1 After method: an introduction

If this is an awful mess . . . then would something less messy make a mess of describing it?

> 'There is no use in trying,' said Alice; 'one can't believe impossible things.' 'I dare say you haven't had much practice,' said the Queen. 'When I was your age, I always did it for half an hour a day. Why, sometimes I've believed as many as six impossible things before breakfast.'
>
> (Lewis Carroll, *Alice in Wonderland*)

How might method deal with mess?

Look at the picture above, and at the question posed by the caption. This book is about that caption, and about what happens when social science tries to describe things that are complex, diffuse and messy. The answer, I will argue, is that it tends to make a mess of it. This is because simple clear descriptions don't work if what they are describing is not itself very coherent. The very attempt to be clear simply increases the mess. So the book is an attempt to imagine what it might be to remake social science in ways better equipped to deal with mess, confusion and relative disorder.

No doubt some things in the world can indeed be made clear and definite. Income distributions, global CO_2 emissions, the boundaries of nation states, and terms of trade, these are the kinds of provisionally stable realities that social and natural science deal with more or less effectively. But alongside such phenomena the world is also textured in quite different ways. My argument is that academic methods of inquiry don't really catch these. So what are the textures they are missing out on?

If we start to make a list then it quickly becomes clear that it is potentially endless. Pains and pleasures, hopes and horrors, intuitions and apprehensions, losses and redemptions, mundanities and visions, angels and demons, things that slip and slide, or appear and disappear, change shape or don't have much form at all, unpredictabilities, these are just a few of the phenomena that are hardly caught by social science methods. It may be, of course, that they don't belong to social science at all. But perhaps they do, or partly do, or should do. That, at any rate, is what I want to suggest. Parts of the world are caught in our ethnographies, our histories and our statistics. But other parts are not, or if they are then this is because they have been distorted into clarity. This is the problem I try to tackle. If much of the world is vague, diffuse or unspecific, slippery, emotional, ephemeral, elusive or indistinct, changes like a kaleidoscope, or doesn't really have much of a pattern at all, then where does this leave social science? How might we catch some of the realities we are currently missing? Can we know them well? *Should* we know them? Is 'knowing' the metaphor that we need? And if it isn't, then how might we relate to them? These are the issues that I open up in this book.

I don't have a single response to these questions. The book is intended as an opening rather than a closing. In any case, if much of reality is ephemeral and elusive, then we cannot expect single answers. If the world is complex and messy, then at least some of the time we're going to have to give up on simplicities. But one thing is sure: if we want to think about the messes of reality at all then we're going to have to teach ourselves to think, to practise, to relate, and to know in new ways. We will need to teach ourselves to know some of the realities of the world using methods unusual to or unknown in social science.

For example? Here are some possibilities. Perhaps we will need to know them through the hungers, tastes, discomforts, or pains of our bodies. These would be forms of knowing as embodiment. Perhaps we will need to know them through 'private' emotions that open us to worlds of sensibilities, passions, intuitions, fears and betrayals. These would be forms of knowing as emotionality or apprehension. Perhaps we will need to rethink our ideas about clarity and rigour, and find ways of knowing the indistinct and the slippery without trying to grasp and hold them tight. Here knowing would become possible through techniques of deliberate imprecision. Perhaps we will need to rethink how far whatever it is that we know travels and whether it still makes sense in other locations, and if so how. This would be knowing as situated inquiry. Almost certainly we will need to think hard about our relations with whatever it is we know, and ask how far the process of knowing it also brings it into being. And as a theme that runs through everything, we should certainly be asking ourselves whether 'knowing' is the metaphor that we need. Whether, or when. Perhaps the academy needs to think of other metaphors for its activities – or imagine other activities.

Such talk is new but at the same time it is not so new. There are many straws in the social science wind which suggest that it is starting to blow in directions such as these. Over the last two decades methods for the analysis of visual materials, performance approaches, and an understanding of methods as poetics or interventionary narrative have all become important. Students of anthropology, cultural studies and sociology have grappled with ways of thinking about and describing decentred subjectivities and the geographical complexities that arise when intimacy no longer necessarily implies proximity. There is also a developing sense that global flows are uncertain, unpredictable indeed chaotic in the mathematical sense. Many now think that ethnography needs to work differently if it is to understand a networked or fluid world. The sense that knowledge is contexted and limited has become widespread, and feminists have talked of situated knowledges while anthropologists have explored writing and receiving culture.[1] Market research, often more imaginative than academic social science, has developed methods such as tasting panels for understanding the non-cognitive and the ephemeral. And never to be outdone, management consultancy has adopted 'soft methods' for intervening in organisations by turning to dramatisations, enactments and performances.

So the world is on the move and social science more or less reluctantly follows. Agency is imagined as emotive and embodied, rather than as cognitive: the nature of the person is shifting in social theory and practice. Structures are imagined to be more broken or unpredictable in their fluidity. But at the same time, within social science, talk of 'method' still tends to summon up a relatively limited repertoire of responses. The collection and manipulation of certain kinds of quantitative data is emblematic for research methods in many parts of social science including much of sociology, economics, psychology, and human geography. The collection and manipulation of certain kinds of qualitative materials is iconographic in anthropology, cultural studies, science studies, and other parts of sociology and human geography. The quantitative/qualitative iconography – and its division – is built into many courses on research methods. In the English-speaking world it is unusual, perhaps impossible, to qualify as a degree-level social scientist without following such courses and learning the appropriate suites of methods. Indeed, national recognition of social science courses in the United Kingdom now demands that these include both quantitative and qualitative methods, though many students and teachers dislike such courses and find their content to be at best marginally relevant to the research process.

This book makes a sustained argument for a way of thinking about method that is broader, looser, more generous, and in certain respects quite different to that of many of the conventional understandings. It is therefore, in part, an attack on the limits set by such understandings. But there are various reasons why any such attack needs to be cautious. One is that 'social science method' is an encouragingly multi-headed beast. It is already variegated and heterogeneous in its claims, but even more so in its practices. Since I am arguing for greater methodological variety, existing variety is surely welcome wherever it is to be found – which is everywhere. This suggests, then, that the problem is not so much lack of variety in the *practice* of method, as the hegemonic and dominatory pretensions of certain versions or *accounts* of method. I will return to this question, that of the normativity of method, shortly.

Another reason for caution is that standard research methods are often important, not to say necessary. To take one notorious example, it was quantitative epidemiological research that established a plausible link between smoking and lung cancer.[2] Another example with a more social science flavour would be the many studies, again often quantitative, that have revealed strong relations between poor health and a range of social inequalities including poverty.[3] Or between vulnerability to disaster, and age, social isolation and poverty.[4] There are, to be sure, always complexities and ambivalences.[5] Nevertheless studies such as these have been the basis for major health education campaigns. And endless other success stories for standard methods, quantitative and qualitative, could be cited.

It cannot be the case, then, that standard research methods are straightforwardly wrong. They are significant, and they will properly remain so. This is why I say that I am after a broader or more generous sense of method, as well as one that is different. But to talk of difference is indeed to edge towards criticism. As I have suggested above, I want to argue that while standard methods are often extremely good at what they do, they are badly adapted to the study of the ephemeral, the indefinite and the irregular. As I have just suggested, this implies that the problem is not so much the standard research methods themselves, but the normativities that are attached to them in discourses about method. If 'research methods' are allowed to claim methodological hegemony or (even worse) monopoly, and I think that there are locations where they try to do this, then when we are put into relation with such methods we are being placed, however rebelliously, in a set of constraining normative blinkers. We are being told how we must see and what we must do when we investigate. And the rules imposed on us carry, we need to note, a set of contingent and historically specific Euro-American assumptions.[6]

Here the problem is not that our research methods (and claims about proper method) have been constructed in a specific historical context. *Everything* is constructed in a specific historical context and there can be no escape from history. Rather it is that they, or at least their advocates, tend to make excessively general claims about their status. The form of argument is often like this (think, for instance, about rules for statistical sampling, or avoiding leading questions in interviews). 'If you want to understand reality properly then you need to follow the methodological rules. Reality imposes those rules on us. If we fail to follow them then we will end up with substandard knowledge, knowledge that is distorted or does not represent what it purportedly describes.' There are two things I want to say in response to such suggestions about the importance of methodological rule-following. The first is counter-intuitive. It is that methods, their rules, and even more methods' practices, not only describe but also help to *produce* the reality that they understand. I will carefully explore the reasons for making this suggestion in due course. However, for the moment let me simply note that there is a fair amount of heavyweight work on the history of science and social science that makes precisely this argument. Perhaps again counter-intuitively, I will also say that if methods tend to produce the reality they describe, then this may be, but is not necessarily, obnoxious. Again I will return to this argument at some length in due course. But what is important now is to note that if these two claims are right then they have profound implications for our understanding of the nature of research.

There is a further and more straightforward point to be made. This is that claims about the general importance of methodological rules also tend to get naturalised in social science debate. *Particular* sets of rules and procedures may be questioned and debated, but the overall need for proper rules and procedures is not. It is taken for granted that these are necessary. And behind the assumption that we need such rules and procedures lies a further range of assumptions that are also naturalised and more or less hidden. These have to do with what is most important in the world, the kinds of facts we need to gather, and the appropriate techniques for gathering and theorising data. All of these, too, are naturalised in the common sense of research. Yes, things are on the move. Nevertheless, the 'research methods' passed down to us after a century of social science tend to work on the assumption that the world is properly to be understood as *a set of fairly specific, determinate, and more or less identifiable processes*.

Within social science conventions, which are the best methods (and theories) for exploring those somewhat specific processes? This is a matter for endless debate. Neo-Marxists discover world systems, or uneven developments, or they theorise regulation. Foucauldians discover systems of governmentality. Communitarians discover communities and the need for informal association and responsibility. Feminists discover glass ceilings, cultural sexisms, or gendering assumptions built into scientific and social science method. As a part of this, social science common sense also assumes that society changes. Indeed this is one of the rationales for social science: that it can participate in and guide that change. (Witness the health-inequalities finding mentioned above, but also the larger political inheritance of Euro-American social theory.) But, overall, the social is taken to be fairly definite. Such is the framing assumption: that there are definite processes out there that are waiting to be discovered. Arguments and debates about the character of social reality take place *within* this arena. And this is what social science is meant to do: to discover the most important of those definite processes. But this is precisely the problem: *this is not necessarily right*. Accordingly, it indexes the broadening shift that I want to make. The task is to imagine methods when they no longer seek the definite, the repeatable, the more or less stable. When they no longer assume that this is what they are after.

So what are those elusive realities? This is for discussion. I have my own sense of what it is that might be important and this informs my argument. However, I do not want to legislate a particular suite of research methods. To do so would be to recommend an alternative set of blinkers. Instead I argue that the kaleidoscope of impressions and textures I mention above reflects and refracts a world that in important ways cannot be fully understood as a specific set of determinate processes. This is the crucial point: what is important in the world including its structures is not simply technically complex. That is, events and processes are not simply complex in the sense that they are technically difficult to grasp (though this is certainly often the case). Rather, they are also complex because they *necessarily exceed our capacity to know them*. No doubt local structures can be identified, but, or so I want to argue, the world in general defies any attempt at overall orderly accounting. The world is not to be understood in general by adopting a methodological version of auditing.[7] Regularities and standardisations are incredibly powerful tools but they set limits. Indeed, that is a part of their (double-edged) power. And they set even firmer limits when they try to orchestrate themselves hegemonically into purported coherence.

The need, then, is for heterogeneity and variation. It is about following Lewis Carroll's queen and cultivating and playing with the capacity to think six impossible things before breakfast. And, as a part of this, it is about creating metaphors and images for what is impossible or barely possible, unthinkable or almost unthinkable. Slippery, indistinct, elusive, complex, diffuse, messy, textured, vague, unspecific, confused, disordered, emotional, painful, pleasurable, hopeful, horrific, lost, redeemed, visionary, angelic, demonic, mundane, intuitive, sliding and unpredictable, these are some of the metaphors I have used above. Each is a way of trying to open space for the indefinite. Each is a way of apprehending or appreciating displacement. Each is a possible image of the world, of our experience of the world, and indeed of ourselves. But so too is their combination. What this might mean in practice will be explored below. But together they are a way of pointing to and articulating a sense of the world as an unformed but generative flux of forces and relations that work to produce particular realities.

The world as a 'generative flux' that *produces* realities? What does this mean? I can only tackle this question bit by bit, and any answer will be incomplete. Nevertheless, in this way of thinking the world is not a structure, something we can map with our social science charts. We might think of it, instead, as a maelstrom or a tide-rip. Imagine that it is filled with currents, eddies, flows, vortices, unpredictable changes, storms, and with moments of lull and calm. Sometimes and in some locations we can indeed make a chart of what is happening round about us. Sometimes our charting helps to produce momentary stability. Certainly there are moments when a chart is useful, when it works, when it helps to make something worthwhile: statistics on health inequalities. But a great deal of the time this is close to impossible, at least if we stick to the conventions of social science mapping. Such is the task of the book: to begin to imagine what research methods might be if they were adapted to a world that included and knew itself as tide, flux, and general unpredictability.

This will take us, and uncertainly, far from a conventional discussion of method, but also from our common-sense assumptions about the character of the world and how we come to know it. And this in turn means that it is also important to avoid some possible misunderstandings:

- First, as I have tried to insist above, I am not saying that there is no room for conventional research methods. Such is not at all the point of my argument.
- Second, and more generally, I am not saying that there is no point in studying the world. I am not recommending defeatism. On the contrary, the task is to reaffirm a reshaped set of commitments to empirical and theoretical inquiry. The issue is: what might social science inquiry look like in a world that is an unformed but generative producer of realities? What shapes might we imagine for social science inquiry? And, importantly, what might responsibility be in such a world?
- Third, I am not recommending political quietism. I shall have a lot more to say about politics below, but the basic point is simple. Since social (and natural) science investigations interfere with the world, in one way or another they always make a difference, politically and otherwise. Things change as a result. The issue, then, is not to seek disengagement but rather with how to engage. It is about how to make good differences in circumstances where reality is both unknowable and generative.
- Finally, what I am arguing is not a version of philosophical idealism. I am not saying that since the world defies any overall attempt to describe and understand it, we can therefore realistically believe anything about it we like. I also discuss this much more fully below, but everything I argue assumes that there is a world out there and that knowledge and our other activities need to respond to its 'out-thereness'. It is a world, as I've

> suggested, that is complex and generative. I will argue that we and our methods help to generate it. But the bottom line is very simple: believing something is never enough to make it true.

As is obvious, this argument strays into philosophy. Like others working in the discipline of science, technology and society (STS) I have explored how science is practised in laboratories, and it is difficult to do this without tripping over the writings of philosophers of science and social science. Again, like many others in STS, I do not share many of the most widespread philosophical and common-sense understandings about the nature of scientific (and social science) inquiry. To a first approximation, STS argues that science is a set of practices that are shaped by their historical, organisational and social context. It further argues that scientific knowledge is something that is constructed within those practices.[8] Thus though they draw on history and philosophy of science, these kinds of arguments also tread on a lot of philosophical toes. But here we need a health warning. Just as this is not a book on method, conventionally understood, neither is it a text in philosophy of science or social science, conventionally understood. The proof of new ways of thinking about method, or so I take it, lies in their results and their outcomes, rather than in their antecedents. Nevertheless, the arguments that I develop indeed have philosophical antecedents. They draw on parts of the philosophy of science but also on philosophical romanticism and (what is perhaps its contemporary expression) post-structuralism. A few words on these two traditions.

Social science has struggled with the inheritance of philosophical romanticism for at least 200 years (at the same time wrestling with its mirror image, the classical commitment to reason and inquiry, embedded in the Enlightenment project (Gouldner 1973)). I will touch on a few of the relevant arguments later. For now it is simply useful to note that many notable social theorists (to name but a few, Karl Marx, Georg Simmel, Max Weber, Georg Lukács, George Herbert Mead and Walter Benjamin) incorporated important elements of philosophical romanticism in their accounts of the world. This means that in different ways they responded to the idea that the world is so rich that our theories about it will always fail to catch more than a part of it; that there is therefore a range of possible theories about a range of possible processes; that those theories and processes are probably irreducible to one another; and, finally, that we cannot step outside the world to obtain an overall 'view from nowhere' which pastes all the theory and processes together.

A related set of intuitions informs such post-structuralist writers as Michel Foucault, Gilles Deleuze and Jacques Derrida. Instead of assuming that there is a specific external reality upon which we can ground our efforts to know the world, such writers mobilise metaphors such as flux to index the sense that whatever there is in the world cannot be properly or finally caught in the webs of inquiry found in science and social science (or indeed any other form of knowing). And then they talk of 'discourse', 'deferral' or 'episteme' to point to the methodological efforts to make and know limited moments in the fluxes that make up reality.

