. Marcel Lozinski applied this technique in his documentary 'Moje miejsce' [*My place*]. The film comprises of statements of a hotel's employees – from top managers to cleaners – all of whom claim he or she is indispensable for their workplace, or that everything would collapse without them. Through multiple voices the character of this society was pictured, not only the character of the people on screen.

In both 'exploratory' and 'conceptual' documentaries the filmmakers can apply *experimental methods*⁸⁸ ('experimental' meaning here that such methods are invented for a particular research situation, adapted to a given case, and tried out). For example, the director may ask the protagonist to write a diary, fragments of which will be later enclosed in the film (e.g. Karabasz's *Rok Franka W.* [One year in Franek W.'s life]), or may use existing diaries to look for future protagonists (e.g. Kieslowski's *Murarz* [The bricklayer]).

Interestingly, documentary filmmakers who make 'conceptual' films broadly employ *performative methods*, that is ones which intentionally arrange social situations in order to trigger off some behaviour, in which the protagonist's character or society's nature are manifest. Marcel Lozinski's method is an example of the performative method. In his words:

'I try to influence the reality and then treat openly the situation which has been created'; 'The best thing ... is that finally you do not really know what has been staged and what is life' (*Film 36/1976*).

He employed the performative method in most of his documentaries, for example in the film mentioned earlier, *Jak zyc* [Recipe for Life]. The director arranged this situation (a summer camp for young married couples, the rules of the competition, etc.) without revealing this fact to the participants. (Of course, they were aware of being filmed and gave their written consent). Two of the couples were also 'planted' by the director; their task was to challenge the very idea of competition.

⁸⁸ Des Bell, a documentary filmmaker and a sociologist, pointed my attention to experimental methods [private conversation].

How should we view the 'scientificity' of performative method against other, well recognised methods? Such performative methods could be compared to creating experimental situations in natural and social sciences. Those who support the use of experiments would discredit artistic experiments as unreliable (badly controlled, difficult to replicate). From another perspective, qualitative researchers, by and large, criticise experiments on the basis that the subjects are not in their 'natural' environment. There is a strong impression that only 'unarranged' conduct, occurring without the researcher's interference, is regarded as a suitable subject for qualitative social sciences.

This argument could be parried, however, since the subjects of artistic experiments do not know that they are in an arranged situation. Their behaviour is hence still natural. Also, qualitative social scientists widely use interviews, which are an active and deliberate call for information. Interviewing is also a performative action, but in a weaker sense than inducing actions. In short, social sciences are not afraid of bringing about utterances that would not take place otherwise, but are distrustful about stimulating actions.

I do not see a good reason why social sciences should not make use of the performative method, why they should not create situations the outcome of which would help to understand subjects better. I suppose that the stubborn presence of old methodological views lies behind the reluctance to use such performative methods in interpretive social sciences.

Finally, a few words need to be said about the stage of making the film that follows the research, which is *shooting*. Methods of learning about the subject are different to methods of shooting. And so the filmmaker may get to know the people in question mainly through interviews, using a participatory method, but may film them in an 'observational' ('fly on the wall') way. This choice of the filming method is not only a stylistic one, but epistemological as well. It is such because the manner of shooting decides whether the viewer will be allowed to learn about the filmmaker's role in the creation of the film or not.

The 'candid' (covert) camera (still dominant in factual TV programmes) is the classic method of shooting documentaries. It is supposed to create the impression of the film being an unobtrusive observation, and through that to affirm the viewer that she is watching the reality itself, not its representation. The classic method is based on the overwhelming impression that the camera does not experience – and therefore is objective.

Ambitious filmmakers today quit this approach in favour of a 'postmodern' one, which does not conceal the existence of the filmmaker but rather exposes her active role in the creation of the filmed reality. While the camera does not experience, the camera operator/director does. Every film is a meaningful creation, and as such depends on her experiences; it is her voice. This epistemological turn strikingly reminds one of the turn in social anthropology, and the discussion of more authentic ways of presenting research.

8.2.1.2 Research methods of literary reporters

The primary research activity of literary reporters is travelling to a certain place or places to acquire first-hand knowledge. Kapuscinski calls it 'the reporter's journey'. This kind of journey is all but tourism or tramping around the world: it is a conscious exploration of a certain phenomenon or a society. The reporter takes a cognitive attitude; she filters information through it. Her perception is set at an angle similar to that of an interpretive social scientist's:

'As for me, the most precious journey is the reporter's one - ethnographical, anthropological one – which aims at bettering one's knowledge of the world, of history, of changes taking place, and then disseminating this knowledge. Such a journey demands concentration and attention, but thanks to them I can better understand the world and mechanisms in it'. (Kapuscinski 2003: 12)

'In a reporter's journey there is no room for tourism whatsoever. This journey demands hard work and huge theoretical preparations. Learning about the region one is travelling to. Such a journey does not know any relaxation'. (ibid 13)

A reporter's journey should not be in a hurry. Indeed, it is more about long stopovers than moving around. Bouvier writes on this matter: 'Six months of hibernation [passing the winter in Tabriz – F.S.] had made us into Tabrizi' (1992: 168). As such, it resembles participant observation and questions a characteristic statement of

Mary Luise Pratt: 'The authority of the ethnographer over the 'mere traveller' rests chiefly on the idea that the traveller just passes through, whereas the ethnographer lives with the group under study' (Pratt 1986: 38). Also, reporters often return to some places repeatedly, which is quite common in anthropology. To give an example, Kapuscinski spent overall thirty years in Africa and Latin America.

What distinguishes reporters from qualitative researchers is that the place they travel to may be the whole region or state, which entails that the reporter translocates herself significantly within it. Further, since the reporter – unlike the qualitative researcher – is disposed to participate in individual events, there exists an element of 'grasping the moment' in her work:

'[Reporter's journey] involves full concentration, focus. We need to be aware that the place we have reached may be given to us only once in a lifetime. We'll never come back here, but we have one hour to get to know it. In one hour's time everything has to be witnessed, heard, memorised. We need to preserve the mood, situation, atmosphere'. (Kapuscinski 2003: 13)

As for the data collection, Kapuscinski uses the classic method: '...for me data collection first of all consists in reaching people and creating situations when I am most absent and they are most natural' (2003: 48). This is why he deliberately does not use the tape recorder: 'From my experience I have learned that people sat next to the microphone begin talking in a different way, they formulate thoughts differently. All genuineness and naturalness of language is lost; it becomes more formal, artificial and constrained' (ibid 47). Apart from freezing people, the tape recorder is not needed for one more reason. Kapuscinski quotes Gorki: '...what is important – you will remember, and what you will forget – is not worth writing about anyway' (ibid 47).