Philosophical romanticism and post-structuralism have informed some versions of social science (and especially qualitative) method. They have inspired various empirically grounded styles of investigation in sociology, anthropology, cultural studies, feminism, human geography, and science, technology and society (STS). It is, for instance, possible to go to *verstehende* sociology, symbolic interactionism, to anthropology and cultural studies of difference, to post-colonialism, to actor-network theory, or to parts of feminist technoscience studies to see what these intuitions might mean in methodological practice.[9] But even so, as I have noted, more often social (and still more natural) science 'method talk' connotes something quite different – that is a particular version of rigour. This is the idea that it is important to obtain the best and technically robust possible account of reality, where reality is assumed, as I have suggested above, to be a pretty determinate set of discoverable entities and processes. That such is what the world *is*: a set of possibly discoverable processes.[10]

My aim is thus to broaden method, to subvert it, but also to remake it. I would like to divest concern with method of its inheritance of hygiene. I want to move from the moralist idea that if only you do your methods properly you will lead a healthy research life – the idea that you will discover specific truths about which all reasonable people can at least temporarily agree. I want to divest it of what I will call 'singularity': the idea that indeed there are definite and limited sets of processes, single sets of processes, to be discovered if only you lead a healthy research life. I also want to divest it of a commitment to a particular version of politics: the idea that unless you attend to certain more or less determinate phenomena (class, gender or ethnicity would be examples), then your work has no political relevance. I want to subvert method by helping to remake methods: that are not moralist; that imagine and participate in politics and other forms of the good in novel and creative ways; and that start to do this by escaping the postulate of singularity, and responding creatively to a world that is taken to be composed of an excess of generative forces and relations.

To do this we will need to unmake many of our methodological habits, including: the desire for certainty; the expectation that we can usually arrive at more or less stable conclusions about the way things really are; the belief that as social scientists we have special insights that allow us to see further than others into certain parts of social reality; and the expectations of generality that are wrapped up in what is often called 'universalism'. But, first of all we need to unmake our desire and expectation for security.

Method, as we usually imagine it, is a system for offering more or less bankable guarantees. It hopes to guide us more or less quickly and securely to our destination, a destination that is taken to be knowledge about the processes at work in a single world. It hopes to limit the risks that we entertain along the way. Method, then, may allow us to learn that particular hypotheses are wrong: this is an important part of methodology's self-presentation, and it has important merits.[11] It may also allow us to learn that *particular methods* are flawed. But as a framework, method *itself* is taken to be at least provisionally secure. The implication is that method hopes to act as a set of short-circuits that link us in the best possible way with reality, and allow us to return more or less quickly from that reality to our place of study with findings that are reasonably secure, at least for the time being.[12] But this, most of all, is what we need to unlearn. Method, in the reincarnation that I am proposing, will often be slow and uncertain. A risky and troubling process, it will take time and effort to make realities and hold them steady for a moment against a background of flux and indeterminacy.

There is a beautiful book by David Appelbaum called *The Stop* (1995). This contrasts the quickness of seeing with the groping of the blind person. It seems to us, he says, that the blind person lacks vision. No doubt this is right. But Appelbaum's argument is that the groping, the halting progress with a stick, also has its privileges. The blind person sees what the person with vision does not, because she moves tentatively. Because instead of making use of direct lines of vision to distant objects, she gropes her way across the terrain. But Appelbaum argues that in the groping there is a kind of poise, what he calls a 'poised perception'. This is:

> a gathering unto a moment of novelty. It is perception of traces of hidden meaning. It is the perception that belongs to the stop.
>
> (Appelbaum 1995, 64)

Understood in this way, blindness implies a range of sensitivities and sensibilities to that which passes the sighted person by. Blindness is no longer a loss. Or if it is a loss, it is also a gain. I take my lesson here from Appelbaum. This is a book about method – and reality – that is also about the stop. The stop slows us up. It takes longer to do things. It takes longer to understand, to make sense of things. It dissolves the idea, the hope, the belief, that we can see to the horizon, that we can see long distances. It erodes the idea that by taking in the distance at a glance we can get an overview of a single reality. So the stop has its costs. We will learn less about certain kinds of things. But we will learn a lot more about a far wider range of realities. And we will, or so I also argue, participate in the *making* of those realities.

Method? What we're dealing with here is not, of course, just method. It is not just a set of techniques. It is not just a philosophy of method, a methodology. It is not even simply about the kinds of realities that we want to recognise or the kinds of worlds we might hope to make. It is also, and most fundamentally, about a way of being. It is about what kinds of social science we want to practise. And then, and as a part of this, it is about the kinds of people that we want to be, and about how we should live (Addelson 1994). Method goes with work, and ways of working, and ways of being. I would like us to work as happily, creatively and generously as possible in social science. And to reflect on what it is to work well.

Appelbaum writes that 'the danger of method is that it gives over to mechanical replacement' (Appelbaum 1995, 89). 'Mechanical replacement' has nothing to do with machines. Rather it has to do with the automatic. My hope is that we can learn to live in a way that is less dependent on the automatic. To live more in and through slow method, or vulnerable method, or quiet method. Multiple method. Modest method. Uncertain method. Diverse method. Such are the senses of method that I hope to see grow in and beyond social science.

The pleasures of reading

Why do the books fall into two heaps, the novels on the one hand, and the academic volumes on the other? Why do the novels get themselves read at the weekends, or on holidays, or in the ten minutes before falling asleep at night? Why do the work-books get read in the day, at prime times?

Then again, another kind of question. *How* do these different kinds of books get read? Why is it that reading a novel brings pleasure not only for its plot and its characterisation, but also for its use of words? If we reflect on the sheer pleasure of reading a well-crafted novel, one in which the words are carefully chosen, put together just right, then we may ask the question: what is the pleasure in reading an academic book? And how many academic books are really well written at the word-level? At the level of crafting?

How these two kinds of books get read is often, perhaps mostly, very different. If we read novels we read them, often, as an act in itself, for the pleasure of the read, the 'good read' of the airport novel, or the crafted text of a Barbara Kingsolver or a Penelope Lively or a J.M. Coetzee. They are pleasures in themselves, intrinsic. Whereas I guess we do not often read an academic book for the pleasure of the read itself, the pleasure, so to speak, of the journey. Rather we read it for the destination, where it will take us, where we will be delivered. We take pleasure, to be sure, in a well-crafted academic book – the ones that come to mind for me are, perhaps, mostly by historians. But the interest is different.

Perhaps, then, the distinction is between means and ends. Novels are ends in themselves, worth reading in their own right. Academic writings are means to other ends. The textures along the way, the actual writing, these are subordinate to those ends. It may be more agreeable to travel first class than third, but in the end we all arrive at the same destination.

What difference would it make if we were instead to apply the criteria that we usually apply to novels (or even more to poetry) to academic writing? Wouldn't the library shelves empty as the ranks of books disqualified themselves? What would we be left with? And, more importantly, if we had to write our academic pieces as if they were poems, as if every word counted, how would we write differently? How much would we write at all?

Of course we would need to imagine representation in a different way. Poetry and novels wrestle with the materials of language to *make* things, things that are said to be imaginary. It is the making, the process or the effect of making, that is important. The textures along the way cannot be dissociated from whatever is being made, word by word, whereas academic volumes hasten to describe, to refer to, a reality that lies outside them. They are referential, ostensive. They tell us how it is out there.

How, then, might we imagine an academic way of writing that concerns itself with the quality of its own writing? With the *creativity* of writing? What would this do to the referent, the out-thereness?

STS

Arguments from the discipline of STS (science, technology and society) will play an important part in the argument of this book. Thus, though I weave together a number of sources, the shifting ground on which I stand comes first and foremost from STS. A few words, then, on STS, and its role in the argument.

STS is the study of science and technology in a social context. The basic intuition is simple: it is that scientific knowledge and technologies do not evolve in a vacuum. Rather they participate in the social world, being shaped *by* it, and simultaneously *shaping* it. Some of the implications of this intuition are uncontentious. Who is going to deny the social significance of genomics or informatics, or try to argue that these are not shaped by social and economic concerns? Other implications are less obvious and much more controversial. Is the structure of current scientific reasoning patriarchal? Is the content of scientific knowledge at the same time essentially social? Does scientific theory and practice necessarily carry and enact social and political agendas? Is the distinction between scientific inquiry and knowledge on the one hand, and other forms of inquiry or experience on the other, a social contingency? Is the knowledge produced by scientists more or less contexted and local rather than possibly universal? Does scientific practice help to produce the objects that it describes and explains? Many STS scholars would answer 'yes' to each of these questions. But as is obvious, all, in greater or lesser degree, run counter both to common-sense and to many versions of the received philosophical wisdom.

So as I deploy the STS arguments (together with related positions developed in such disciplines as anthropology, sociology and cultural studies) these are going to take us to some more or less unfamiliar and sometimes anxiety-provoking territory. But this is precisely my object. STS work over a period of thirty years has made a series of strong and counter-intuitive claims about the character of science. These have profound potential implications for the conduct of natural science. But if they have implications for natural science, then so, too, they are potentially important for social science. So it is a source of some frustration that those arguments – and their implications – have not been more important in social science and its thinking about method and methodology. And such, to be sure, is the object of this book. I work through some of the STS findings in the context of social science, and in doing so attempt to destabilise some standard versions of social science wisdom. All this means that my argument moves between natural and social science. There are certainly important distinctions between the two, but here, for the most part, I trade upon their commonalities. These, I take it, are instructive and important. And this is where I start.

The argument outlined

In Chapter 2 I offer an account of a laboratory ethnography described by STS writers Bruno Latour and Steve Woolgar. The issue is: how is scientific knowledge produced? Their answer is: in a more or less messy set of practical contingencies. But what is most startling is their additional claim that in its practice science *produces* its realities as well as describing them. This is the cornerstone of my own argument. It runs counter to common-sense, and is also easily misunderstood, since it sounds as if it is a way of saying that 'anything goes'[13] and one can believe what one wants. But this isn't right. If realities may be built, Latour and Woolgar also show that it is difficult to do this. In practice bright ideas are very far from realities. And it is the word 'practice' that is the key. If new realities 'out-there' and new knowledge of those realities 'in-here' are to be created, then practices that can cope with a hinterland of pre-existing social and material realities also have to be built up and sustained. I call the enactment of this hinterland and its bundle of ramifying relations a 'method assemblage'.

But do those practices narrow down, converge, to make a single reality? In Chapter 3, I follow an account by Annemarie Mol of the practices of medical diagnosis, and argue that they don't. She shows that different practices tend to produce not only different *perspectives*, but also different *realities* – even for what otherwise might seem to be single-disease conditions. She calls this 'the problem of multiplicity'. But if there are different realities, then lots of new questions arise. How do they relate? How do we choose between them? How should we choose between them? One possibility is that we need what Mol calls an *ontological politics*. If truth by itself is not a gold standard, then perhaps there may be additional *political* reasons for preferring and enacting one kind of reality rather than another. Such, at any rate, is a possibility.

If realities made in methods are multiple, then do they have to be definite and fixed in form? Chapter 4 answers this question by saying 'no'. Using two more case studies – the treatment of alcoholic liver disease in the UK NHS, and a water pump in Zimbabwe – it shows how realities may change their shape or become more or less indefinite. But is this okay? Are the bush-pump or alcoholic liver disease not just definite objects that we haven't quite understood? Is our vagueness a sign of methodological failure? The answer is, perhaps, but I don't think so. Instead I argue that (social) science should also be trying to make and know realities that are vague and indefinite *because much of the world is enacted in that way*. In which case it is in need of a broader understanding of its methods. These, I suggest, may be understood as methods assemblages, that is as enactments of relations that make some things (representations, objects, apprehensions) present 'in-here', whilst making others absent 'out-there'. The 'out-there' comes in two forms: as manifest absence (for instance as what is represented); or, and more problematically, as a hinterland of indefinite, necessary, but hidden Otherness.

But if this is so, then how might we know about the indefinite or the non-coherent? Clearly we cannot know the indefinite without limit. It ramifies on for ever. But at least we can explore the issue, and this is the topic of Chapter 5 where I consider the character of allegory as a method for non-coherent representation. Again I work through cases. I argue that a rundown set of premises can be understood as an allegory for health-service disorganisation because it is tolerant of realities that are multiple, diffuse and non-coherent. Again, following work by Vicky Singleton, I suggest that the UK cervical smear programme is held together as much by inconsistency as consistency – that is by the ubiquitous practice of the allegorical. Finally, I argue that the horrors of a train collision can also be understood as a performative allegory for railway disorganisation – but also of pain and suffering. All of these are modes of knowing, methods assemblages, that do not produce or demand neat, definite, and well-tailored accounts. And they don't do this precisely because the realities they stand for are excessive and in flux, not themselves neat, definite, and simply organised. But this does not mean that they are not good methods.

So method assemblage works in and 'knows' multiplicity, indefiniteness, and flux. But how might we think about this? What *are* methods – or methods assemblages? In Chapter 6 I explore this issue by discussing materials from three very different sites of inquiry: management techniques, sociological ethnography, and religious experience. I argue that all of these are method assemblages because they detect, resonate with, and amplify particular patterns of relations in the excessive and overwhelming fluxes of the real. This, then, is a definition of method assemblage: it is a combination of reality detector and reality amplifier.

Chapter 7 returns to the question of truth and asks what follows if this is no longer a methodological gold standard. If it is no longer the only 'good'. Politics, we have seen, is another, 'good', but there are further possibilities. Others might include the aesthetic (beauty), and the spiritual or the inspirational. I develop this argument by looking at forms of method assemblage where there is little attempt to distinguish between such goods. Using materials drawn from Australian Aboriginal practices and the writing of Helen Verran and David Turnbull, I show that few Euro-American assumptions about representation and reality hold in Aboriginal cosmology. There is no universal reality. Realities are not secure but instead they have to be practised. And the world is not passive, waiting to be seen by people. Aboriginal cosmology both puts together goods that are usually held apart in Euro-American metaphysics, and it is explicit that all is enactment. To say this is not to say that science and social science practice should follow the Aboriginal model – but it shows once more that the metaphysics of method are, in principle, endlessly variable.

The argument of the book raises a series of more or less radical questions about method, and I review these in Chapter 8. I press for a more generous, and inclusive approach to method, and as a part of this briefly touch on a series of destabilising questions about the character and role of academic inquiry, and about knowledge more generally. This is because the division of labour which founds the academy, between the good of truth and such other goods as politics, aesthetics, justice, romance, the spiritual, inspirational and the personal, is in the process of becoming unravelled. This implies that we need to look not only at our practices but also at our institutions if we are to create methods that are quieter and more generous. Perhaps the model that we need, or one of the models, is that of 'partial connection' (Strathern 1991). At any rate, if the argument works at all then we need to find ways of living in uncertainty. The guarantees, the gold standards, proposed for and by methods, will no longer suffice. We need to find ways of elaborating quiet methods, slow methods, or modest methods. In particular, we need to discover ways of making methods without accompanying imperialisms.

INTERLUDE:
Notes on empiricism and autonomy

Euro-American common sense tends to the reflex that it is important to stipulate the conditions under which science can be properly carried out. This is because scientific inquiry needs to be protected from the distortions that might come from outside. The idea that science needs to be protected in this way is often (though not always) linked to 'empiricism' and to 'positivism'. *Empiricism* is a family of traditions in the philosophy of science which argue that scientific truths grow out of, and are properly generalised from, appropriate empirical observations. *Positivism* is another, closely related, set of traditions which argue that scientific truths are rigorous sets of logical relations or laws that describe the relations between (rigorous) empirical descriptions.

In the social sciences, empiricism and especially positivism are now usually seen negatively. Raymond Williams comments that positivism is a 'swear word by which nobody is swearing' (1988, 239). No doubt this is right. However their basic intuitions are widespread in Euro-American common-sense thinking about science and social science. It is commonly assumed that observations should be unbiased and representative, and that theories should be logical and consistent both with one another, and with observation.

The sociology of science, which was invented by Robert K. Merton (1973a; 1973b) started out on this assumption. There were good reasons for Merton's intuitions. He was writing at the time of Nazi racial science, and Stalinist Lysenkoism (which argued that plants could transmit and inherit acquired characteristics). He argued that these lethal lapses from proper scientific standards were a consequence of the failure to insulate science from political agendas in totalitarian societies. Scientists' capacities for unbiased observation and logical thinking were being eroded by these agendas. Instead science should, he said, be protected by a 'scientific ethos'. First, it should be *universalist*, testing its ideas in terms of: '*preestablished impersonal criteria*: consonant with observation and with previously confirmed knowledge' (Merton 1973a, 270). This meant that the race, gender, politics, or national origins of the scientist were not relevant to truth. Second, it should be *disinterested*. Scientific claims should be assessed independently of local social, economic, political, and personal interests. Third, it should be *sceptical*. Scientists should not take things on trust. (Merton talked of *organised scepticism*.) And finally, it should be *communal*. By this Merton meant that scientists should always publish their results: that science would best advance if it published its findings.

Merton's vision of science throws up some problems. (It is, for instance, difficult to see how scientists are consistently sceptical: in practice if they are to be effective they have to take a lot on trust.) And there are problems, too, with empiricism and positivism (we will encounter some of these below). But this is a convenient place to start because Merton is very clear that anything that interferes with 'empirically confirmed and logically consistent statements of regularities' (1973a, 270) is illegitimate because it detracts from the proper empirical and logical basis of truth. Merton's theory, then, is that research needs to be *disentangled* from the social and the psychological, and entangled solely with logic, with facts, and with methods for determining the facts.[14]

This is a language to which we will return. Different visions of science propose that it should be (or it is) entangled and disentangled with the world in different ways. Empiricism offers one recipe for this. It tells us that science (and social science too) have to be autonomous if they are to work properly. They should be disentangled from the social.