Unlike the anthropologist, who is obliged to carry out her research systematically, a reporter can select information and pick what she finds essential. Such a method is closely related to the fundamental principle of literary reportage and documentary filmmaking – exposing the general through the particular: 'I barely take any notes. I try to remember, and what I later preserve on paper is two-three memorised pictures that yield the essence, the synthesis of a phenomenon and impressions, reflections arising on that occasion' (Kapuscinski 2003: 73).

Unlike anthropologists, Kapuscinski emphasises the role of nonlinguistic information in cognition: 'I perceive information not only from what someone says, but from the whole landscape, climate, from the way people behave, from a thousand details. It all speaks, this whole reality that surrounds me' (2003: 50). Baudrillard's *America* contains passages suggesting the same approach:

'I went in search of astral America, not social and cultural America, but the America of the empty, absolute freedom of the freeways, not the deep America of mores and mentalities, but the America of desert speed, of motels and mineral surfaces. I looked for it in the speed of the screenplay, in the indifferent reflex of television, in the film of days and nights projected across an empty space, in the marvellously affectless succession of signs, images, faces, and ritual acts on the road; looked for what was nearest to the nuclear and enucleated universe, a universe which is virtually our own, right down to its European cottages.' (Baudrillard 1988: 5)

Bouvier also uses alternative sources of knowledge: 'General stores in provincial, little towns and villages reflect the locals' mentality fairly well' (2000: 68).

Kapuscinski also emphasises the role of the reporter's character and her interpersonal skills:

'I reckon that good reporters are modest and able to show respect and esteem to other people. (...) Her life and work's results depend on what she is told, on what other people do for her. In order to be accepted she should learn how to live with people. To me, a reporter has to be a humble and empathic person' (Kapuscinski 2003: 54).

'A reporter is peoples' slave, she can do only as much as they allow her. (...) [the interlocutor] will tell me only as much as she likes, she can tell me nothing. I know that being successful depends on the way the rapport is established' (ibid 59).

The necessity to win the subjects' trust from the very beginning is therefore a feature shared by academic and non-academic researchers.

8.2.2 Artistic v social-scientific research methods: a comparison

Having presented artistic research methods, I am now going to draw comparisons between them and interpretive research methods.

What elements support the claim 'for' the equal 'scientificity' of interpretive research and non-fiction art? The basic similarity of interpretive social sciences and non-fiction art lies in the *first-hand cognitive experience* of the researcher: the validity of findings is essentially derived from this immediacy. In other words, the nature of the subject – the meaningful human behaviour – determines the way it can be comprehended: by understanding and interpretation. But the ability to understand and interpret is possessed naturally by everyone, scientists and non-scientists alike. It cannot be acquired by training.

Secondly, the fact that qualitative research is *not codified*, is flexible and circular, means that both artists and social scientists cannot follow any rigid plan or guidelines. In particular, the stages of research design and data analysis are not too different. Sampling depends on specific circumstances of the study, and interpreting depends heavily on who is interpreting.

Following on, the *research positioning* in artistic research is the same as it is in social-scientific research. In both cases the researcher undergoes the transformation from an outsider to an insider, but tries to keep the balance between familiarising the subject and maintaining cognitive criticism. Also in both cases she stays unembroiled in the structure under study⁸⁹, which means she has the chance to be given access to various positions. Thus Bouvier writes: 'A traveller's social mobility⁹⁰ makes it easier for him to be objective' (1992: 27), and 'travelling gives access to various worlds – beggars, rich people...' (2002: 139). These could be Hochschild's words.

Fourthly, it is admitted that *interpersonal skills* of the researcher play a major role in establishing fruitful relationships with the subjects. But such skills cannot really be acquired by training; they are talents. Social scientists are reluctant to acknowledge this fact, whereas it is a commonplace in documentary filmmaking and reportage that one has to be good at communicating with people and at establishing relationships to be successful in these fields.

⁸⁹ With the exception of *covert* participant observation.

⁹⁰ By 'social mobility' Bouvier means here 'cruising between different social *milieux*'.

Next, the fact that artists do not employ '*remedy*' tools that enhance validity may be irrelevant - since in practice, interpretive social scientists often do not use them either.

Last but not least, artistic researchers are no worse than scientific researchers simply because the latter apply a lot of *technical names* to their research procedures, such as 'sampling', 'fieldnotes', 'data analysis'. This does not mean that qualitative researchers employ certain procedures that are not available to artists. Non-fiction artists and, for that matter, lay people, can potentially carry out certain research procedures even if not being aware of their jargon names. (Whether they actually apply those procedures or not will be discussed shortly). For example, one does not need to know the term 'sampling' to reflect critically whether one's findings are representative or not, and in a negative instance one can extend the number or type of subjects under study. Hence it is not justified to judge that research carried out by interpretive social scientists is more scientific because it contains elements not accessible to non-scientists.

So are there any arguments 'against' granting artistic research a status equal to qualitative research? One such argument would be that *data collection* seems to be more *comprehensive* and more *laborious* in social sciences than non-fiction art. Non-scientific data collection appears to be more intuitive, opportunistic and contingent. This sounds logical: as an artist's work is going to be assessed more by its artistic values than by correctness of its claims, there is less pressure on the artist in this respect than there is on the social scientist. Artists are not *professionally* obliged to carry out thorough sampling, to keep fieldnotes, or to employ 'remedy' tools such as triangulation.

This argument is sound, but in reality there are non-scientists who feel obliged to carry out thorough data collection even if no one is going to check them. The artist's ethos is decisive: some artists would not publish a work if they were not content with their level of expertise on the subject of study, or with the amount of evidence provided, or with the representativeness of the work. Other artists would still produce and publish the work. All this means that we cannot help but consider and compare works individually.

In conclusion, interpretive social science and non-fiction art share a significant portion of their research methods. However, while particular cases of non-fiction art may be at least as 'scientific' as their social-scientific counterparts, other cases may be significantly 'non-scientific'.