2 Scientific practices

> . . . tools only exist in relation to the interminglings they make possible or that make them possible. The stirrup entails a new man–horse symbiosis that at the same time entails new weapons and new instruments. Tools are inseparable from symbioses or amalgamations defining a Nature–Society machinic assemblage. They presuppose a social machine that selects them and takes them into its 'phylum': a society is defined by its amalgamations, not by its tools. Similarly, the semiotic or collective aspect of an assemblage relates not to a productivity of language but to regimes of signs, to a machine of expression whose variables determine the usage of language elements. These elements do not stand on their own any more than tools do.
>
> (Deleuze and Guattari 1988, 90)

> A proposition, contrary to a statement, includes the world in a certain state. . . . Thus a construction is not a representation from the mind or from the society about a thing, an object, a matter of fact, but the engagement of a certain type of world in a certain kind of collective.
>
> (Latour 1997, xiii–xiv)

Inscription devices and realities

In October 1975 a young French philosopher arrived at the Salk Institute in San Diego. Called Bruno Latour, he later wrote that his 'knowledge of science was non-existent; his mastery of English was very poor' (Latour and Woolgar 1986, 273). He watched the work of the Salk Institute endocrinologists for nearly two years and then wrote a book about it with sociologist of science Steve Woolgar. Called *Laboratory Life*, this appeared in 1979 and, with books by one or two others,[15] helped to create a new field, that of the *ethnography of science*.

As we move through the present book we will look over the shoulders of ethnographers as they visit scientific laboratories, clinics, hospitals, religious ceremonies and managerial meetings. We will also watch the work of social scientists – and others – as they produce knowledge in practice. So what do ethnography of knowledge practices tell us? The answer is that ethnography lets us see the relative messiness of practice. It looks behind the official accounts of method (which are often clean and reassuring) to try to understand the often ragged ways in which knowledge is produced in research. Importantly, it doesn't necessarily distinguish very cleanly between science, medicine, social science, or any other versions of inquiry. Distinctions such as these tend to go out of focus in the welter of knowledge practices uncovered by ethnography. It also tends to find continuities between natural and social science. Physicists may have their instruments, but so too do sociologists. Much that we learn about the practice of natural science is also applicable to social science.

Thus the first take-home message from Latour and Woolgar is that what the authors called 'the tribe of scientists' (1986, 17) is not very different from any other tribe. Scientists have a culture. They have beliefs. They have practices. They work, they gossip, and they worry about the future. And, somehow or other, out of their work, their practices and their beliefs, they produce knowledge, scientific knowledge, accounts of reality. So how do they do this? How do they make knowledge?

The ethnographers of science are usually more or less *constructivist*. That is, they argue that scientific knowledge is constructed in scientific practices. This, it should be noted, is *not* at all the same thing as saying it is constructed by scientists. Thus we will see that practices include, and imply, instruments, architectures, texts – indeed a whole range of participants that extend far beyond people. But the process of building scientific knowledge is also an active matter. It takes work and effort. The argument is that it is wrong to imagine that nature somehow impresses its reality directly on those who study it if they just set aside their own biases. The picture of science offered by Merton is not right. But how is this construction done?

Different ethnographers respond to this question in somewhat different ways. However Latour and Woolgar, whom I follow here, explore it materially. They wouldn't call themselves 'materialists' because they do not think that everything derives from, or can be ultimately explained in, material terms. Nevertheless, they are very much into *materiality*. This means that they focus in the first instance on the physical stuff of the laboratory, and how this is laid out architecturally. For instance, it has a chemistry section, a physiology section, and then there is a location with desks and word processors which is mainly to do with paperwork. Then they talk about the way materials move around. Energy, money, chemicals, people, animals, instruments, tools, supplies, and papers of all kinds, move into the laboratory. At the same time, people and (different) papers and maybe instruments, together with debris and waste, move out. Looked at as a system of material production, then, the major product of the laboratory turns out to be *texts*. These are very expensive: at 1979 prices they cost about $30,000 each. No doubt the figure would be much higher now.

If the Salk Laboratory is a system of material production then how are its various material resources turned into texts? Latour and Woolgar trace this through a number of moves. Step one: they observe that 'the desk . . . appears to be the hub of our productive unit' (1986, 48). At the desk two kinds of texts are juxtaposed: on the one hand some come from outside the laboratory, such as scientific articles or books; on the other hand some originate from within the laboratory. But where do these come from? The answer is that they are produced by what they call *inscription devices*.

So this is the second step in their argument. An inscription device is a system (often including, though not reducible to, a machine) for producing inscriptions, or traces, out of materials that take other forms:

> an inscription device is any item of apparatus or particular configuration of such items which can transform a material substance into a figure or a diagram which is directly usable by one of the members of the office space.
> (1986, 51)

For instance, an inscription device might start out with rats. These would be sacrificed to produce extracts which would be placed in small test tubes. Then those test tubes would be placed in a machine, for instance a radiation detector, which would convert them into an array of figures or inscriptions on a sheet of paper. These inscriptions would be said – or assumed – to have a direct relation to 'the original substance'.

At this point, stage three, something interesting happens. Latour and Woolgar argue that *the process of producing the traces melts into the background*:

> The final diagram or curve thus provides the focus of discussion about properties of the substance. The intervening material activity and all aspects of what is often a prolonged and costly process are bracketed off in discussions about what the figure means.
> (1986, 51)

The argument is thus that *the materiality of the process gets deleted*. (Perhaps this is why 'constructivism' is often mistakenly thought to be about a purely human activity.) For what is subsequently manipulated is not the rats themselves. It is not even the extracts from the rats. Rather it is curves derived from figures from the relevant inscription devices. It is the curves that get juxtaposed with one another on the desks of the researchers.

The fourth step in the story is a process of isolating, detecting, and naming substances:

> Samples of brain extract are subjected to a series of *discriminations*. . . . This involves the use of some stationary material (such as a gel or a piece of blotting paper) as a selective sift which delays the gradual movement of the sample of brain extract. . . . As a result of this process, samples are transformed into a large number of fractions, each of which can be scrutinised for physical properties of interest. The results are recorded in the form of several peaks on graph paper. Each of these peaks represents a discriminated fraction, one of which may correspond to [a] . . . discrete

> chemical entity. . . . In order to discover whether the entity is present, the fractions are taken back to the physiology section of the laboratory and again take part in an assay. By superimposing the result of this last assay with the result of the previous purification, it is possible to see an overlap between one peak and another. If the overlap can be repeated, the chemical fraction is referred to as a 'substance' and is given a name.
>
> (1986, 60)

This is very important. Latour and Woolgar are telling us that it is *more or less stable similarities between curves* that allow the scientists to say that they have isolated a 'substance'. It is the relative similarities of successive curves that allow the laboratory workers to name a 'substance'. By contrast, 'elusive and transitory' substances – witnessed by curves that appear and disappear – come to be known as 'artefacts' and are disregarded.

Though some of their language is unusual, and, yes, they have taken us away from empiricism, perhaps what Latour and Woolgar have told us so far is not too surprising. But with the next step we move towards the unexpected:

> The central importance of this material arrangement [of laboratory inscription devices] is that none of the phenomena 'about which' participants talk could exist without it. Without a bioassay, for example, a substance could not be said to exist. The bioassay is not merely a means of obtaining some independently given entity; the bioassay constitutes the construction of the substance.
>
> (1986, 64)

'Without a bioassay, for example, a substance could not be said to exist.' And this is not simply a way of speaking. Here they are again:

> It is not simply that phenomena *depend on* certain material instrumentation; rather, the phenomena *are thoroughly constituted by* the material setting of the laboratory. The artificial reality, which participants describe in terms of an objective entity, has in fact been constructed by the use of inscription devices.
>
> (1986, 64)

This, then, is their fifth point. It is that *particular realities are constructed by particular inscription devices* and practices. Let me emphasise that: *realities* are being *constructed*. Not by people. But in the practices made possible by networks of elements that make up the inscription device – and the networks of elements within which that inscription device resides. The realities, they are saying, simply don't exist without their matching inscription devices. And, implicitly at least, they are also saying that such inscription devices – and even more so their particular products – are elaborate and networked arrangements that are more or less uncertain, more or less able to hold together, and more or less precarious.

As is obvious, this is an account of scientific inquiry that departs from the most common-sense – and indeed philosophical – understandings of the nature of reality and the ways in which we know it. It is certainly not empiricist: Merton, along with many others, assumes that there is a reality out-there of a definite form waiting to be discovered, if only we can get it right. But one does not have to be an empiricist to feel that this is a good intuition. The same hunch underpins much more elaborate understandings of science – for instance the various versions of realism. So what does it mean to assert the contrary – to say that particular realities are *constructed* in networks of practices that include inscription devices and their contexts? What does it mean to say that without a bioassay a substance *could not be said to exist*? These are the puzzles that Latour and Woolgar leave us with. And they are puzzles central to the argument of this book.

A perspective on reality

Linear perspective. The art historians[16] tell us that this, known in antiquity and lost in the Dark Ages, was rediscovered in the early years of the fifteenth century by the Florentine architect Filippo Brunelleschi. In effect Brunelleschi asked himself the following question: is it possible to make a drawing of a building which looks exactly like the building itself? His answer, in an ingenious experiment with a mirror, was yes, it was.[17] With appropriate care a depiction could indeed reproduce the proportions of the object that it represented. The system of linear perspective so derived was developed and formalised by a further Renaissance architect, Leon Battista Alberti in his *Della Pittura* which appeared in 1435. The art, or the science, he told his readers, is to think of a picture as if it were a window, looking out in the direction of whatever is to be drawn. Or better, to think of it as an initially transparent screen, through which the external world can be seen.

But how to do this? Alberti makes two moves. The first is to imagine that there are lines of sight, coming from outside the window/screen, and passing through the screen to the eye of the painter. If the painter can mark the point where they pass through the screen on the way to his eye, then he or she will successfully mimic whatever is outside. The lumpy three-dimensional reality beyond the figurative window is thus converted into a two-dimensional representation. The first move, then, imagines a cone of vision starting, or ending, at the eye. Lines of sight beginning or ending in the eye, fan out, through the figurative window to the objects in the world beyond that window.

The second is to invent something that is usually called the vanishing point. The issue here is, how best to preserve the proportions of objects that are out there, in the world, when they are being transformed into a representation on a two-dimensional surface. Alberti suggests, in part, that we imagine a second cone, another fan. But this time, instead of converging in the eye of the artist, it converges on the *other side* of the picture/window, in the middle of the field of view, at a distant point on the horizon, directly opposite the eye of the artist. This, then, becomes the point at which those edges of objects in the real world that are at right angles to the picture/window tend to converge. To help in painting the artist now needs to draw this second cone, to depict it on the two-dimensional surface of the picture/window.

What form does this take? The answer is that it becomes a set of lines radiating out from a single point on the surface. This becomes the vanishing point. And the location of the vanishing point is fixed because it is where the line joining the centre of the two cones that have been created – the one converging on the distant vanishing point out there in the world, and the other, in here, in the eye of the artist – passes through the surface of the picture/window.[18]

This theorising is only a small part of the story. The conventions of linear perspective were being developed in the last years of the fourteenth century among artists in Italy. Art historians such as Norman Bryson (1983) show that it indeed took several generations for the new techniques to become established in the repertoire of the Renaissance artists.[19] This is partly because there were other powerful representational traditions available, for instance to do with the all-seeing eye of God, and symbolisms attached to various depicted features of nature or the gesture. Nevertheless it led to such powerful representations as Raphael's *Marriage of the Virgin*.[20]

Five assumptions about reality

To make sense of the stories about the Salk Laboratory and Western perspectivalism I need to talk about 'reality'. I need to talk about what is or isn't out there in the real world. That is, I need to engage with what philosophers variously call 'metaphysics' or 'ontology'. *Ontology* is the part of philosophy concerned with what there *is* and what there could be.[21] Philosophers talk of *metaphysics* when they are thinking about the untestable and often implicit assumptions that frame experience. From a philosophical point of view we all work in terms of more or less unexamined metaphysical (and ontological) assumptions. This is not a problem: there is no choice! But my interest is in the assumptions that these two stories make about reality, and in particular with Latour and Woolgar's surprising conclusion that specific realities are constructed in sets of practices that include particular inscription devices. At the same time, I am also interested in why it is that we might find this thought surprising.

In order to think about this I want to tease out some of the metaphysical assumptions that Euro-American people tend to carry when they, when we, think about what it is that scientists or social scientists are up to in the world. Or lay people. When we think, in other words, about reality, about what is, about ontology.

First, and most generally, it appears that our experience is widely if not universally built around the sense that there is, indeed, a *reality that is out there* beyond ourselves. Note that if we assume this then we are not committing ourselves to anything very specific. Indeed, I have phrased this in a way that is deliberately both general and diffuse. The out-thereness could take a variety of different forms. Let's think of this as a 'primitive' or 'originary' version of reality and simply talk of it as '*out-thereness*'. But for most Euro-Americans, at least most of the time, the sense of reality we carry is considerably more specific. So what does this include? Here are some additional suggestions:

Most of us would, I guess, implicitly commit ourselves to the further sense that this external reality is *usually independent of our actions and especially of our perceptions*.[22] Note that this – I will call it a commitment to '*independence*' – is not the same thing as out-thereness in its primitive form. As I have just noted, at least in principle, out-thereness might be experienced as much more closely related to our perceptions and our actions, much more dependent on them, than is generally the case in Euro-America.[23] I say 'generally' because there are at least parts of contemporary science – quantum mechanics is an example – in which the reality in question is taken to be closely related to any attempt to measure it.

Another more or less related common-sense is that this external reality comes before us, that it *precedes us*. Again this is not the same as the primitive commitment to out-thereness. It is a possible version or specification of it – but alternatives can be imagined. One could imagine, for instance, a theology or a metaphysics in which out-thereness was only possible in relation to a knowing and sentient being, or perhaps a set of methods for detecting and apprehending that reality. Versions of this, which are usually taken to be philosophically idealist (though this may be only one of the possibilities), have been considered from time to time in Western metaphysics. But, aside perhaps from some physicists in their professional lives, Euro-America mostly doesn't sense things that way. I will call this particular version of out-thereness '*anteriority*'.

A further common-sense is that external reality has, or is composed of, a set of *definite forms or relations*. Again, this is not entailed in the primitive commitment to out-thereness. Rather it is a possible operationalisation or version of it. One might, for instance, live in a world in which what went on was always vague, diffuse, uncertain, fluid, elusive and/or undecided – and was taken to be so. But though the social world may sometimes be apprehended in this way, Euro-American empirical experience mostly doesn't work like this. Instead it buys into an assumption that the world is more or less specific, clear, certain, definable and decided. It agrees, to be sure, that we may dream or imagine in ways that are vague and indefinite – but this has little to do with reality. It also agrees that individuals or groups may be vague and unclear (or simply mistaken) about the character of that world: our methods for finding out about it may be underdeveloped, distorted or themselves be vague. But this is usually seen as a failure in the attempts of those involved to gather proper knowledge, rather than being an attribute of the world itself. This I will call the assumption of '*definiteness*'.

Another common-sense is that the world is shared, common, the *same everywhere*. Once again, this is not implied in the primitive commitment to 'out-thereness'. Different people, groups or cultures might exist in different worlds. One could imagine multiple versions of the real (which is not the same thing as multiple perspectives on the same reality). Indeed this possibility is sometimes entertained, perhaps in a somewhat metaphorical form, in the context of social life, with the idea that different people live in different 'social worlds'. But nonetheless, again some parts of physics excepted, this would not be a common Euro-American intuition with respect to the physical world, or indeed in the end in the social world. Instead most Euro-Americans would be committed to what I will call '*singularity*'.

It is easy to think of other possible forms or specifications of the real. It is tempting, for instance, to think of *constancy* as a further category. (Do objects or processes in general stay the same unless they are disturbed? Most Euro-Americans would probably say yes.) Another, to which I will return at the end of the book, is *passivity* (in Euro-American versions of the real, the latter is usually 'disenchanted' and rendered passive). Yet another, though perhaps it does much of the same work, is *universalism*. But this initial list will do for the moment, because it allows us to distinguish between (a) Albertian perspectivalism, (b) Latour and Woolgar's understanding of scientific inquiry at the Salk Institute, (c) the scientists' own apparent understanding of their work (which is probably not so very far from that of Merton), and (d) our own possible surprise at the conclusion proposed by Latour and Woolgar.