8.2.3 Artistic v social-scientific 'research reporting'

Let us recall that research reporting is crucial for determining the level of validity and reliability of findings. We can distinguish *methodological* reporting and reporting of *evidence*. The latter is one of the two elements responsible for the validity of findings; the other one concerns whether procedures have been matched well to the situation. Methodological reporting influences the reliability of findings, as it allows for the assessment of whether research procedures were carried out correctly.

8.2.3.1 Reporting of evidence

Non-fiction works are usually abundant in evidence supporting the correctness of interpretations being made. After all, they are by definition representations or documentations of the reality. Quoting excerpts from conversations in literary reportage, and the form of 'talking heads' in documentaries are the most common techniques⁹¹. Sometimes in more ambitious works such evidence is left without interpretive commentary, letting readers/viewers enjoy formulating the interpretation independently.

When it comes to showing evidence of representativeness of statements however, non-fiction works are unsatisfactory. Artists most often generalise as they wish, sometimes from a few cases. It may not be that important in the case of 'internal'

⁹¹ In literary reportage and documentary film, just like in sociology and anthropology, only the most important, essential fragments of interviews conducted by the researcher appear. There are, however, some exceptions: for example Dwyer (1982), Castaneda (1973), Naipaul (1990). Dwyer and Naipaul provide complete records of long interviews. Presenting the whole conversation - including the author's questions, misunderstandings etc. - is closer to the real experience. This does not mean that the records are completely unedited. Dwyer occasionally comments on the dialogues in footnotes, while Naipaul inserts commentaries in between his interlocutor's statements (Naipaul 1990: 11n).

validity, as artists usually have good knowledge of the case under study, but it is unacceptable in the case of 'external' representativeness.

Building up the validity. Validity of findings is based first of all on providing direct evidence. Where such evidence is missing, building up authority is perhaps its strongest substitute. Kirk and Miller observed that '...the sensitive, intelligent fieldworker armed with a good theoretical orientation and good rapport over a long period of time is the best [validity] check we can make' (1986: 32). It should not be surprising then that authors want to come across as such! Specific techniques are applied to construct authority and in consequence validity.

In *documentary film* the 'voice of God' type of commentary is the simplest way to create the authoritative voice. Changes of the position and angle of the camera as well as making people ignore the camera also create the illusion of the objective, omniscient author. At the opposite end there is a moving hand-held camera, long unedited shots and people who look and talk into the camera, which gives the viewer the impression that the film has a concrete maker, that it is a subjective first-person look rather than a third-person objective 'window'. This 'subjective' approach is used widely nowadays under the influence of the postmodernist critique of classic 'fly on the wall' style, which is claimed to be deceptive.

In *anthropology* the main technique is the 'ethnographic present', which 'locates the other in a time order different from that of the speaking subject; field research on the other hand locates both self and other in the same temporal order' (Pratt 1986: 33). The ethnographic present relies on third-person voice. Another technique is to precede a third person fieldwork monograph by a first person description of entering the group, which creates the authority necessary in an anthropological work. (Pratt noticed interestingly that this personal narrative is indebted to travelogues.) Saying 'I was there' is to emphasise the validity of the data, the fact that it is first-hand.

Similarly to anthropology, in *literary reportage* the authority and validity follow from the author's personal experience and her knowledgeability about the described reality. Third-person voice is a commonly used technique. Another technique consists in creating the impression that the reader is transported to another reality with the author as her guide. (Crapanzano about Goethe (1986: 66); also

Kapuscinski). Anthropologists sometimes also apply this technique - e.g. the famous introduction to Malinowski's *The Argonauts of the Western Pacific*:

'Imagine yourself suddenly set down surrounded by all your gear, alone on a tropical beach close to a native village, while the launch or dinghy which has brought you sails away out of sight. (...) Imagine further that you are a beginner, without previous experience, with nothing to guide you and no one to help you. (...) This exactly describes my first initiation into field work on the south coast of New Guinea. I well remember the long visits I paid to the villages during the first weeks; the feeling of hopelessness and despair after many obstinate but futile attempts had entirely failed to bring me into real touch with the natives, or supply me with any material. (...) Imagine yourself then, making first entry into the village...' (Malinowski 1922: 4).

In other words, both anthropology and literary reportage heftily juggle third- and first person accounting. (First-person convention does not however serve in this case the purpose of deconstructing one's own authority but rather building it up). Today many documentary filmmakers and literary reporters abandon third-person voice, since 'The literary devices of the passive third person cause statements to appear to be authorless, authoritarian, objective, and hence in keeping with the prevailing positivist/empiricist philosophies of science' (Ruby 2000: 159).

Another technique fulfils the role of enhancing generalisability and validity by suggesting that the author has extensive evidence. It consists in manipulating the 'size' of the subject, e.g. 'Nobody remembered a drought that horrible and that long-lasting.' From the point of view of scientific logic this sentence is false or lacks empirical grounds. However, it clearly and strongly illustrates for the reader the significance of the thing described and the nature of the phenomenon. Interestingly, anthropology does the same by generalising freely! This is suggested by Vincent Crapanzano in *Hermes' Dilemma* (1986). First he analyses the style of Catlin, a travelogue writer: 'Catlin rambles, generalises, simplifies, exaggerates, and embellishes' (1986: 56) and then suggests that interpretive academic authors such as Geertz do the same, e.g. in the sentence 'Everyone ignored us [Geertz and his wife] in a way *only a Balinese can do*' (Geertz 1973: 412 cited in Crapanzano 1986: 70, emphasis mine).

Another similar technique that establishes authority is 'quoting' a collective subject, e.g. '*What really matters is whether you have anything to put in the pot*, I was told by the residents of Kabul'. Quoting is the main method of showing that what we know is

based in the empirical - that indeed we *heard* what we quoted. Relating this to the collective subject (the residents of Kabul) is to convince the reader that all the people under study said it. The point is to make the impression that we are well acquainted with the people we interview. This technique deftly combines two mutually exclusive things: a quote, which endows what is said with the status of 'evidence', and the collective subject, which suggests representativeness.

Generally speaking the above techniques consist in 'playing sets (aggregations, classes)' to achieve a desired rhetorical/stylistic effect. The sets are increased or decreased and put together. This is distorting the scale of things, manipulating it. The realness of an individual voice/case is used and transposed on bigger sets. The question arises whether the ability to make a generalisation based on one event (e.g. Bouvier on the bowl of soup) exposes us to the danger of false generalisations. Ultimately we are restricted to trust the creator that her knowledge of the described reality is thorough and not based on a one-off experience.

Another writing technique that enhances validity but may be suspicious from empirical point of view is exploring intentions of the means/ends type. 'One-eyed Mulla Omar wasn't a king, and he needed the prophet's cloak precisely to become one' (Jagielski 2002: 22). This sentence is dubious because its empirical basis is unknown. We do not know whether this was Mulla Omar's actual intention. (In the case when the reporter writes about something that does not come directly from an interview with the informant, she frequently does not mention where she got this particular information/interpretation: whether she came up with it or heard it from people). However, the same problem occurs commonly in interpretive social sciences, to name Goffman for example.

8.2.3.2 Reporting of the methodological side of research

Usually non-fiction artists do not report research decisions and problems at all. We do not usually know how long the author/filmmaker spent with her subjects, how many representative subjects she got to know well, what challenges she encountered, etc. If one finds reporting of 'fieldwork' problems, it is of the kind 'I got stuck in the village surrounded by the army and could not reach the guerrillas'.

There are two exceptions to the above. Firstly, non-fiction writers use specific conventions to show they have gained the trust of respondents. The same conventions are used by qualitative researchers. Baumann for example writes: 'Fieldwork and the curiosity for local knowledge began to imprint their own stamp on my daily routines. Gradually, the "Railway Tavern" turned into a living-room away from home, and the gritty living-room of my house into a place where Narinder and Balbin, Joshua, Sukhbir, and Syd would drop in to have an illicit cigarette after school or a drink after work' (Baumann 1997:2). This significantly helps building up reliability.

Additionally, 'postmodern' documentaries picture not only the subject but also the process of how the film was made. The idea is that showing the relationship between the filmmaker and the subjects tells us more about the subjects, lets us criticise the message by deconstructing filmmaker's authority, and through this allows for evaluating the reliability of claims. In this sense such films are methodologically reflexive.

Chapter 9

Conclusion

The aim of this dissertation was to take seriously the problem of the scientific status of interpretive inquiry rather than to find a definite 'yes/no' answer to this problem. In the first part of this Conclusion I would like to review the findings of the thesis around this issue. I will then point towards some ongoing puzzles which arise from the arguments made within the thesis but which require further exploration in the future.

9.1 Overview of findings

One contribution of the thesis was to propose a framework within which the scientific status of interpretive social inquiry could be assessed. Here the aim was to offer an account of scientificity which would capture core features of science agreed on even by philosophers with quite different positions. I suggested that science was best characterised by: (a) the theoretical explanation of phenomena; (b) the pursuit of methodologically rigorous investigation, and (c) the professional assessment of the shortcomings of research by others.

Before investigating whether interpretive inquiry had these characteristics, I had to come to some account of interpretivism itself. This involved identifying the different 'European' and 'American' traditions of interpretive thought, with their respective focuses on culture and action. I argued that these traditions were usefully brought together in Fay's account of interpretive thought. However, one contribution of my discussion here was to point out certain ways in which Fay's analysis needs to be developed. The most important of these was the addition of the notion of 'horizontal' interpretation. This involves not a move to a 'lower' level of interpretation beneath that of the intentions of actors, such as that which Fay identifies as 'constitutive meanings'. Instead, it involves a broadening out of the interpretation horizontally,

and filling in those side-effects which are not explicitly intended but are nevertheless meaningful. I suggested that aspects of Geertz's analysis of Balinese cockfights had this horizontal character, although he lacked the category to express it in this way. In my view horizontal interpretation is an important notion that could be productively developed in the future. The final part of my initial account of interpretivism involved noting that although interpretive thought has been subject to well-known criticisms, particularly those offered by Bhaskar, Giddens, and Habermas, the question of the scientificity of interpretive inquiry itself tended to be touched on briefly, at best, by these writers.

Having characterised interpretive inquiry in this way, I moved on to consider whether it met the 'theory' part of the criteria for scientificity. In other words, I asked the question: 'can interpretations be considered theoretical'. The answer to this question was not an all or nothing matter as I argued that there are five aspects to scientific 'theory'. I came to these five by the same procedure mentioned above, namely identifying features of 'theory' that philosophers of science from different positions would agree were important. Two of these aspects turned out to be particularly important for understanding the scientificity of interpretive inquiry.

The first was the view that scientific theory is 'explanatory'. I argued that this is usually analysed in a way which biases the very notion of explanation towards the natural sciences, by focusing on *efficient* causes as explanations. I attempted to modify this by arguing that in the social sciences, it is important to identify *teleological* causes which capture people's abilities to meaningfully come to their own decisions, rather than seeing people's behaviour as fully (efficiently) caused by, say, pre-existing norms.

The other important aspect of 'theoretical' knowledge is that it involves the production of new understandings. This initially appeared a hard requirement for interpretive inquiry to meet, given that one major concern of interpretive inquiry is to capture the meanings of actors, rather than to impose meanings on their behaviour. However, I argued that it is possible to produce new understandings without correcting actors' knowledge. Some of these are already established, such as critical interpretivist production of new knowledge by engaging in a dialogue about the values and meanings of researcher and researched. However, I added to this

various other cases, arguing, for example, that although 'action' concepts cannot be factually incorrect, definitions of the situation can be, such as a belief about whether or not a bank is unsound. This means that the researcher can get to know both 'definitions of the situation' and the situation itself. I also noted that as well as 'theoretical' reasons for generating new categories in research, there are 'practical' ones, which result from the need to take categories out of their existing context and present them to readers unfamiliar with this context.

The ultimate conclusion of the theory discussion was that the two most important criteria for research being theoretical could be met by interpretive inquiry, albeit in slightly modified ways. Although there was still an issue with the possibilities of precision and generality of interpretive accounts, these could nevertheless be considered substantially theoretical in character.

Of course, an important feature of the thesis is that it not only considers 'philosophical' questions about the status of interpretive inquiry, but looks at actual case studies of interpretive research. In relation to the issue of theory, this meant looking at whether three pieces of qualitative research, by Hochschild, Baumann and Wagner, could be said to be 'theoretical' in the sense developed in this thesis. Again, the most important issues here were whether the interpretations developed in their work were explanatory, and whether they produced new knowledge. Starting from explanation, I argue that Hochschild's and Baumann's research does involve teleological explanation, and Wagner's research also has explanatory aspects. In relation to the issue of new knowledge, all three writers do attempt to produce this in some way. One productive aspect of their approaches is their attempt to map out the concepts and beliefs of a range of actors and draw these together in one account. This means that the research report does embody knowledge that no *single* actor had. Also, in the case of Wagner, we see him capturing the conceptualisations of one sector of the population, the homeless, and making these available to others who were not aware of them. 'New' knowledge claims were not always unproblematic, however, especially in the case of Baumann's work. Although some of his new technical concepts merely abstracted somewhat from the context, others, such as his use of 'culture' and 'community' diverged from the usages of his participants. Overall, then, the criteria developed in the analysis of 'theory' allowed us to identify both the strengths and weaknesses of actual research.