First, then, Albertian perspectivalism. To work within this is surely to be committed to the entire list. *Out-thereness* of course: perspectivalism precisely depends on a distinction between observer and observed. *Independence*? Though perspectivalism has also been an imaginative and creative tool for Western artists for at least five hundred years, in the first instance (think of Brunelleschi) the issue is to find ways of representing the world out there. *Anteriority*? Again, thinking of Brunelleschi, it has to do with the representation of a pre-existing world. It assumes that there is a world out there already in place that is waiting to be depicted. *Definiteness*? Yes again. Importantly, the apparatus of perspectivalism articulates a most specific and precise version of what it is to be definite. Thus the system is a projection that rests on the assumption that the real world is a Euclidean space, and that space is populated with representable objects possessed of Euclidean volumes. The art, or the science, is to discover and follow the rules that allow the relevant definite three-dimensional volumes to be transcribed on to a two-dimensional surface. And finally *singularity*? Again yes – and again linear perspective has its own particular take on this. If space is Euclidean, and it is populated with objects with specific volumes, then it follows that representational eyes in different places will see different views or perspectives. At the same time, since the rules are explicit, they precisely provide for the projection of a single three-dimensional real-world object from several different perspectival viewpoints. Perspectivalism is thus most strongly committed not only to a specific version of definiteness, but also, and as a part of this, to a specific and spatially-based version of singularity. Knowledge of the world resides in the subject.

So much for perspectivalism. Its version of out-thereness is highly specified. But what of the scientists in the Salk Laboratory? Look at this snippet of conversation between two of the Salk scientists as reported by Latour and Woolgar in *Laboratory Life*:

> *Dieter*: Is there any structural relation between MSH and Beta LPH?
> *Rose*: It's well-known that MSH has parts in common with Beta LPH. . . . Would you have expected finding proteolytic enzymes in the synaptosome?
> *Dieter*: Oh yes.
> *Rose*: Well, has it been known for a long time?
> *Dieter*: Well yes and no . . . there is a paper by Harrison showing that they do not obtain.
>
> (1986, 160)

Like any other conversation, this can be interpreted in various ways. However, the most straightforward reading suggests that Rose and Dieter, like the Albertian artist, are committed to and assume all five of the features of reality mentioned above. *Primitive out-thereness*? Yes. MSH and Beta LPH are only two of the external entities that appear in the conversation. *Independence*? Yes, each of these compounds is taken to have features independent of the beliefs, ideas, or practices of the scientific community. *Anteriority*? Yes, they pre-exist any attempt to get to know them. *Definiteness*? Yes indeed, that is what the conversation is all about. MSH, Beta LPH and proteolytic enzymes are all assumed to have definite attributes. The difficulty Rose and Dieter are wrestling with in the second part of the conversation doesn't call this into question: it is rather that the definite features of the enzymes appear to be in doubt amongst the relevant scientists. And finally, *singularity*? Again, yes of course. MSH is an object. It is a single object. It is a single object that can be compared with Beta LPH. It is not, it cannot be, different things in different places.

So Rose and Dieter are committed to a set of assumptions about reality very similar to those articulated in Euclidean perspectivalism. The only difference is in the way in which definiteness and singularity are detected. In perspectivalism they are specified in geometrically spatial terms, while endocrinological definiteness and singularity are generated in an alternative, chemically defined, manner.

But what of Latour and Woolgar? What of *their* assessment of the practices of the scientists? What of their counter-intuitive conclusion that particular realities do not exist without sets of practices that include inscription devices and the networks within which these are located? To tackle these questions we need to return to the Salk Laboratory.

The hinterland

Latour and Woolgar insist that science has to do with the *manipulation of inscriptions and statements*. As I have already noted, the desk of the Salk scientist, so central to scientific production, is covered with texts. Some derive from local inscription devices, and others from beyond the laboratory – papers, reviews and preprints written by scientists elsewhere. So the argument is that texts are put together and played off against one another. And the purpose of all this? It is to produce statements that carry authority, that tell about the outside world.

What do these statements look like? Latour and Woolgar divide them into a number of categories. Some are unconditional. They simply describe the outside world without qualification. For instance: 'Ribosomal proteins begin to bind pre-RNA soon after its transcription starts' (1986, 77). And, closely related to these, there are statements that are hardly statements at all because everyone takes them for granted anyway. These are only made explicit when talking to students or outsiders. Then, and usually (though not always) with less authority, there are statements that include what Latour and Woolgar call *modalities*. Modalities are qualifications or contexts that turn up within the text. They may be references to authors or to the way in which the statements were produced:

> [T]his method has *first* been described by Pietta and Marshall. If Pietta and Marshall have a strong reputation this might add to the strength of the claim.

However other modalities tend to undermine credibility:

> Recently Odell [ref.] has reported that hypothalamic tissues, when incubated . . . would increase the amount of TSH.
>
> (1986, 78)

The words '*has reported*' suggest an agnosticism about this claim which is therefore seen as uncertain. Yet other modalities turn statements into mere speculations or possibilities, and are even more erosive:

> There is also this guy in Colorado. They claim that they have got a precursor for H . . . I just got the preprint of their paper.
>
> (1986, 79)

A lot of the time, then, scientists are comparing statements of differing degrees of strength, selecting and playing them off against one another in the process of trying to create unqualified statements. The practice is similar to the comparisons between the curves produced by inscription devices. We have seen that if these map on to one another it may become possible to say that a 'substance' has been discovered. It may be possible to give it a name. It is the same with the relations between statements and their modalities. Similarities, overlaps, stabilities, repetitions, or positive relations between statements tend to increase their authority. If all goes well it may become possible to make statements that assert unqualified claims about substances and realities, pin these down, fix them, and make them definite. But this is only one possibility. In practice, Latour and Woolgar suggest that most statements are qualified and uncertain. Never achieving a modality-free existence, their speculative lives tend to be more or less brief.[24] Overall then, in the Salk Laboratory:

> The aim of the game was to create as many [unqualified] statements . . . as possible in the face of a variety of pressures to submerge assertions in modalities such that they became artefacts. . . . the objective was to persuade colleagues that they should drop all modalities used in relation to a particular assertion.
>
> (1986, 81)

This form of words suggests that science is a literary exercise. It is about the fate of statements as they interact with one another. This is not exactly wrong, but it is also misleading because, crucially, *science is not just a literary exercise*. Natural (and social) science works with statements of a particular provenance. Thus statements do not idly freewheel in mid-air, or drop from heaven. They come from somewhere. Thus we can all dream up wish lists about the character of reality, but without support from other statements or inscriptions of an appropriate provenance they do not go very far. So we might put it this way: *if a statement is to last it needs to draw on – and perhaps contribute to – an appropriate hinterland*. But what is the nature of that hinterland?

We already have a partial answer for science. A part of the hinterland of a statement is other related statements. Is it consistent with these? Do they tend to support it? If the answer is 'yes' then they tend to add to its authority. But we have also seen that this is only a part of the story. Scientific statements also draw more or less directly from a network or a hinterland of appropriate *inscription devices*. Do the practices in which these are embedded produce figures that can be compared and tend to reinforce one another? If the answer is 'yes' then the authority of a statement increases. If it is 'no', then the statement is likely to enter the limbo of the might-have-beens. This, then, is the most important point: it is the character of this hinterland and its practices that determines what it is to do science, or to practise a specific branch of science. To a first approximation, then, science is an activity that involves the simultaneous orchestration of a wide range of appropriate literary *and* material arrangements. It is about the orchestration of suitable and sustainable hinterlands.

Inscription devices: Latour and Woolgar are canny in the way they use this term. An inscription device may be, but is not necessarily, a technology or an instrument. More generally, it is a set of arrangements for labelling, naming and counting. It is a set of arrangements for *converting relations from non-trace-like to trace-like form*. It is a set of practices for shifting material modalities. This is their understanding of the special materiality of science. It is the process of *making* particular kinds of relations in an experimental and instrumental set-up, and turning these into traces. This is why they insist that:

> We do not wish to say that facts do not exist nor that there is no such thing as reality. In this simple sense our position is not relativist. Our point is that 'out-there-ness' is the *consequence* of scientific work rather than its *cause*. We therefore wish to stress the importance of timing.
>
> (1986, 182)

The practices of science make relations, but as they make relations *they also make realities*. This is why Latour and Woolgar are interested in timing. Beforehand things are not clear and the realities in question are not yet made. Afterwards they are.[25] This means that scientific work is both robust *and* insecure. Its insecurity, typically invisible to outsiders, is apparent to anyone who visits a laboratory or knows anything about the actual conduct of science. As I have noted, things go endlessly wrong. This radiation counter is not calibrated, those rats are ill, or the new serum samples are odd. The deliveries of oxygen have been held up. And even (and perhaps more tellingly) when everything is going well experiments tend to produce traces that contradict one another and erode rather than strengthen putative accounts of reality. The future of reality is always at risk in a sea of uncertainty. It is extremely difficult to build stable relations in the laboratory. It is extremely difficult to build relations that will produce more or less stable traces.

Here is Latour describing himself stumbling round the laboratory:

> He had to remember in which beaker he had put the doses, and made a note, for example, that he had put dose 4 in beaker 12. But he found that he had forgotten to make a note of the time interval. With pipette half-lifted, he found himself wondering whether he had made a note before or after the actual action took place; obviously, he had not made a note of when he had made a note! He panicked and pushed the button of the Pasteur pipette into beaker 12. But maybe he had now put *twice*

> the dose into the beaker. If so, the reading would be wrong. He crossed out the figure.
>
> (1986, 245)

Methodical procedures and meticulous note-keeping are necessary. Otherwise a day's work is lost. (Lest it be thought that Latour was particularly clumsy let me add that I was responsible for similar minor debacles in the course of my own laboratory ethnographies.) So the practices of science are quite obsessively textual. Labelling, naming, writing down, noting – they are fixated on the business of keeping tabs on things. And if this fails then the work of the laboratory also fails.

The precariousness of the process of producing stable traces about stable realities is also witnessed by another well-documented feature of laboratory science: *the fact that it is often surprisingly difficult to reproduce the novel findings of one laboratory in other laboratories*. It is not uncommon that a statement generated from the inscription practices in one laboratory cannot be reproduced elsewhere.[26] Is this a cause for suspicion? Is the new claim about reality doubtful? The answer is yes to both questions. If statements do not map on to one another, if the patterns do not repeat themselves, then the realities they report are being undermined. It comes to look as if the statement reported not a fact but an artefact. But what does this mean? Answer: if the creation of facts is a relational activity – a question of assembling and fine-tuning the appropriate inscription devices – then it is equally possible that what is happening is a failure in such fine-tuning. If this is the case then it may be that there is need for more training, new and special equipment, the production of particular test samples (Salk Institute work was crucially dependent on these), the specialist manual skills of a particular experimentalist or technician, or the competence of an in-house computer programmer. If people can be trained or travel, if the precise experimental set-up can be reproduced, if novel equipment can be built – in short, if the relations in one laboratory can be configured in another – then the reality in question may be reproduced. As Latour and Woolgar bluntly put it:

> In no instance did we observe the independent verification of a statement produced in the laboratory. Instead, we observed the extension of some laboratory practices to other arenas of social reality, such as hospitals and industry.
>
> (1986, 182)

Or, even more pithily:

> . . . if you carry out the same assay you will produce the same object.
>
> (1986, 183)

If this is not possible, if 'the same assay' is not carried out, then the reality disappears into a limbo of questionable modalities.

This, then, is the implication of Latour and Woolgar's argument. Contrary to Euro-American common sense, they are telling us that it is not possible to separate out (a) the making of particular *realities*, (b) the making of particular *statements* about those realities, and (c) the creation of *instrumental, technical and human configurations and practices*, the inscription devices that produce these realities and statements. Instead, *all are produced together*. Scientific realities only come along with inscription devices. Without inscription devices, and the inscriptions and statements that these produce, there are no realities.

Where does this leave 'out-thereness'? We've seen that Latour and Woolgar treat this as the '*consequence* of scientific work rather than its *cause*'. But the implications of their argument are now clearer and we can return to the list of out-therenesses:

Independence: is external reality independent of our perceptions and actions? The answer is: it depends on what we mean by 'our perceptions and actions'. For individuals or particular sites of scientific production the answer is – largely – yes. It is difficult to imagine circumstances in which we could imagine, perceive, or act realities into being individually, or in our work. In that sense the outside world is independent of us. But collectively and in the longer run the answer is different. This is because particular realities are brought into being with and through the arrays of inscription devices and disciplinary practices of natural and social science. Reality, then, *is not independent of the apparatuses that produce reports of reality*.

Anteriority: does external reality precede our reports of it? The answer, again, is that it depends. In general the answer is no, it doesn't. Reality and the statements that correspond to it are produced together in the disciplinary and laboratory apparatuses of inscription. But in specific circumstances (and we are all, and all the time, in specific circumstances), there is always also a large hinterland of inscription devices and practices already in production. This means that an equally large hinterland of statements, and realities that relate to those statements, are already being made. There is a backdrop of realities that cannot be wished away.

Definiteness: does external reality come as a set of definite forms and relations? Again, the answer is both yes and no. Where statements fit together and reinforce one another the corresponding objects are named and acquire a definite form. Where this does not happen they do not. And, as Latour and Woolgar show, though the aim of the game is to make definite statements that correspond to definite realities, much of the time scientific inquiry deals with uncertainty, fuzziness and undecidability. An example: Latour and Woolgar describe the way that for a seven-year period starting in 1962 there was uncertainty about the existence and the character of a substance of particular interest to the Salk endocrinologists which came to be known as TRF. This changed in a way that was scientifically unsatisfactory because it was fuzzy, vague, and shifting. There were doubts about its very existence. It was only after 1966 that it became possible to talk of 'TRF' as a substance – and the chemical character of that substance was only turned into a firm statement in 1969. The moral of the story is that sometimes things are definite and sometimes they are not.

Singularity: is the world shared, is it common, is there a single reality? For Latour and Woolgar the answer is 'yes', but only *after* the controversies have been resolved and the statements reporting on nature have become fixed, definite and unambiguous. Before this happens not only is reality indefinite, but at least at times of scientific controversy it is also multiple. Multiplicity is the product or the effect of different sets of inscription devices and practices, for instance in different laboratories, producing different and conflicting statements about reality. Nevertheless, the end point – difficult but in their view none the less sometimes achieved in science – is a single reality and a single authorised set of inscription devices.

In sum, Latour and Woolgar take us some distance from everyday Euro-American expectations about out-thereness. Reality is neither independent nor anterior to its apparatus of production. Neither is it definite and singular until that apparatus of production is in place. Realities are made. They are *effects of the apparatuses of inscription*. At the same time, since there are such apparatuses already in place, we also live in and experience a real world filled with real and more or less stable objects.

A routinised hinterland: making and unmaking definite realities

So why is scientific reality relatively stable, at least a lot of the time? Latour and Woolgar suggest that we might think about this in terms of *cost*. The argument is that undermining the relations embedded in received statements is expensive:

> the set of statements considered too costly to modify constitute what is referred to as reality. Scientific activity is not 'about nature,' it is a fierce fight to *construct* reality. The *laboratory* is the workplace and the set of productive forces, which makes construction possible. Every time a statement stabilises, it is reintroduced into the laboratory (in the guise of a machine, inscription device, skill, routine, prejudice, deduction, programme, and so on), and it is used to increase the difference between statements. The cost of challenging the reified statement is impossibly high. Reality is secreted.
>
> (1986, 243)

'Reality is secreted.' Notice that this posits a kind of feedback loop. Statements stabilise, and then recycle themselves back into the laboratory. This means that once they are demodalised, *yesterday's modalities become tomorrow's hinterland*. And, as a part of this they tend to change in their material form:

> The mass spectrometer is the reified part of a whole field of physics; it is an actual piece of furniture which incorporates the majority of an earlier body of scientific activity.
>
> (1986, 242)

So why and how do they change their material form? A part of the answer is that it is easier to produce statements about realities – easier to produce realities – when these take standardised and transportable forms. Latour and Woolgar talk of reification, but perhaps the notion of *routinisation* better draws attention to what is most important. We saw above that the practice of fitting bits and pieces together to produce more or less stable traces is a precarious business. Much goes wrong in laboratory science. But if machines and skills and statements can be turned into packages,[27] then so long as everything works (this is always uncertain) there is no longer any need to individually assemble all the elements that make up the package, and deal with all the complexities. It is like buying a personal computer rather than understanding the electronics, and the physics embedded in the electronics and assembling one out of components. Thus in the above example the field of physics that is the hinterland of the mass spectrometer can be taken for granted. It does not have to be rebuilt or even understood by those who use the instrument. One sociology of science literature talks of 'standardised packages'. This is the point: in this way of thinking all the reality-describing and reality-making of natural (and social) science practices surfs on more or less provisional standardised packages that are, form part of, or support, inscription devices and practices. At the beginning of this chapter I cited Latour:

> A proposition, contrary to a statement, includes the world in a certain state . . . . Thus a construction is not a representation from the mind or from the society about a thing, an object, a matter of fact, but the engagement of a certain type of world in a certain kind of collective.
>
> (Latour 1997, xiii–xiv)

Latour, here twenty years on, is talking about Isabelle Stengers's philosophy of science[28] (and his talk of propositions rather than statements is a small but potentially misleading change in vocabulary). But the overall argument remains the same. It is not a matter of words representing things. Words and worlds go together. Propositions (as he is now calling them) include realities – include a collective. Include and grow from what I am calling the hinterland.

Certain additional consequences follow. The hinterland produces specific more or less routinised realities and statements about those realities. But this implies that countless other realities are being *un-made* at the same time – or were never made at all. To talk of 'choices' about which realities to make is too simple and voluntaristic. The hinterland of standardised packages at the very least shapes our 'choices'. We who 'choose' embody and carry a bundle of hinterlands. Nevertheless there are a whole lot of realities that are not, so to speak, real, that would indeed have been so if the apparatus of reality-production had been very slightly different.

A further and related implication is that the hinterland produces certain *classes* of realities and reality-statements – but not others. Some kinds of standardised inscription devices and practices are current. Some classes of reality are more or less easily producible. Others, however, are not – or were never cobbled together in the first place. So the hinterland also defines an overall geography – a topography of reality-possibilities. Some classes of possibilities are made thinkable and real. Some are made less thinkable and less real. And yet others are rendered completely unthinkable and completely unreal.

The economic metaphor suggests that it is easier and cheaper to create new inscription devices, new statements and new realities by building on to the routinised black boxes that are already available. It also suggests that as the process goes along it becomes more and more difficult and expensive to ignore or to undo the routines and create others and alternative realities. Latour and Woolgar again:

> Once a large number of arguments have become incorporated into a black box, the cost of raising alternatives to them becomes prohibitive. It is unlikely, for example, that anyone will contest the wiring of the computer . . . or the statistics on which the 't' test is based, or the name of the vessels in the pituitary.
>
> (1986, 242)

For individual practitioners it is often, perhaps usually, best to borrow from and make use of a very extensive and expensive set of inscription devices, because these would be extremely costly to overturn. Latour and Woolgar offer an example of this:

> when Burgus used mass spectrometry to make a point, he made it difficult to raise alternative possibilities because to do so would be to contest the whole of physics. Once a slide has been shown with all the lines of the spectrum corresponding to one atom of the amino acid sequence, no one is likely to stand up and object. The controversy is settled. But if a slide is presented which shows the spots of a thin-layer chromatography, ten chemists will stand up and assert that 'this is not a proof'. The difference, in the second case, is that any chemist can easily find fault in the method used.
>
> (1986, 242)

It is also a practical point for working scientists in another way too. Should they build on a particular standardised package or, alternatively, raise the stakes and the costs, go against the grain, and try to reorganise the hinterlands to generate one that is new? This is not a possibility open to most practitioners, even in the most straightforward economic terms. The money and the time to undo (say) the physics that lies behind mass spectroscopy and build an alternative set of inscription devices with their corresponding reality-statements and realities is not likely to be available.

In this argument, it is the hinterland of scientific routinisation, produced with immense difficulty and at immense cost, that secures the general continued stability of natural (and social) scientific reality. Elements within this hinterland, even sections of it, may be overturned (perhaps this is what Thomas Kuhn, whom we will touch on below, meant when he talked of 'scientific revolutions'). But overall and most of the time Latour and Woolgar are telling us that it is the expense of doing otherwise that allows the hinterlands of scientific reality to achieve relative stability. So it is that a scientific reality is produced that holds together more or less. That appears to be – and in a real sense is – independent of our particular scientific perceptions and actions. That appears to – and in a real sense does – predate those actions, is anterior to them. That is, indeed, definite. That is, in this account, singular – though the issue of singularity is one to which I will return in the next chapter.

A note on Foucault: limits to the conditions of possibility?[29]

The apparatuses of scientific (and arguably of social science) production produce something akin to what Michel Foucault described as the conditions of possibility. If we go with the economic metaphor then they set necessary limits – more or less permeable, but nevertheless limits – to those conditions.

So how does the present argument differ from that of Foucault? One answer has to do with empirical scope. Foucault and his interpreters insist that there is endless possibility for variation and creative innovation within the existing conditions of possibility.[30] Nevertheless, it is also well known that Foucault argued that the current conditions of possibility were established at the end of the eighteenth century in a set of strategies laid down within what he called the modern episteme. The argument is that at the beginning of the twenty-first century we are still being produced by that episteme and its conditions of possibility.

This may or may not be right. However, the picture of natural (and social) science production proposed by Latour and Woolgar and other STS scholars is drawn on a smaller scale. Perhaps there are larger limits set by modern disciplinary strategies that lie within and are being enacted by the different inscription devices and practices of modern natural and social science. But Latour and Woolgar's suggestion is more modest. It is that the limits to scientific knowledge and reality are set by *particular and specific sets of inscription devices*. The relations between these become an empirical matter.

Given the flexibility of the modern episteme, the position is not necessarily inconsistent with that of Foucault. Further it shares with him the commitment to the idea that it is not simply knowledge of realities, but also realities themselves, that are generated in the practices of production. My question, and one to which I will return in Chapter 3, has to do with singularity. Latour and Woolgar tend to assume that inscription devices (and so their hinterlands) mesh together fairly well. This seems to me uncertain.

Covering up the traces

But then there is the great question: why doesn't it look that way? Why is it not obvious that inscription devices produce not only the statements about reality but also the realities themselves? How come people don't see that 'phenomena *are thoroughly constituted by* the material setting of the laboratory' (1986, 64)? Why is it that reality is taken to be independent, anterior, definite and singular? How come scientists are said to 'discover' a reality that is anterior, definite, and all the rest?

Latour and Woolgar have given us the elements that we need to answer these questions. Thus we have seen that the object of scientific practice is to make unqualified statements about reality. All the qualifying modalities need to be deleted. We have also seen that it is important to routinise statements by turning them into taken-for-granted assumptions, instruments, or skills. The more the hinterland is standardised and (at least in certain respects) the more it is concealed, the better.

But this means that as the modalities disappear, so too do almost all of the processes in which statements and realities are produced. The largest part of the work that has gone into their production is deleted. In the end, the inscription devices themselves disappear, though those that are most novel are likely to retain a foothold in the 'methods section' of scientific papers. But it is the 'subjective' and the 'personal' that disappears first. The traces and the statements in the laboratory are used 'in such a way that all the statements were seen to relate to something outside of, or beyond, the reader's or author's subjectivity' (1986, 84).

This deletion of subjectivity is crucial. In natural and social science research statements about objects in the world are supposed to issue from the world itself, examined in the proper way by means of proper methods, and not from the person who happens to be conducting the experiment. If this is not achieved, then independence and anteriority are not achieved either. If the scientist appears in her text, if she appears as a person, then this undermines any statement about reality.

So what is the consequence of this process of deletion? Latour and Woolgar suggest that scientific statements should be seen as 'split entities':

> On the one hand, it is a set of words which represents a statement about an object. On the other hand, it corresponds to an object in itself which takes on a life of its own. It is as if the original statement had projected a virtual image of itself which exists outside the statement.
>
> (1986, 176)

So there is deleting and splitting. But then something else happens to complete the effect: there is a causal reversal or inversion. It is no longer the case that the manipulation of inscriptions is seen to produce particular realities. Instead it is the *realities that come first*:

> Before long, more and more reality is attributed to the object and less and less to the statement *about* the object. Consequently an inversion takes place: the object becomes the reason why the statement was made in the first place.
>
> (1986, 177)

This is the way in which reality becomes the determining factor. It is no longer the processes of comparing, contrasting, and weighing up inscriptions that produce reality. It is no longer the long sequence of actions, events and negotiations in which appropriate inscription devices are brought together and arrayed. Least of all is it the uses made of the special skills of particular technicians or programmers. It is not arguments, debates, discussions or controversies that *produce* reality. It is not the work that lies behind those debates and discussions. Rather it is *reality* that settles any disagreements. It is reality that produces statements.

> The thing and the statement correspond for the simple reason that they come from the same source. Their separation is only the *final stage in the process of their construction*.
>
> (1986, 183)

The result is a sense of a world that is out-there in far more than the primitive or originary sense. It is an out-thereness that is also assumed to be independent of and prior, anterior, to our scientific attempts to know it. It is assumed to be definite – even if we do not yet know that definite form because we have not acquired the methods we need to know it. And it is assumed to be singular.

Latour and Woolgar's proposal, then, is that this bundle of out-therenesses can be understood as an accomplishment rather than something that defines and sets limits to the ways in which we can properly know the world. Indeed, it is that out-thereness is better understood as an accomplishment rather than something given in the order of things. In short it is that the embedded hinterland of scientific method, the practices that it carries, work to *produce* a reality that is independent, anterior, definite and singular.

This is the bottom line of their ethnography of science. The hinterland of methods *enacts* realities. And (one can turn this round) those realities then enact the conditions of possibility of further research. They do not do so wantonly. They do not do so randomly. This is not a matter of the will, of lust, of desire, or of political visions. Nothing can be made real without the ramifications of an appropriate hinterland. But none the less, realities are enacted. If this is difficult then this is because it questions the self-evidence of Euro-American metaphysics; because it undermines the necessity of the methods that we happen to have available to us; because it presents us with possibilities (a reality *enacted*?) that are dangerous and potentially destabilising at least in principle not only to the metaphysics in which our methods are embedded, but also to the particular realities which they produce.

The method assemblage

Latour and Woolgar's proposal is that out-thereness is accomplished or achieved rather than having a prior and determinate form of its own. Realities are produced along with the statements that report them. The argument is that they are not necessarily independent, anterior, definite and singular. If they appear to be so (as they usually do), then this itself is an effect that has been produced in practice, a *consequence* of method. This suggestion flies in the face of most Euro-American metaphysics, including the more standard versions of the philosophy of science and social science.

Confronted with this claim we have a choice. We might opt to stick with a standard version of metaphysics. We could insist that the argument is wrong, and that whatever is out there is (at least usually) independent, anterior, definite and singular. If we take this line then it follows that we should continue to design our research methods along the current lines. We will need to think of our methods as tools for discovering a reality, or aspects of a reality, that is out there in a fairly definite form but is more or less hidden to us. This is comfortable, reassuring, and fits many understandings of methods. However, there are good reasons for considering the less conventional alternative: that the metaphysics are not right.

There is much that might be said about this. Here are a few thoughts. First, even though its argument is unfamiliar, it is plausible. Even if it doesn't fit the standard Euro-American justifications, Latour and Woolgar's account fits the *practices* of natural and social science. The findings of their ethnography are neither empirically weird nor theoretically strained. They explain perfectly well why scientists (and social scientists and lay people) tend to be committed to a strong version of out-thereness. But at the same time they also show how this is consistent with the idea that out-thereness is something enacted in practice. As I have shown above, scientists are caught up in a hinterland that has both been created and yet is relatively obdurate because it is too difficult to overturn.[31]

Latour and Woolgar's argument applies just as well to our social sciences.

We too have our instruments of research. We too reflect on and work within the obdurate realities produced by the hinterland of those instruments. For instance, statistics do not exist *sui generis*. As is obvious, they have to be created. Indeed there has been considerable historical work on the way in which this has been achieved over a couple of centuries or more through the medium of elaborate systems of tallying, measuring and quantifying in such forms as censuses, timekeeping (or time-making), surveying and economic data-creation. Such apparatuses, the hinterlands of much of social science, embed and enact many assumptions about the nature of the social. Arguably, 'the social' was brought into being in these apparatuses, as they developed and carried strategies of social and state control. By now however, with so many daily practices (public and private) dependent on official and other statistics, their reversibility is in doubt. It is possible to tinker with them – but overall, undoing them would be extremely expensive both literally and metaphorically. The result is both that we have come to live, and are made, in a social reality that is partly quantitative in quite specific ways, and that much of this hinterland is bundled into and constitutive of social science research.[32] We might add that parts of it have also been produced by social science.[33]

None of this is to say that these statistics are wrong. They may be criticised for this or that particular failing, but this is not the point. Rather they and the relations in which they are located are hinterlands and social realities out-there that both enable and constrain any work in social science. They set limits to the conditions of social science possibility. Overall, then, this is the first reason for taking the arguments of Latour and Woolgar seriously. Though their argument about enacted realities sounds counter-intuitive, it is consistent with our Euro-American intuitions that realities, natural and social, are pretty solid. To say that something has been 'constructed' along the way is not to deny that it is real.

Second, and just as important, their argument helps us to think differently and more creatively about method. In particular, the suggestion that specific forms of out-thereness are enacted and re-enacted makes it possible to think about which realities it might be best to bring into being. This, as I hope I have made clear, is not a simple or trivial question of choosing the version of out-thereness that happens to suit. 'Choice', if this is an appropriate term at all, is limited by the need to relate to and build appropriate hinterlands that will sustain statements about reality. Philosopher Isabelle Stengers puts the argument in slightly different terms:

> no scientific proposition describing scientific activity can, in any relevant sense, be called 'true' *if it has not attracted 'interest'*. To interest someone does not necessarily mean to gratify someone's desire for power, money or fame. Neither does it mean entering into preexisting interests. To interest someone in something means, first and above all, to act in such a way that this thing – apparatus, argument, or hypothesis . . . – can concern the person, intervene in his or her life, and eventually transform it. An

> interested scientist will ask the question: can I incorporate this 'thing' into my research?
>
> (Stengers 1997, 82–83)

So this is not a trivial matter. 'Interesting' is not necessarily easy. Nevertheless, the implications are profound. If out-therenesses are constructed or enacted rather than sitting out there waiting to be discovered, then it follows that their truth or otherwise is only one of the criteria relevant to their creation. Politics in one form or another also becomes important. But the moment we acknowledge this we are faced with new questions. What kind of out-therenesses are possible? Which are so embedded that they cannot be undone? Where might we try to undo or redo them? How might we try to nudge research programmes in one direction rather than another?[34] To bend a phrase, if we think in this way then reality is no longer destiny.

In the rest of the book I pursue this non-conventional option. The stakes for politics, but also for truth, are surely so high that it would be mistaken not to try to think these through. But if we are to do this there are at least two reasons why we need a better vocabulary for talking of method. The first has to do with *symmetry* and the second with the character of the *hinterland*.

As I indicated in the introduction, conventional talk of 'method' is closely associated with rules and norms for best practice. Indeed, though method is usually more than this, it sometimes becomes indistinguishable from lists of do's and don'ts. But if we want to think about more generous versions of method we need to think seriously about methods that ignore the rules. Here the sociologists of science are helpful. I will discuss their notion of 'symmetry' more fully at the end of Chapter 5. However, for the moment I just need to say that the idea of symmetry suggests that we shouldn't let our ideas about what is true or false (in science or anywhere else) affect how we look at our subjects. For instance, if we build our assumptions about the nature of good methods into our investigations of method then we are likely to come to conclusions that mirror those assumptions. We are likely to find that 'good methods' produce 'good results'. We will tend to reproduce the current workings of method. The alternative is to follow Latour and Woolgar. As we have seen, they disentangle the asymmetrical normativities of standard methods-talk ('this is good science, and this is bad') from their stories about how methods work in practice. In this respect their inquiry is symmetrical – but so too are the terms of their analysis. This, then, is the first reason for devising a new vocabulary.[35]

The second reason relates to the hinterland of method. I have argued that method and its out-therenesses are made out of, and help to make, an appropriate hinterland. I have also suggested (and this is the important point) that the hinterland ramifies out for ever. This means that method extends far beyond the limits that we usually imagine for it. Going beyond laboratory benches, reagents and experimental animals, or questionnaires, interview design protocols, and statistical or qualitative data-analysis packages it extends into tacit knowledge, computer software, language skills, management capacities, transport and communication systems, salary scales, flows of finance, the priorities of funding bodies, and overtly political and economic agendas. The list is endless. All of these form a part of the hinterland of research. Its boundaries are porous and extend outwards in every direction. However, the problem is that the word 'method' doesn't really catch these ramifications. To take one instance, it doesn't catch the way in which discourses about 'users' have become integral to most funded research in the UK over the last twenty years; or the ways in which related assumptions about audit have been embedded in the practice of research. This, then, is the second reason why we need a new vocabulary. We need a way of talking that helps us to recognise and treat with the fluidities, leakages and entanglements that make up the hinterland of research. This would allow us to acknowledge and reflect not only on what happens in laboratories or in the offices of social scientists, but also in the missing seven-eighths of the iceberg of method.

In order to do this I propose a (partial) neologism. When I want to refer to method in this extended manner I will usually speak of *method assemblage*. I will return to and redefine this term several times in what follows, and especially in Chapters 3 and 5. However I will start by noting that the term 'assemblage' comes from the English translation of Deleuze and Guattari's *Mille Plateaux* (see the citation that begins this chapter).[36] Helen Verran and David Turnbull say that for these authors an assemblage:

> is like an episteme with technologies added but that connotes the ad hoc contingency of a collage in its capacity to embrace a wide variety of incompatible components. It also has the virtue of connoting active and evolving practices rather than a passive and static structure.
>
> (Watson-Verran and Turnbull 1995, 117)

Here Verran and Turnbull have caught exactly what is needed. An *assemblage* (without the method) is an episteme plus technologies. It is ad hoc, not necessarily very coherent, and it is also active.