Having dealt with the first criterion of 'science', I subsequently moved on to Part III, where I discussed the further two criteria, which are 'research method' and 'quality control'. Firstly, I addressed the question of whether interpretive inquiry is really 'methodical', by carrying out a close reading of five qualitative research textbooks. I applied the most common understanding of 'scientific method', as a method leading to *valid* and *reliable* results.

My enquiry incorporated two perspectives. To start with, I studied what qualitative methodologists think about this type of inquiry - which elements of it lead to validity and reliability and which do not. I also paid attention to different 'tools' that are recommended as a remedy for those non-scientific elements. My analysis showed that interpretive research is a mix of scientific and non-scientific components - sampling, fieldnotes, long-term fieldwork and reporting evidence contribute to validity, but the lack of precise rules about how to carry out research procedures undermines the reliability of findings and their evaluation. Qualitative methodologists, however, postulate the use of some 'remedy tools' that can neutralise the distortion potentially resulting from the lack of codified rules, such as triangulation and member's check. Summing all that up, the scientificity of interpretive inquiry does not look excellent but on the whole the judgement should be positive rather than negative.

Following that, I studied what methodologists think about the *practice* of qualitative inquiry. Here the image was different and fairly negative. Practitioners seldom comply with the methodologists' recommendations, both during research and when writing up research reports. Overall, my study showed that the scientificity of interpretive inquiry is undermined more by the 'practice' of it than by the 'theory'.

Regarding the third and last criterion of 'scientificity' - *quality control* - it turned out from our discussion that it is again the 'practice' that throws negative light on this element of interpretive inquiry. I argued that the main culprit here is poor research reporting, but there are intrinsic problems as well. As for the latter, in the case of study replication there is a problem with ever-changing social reality, while in the case of peer review, the lack of agreement on the criteria of assessment results

from the lack of codified rules for carrying out research procedures. Additionally, I pointed at a number of smaller factors contributing to the problem.

Realistically I would not count on any improvement of research reporting practice. The fear of criticism is too powerful for the researchers to reveal their errors voluntarily. As it is never going to happen that an erroneous study will be appreciated, perhaps the best working solution would be to routinely employ the procedure of independent *auditing* when carrying out research (Lincoln and Guba's proposal). The practical difficulties would be significant though: who appoints the auditors, who pays for their work, etc⁹².

As in the part on theory, I complemented the analysis of methods with the three case studies. The function of this chapter was both illustrative and explanatory. I applied the same scheme to all studies, by dividing the discussion into two main sub-sections: 'reporting evidence' and 'reporting research'. As for reporting evidence, the study showed that our three researchers are relatively strong on this point. However, in the case of Hochschild we found that she rhetorically manipulated data presentation, while Baumann's weakness was a significant amount of second-hand evidence.

By contrast, research reporting was much less impressive. None of the researchers managed to do consistently well on all aspects of the research. Hochschild had obvious difficulties with sampling and justifying her procedures. Some of Baumann's evidence put into question the extent to which he was trusted by his respondents. Finally, Wagner (similarly to Hochschild) had problems with generalising his findings onto other homeless populations, as well as lacking a detailed description of research procedures.

Interestingly, our case studies have exposed one important difference between interpretive theory and practice. When discussing interpretive theory, we had a tendency to place the adequacy or truthfulness of the interpretation in the centre of attention. It turned out however that the practitioners of interpretive research worry

⁹² Interestingly, auditing does already take place commonly in one context, namely in PhD projects! Unfortunately, whoever obtains this title is automatically freed from the burden of supervision in her future research. This is good for the researcher's comfort, but not for the quality of social research.

more about sampling, gaining respondents' trust, etc. - problems that are common to all research rather than just to the interpretive approach. Thus an important conclusion from scrutinising case studies is that the interpretive approach does not bypass common research problems.

Finally, problems with the scientific status of interpretive inquiry pushed me to investigate the difference between findings achieved with this method and findings achieved by non-scientific researchers, particularly by non-fiction writers and filmmakers. This required a study of the latter's methods. Before doing that, however, I had to identify the characteristic features of various non-fiction literary forms (reportage, essay, and travelogue), and of the documentary film. I also compared them with features of interpretive social sciences. Here I pointed at elements such as 'exposing the general through the particular', 'humanistic exploration' of the subject matter, and 'horizontal interpretations'.

Having done this preliminary task, I studied the research methods used by literary reporters and documentary filmmakers. I claimed that both of them carry out their own fieldwork with observation and interviews (except in the case of 'conceptual' documentaries), which means that the core of their approach is the same as that of interpretive social scientists. However, I also noted that 'non-scientific' fieldwork tends to be shorter and less comprehensive than 'scientific' fieldwork.

We have therefore seen some differences testifying to the interpretive social sciences' superiority over non-scientific inquiry; however, these differences are not significant enough to automatically give credit to interpretive social scientific findings and refuse it to non-fiction arts. Also, our exploration has shown that one should be cautious about different elements when studying social scientific reports and when reading non-scientific books: the two have different aims and thus different strengths and weaknesses. We found that non-fiction art is better at formulating 'horizontal' interpretations. Interestingly, social sciences seem to be, on the one hand, suspicious about such interpretations (as they usually cannot be supported by direct evidence) and, on the other hand, attracted to them: for example, Geertz's article about Bali (heavy with horizontal interpretations) is a very popular, classic piece of social-scientific interpretive inquiry.

Our analysis of methods perhaps suggested that interpretive social scientists could use the existing pieces of non-fiction art at the stages preceding research, as hypotheses in need of a thorough empirical grounding.