In Deleuze and Guattari the English term 'assemblage' has been used to translate the French 'agencement'. Like 'assemblage', 'agencement' is an abstract noun. It is the action (or the result of the action) of the verb 'agencer'. In French 'agencer' has a wide range of meanings. A small French–English dictionary tells us that it is 'to arrange, to dispose, to fit up, to combine, to order'. A large French dictionary offers dozens of synonyms for 'agencement' which together reveal that the term has no single equivalent in English.[37] This means that while 'assemblage' is not exactly a mistranslation of 'agencement' much has got lost along the way.[38] In particular the notion has come to sound more definite, clear, fixed, planned and rationally centred than in French. It has also come to sound more like a state of affairs or an arrangement rather than an uncertain and unfolding process.[39] If 'assemblage' is to do the work that is needed then it needs to be understood as a tentative and hesitant unfolding, that is at most only very partially under any form of deliberate control. It needs to be understood as a verb as well as a noun. Here is Derrida (of course in translation):

> . . . the word sheaf seems to mark more appropriately that the assemblage to be proposed as the complex structure of a weaving, an interlacing which permits the different threads and different lines of meaning – or of force – to go off again in different directions, just as it is always ready to tie itself up with others.
>
> (Derrida 1982, 3)

Note that. A '*complex structure of a weaving*'. A '*sheaf*'. And here are Deleuze and Claire Parnet:

> In a multiplicity, what counts are not the terms or the elements, but what there is 'between', the between, a set of relations which are not separable from each other.
>
> (Deleuze and Parnet 1987, viii)

So assemblage is a process of bundling, of assembling, or better of recursive self-assembling in which the elements put together are not fixed in shape, do not belong to a larger pre-given list but are constructed at least in part as they are entangled together. This means that there can be no fixed formula or general rules for determining good and bad bundles, and that (what I will now call) 'method assemblage' grows out of but also *creates* its hinterlands which shift in shape as well as being largely tacit, unclear and impure.

But what is *method* assemblage? In Chapter 5 I will define this as the enactment or crafting of a bundle of ramifying relations that generates presence, manifest absence and Otherness, where it is the crafting of presence that distinguishes it as *method* assemblage. But I need to build towards this definition, so the work of Latour and Woolgar suggests a provisional and more specific possibility. Method assemblage may be seen as the crafting of a hinterland of ramifying relations that distinguishes between: (a) 'in-here' statements, data or depictions (which appear, for instance, in science and social science publications, and include descriptions of method); (b) the 'out-there' realities reflected in those in-here statements (natural phenomena, processes, methods, etc.); and (c) an endless ramification of processes and contexts 'out-there' that are both necessary to what is 'in-here' and invisible to it. These might range from things that everyone in question knows (how to do chromatography), through mundanities that no one notices until they stop happening (the supply of electricity), to matters or processes that are actively suppressed in order to produce the representations that are taken to report directly on realities (these would include the active character of authorship or the trail of continuities between statements and the realities that they describe).

INTERLUDE:
Notes on paradigms

Kuhn's book, *The Structure of Scientific Revolutions* is the best-known example of a body of work that drove a coach-and-horses through the empiricist and positivist vision of science.[40] Kuhn works by means of exemplary historical cases and his argument defies easy summary. For him exemplars are lessons on how to see and understand the world. To be a scientist is to work through cases. Quick accounts don't get to the heart of things at all. Talk and statements are only the tip of the iceberg. So if I say that three features of his account of science are important for us, though this isn't exactly wrong it is also at odds with the deeper sense of his story. Nonetheless:

First, scientists don't come to their work naïve but with a whole package which he calls a *paradigm*. This includes law-like generalisations, implicit assumptions, instrumental and embodied habits, working models, and a general and more or less implicit world-view. As I've just noted, it also includes exemplary applications of relevant models and theories. Scientific training, says Kuhn, is about learning to see chosen empirical circumstances in terms that fit how other paradigm-sharing scientists see them: to see particular circumstances as instances or applications of relevant models and theories. He writes that students

> regularly report that they have read through a chapter of their text, understood it perfectly, but nonetheless had difficulty in solving a number of the problems at the chapter's end. Ordinarily, also, those difficulties dissolve in the same way. The student discovers, with or without the assistance of his instructor, a way to see his problem as *like* a problem already encountered. (Kuhn 1970, 189)

You simply have to go and do the experiments and learn how to *see* them properly. Book-learning will not do.

Second, scientists are *puzzle solvers*. The world presents empirical and theoretical puzzles that can be solved by applying, adapting, and extending the paradigm. This, indeed, is what it is to be a scientist: a puzzle solver who is committed to this package, applies it, and extends it.

Third, very rarely paradigms fail. Systematic attempts to resolve some important puzzle do not work. If this happens for long enough then a sense of crisis develops that may lead to a '*scientific revolution*' in which one paradigm is replaced by another. But this is unusual. Most scientists are engaged in the creative and mundane process, puzzle solving.

Kuhn's account has many similarities with that of Latour and Woolgar (no surprise, for these authors come after and draw from Kuhn). We can see the Salk scientists as puzzle solvers who draw on and are entangled with a hinterland of more or less standardised instrumental, theoretical, and embodied resources. Furthermore, much of that hinterland is *tacit*: paradigms are embodied in craft skills, unspoken assumptions, and inscription devices.[41] Knowledge is not primarily an explicit set of statements and theories. It is a more or less inexplicit and indeterminate hinterland.

This means that the entanglements of Kuhn's picture of science are quite unlike those proposed by Merton. First, they are much less clear. Second, the empirical has a quite different significance because in Kuhn's way of thinking it *is not possible to make observations of nature in a neutral way*. Instead, what scientists observe, and how they observe it, is always tied up with their paradigm:[42] recognition of similarity in scientific observation is *acquired*:

> What is built into the neural process that transforms stimuli to sensations has the following characteristics: it has been transmitted through education; it has, by trial, been found more effective than its historical competitors in a group's current environment; and, finally, it is subject to change both through further education and through the discovery of misfits with the environment. Those are characteristics of knowledge, and they explain why I use the term. But it is a strange usage, for one other characteristic is missing. We have no direct access to what it is we know, no rules or generalizations with which to express this knowledge.
>
> (Kuhn 1970, 196)

Observations could not be neutral. They cannot be disentangled from the context of training or the process of puzzle solving which makes up the hinterland. So though *scientists solve real empirical puzzles* the reality they are dealing with is partially dependent on the paradigm itself. Out-thereness is not wanton or fickle. It cannot be created willy-nilly. But the particular forms it takes are more or less specific. Kuhn's vision of knowledge is pragmatic: paradigms are tools for handling out-thereness. But they also in part *enact* that out-thereness. So, though there are differences between Kuhn on the one hand, and Latour and Woolgar on the other, perhaps it is only pushing Kuhn's vision of science a little to say that specific versions of out-thereness are not independent and prior to the paradigm. And that if they are definite and singular they only become so in relation to a particular paradigm.

The structure of scientific entanglement is far removed from that of Merton.

3 Multiple worlds

Different sites

The picture of method starts to shift. The argument is no longer that methods *discover* and depict realities. Instead, it is that they participate in the *enactment* of those realities. It is also that method is not just a more or less complicated set of procedures or rules, but rather a bundled hinterland. This stretches through skills, instruments and statements (in-here enactments of previous methods) through the out-there realities so described, into a ramifying and indefinite set of relations, places and assumptions that disappear from view. So what follows from this? This is the issue that I tackle in the remaining chapters of this book. What are the realities that are made in method? What are the forms of the out-thereness? What realities, out-therenesses, *might* be made in method? How do in-herenesses get made, and what might they look like? How are different realities, different methods, and different in-herenesses entangled with one another?

The inquiry needs to be practical: an exploration of method-in-practice. So what happens to different methods in practice, and how do they relate to one another? To explore this question we move to a large university hospital, 'Hospital Z' in the Netherlands, and follow philosopher Annemarie Mol. Mol is watching the doctors and the patients as they work with lower-limb atherosclerosis. This condition is mundane, indeed prosaic, but it is also distressing, more or less painful and handicapping, and sometimes deadly. Mol's question is the following: what *is* it, lower-limb atherosclerosis? To answer this question she takes us to a number of locations, starting with a surgeon's *consulting room*:

> The surgeon walks to the door and calls in the next patient. They shake hands. . . . The patient, a women in her eighties, takes a chair at the other side of the desk, clutching her handbag on her lap. The doctor looks in the file in front of him and takes a letter out. 'So, Mrs Tilstra, here your general practitioner writes you've got problems with your leg. Do you?' 'Yes, yes, doctor. That's why I come here.' 'Tell me, then, what are those problems. When do you have them?' 'Well, what can I say? It's when I try

> to do something doctor, move, walk, whatever. Like, I used to walk the dog for long stretches, but now I can't, I hardly can. It hurts too much.' 'Where does it hurt?' 'Here, doctor, mostly down here, in my calf it does. In my left leg.' 'So it hurts in your left calf when you walk. Now how many metres do you think you can walk before it starts hurting?' 'What can I say? I think it must be, well, some, not a lot, some 50 metres I guess.' 'Good. Or not good. Well. And then, can you walk again, then, after some rest?' 'Yeah, if I wait for a while, after that, yes, I can. Yes.'
>
> (Mol 2002, 21–22)

Mrs Tilstra is describing a complaint which the medical professional call 'intermittent claudication'. This is intermittent pain on walking. She wouldn't have been talking to the surgeon unless she'd already talked to her general practitioner. And she wouldn't have talked to her general practitioner unless she had pain in her leg when she was out walking the dog. But – or so says Mol, following Latour and Woolgar – she didn't actually have a condition called 'intermittent claudication' until she presented herself at the surgery. Before that the pain was 'diffuse' (2002, 22). Mol continues:

> This does not imply that the doctor brings Mrs Tilstra's disease into being. For when a surgeon is all alone in his office he may explain to the visiting ethnographer what a clinical diagnosis entails, but without a patient he isn't able to *make* a diagnosis. In order for 'intermittent claudication' to be practised, two people are required. A doctor and a patient.
>
> (2002, 23)

Intermittent claudication calls for both a patient and a doctor. If it is to be enacted it needs to be crafted out of a story by the former and the embedded knowledge of the latter. Here we see the bundling of a hinterland. We also sense shades of Kuhn's scientists: the surgeon skilfully sees the similarity between Mrs Tilstra's case and all the other cases of intermittent claudication that he's seen before. Because sometimes the stories do not fit. For instance, Mr Zender also talks to the surgeon about pain in his legs, but this happens when he is sitting, not walking. Something is wrong with Mr Zender's legs, but not intermittent claudication. As the surgeon puts it: 'You may have pain in your legs alright. But there's nothing wrong with your leg arteries' (2002, 42).

The practice of intermittent claudication grows out of a specific hinterland that includes the story of the patient and the skill of the physician – and the latter includes a theory of its origin. This says that intermittent claudication is caused by inadequate supply of blood to the legs. This occurs when the legs need more oxygen, which is usually in exercise. And this, in turn, is caused by atherosclerosis, which is why Mr Zender's problems don't fit. But is that all? A story and a theory? Often not. Frequently the body of the patient is also important in the consulting room. It doesn't speak about intermittent claudication, but in the hands of the examining surgeon it may come up with corroborating evidence:

> . . . the vascular surgeon holds Mr Romer's two feet in his full hands to estimate and compare their temperature. He observes the skin. And with two fingers he feels the pulsations of the arteries in the groin, knee and foot.
>
> (2002, 25)

One foot is warmer than the other – a sign of a poor blood supply to the second. The skin is poor on the second too, a further sign. And the pulsations in the same leg are weak at the ankle, which is a third sign. So the body is important too. It is best if it corroborates the story of the patient, which it does for Mr Romer. But sometimes it doesn't. For instance, surgeons say that patients sometimes learn stories from the television or something that they heard at a party, and sound as if they have intermittent claudication. But then a physical examination produces warm legs with strongly pulsating foot arteries. This isn't common, but it can happen.

But let's stop at this point with the clinic and shift to a second site. A few floors down in the same hospital there is the *pathology laboratory*. This has a large fridge and, on the day Mol visits, the fridge contains a foot and the lower part of a leg. This was amputated the previous day and sent to the laboratory to assess the state of the blood vessels (2002, 33). So what does this mean in practice? The answer is, first, that the pathologist cut out pieces of the artery and put them into containers with preserving fluid. Then a technician decalcified the artery and sliced thin sections from it. Then she stained those sections, and fixed them onto glass slides (2002, 37–38). After this it was possible to examine them microscopically. Here is the pathologist talking with Mol as they look together through the microscope at one of the sections of artery:

> 'You see, there's a vessel, this here, it's not quite a circle, but almost. It's pink, that's from the colourant. And that purple, here, that's the calcification, in the media. . . . Look, all this, this messiness here, that's an artefact from that.' He shifted the pointer to the middle of the circle. 'That's the lumen. There's blood cells inside it, you see. . . . And here, around the lumen, this first layer of cells, that's the intima. It's thick. Oh wow, isn't it thick! It goes all the way from here, to there. Look. Now there's your atherosclerosis. That's it. A thickening of the intima. That's really what it is.' . . . And then he adds, after a little pause: 'Under a microscope.'
>
> (2002, 30)

The pathologist is talking about something like a more or less furred-up pipe. The scale in the pipe, the furring, is the thickened intima. In the textbooks and in the expertise of the physicians a 'furred' artery with a thickened intima impedes the flow of blood – in which case the story fits Mr Romer's examination described above. If the flow is impeded there is little or no pulse, and not enough oxygen is being carried into the afflicted foot. And it fits Mrs Tilstra's story too because intermittent claudication, pain on walking, is caused, as we have seen, by the lack of oxygen.

So lower-limb atherosclerosis is produced in the clinic and the pathology laboratory. But it is also enacted in the *radiology department*. Here the patient lies on a table, and a needle is inserted into the artery in the groin – a tense moment for the professionals, for things may go wrong. The needle is followed by a catheter. Then everyone apart from the patient retreats into a neighbouring room behind a lead screen, and two buttons are pressed. One injects X-ray opaque dye through the catheter into the artery and the other starts the X-ray machine which takes a series of pictures of the leg. If all goes well this produces a series of angiographic pictures (the technique is called *angiography*) which show a two-dimensional version of the lumen, the un-furred sections of the leg's vascular system. This is a visual representation of the places where the blood (and the opaque dye) can get. So the result is a bit like a route-map, that can then be displayed and discussed. Where, and how much, is the stenosis, the reduction in flow?

> Decision making meeting. The light box. A surgeon walks up to the angiography under discussion. 'How much did you make of this?' he asks the radiologists, his finger pointing towards a stenosis. '70%. Come on, that's not 70%. If you compare it with the earlier part there, if you take that bit as the normal part, up here, I'd say it's almost 90%, this lumen loss.'
>
> (2002, 73–74)

Like the pathology laboratory, the radiology department has its own methods and practices. Its hinterland includes: the X-ray machine; the dyes; the catheters; the lead screens; the surgical incision; the antisepsis; the sedated patient; the table on which he lies; and a whole lot more. But here the product is not a microscope slide. Instead it is an angiograph, another quite different version and visualisation of lower-limb atherosclerosis. It is another way of thinking about lumen loss, though this time it does not directly visualise the thickening of the intima. And it is a visualisation that can lead to the kind of debate cited above, for differences between estimates of lumen loss tend to be high.

Clinic, pathology laboratory, radiology department. Three locations. But here is a fourth which is another quite different way of detecting and locating the narrowing of blood vessels, the stenoses. Called duplex, this uses *ultrasound*. A small probe is pressed on to the skin of the patient above a blood vessel – though first it is necessary to find the blood vessel, and make sure that the probe is in good contact with the skin (a special gel is spread over the skin).

The probe emits ultrasound, and detects reflected ultrasound waves. The operator is looking for a Doppler effect, differences in the reflected wavelengths caused by blood flow. These appear as colours on a screen. In particular she is looking for variations in the speed of flow since (it is assumed that) blood will flow more quickly where the vessel is partially restricted (where the lumen loss is greatest) and more slowly where it is not. In practice she tries to compare velocities (usually peak systolic velocity, PSV) for a healthy and a partially occluded artery, in order to calculate a 'PSV-ratio' (2002, 55–57). Then she converts this, more or less controversially, into a figure for lumen loss:

> PSV-ratio smaller than 2.5: a stenosis smaller than 50%. PSV-ratio equal to or larger than 2.5: a stenosis larger than 50%. No sign: occlusion.
>
> (2002, 78)

This is another method with its own specific hinterland. But just as angiography differed from pathology, which in turn differed from the clinic, so duplex differs from angiography, having its own set of devices, skills, and people. The patient is prepared in a different way (and much less invasively than for angiography). Indeed, the physics built into the devices are different too, since electronics are supplemented by acoustics for duplex while a more or less nineteenth-century version of electromagnetic theory is built into angiography.

And then there is the *operating theatre*. Mol:

> It is a fat leg. Nurses have coloured the inside of the thigh yellow with iodine. The surgeon makes a sharp straight cut that opens up the skin. The fat underneath it is carefully separated by a resident. Blood repeatedly obscures the view. Tissues are used to absorb it. Small vessels are closed off with a small pin. Larger ones tied off with blue threads. Heparin is added to prevent the blood from clotting. . . . The entire cut is then widened with a . . . clamp. . . . Ah, finally, there is the artery. An orange plastic thread is put around to mark it. Then a similar search for the artery is repeated just above the knee. . . . The surgeon makes two incisions in the vessel wall. . . . With a knife the resident loosens the atheromatous plaque from the rest of the arterial wall where the artery is opened up. He then inserts the ring of a stripper around the plaque. . . . The stripper is moved upward. Slowly. When it finally arrives in the groin, the entire stretch of atheromatous plaque has been loosened. . . . With tweezers the surgeon draws it out. He drops it in a small bin. There goes the thickened intima. With lots of debris attached to it. Its bright white contrasts with the greyish artery.
>
> (2002, 90)

This is a description of endarterectomy, one of the surgical procedures for removing the thickened intima which causes arterial stenosis. It is, again, its own set of arrangements, its own method assemblage. The surgeons may use the angiography as a kind of route-map in order to decide where to make the incision. If this happens than the angiography and everything that produces it form one part of the surgical hinterland. But others include the skills of the surgeon, the various tools of his trade: scalpels, clamps, stripper (a remarkably crude instrument in the form of a ring attached to a stiff wire), heated pin, heparin, tissues, swabs, the apparatus of anaesthesia, and all the other elements of the operating theatre. So the bundled hinterland of the operating theatre turns the thickened intima into the form of white debris that can be dropped into a bin. Again, intima and the stenosis take their own particular form in the operating theatre. Forms both similar to and different from those in the other method assemblages.

A single story

So there are many sites: the clinic (which can be divided between the patients' complaints and the physical examination); the pathology laboratory; the radiology department; duplex; and the operating theatre. Mol describes at least five locations at which lower-limb atherosclerosis appears, and she could find more. She writes, for instance, of 'walking therapy', an alternative, non-invasive treatment for intermittent claudication.[43] She also visits the haematology department, where there is research into the formation of the plaque which leads to stenosis and lumen loss. But let us stop at this point. We have a large number of locations and each is its own method assemblage, its own set of health-related crafts and practices. And, if we follow the logic proposed by Latour and Woolgar, then we also need to add that *each of these method assemblages is producing its own version of atherosclerosis*: that there are *multiple atheroscleroses*. But what should we make of this startling conclusion? Are we happy to see the erosion of reality as singular?

If you ask the professionals, they usually *talk* about a single object, or about a set of objects and processes that fit together to produce a single reality. I touched on this above. Thus they say that long-term changes in the blood, perhaps partly due to diet and insufficient exercise, may lead to a slow build-up of atheromatous plaque and thickening of the intima. At first this has little effect, but at a certain point lumen loss becomes so great that blood flow is reduced. Then the patient experiences pain on walking – intermittent claudication – and he or she is likely to go to the doctor's surgery. When this happens other symptoms become visible – for instance the relative absence of pulsations, and the discovery of temperature differences between the legs. With further investigation additional symptoms become visible, and their localisation becomes possible. Duplex may be used to locate partial stenosis by measuring increases in the maximum speed of blood flow, or angiography to pinpoint partial or total stenosis.

So much for the story of origins and diagnosis. But what of treatment? In milder cases this may take the form of walking therapy.[44] Otherwise the alternative is surgery. I have described one version of this above. Endarterectomy is an operation in which the offending plaque is physically stripped from the inside of the artery. But there are at least two further possibilities. In an operation called percutaneous transluminal angioplasty (PTA) the stenotic vessel is inflated from the inside using a device like a tiny inflatable balloon on the end of a tube which has been inserted into the vessel. The object in PTA is to push aside the plaque and increase the diameter of the lumen. A third possibility is to create a bypass round the sclerosis. And a fourth – necessary if the blood flow is so poor that there is risk (or the reality) of gangrene – is amputation. And it is only with this fourth form of intervention that the practices of the pathology laboratory become possible: cutting and preparing thin cross-sections of vessels with stenosis, and observing the growth of the intima and the loss of lumen under the microscope.

Differences in perspective

To write in this common, indeed habitual, way is a form of perspectivalism. It buys into, enacts, and presupposes a classic Euro-American version of out-thereness. An atherosclerotic reality out there is made *anterior* to, and *independent* of medical intervention. It is both *definite* in form and *singular*. With this framing of out-thereness the first task is to work out what reality *is*: for instance, the condition of Mrs Tilstra's leg vessels, and the location of stenoses in those vessels. Then, the second task is to intervene in a way that will help her. Surgeons and their medical colleagues are committed to a strong version of out-thereness: like perspectival artists, or the Salk Laboratory scientists, they assume that they are all addressing the same reality. And, to be sure, sometimes everything works out smoothly. Pain on walking, clinical examination, angiography, duplex, surgical intervention, and pathology – all may fit together to produce a single co-ordinated atherosclerosis.

Sometimes. But sometimes relevant practitioners instead find that they are faced with poorly co-ordinated realities. So what do they do then? Here is one example:

> The pathologist: 'You, since you're so interested in atherosclerosis, you should have been here last week. We had this patient, a woman in her seventies. She had renal problems. Severe ones too. So she was admitted. And the next day she died. Paff, from one moment to the next. The nephrologists were aghast, and so, of course, was her family. So we were asked to do an obduction. Her entire vascular system was atherosclerotic. One of her renal arteries was closed off, the other almost. It was a wonder her kidneys still did anything at all. It was hard to see where they got their blood from. And it was more or less the same for every other artery we took out: they were all calcified. Carotids, coronary arteries, iliac arteries: everything. Thick intimas, small lumens. And she'd never complained. Nothing. No chest pain, no claudication, nothing.'
>
> (2002, 45–46)

Here is a second example:

> 'Here, look at this. Have you seen the pressure measurements of Mr Iljaz? It's unbelievable. I can't believe it. If you look at these numbers he can hardly have any blood in his feet at all. And he came to the outpatient clinic all alone, on his motorbike. Said he had some pain. I can't believe it. Some pain. On these figures alone I'd say here's someone who can't walk at all. Who's screaming.'
>
> (2002, 64)

And here is a third:

> [Mrs Takens's bypass] might be occluded, for the angiographic picture shows no dye beyond a critical point: the white stops abruptly. The duplex, however, still shows a peaking graph below this point. Flow. One of the radiology residents asks: 'In a case like this, when the angio says "closed" and the duplex says "open": what should one believe?' Two surgeons, speaking with a single voice, say: 'Duplex'.
>
> (2002, 83)

These are examples of contradictions: between the results of the pathology laboratory and the life of the patient (first case); between the life of the patient and measurements from the clinic (second case); and between angiography and duplex (third case). But what is their significance?

First, contradictions are important in the day-to-day practice of medicine. For though medical professionals usually work with a strong, perspectival version of out-thereness, this is only a means to the more important end of intervening and helping the patient. Their major preoccupation is in working out *what to do*. In an ideal world all the indications add up and fit together: they are different but compatible in-here perspectives on a single out-there reality. But since the world is not perfect often those involved need to work out how to act in the face of conflicting indications. Their world is quite unlike that of the Salk scientists. The latter are concerned with fixing reality, with truth, but in practice if not in theory the medical professionals often have to work with multiple possible truths.[45]

Second, this points to the need for judgements, and for rules of thumb for making judgements. These grow out of past experience, research, conversation and reading. The craft of surgery. For instance, why did the two surgeons both say 'duplex' in the same breath in the quotation immediately above? Mol's text continues so:

> And then one of . . . [the surgeons] tells how he once studied seventeen cases like this: patients whose angiography showed an occlusion while their duplex showed flow. In all seventeen cases duplex proved to be in line with

> the findings upon operation. 'It was only seventeen cases, so I couldn't publish it. But there were no exceptions.'
>
> (2002, 83)

Here surgery is being used as the 'gold standard' to determine the nature of atherosclerosis, and duplex is being treated as the better guide to reality. But Mol also shows that in many (perhaps most) other contexts it is angiography that is used as the 'gold standard' to determine the accuracy of duplex (a much more recent technology) rather than the other way round. The implication is that that what counts as the best depends on circumstances (2002, 56). Rules, as Wittgenstein (1953) long ago showed, do not suggest their own proper application.

So there are more or less variable and situated rules for discriminating between contradictory versions of atherosclerotic reality, and deciding, in practice, what that reality is. For fixing it in practice. And, given the frequency of contradictions, such rules are endlessly deployed. How, for instance, could Mr Iljaz have come to the hospital outpatient clinic on his motorbike when he had so little blood in his feet? When he should have been screaming in pain? This is a puzzle:

> 'Yeah, that really is something' . . . nods [a senior internist], 'but we've seen cases like that before. Probably these people have only become worse very gradually. What happens is that their muscle metabolism alters. As long as people have time for it, the adaptation may go a very long way.'
>
> (2002, 65)

This is one way of explaining away inconsistency, of turning it into *apparent* inconsistency. Another is the possibility that his diabetes may have led to degeneration of the peripheral nerves – in which case Mr Iljaz may not be able to feel pain in his legs. A third is that Mr Iljaz, who is an immigrant, may not speak Dutch well enough to explain how much pain he is actually feeling:

> 'Yeah, come to think of it, he may have underreported his complaints. His Dutch was poor.'
>
> (2002, 66)

These are perspectives on difference that tend to *explain it away*. But what are the implications of this? It is tempting to say that the professionals are trying to cover up inconsistencies and even their incompetences. However this suggestion, common though it is in the literature of medical sociology, is surely only part of the story. More important in the present context is that such stories *help to sustain a strong perspectival and singular version of out-thereness* even as they manufacture multiple realities. They assume, and at the same time help to enact, the standard version of Euro-American metaphysics while also crafting something different. The implication is that in the present incomplete, uncertain, and untidy circumstances, we may not have full insight into either Mr Iljaz's particular condition or atherosclerosis in general. But nevertheless his condition, and the disease, are both visible out-there. They are out-there not just vaguely, but in all the different specifics we have discussed. They are *independent* of the investigations of medical science, they *precede* diagnosis, they are *definite*, and they are *singular*. It is a technical or practical matter if we are not yet properly clear of their attributes. In this way Euro-American metaphysics preserves itself in the face of possible contra-indications.

Multiplicity, enactment and objects

As I have hinted above, Mol wants to take us in another direction. Instead of singularity she is interested in difference and multiplicity:

> If a relation between the atherosclerosis of pathology and the atherosclerosis of the clinic is made, in practice, their objects may happen to coincide. But this is not a law of nature.
>
> (2002, 46)

Notice what is happening here. Mol is shifting the focus *from representation to the object itself*. Perhaps representations are being crafted too, but in-hereness is also a matter of objects, things. But why? And how? In what I have written above I have touched on the pivotal moment in her data. It happened at the moment in the pathology laboratory, when she was peering with the pathologist through the microscope at the cross-section of the artery from the amputated leg. Because what the pathologist said, I repeat the words, was: '"Look. Now there's your atherosclerosis. That's it. A thickening of the intima. That's really what it is. *Under a microscope*"' (2002, 30). Mol writes:

> My endeavour hangs on this last addition. The pathology resident utters it as if he is saying nothing special. 'Under a microscope'. But it implies a lot. Without this addition, atherosclerosis is all alone. It is visible *through* a microscope. A thickened intima. . . . There's something seductive about it: to use instruments as 'mere' instruments that unveil the hidden reality of atherosclerosis. . . . But when 'under a microscope' is added, the thickened intima no longer exists all by itself – but through the microscope. What is foregrounded through this addition, is that the visibility of intimas *depends on* microscopes. And, for that matter, a lot more.
>
> (2002, 30–31)

Objects, then, don't exist by themselves. They are being *crafted*, assembled as part of a hinterland. Like representations they are being enacted 'in-here', while sets of realities are being rendered visible out-there, and further relations, processes and contexts that are necessary to presence are also disappearing.

Unlike representations, however, objects do not *describe* the visible realities 'out-there'. This is method assemblage where the relations are different. Perhaps the in-here is being *made* by its visible out-there realities, or *caused* by them, or *shaped* or *given form* or *influenced* by them. (An atherosclerotic blood vessel might be caused by blood physiology, or influenced by poor diet, or both, depending on one's interest.) Then again, perhaps it is (also) having an effect, or shaping, or giving form or influencing the out-there. (Atherosclerosis enacted as angiography may have implications for the subsequent actions of surgeons or physical therapists.) For objects, then, the relations between the in-here and the visible out-there are complex, contingent and variable, and the traffic may be two-way.

If objects are enacted in this way, then this suggests that we need a second understanding of method assemblage to put alongside what has already been said about representation. We need to say that method assemblage may also craft hinterlands in the form of (a) in-here *objects*, (b) visible or relevant out-there *contexts*, as well as (c) out-there processes, contexts, and all the rest, that are both *necessary and necessarily disappear* from visibility or relevance. At the same time, however, if we focus on practice in this way then the perspectival pressure to singularity is weakened. And this is where the question of difference, of multiplicity, raises its head: when medicine talks of lower-limb atherosclerosis and tries to diagnose and treat it, in practice *at least half a dozen different method assemblages* are implicated. And the relations between these are uncertain, sometimes vague, difficult, and contradictory. This is what Mol calls the *problem of difference*. Because if we ruthlessly stick with the logic proposed by Latour and Woolgar, and pressed by Mol, then Euro-American perspectivalism will no longer do. We are not dealing with different and possibly flawed perspectives on the *same* object. Rather we are dealing with *different objects produced in different method assemblages*. Those objects overlap, yes. Indeed, that is what all the trouble is about: trying to make sure they overlap in productive ways. Ways that make it possible to intervene and help Mr Iljaz and Mrs Takens. So they overlap, *but they are not the same*. Different realities are being created and mutually adjusted so they can be related – with greater or lesser difficulty.

This is the point of Mol's intervention. Bar one subtle but devastating difference, her position is similar to that of Latour and Woolgar. And the difference? It is that medical inquiry and intervention *may* lead to a single reality, *but this does not necessarily happen*. In thinking of this Mol finds it helpful to distinguish between 'construction' and 'enactment':

> The term 'construction' was used to get across the view that objects have no fixed and given identities, but gradually come into being. During their unstable childhoods their identities tend to be highly contested, volatile, open to transformation. But once they have grown up objects are taken to be stabilized.
>
> (2002, 42)

Latour and Woolgar talk about *construction*. Their stories are full of talk about the vaguenesses of objects as they took (or failed to take) shape in the laboratory. They talk of the chosen few that made it through to the stable maturity of a perspectival 'closure'.[46] They add, as we have seen, that closure can in principle be undone, but also note that this is unusual because it is usually too expensive to undo the hinterland and remake it in some other form. TRF? The mass spectrometer? Closure has been achieved. The object has been constructed. A single hinterland is in place. No more questions.

So that is *construction*. But what of *enactment*? Mol:

> like (human) subjects, (natural) objects are framed as parts of events that occur and plays that are staged. If an object is real this is because it is part of a practice. It is a reality *enacted*.
>
> (2002, 44)

'Plays that are staged', writes Mol, pointing to the role of performance. But this is not an updated version of Goffman's dramaturgical sociology. Goffman distinguishes between *presentations* of self on the one hand, and *self* as a hidden reality lying behind and producing those presentations, on the other.[47] But this is precisely what Mol is trying to avoid. Her argument is much more closely related to recent writing in the philosophy, sociology and history of performance that emphasises the *performativity* of enactment than it is to Goffman's approach. It is these writings – in science studies, feminist theory, and cultural studies – which in one way or another have started to explore the possibility that there is a two-way traffic between enactments on the one hand, and realities on the other.[48] Enactments, it is being argued, don't just present something that has already been made, but also have powerful productive consequences. They (help to) make realities in-here and out-there.

To talk of enactment, then, is to attend to the continuing practice of crafting. Enactment and practice never stop, and realities depend upon their continued crafting – perhaps by people, but more often (as Latour and Woolgar imply) in a combination of people, techniques, texts, architectural arrangements, and natural phenomena (which are themselves being enacted and re-enacted). So Mol throws away the notions of construction and closure. Yes, of course, there are often *practical closures*. For the moment, in the present circumstances, and notwithstanding his apparent lack of pain, Mr Iljaz has really severe lower-limb atherosclerosis. But what there aren't are closures in general. Beware, Mol is telling us. If we attend to practice and to objects we may find that no objects are ever routinised into a reified solidity. We may find that there are no irrevocable objects bedded down in sedimented practices. We may find that the hinterlands are not set in stone. And if things seem solid, prior, independent, definite and single then perhaps this is because they are being enacted, and re-enacted, and re-enacted, in practices. Practices that continue. And practices that are also multiple. This is a way of thinking:

> that does not simply grant objects a contested and accidental history (that they acquired a while ago, with the notion of, and the stories about their *construction*) but gives them a complex present, too, a present in which their identities are fragile and may differ between sites.
>
> (2002, 43)

So Mol follows the lead suggested by Latour and Woolgar – but then shifts us in two ways. She moves from representations to objects, and as she does so, she also *does away with singularity*. In-hereness and out-thereness can be, and indeed usually are, *multiple*.