9.2 Ongoing issues

Without claiming that my dissertation has managed either to fully complete the task set at the beginning or to provide correct answers, I would finally like to point to one area which certainly requires further exploration, and make some initial observations about it. My analyses have exposed the fact that social sciences, focused on polishing theory and methods, have significantly neglected the theory – method link. This is obvious when reading methodological textbooks, research reports, and social theory alike. This is perhaps natural, for there are many more experts either on theory or on methods than on both, but this is nonetheless unfortunate. That qualitative methods textbooks do not really discuss the relation between theory and qualitative research is symptomatic of the fact that this relation is underdeveloped *across* social sciences. Quantitative methodologists and non-interpretive theorists have an identical problem with finding the ‘meeting point’. In this section I would like to offer some comments on two aspects of the theory-method link. The first of these is the problematic link between *interpretive* theory and *qualitative* research, and the second of these is the problematic link between (any) *theory* and (any) *research*. What is stipulated by my argument is that *starting* with the link between theory and method would ultimately advance both of them more than if they are dealt with separately.

9.2.1 The problematic link between *interpretive* theory and *qualitative* research

Within the thesis, I have taken qualitative research as the mode of investigation that best exemplifies interpretive social science. I suggested that the best way to study interpretivism ‘in action’ is to look at qualitative research reports. In defence of these claims, it is certainly the case that qualitative researchers typically see themselves as inspired by ‘anti-positivist’ social theory, even if they do not always soundly locate themselves in an alternative position. Further, qualitative researchers clearly have *some* concern with issues of meaning that are highlighted by

interpretive thinkers, much more so than quantitative researchers. However, in this final chapter of the thesis I want to argue that the parallel between qualitative research and interpretive social science is not exact. During my reading of qualitative methodology textbooks I was able to infer a number of differences between their authors' view of the relationship between interpretive theory and qualitative methods, and interpretive theorists' view. Below I shall discuss these distinctions, and argue that this represents a partial divergence between interpretive social theory and qualitative research methodology.

9.2.1.1 Different directions

To begin with, views of the relationship between interpretive theory and qualitative methodology vary: qualitative methodologists and researchers claim that qualitative research is based on interpretivism (particularly on symbolic interactionism; see Flick 1998: 17-25, 144, Lofland 1971: 11), while interpretive theorists do not say explicitly that it is qualitative methods that should be applied.

To be more precise, theorists who advocate interpretive social science postulate 'understanding' but hardly go into specific methodological matters, such as face-to-face interaction or interviewing. Schutz's and Weber's method is not directly empirical; Winch and Taylor do not make methodological remarks at all; Blumer is probably the methodologically most conscious of all of them, but still failed to produce a study in which the (interpretive) theory - (qualitative) method link would be proved working. (For example, his paper 'Social unrest and collective protest' (1978) is not an empirical work).

9.2.1.2 Interpretivism, functionalism, structuralism?

Contrary to Flick's claim that symbolic interactionism is the theoretical basis of qualitative research, carrying out qualitative research does not necessarily mean that the researcher supports interpretive assumptions. In practice, those who hold structural or functional assumptions also often apply qualitative methods. In such a case, one explores the subjects' point of view only to find out about the hidden

functioning of institutions and structures by comparing what the subjects say with what they do. ('Treat what your subjects say as data, not as truth'). Meanings are treated not as ultimate drivers of conduct (and ultimate pieces of social analysis) but rather as resources or tools in the hands of groups having divergent interests and conflicts. Insight into meanings is necessary to have insight into social mechanisms⁹³. Gellner's study of Moroccan Berbers and their institution of *igurramen* (1987: chapter 2) exemplify this kind of relationship between non-interpretive social theory and qualitative research. Gellner found that the key to understanding this institution is not what Berbers say it is (and they truly believe in it), but what functions it serves and how it is structurally reproduced. These latter issues looked differently in reality and in Berbers' account of them, and therefore any attempts at making sense of subjects' 'point of view' would be spurious.

9.2.1.3 Classic or modern epistemology?

The epistemology supported by qualitative methodologists often seems not to accord with interpretivism. Methodologists send contradictory messages here. On the one hand they put stress on 'raw data' being independent from the researcher, and on data analysis as a different stage of the research process than data collection (Lofland 1971: 62). Such a conception of 'data' reminds one of classical epistemology, with the researcher being the neutral cognitive tool. Data is what is logged and not what the researcher makes of it (Lofland 1971: 44). Lofland says: 'try to capture raw behaviour' (1971: 64). Fieldnotes should distinguish 'raw data' from 'interpretation' (Seale 1999: 149). Data is understood as something that is 'true' or 'false' (Lofland 1971: 50), not as something that is better or worse interpreted. I believe this stance is against interpretivism because as a logical result, someone else could interpret the data: the roles of 'data collector' and 'data analyst' are claimed to be technically separable. This fits quite straightforwardly with quantitative methods and positivistic ideals.

But on the other hand methodologists say that data analysis happens at the same time as data collection (Lofland 1971: 131, Burgess 1984: 166, Seale 1999: 150).

⁹³ Note that this kind of structuralism (focused on conflict and interests) is very different to structuralism that looks for relationships between objective independent and dependent variables. The latter applies quantitative rather than qualitative methods.

This testifies to the fact that data is not raw – it is as much understood as it is perceived and measured. As a well known philosophical argument holds, one cannot precisely distinguish observation from interpretation (Seale 1999: 163). Observations are not theory-free, or 'raw'. This accords with interpretivism.

(This throws some light on the procedure of coding. It is claimed that coding takes place after fieldwork. But if interpretation happens in the field - if data analysis is not separate from data collection - the researcher already has codes in her mind and it is simply misleading to claim that coding is a separate stage.)

9.2.1.4 Interpretation - interviews - participant observation

I said earlier that qualitative methodologists see interpretivism as the theoretical basis of qualitative research. But they are not really clear on the details of this relationship and prefer safe, sketchy accounts. Flick for example says that three kinds of interpretive theories entail the need for qualitative research - symbolic interactionism, ethnomethodology, and structuralism/psychoanalysis - but does not really take it any further (1998: chapter 2).

That methodologists neglect the problem of interpretation transpires when they discuss particular qualitative techniques, most notably in-depth interviews and participant observation. For someone interested in the theory-method link the obvious question is how interpretation relates to interviews and participant observation. But methodologists do not take this up thoroughly. In the case of interviewing it is clear that research aims at understanding subjects' meanings. But what is participant observation for? Here methodologists and theorists differ significantly.

The latter are not explicit about this, but there is a strong suggestion that in their view participating in subjects' lives serves the understanding of meanings that are 'tacit', that cannot be researched or understood by the means of interviewing. Also, learning how to *do* things properly helps enormously with understanding concepts that subjects try to elucidate to the researcher in interviews. (Which reminds one of the Wittgensteinian argument about understanding as 'ability to go on' in real-life

practice). Therefore interpretivists see first-hand accounts as helping the correctness of interpretation rather than verifying facts. They see the researcher as trying to 'become a native' herself.

Methodologists see things differently: the art of being a qualitative researcher is as much about becoming an insider as it is about staying as an outsider at the same time. The researcher should not let herself fall into the trap of 'going native', for it would mean losing the distancing necessary for carrying out research (Lofland 1971: 14). Flick states that 'To lose critical external perspective and to unquestioningly adopt the viewpoints shared in the field is known as 'going native' (1998: 142). Burgess speaks in a similar tone: 'This idea of the researcher as a stranger rests on Simmel's notion of the individual who is free of commitments to those who are studied and therefore more likely to be objective' (1984: 23).

The methodologists point here at the fact that participant observation gives the researcher a chance to compare what is said in interviews with what is done in reality. Such comparison serves two purposes, and both of them fall under the motto 'treat what subjects say as data, not truth'.

Firstly, during participant observation the researcher has a chance to identify those meanings that subjects present as reality but that in fact are only their expressions of what reality should be like (Flick 1998: 134, 145). In other words, some meanings are 'reserved' for outsiders only (ibid 58), but since fieldwork is the process of transformation from the outsider to the insider, the researcher is able to identify and reject such 'official versions'. This purpose of participant observation, although overlooked by interpretive theorists, would not meet their dissent. Although at first sight the subjects' meanings are denied, this is misleading, since the purpose is learning about the 'real' meanings held by them.

But secondly, according to methodologists 'treating what subjects say as data, not truth' means that participant observation allows the researcher to find her way when subjects hold meanings that are truly believed to be true, but are nevertheless divergent from reality. (E. g. Gellner about Berbers and *igurammen*, 1987: chapter 2). Here we are not talking about 'official versions' for outsiders but rather about genuinely inadequate knowledge.

Since there is an assumption here that such inadequate meanings result from hidden social mechanisms and masked interests, this kind of participant observation does not accord with interpretive assumptions. This is a truly interesting finding: qualitative methodologists are eager to say that qualitative research is based on interpretivism, but actually they let anti-interpretive assumptions in by the back door. Take the following quotations:

[the central theme of research is] 'things are not (or are not only) what they seem to be' (Lofland 1971: 122).

'Because all social analysts play off what is ordinarily believed or felt to be known, an analysis is interesting only insofar as it departs from what is already seen as 'obvious'...'The perspective in this chapter helps explain the propensity we find among social analysts to coin new words and to imbue old words with new meanings'...'The goal is revitalized and rethought vision...' (Lofland 1971: 127)

'Generating accounts that do not challenge the common-sense evaluations of respondents is one of the easier tasks in social research' (Seale 1991: 69); '[Grounded theory] can aid in taking researchers beyond common-sense reporting of participants' categories' (Seale 1999: 96).

9.2.1.5 'The content of meaning' or 'causality and distribution of phenomena'?

However, qualitative methodologists differ most significantly from interpretive theorists in yet another respect: the understanding of the structure of the research problem. The 'research problem' means something else for qualitative methodologists than for interpretive theorists. The difference concerns *what* should be researched, both what object and what aspect of the object.

For interpretive theorists, alien meaning is the problem. This is why they are focused on face-to-face interactions and everyday practices. Theorists are more 'anthropological' than 'sociological': they research meanings held by people rather than social entities and phenomena.

For qualitative methodologists, the research problem is never as 'simple' as that. Understanding meanings is necessary, certainly, but it is only a step towards solving a more advanced 'puzzle'. Methodologists go far beyond that – firstly, they research

people to understand sociological entities, and secondly, they want to learn about the structure, causes, and consequences of these entities. The research problem is the researcher's construct, not having the counterpart in the subjects' vision of social reality. For example, Lofland (1971: chapter 6) talks about eleven 'thinking units' that the qualitative researcher should study:

Meanings

Practices

Episodes

Encounters

Roles

Relationships

Groups

Organisations

Settlements

Social worlds

Lifestyles

According to Lofland, 'the scale of social organisation is increasing as we move from unit to unit. Each new unit introduced contains units discussed prior to it rather than being separate from them' (1971: 71). What is striking is that interpretivists, unlike qualitative methodologists, miss out the whole middle of the list (roles, relationships, groups, organisations, settlements). But not only that: unlike theorists, methodologists do not stop at asking 'what does it mean' or 'what do they mean', they also ask other questions (Lofland 1971: chapter 7):

What is the unit's type?

What is the unit's structure?

What is the unit's frequency?

What are the unit's causes?

What are the unit's processes?

What are the unit's consequences?

What are people's strategies?

Note that all but one of these questions has units' as their object – only one asks about 'people'! It is virtually impossible to find a similar list of research questions in interpretive theoretical texts, such as Winch's *The Idea of a Social Science* or Schutz's *Phenomenology of the social world*. In fact, it seems as if the two groups spoke different languages.

The two questions that point to what I believe is the crucial difference between methodologists and theorists are 'What are the unit's causes?' and 'What is the unit's frequency?'

As for **causality**, it can be said that some methodologists use the language that emphasises 'conditions' and 'variables', that is not context-sensitive, and that aims at generalization. For example, Flick refers to grounded theory as embodying the following paradigm model: "causal conditions => phenomenon => context => intervening conditions => action/interaction strategies => consequences" (1998: 181). This clearly reminds one of a positivistic rather than an interpretive framework and shows that methodologists treat interpretation of meaning merely as one building block in the wall of research. Although this element is the prerequisite of the research, it does not by any means exhaust it. It is like the first step of the staircase. Further steps are 'conditions' and 'variables'. Flick is straightforward in this regard: 'Grouping the data according to the coding paradigm allocates specificity to the theory and enables the researcher to say: 'Under these conditions [listing them] this happens; whereas under these conditions, this is what occurs' (Flick 1998: 183).

Regarding **frequency**, it seems clear that theorists are far more interested in meaning as such than in its distribution; therefore what they typically understand by 'research' is a kind of 'community' study, where a given practice, a concept or a 'definition of the situation' is interpreted. Methodologists, by contrast, are more interested in the distribution of meaning than in its content. The aim of qualitative method is to find out about different views:

'Rapid social change and the resulting diversification of life worlds are increasingly confronting social researchers with new social contexts and perspectives. These are so new for them that their traditional deductive methodologies – deriving research questions and hypotheses from theoretical models and testing them against empirical evidence – are failing in the differentiation of objects' (Flick 1998: 2).

'The research issue is the distribution of perspectives on a phenomenon or a process. The underlying assumption is that in different social worlds or groups, differing views can be found' (Flick 1998: 184);
'Qualitative research demonstrates the variety of actors' perspectives' (Flick 1998: 6)⁹⁴;

Therefore methodologists take it for granted that the researcher is going to take on board the issue of generalizability, and that a good piece of research cannot omit this issue (even if it is a single case study, the researcher should try to elaborate upon its generalizability). It is supposed that the researcher is going to look at and compare a number of cases – be it groups, social roles, social positions, social actions, etc (Burgess 1984: 59). In other words, for interpretive theorists it is *meanings* that are unclear, whereas for qualitative methodologists it is *distribution/variation* of meanings that is unclear. I will call this latter kind of research 'variation studies' or 'problem studies', as it studies separate cases (communities, individuals), and will present it in contrast to 'community studies'.

(It should be noted that providing the reader with such a typology of various logics of qualitative enquiry is what textbooks of social theory and social research fail to do. Instead of that, they focus on particular research techniques, such as participant observation, long interviews, focus groups, etc).

'Community studies' mean that a particular group or structure of people is treated as *unique* and constitutes the subject of research. There are plenty of examples of 'community' research in interpretive social sciences, Malinowski's study of Trobrianders or Evans-Pritchard's fieldwork among the Azande as two examples.

In the case of 'problem studies' however, the subject of research is constructed by the researcher, which means that whoever is studied, is treated as a representative (rather than unique) case. The research does not refer to any particular existing community⁹⁵, even if it studies only one case/community (e.g. Whyte's study of an Italian slum in a big American city, 1955)⁹⁶.

⁹⁴ Flick says that qualitative research method is particularly applicable these days as the contemporary world is characterized by enormous plurality of ideas, opinions and practices. Quantitative, deductive method by its nature neutralizes all this diversity of meanings (Flick 1998: 2).

⁹⁵ It could be tempting to simply call 'community studies' 'anthropology' or 'ethnography', and 'problem studies' 'qualitative sociology'. But this is not where the real difference between anthropology and sociology lies. It happened historically that anthropologists in the past were

Goffman's study of 'asylums' (1968) is a good example of this type of research. It has as its subject a number of institutions (boarding schools, prisons, monasteries, mental health hospitals, etc) that, according to the researcher, possess common characteristic regarding the way they function and the processes their residents undergo. But it is obvious that members of these different institutions do not form one big community or social structure.

It is interesting that practitioners may often be inclined to make their life easier and narrow the research to one case study, which unintentionally falls in line with how theorists understand interpretive research. Methodologists oppose that: 'Many field studies appear to be located on a single site, e.g. factory. (...) However, [Strauss] maintains that a number of locations need to be selected for study in order that a broad perspective of the institution can be obtained' (Burgess 1984: 59).

Summing up, the parallels between interpretive theory and qualitative methodology are certainly not exact. Indeed, many qualitative methodologists see the purpose of research in a similar way to quantitative methodologists: research is about causes, conditions and distribution of phenomena. Only the means are different – understanding people rather than correlating numbers. This is not to say that there is no 'interpretive' element in qualitative research, and the presence of this element

researching well-defined communities while sociologists often were choosing constructed aggregates of people for their subject, but this difference is not relevant any longer. Consequently, the choice of techniques is not determined by the researcher's discipline: nowadays qualitative sociologists are as likely to use participant observation as anthropologists are likely to use other methods.

⁹⁶ The fact that qualitative researchers study either people or questions has significant consequences. In case of 'community studies', it means that it is practically very difficult to design research or even formulate research questions before entering the field:

'You can't specify the questions you're going to ask when you move into the community; you don't know how to ask questions yet. You can't define a sample; you don't know what the range of social types is and which ones are relevant to the topics you're interested in' (Agar cited in Burgess 1984: 34).

(This used to be particularly true in the past; it is difficult to imagine Malinowski having precise research questions in his mind when boarding the ship to Trobriand Islands. Nowadays there is so much literature on each community on earth that the researcher is likely to have at least a vague idea what she wants to focus on). 'Problem studies', on the other hand, allow for and in fact require elaborate research design. They also, however, come to logical difficulty with generalising their findings. They produce results that are putatively general – say, how 'total institutions' develop similar mechanisms to manage their residents – but these generalisations are always conditional, since individual cases and contexts vary (rarely any actual case fits perfectly the general image constructed by the researcher).

justifies the connections made in the thesis between this approach and interpretive theory. However, it is to acknowledge that qualitative research methodologists see interpretation as just one part of social science, rather than being pure interpretivists as such. It seems that for qualitative methodologists, qualitative research is a *better, more appropriate* tool than quantitative methods. For interpretive theorists however, understanding is the *only* tool possible.

9.2.2 The problematic link between (any) theory and (any) research

The lack of clarification of the relationship between interpretive theory and qualitative method does not however exhaust the list of sins of methodological textbooks (and social sciences in general). The theory – method link is problematic in an even more fundamental sense than ‘what method to link to what theory’: namely, it is problematic if interpretive theory needs research method *at all!* I have already mentioned this issue in chapter 4; I asked there the question ‘why not let people represent themselves?’ If we think about it, there can be no question more fundamental to be discussed at the opening sections of any research report or research textbook than ‘Why carry out research at all?’. Yet both methodologists and practitioners seem to take the reason for carrying out research for granted. There is no question ‘what is the research *for?*’, there is only ‘*how* to do the research?’. Particularly with methods textbooks, the impression is that their authors begin with: ‘So, you’ve decided to carry out qualitative research. We’ll help you to do it correctly’.

I believe that interpretive social sciences and qualitative methods textbooks should explain why interpretivism needs empirical research, instead of taking it for granted. By saying this, I do not suggest that interpretivism does not need empirical research – actually, I listed a number of practical, methodological, and theoretical reasons for carrying out research. I think it is important to develop this area of study further, and indeed the areas of exploration are both vast and exciting.

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