Virtual singularity

Here is a case of a single enactment that turns out to be multiple. It is the decision made by the British government in 1965 to cancel a warplane called the TSR2. This is an account of that decision by one of the participants:

> The discussion showed there had been a certain divergence amongst those concerned. James Callaghan, as Chancellor of the Exchequer, wanted to cancel the plane altogether for purely financial reasons. Ranged against him were (a) Denis Healey, who wanted to cancel the TSR2 and to substitute the American F111A, which would mean a certain saving of money but an enormous increase of outlay in dollars; and (b) Roy Jenkins, who wanted to cancel the TSR2 and replace it with a British plane – which was roughly George Brown's view as well; and (c) George Wigg, who held the view that we might have to cancel both but we mustn't make any decision until we had finished the strategic reappraisal which would show what kind of plane was required.
>
> (Crossman 1975, 190–191)

And here is a second account by another participant:

> But we had to have a decision, and the Cabinet was called again for 10.00 p.m. By midnight I had to resolve a difficult . . . decision. The Cabinet was split three ways: some favoured continuing with TSR2; some favoured its outright cancellation; and the third group supported the Defence Secretary's view that TSR2 should go but that its military role should be taken over by an order for American Phantoms, together with one for a number of F111As.
>
> (Wilson 1971, 89–90)

These accounts are not quite the same. Indeed, as the following table suggests, they only partially overlap:

	Crossman account	*Wilson account*
Cancel	✓	✓
Cancel and buy F111A	✓	
Cancel and buy British plane	✓	
Probably cancel but wait for strategic review	✓	
Continue with TSR2		✓
Cancel and buy F111A and Phantoms		✓

Euro-American common sense suggests that we should think about the differences perspectivally. In this way the reality out-there is independent of, and prior to, the descriptions of that reality in-here. It is also definite and singular. If we think this way then we can assume that there was a single decision – and probably a single decision-making moment. At the same time, and as a part of the process of decision-making, we can assume that this was preceded by the elaboration of a number of specific options. But if we say this, then what should we make of the differences between the accounts?

The answer is that they are smoothed away. As with the differences between different versions of atherosclerosis, other explanations are found for the disparities. Perhaps those who wrote the accounts forgot what happened, or misunderstood it. Perhaps their accounts are self-serving. There are various possibilities, but they are all perspectival. They all preserve the assumption that there was indeed a single and definite decision made, selected from a single and definite range of options. Euro-American metaphysics in this way sustains itself.

But there is also the alternative multiple possibility, the proposal made by Mol. This is that the different participants were making *different* decisions, and that they simply thought they were making a single decision. Then, somehow or other, they co-ordinated themselves. Imagined themselves to be making the same decision. Displaced the possible differences, kept them apart. Perhaps we might call this 'virtual singularity'. But if we do so then the 'virtual' has nothing to do with cyberspace, but rather with the glass blocks in school optical experiments which seem to show pins located in places where they are not really to be found.[49]

Multiplicity and fractionality

If we are interested in multiplicity then we also need to *attend to the craftwork implied in practice*. Remember the reversal described by Latour and Woolgar. They said: one, that practices simultaneously produce statements about realities and the realities they describe; and two, that when the modalities disappear, the realities are suddenly turned into the causes of those statements. Perhaps this is right, but Mol is issuing a methodological warning. If we want to understand practice, and the objects generated in practice, then we need to make sure that we don't get caught up in that reversal ourselves. This is because it is misleading. Realities are not explained by practices and beliefs but are instead produced in them. They are produced, and have a life, in relations. So what we need is ethnography or what Mol calls praxiography:

> after the shift from an epistemological to a praxiographic appreciation of reality, telling about what atherosclerosis *is*, isn't quite what it used to be. For somewhere along the way the meaning of the word 'is' has changed. Dramatically. This is what the change implies: the new 'is' is one that is situated. It doesn't say what atherosclerosis is by nature, everywhere. It doesn't say what it is in and of itself, for nothing ever 'is' alone. To be is to be related. The new talk about what is, does not bracket the practicalities involved in enacting reality. It keeps them present.
>
> (2002, 53–54)

A praxiography allows us to investigate the uncertain and complex lives of objects in a world where there is no closure. Where, willy-nilly, there is no singularity. It allows us to explore the continued enactment of objects. And as a part of this, it allows us to investigate the multiplicity of those objects, the ways in which they interact with one another:

> Ontology in medical practice is bound to a specific site and situation. In a single medical building there *are* many different atheroscleroses. And yet the building isn't divided into wings with doors that never get opened. The different forms of knowledge aren't divided into paradigms that are closed off from one another. It is one of the great miracles of hospital life: there are different atheroscleroses in the hospital but despite the differences between them they are connected. Atherosclerosis enacted is more than one – but less than many. *The body multiple* is not fragmented. Even if it is multiple, it also hangs together. The question to be asked, then, is how is this achieved.
>
> (2002, 55)

And that is what the largest part of Mol's book is about: the practices that generate that apparently oxymoronic object, the body multiple. But what are they? Mol's work suggests that there are a number of ways in which differences are regulated. Some are more or less perspectival:

- Important is the idea that there is indeed only one body, so that any differences are a consequence of failures or limitations in practice. Mol calls this '*layering*'. Symptoms or diagnostic signs which may be at odds with one another are distinguished in negotiation from the underlying condition itself which is taken to be consistent. Crucial, then, to the body multiple is a continued faith in the body singular.
- A *single narrative* may also be important, a narrative that smoothly joins theories about the aetiology of atherosclerosis with its anatomical, physiological and diagnostic expressions. Expressions that are in turn linked to judgements about the possibility and desirability of particular interventions. The larger narrative, then, smoothes together a single coherent object that it describes and explains.
- *Translations* also help co-ordination of multiples. These are processes in which one thing is turned into another. We have come across a number of examples: angiographs were being (controversially) converted into percentages of lumen loss; and so too were PSV ratios.
- *Submission* is a hierarchical version of translation. We saw one context in which angiography (often the 'gold standard') lost out to duplex. So the lesson is that local hierarchies and submissions are important but these too are made and remade, and they are not necessarily consistent.[50]
- *Rationalisations* may have a crucial role too. They take the form of additional layers of narrative that explain apparent inconsistencies away – like Mr Iljaz's arrival at the hospital on his motorbike when he should have been screaming with pain.

These are ways of handling multiplicities that reconcile different atheroscleroses and patch them together into singularity. They are perspectival because, at the same time they also work to preserve a general commitment to ontological singularity. Mol points to further strategies that also sustain this general commitment but do so without producing a single atherosclerosis:

- *Mutual exclusion*: some things exclude one another. It isn't possible to take cross-sections of an artery from a leg vessel that is attached to a living patient. Conversely, legs that have been amputated cannot be cross-examined for complaints about intermittent claudication. Here the clinic and the pathology department exclude one another. 'The incompatibility is a practical matter. It is a matter of patients who speak as against body parts that are sectioned' (Mol 2002, 35–36). So many practices and the realities that they enact are parallels, alternatives, collaterals, streams of activity that never come together.
- *Creating different objects*: sometimes it is said that different practices are in fact producing different objects rather than conflicting versions of the same object. Mol describes a comparative study of PTA (stretching the stenosed

vessel with the little balloon) and walking therapy. The result suggested that PTA improved pressure measurements but made no difference to the distance of onset of pain when walking. But for walking therapy it was the other way round. Does this mean that the findings are inconsistent? Possibly, but in practice those reporting the study said that the patient was suffering not from one, but two atheroscleroses: 'pressure-atherosclerosis' and 'walking-atherosclerosis'. Two singular objects replaced a single multiple.

- *Creating composite objects*: but if objects can be separated, they can also be recombined to produce composite entities. And this is what happened to these two different atheroscleroses. 'In the "criteria for success according to Rutherford" improvement is defined in a composite way. It is a combination of clinical symptoms and ankle–arm [blood pressure] index' (Mol 2002, 68, citing F. van der Heijden). This is one atherosclerosis with two parts. 'Addition', Mol observes, 'is a powerful way of creating singularity'.
- *Locating in different places*: finally Mol notes that 'Incompatibilities don't stop patients getting diagnosed and treated. Work may go on so long as the different parties do not seek to occupy the same spot. So long as they are separated between sites in some sort of *distribution*' (2002, 88). Separation may occur *over time* (patients move from one site to another and can't be in all of them at the same time (2002, 115)). It may occur between *different patients* (who are operated on in different and mutually incompatible surgical ways). It may occur as *mutual recognition* (the distribution between atherosclerosis as a gradual process of deterioration, and its reality as a serious condition in the here-and-now (2002, 116)). Or finally, separation may occur by acknowledging *differences in the conditions of possibility*. (Surgery is necessary at present, but future work in haematology will hopefully prevent the development of atherosclerosis and surgeons will find, as they did with stomach ulcers, that they are no longer needed.)

There are many ways of reconciling difference and avoiding multiplicity. Some are perspectival, and others are not. Together, however, they work to push the possibility of multiplicity off the agenda. Rendered invisible, it becomes a part of the out-there that is arguably necessary to the practices in question but cannot be acknowledged. By contrast, if we attend to practice we tend to discover multiplicity. But here is another important point. We discover multiplicity, *but not pluralism*. For the absence of singularity does not imply that we live in a world composed of an indefinite number of different and disconnected bodies, atheroscleroses, hospital departments, or political decisions. It does not imply that reality is fragmented. Instead it implies something much more complex. It implies that the different realities *overlap and interfere with one another*. Their relations, partially co-ordinated, are complex and messy:

> [The term atherosclerosis] . . . is a co-ordinating mechanism operative in conjunction with the various distributions. It bridges the boundaries between the sites over which the disease is distributed. It thereby helps to prevent distribution from becoming the pluralizing of a disease into separate and unrelated objects.
>
> (Mol 2002, 117)

I cited Mol similarly above: '*The body multiple* is not fragmented. Even if it is multiple, it also hangs together' (2002, 55). Hinterlands partially intersect with one another in complex ways, and the practices bundling those hinterlands together generate complex objects. We will, I think, need a range of different metaphors if we are to start thinking this well, but here is a first possibility. Perhaps we should imagine that we are in a world of *fractional objects*. A fractional object would be an object that was more than one and less than many. The metaphor draws on an elementary version of fractal mathematics. Thus a fractal line is one that occupies more than one dimension but less than two. Perhaps, indeed, when we visit the hospital (or anywhere else) we are in a world of fractionality. We are in a world where bodies, or organisations, or machines are more than one and less than many. In a *world* that is more than one and less than many. Somewhere in between.[51]

Partial connections

The difficulty of the talk of fractionality suggests that we are pressing up against the conditions of possibility. The dominant enactments of Euro-American metaphysics make it very difficult to avoid singularity on the one hand, and pluralism on the other. Either there is a single world, or there are lots of different worlds. This is what seems to be the choice.

If there is a single world, then in science or in social science it is our duty to try to provide an account of it. And if there are lots of different worlds? Then we are faced with two alternatives. Either the different worlds are components of a single larger world, in which case, once again, it is our duty to try to provide an account of it. Or, alternatively, the different worlds have nothing to do with one another. And if they have nothing to do with one another? Then we are confronted with what are usually taken to be the horrors of relativism. So what are those horrors?

These come in three closely related versions. First there is *epistemological* relativism. This says that the knowledge in your culture is just as good as the knowledge in my culture, so there are no grounds for claiming that my account of out-thereness is any better than yours. Second, there is *ethical* relativism. This says that ethics are situated and local, and there are no grounds for claiming that my ethical standards are any better than yours. And third, there is *political* relativism. The argument takes the same form again: there are no reasons for preferring my politics over yours. We should live and let live.

Notice, though, what is happening here. We are being pressed, all the time, to make a choice between singularity and pluralism. Either there is one, one reality, one ethics, one politics, or there are many. There is nothing in between. This pressure to dualist choice is why I take it that we are being pushed up against the enacted limits of Euro-American metaphysics – and, to be sure, being asked to re-enact it. But the dualism imposed by the choice does not follow. Something in between is a possibility.

One way to see this is to think in empirical mode and ask the question: how far do arguments carry in practice? How far, for instance, do arguments about claudication carry? Are they only valid in the place that makes them? In one cultural location? Or do they travel universally? Setting the choice up like this, as an empirical version of the epistemological dualism, reveals that the choice is forced. For the empirical and matter-of-fact answer is that arguments about claudication travel so far, but only so far. The same is the case for any other argument about out-thereness. How far does it carry? So far, but only so far. The overlaps, for instance, between the arguments made by Australian Aborigines and Euro-American technoscience are limited (we will discuss some of these in Chapter 7). The arguments carry only so far. But often enough they do carry in some measure. And an empirical version of the ethical question – or indeed the political question – leads us to similar conclusions. How far do our ethical or political arguments carry? Answer: they go some way, but only so far. It is indeed a commonplace that people disagree over what a good world would look like.

So how should we respond to this? There are three options. It is possible to insist on singularity, and insist that those who do not see it our way are suffering from impaired vision: that their empirical, ethical or political perspective on reality is flawed. To do so is to re-enact Euro-American singularity. Alternatively, it is possible to insist on pluralism, and the essential irreducibility of worlds, of knowledges, of ethical sensibilities, or of political preferences, to one another. This is the relativist response. But there is a third option, or a family of options, in-between. It is possible to observe, in one way very matter-of-factly, that the world, its knowledges, and the various senses of what is right and just, overlap and shade off into one another. That our arguments work, but only partially. That is how it is. But how to *think* this? How to think the in-between?

Here is a possibility. Feminist technoscience writer Donna Haraway, and following her, anthropologist Marilyn Strathern, talk of *partial connections* (Haraway 1991a; Strathern 1991). This is partly a matter of partial connections between different people – or different groups of people. But it is more complicated than this because it also has to do with partial connections *within the same person*. We do not, this is the argument, have single identities. Strathern notes, for instance, that Strathern-the-feminist is not the same as Strathern-the-anthropologist. They write in different ways in different circumstances and for different audiences. At the same time, however, neither are they entirely separate from one another. Strathern-the-feminist is *included* in Strathern-the-anthropologist. Strathern-the-anthropologist writes in a way that is informed by, but not reducible to, Strathern-the-feminist. And the same is the case the other way round.

Strathern's argument is informed both by her reading of (and exposure to) the indigenous cultures of Papua New Guinea, and by contemporary debates about identity politics: the realisation that political alliances which depend on single identities are usually counterproductive in Euro-America where most people are better understood as having multiple, shifting, and partially connected identities.[52] But the position is very close to one of the arguments made by Mol. The crucial word is *inclusion*. The argument is that 'this' (whatever 'this' may be) is included in 'that', but 'this' cannot be reduced to 'that'.

Another example. Mol shows that clinical diagnoses often depend on collective and statistically generated norms. What counts as a 'normal' haemoglobin level in blood is a function of measurements of a whole population. She is saying, then, that individual diagnoses *include* collective norms, though they cannot be reduced to these (Mol and Berg 1994). At the same time, however, the collective norms depend on a sample of clinical measurements which may be influenced by assumptions about the distribution of anaemia – though it is not, of course, reducible to any individual measurement. The lesson is that the individual is included in the collective, and the collective is included in the individual – but neither is reducible to the other.

It appears, then, that in practice there are plenty of partial connections, partial inclusions, partial relations. It also appears that these do not reduce to one another. Haraway:

> Irony is about contradictions that do not resolve into larger wholes, even dialectically, about the tension of holding incompatible things together because both or all are necessary and true.
>
> (Haraway 1991a, 149)

So there is inclusion, contradiction, and sometimes, if we follow Mol, co-operation too. But there is never collapse into singularity. And the arguments against identity politics are just as applicable to the out-therenesses of objects, of non-social realities.

Ontological politics

> what is ceaselessly perfected is a history of erasure.
>
> (Appelbaum 1995, 17)

By now we are well into this journey of erosion – the erosion of the self-evidences of Euro-American metaphysics and their versions of in-hereness and out-thereness. Realities, yes, they are real enough. Relativism is not an issue. One does not have to buy into Euro-American metaphysics to retain a commitment to out-thereness. So, yes, there is resistance. There is stuff. But the character of that stuff becomes less clear, less self-evident. Hinterlands are complex and ramified and only contingently coherent. Thus we have seen that out-thereness is not independent of practice in general, but only in particular. Though, of course, since we are all somewhere in particular, situated, we do not notice the distinction very much. We have seen that it is not prior to practice in general but only in particular. Though, again, since we are all somewhere in particular, we all live within a set of hinterlands of anteriority. Definiteness – I shall talk more carefully of definiteness in the chapters that follow, but again the formula applies. In general nothing is definite. Only in particular. And finally singularity. Mostly, yes, like the physicians and surgeons in Hospital Z, we find singularity. We *make* it. We live within it. But singularity likewise is very specific, very local. And it includes multiplicity.

So singularity is not only the product of specific enacted and visible out-therenesses – though their production of singularities is crucial – but also of a series of mechanisms for avoiding the appearance and the experience of multiplicity: for expelling it into invisibility. For, alongside the practices of multiplicity, there are endless practices for insisting on, presupposing, and producing singularity. There are stories about the singular nature of the world, its objects, and its processes. There are perspectivally inspired distinctions between (provisionally) hidden realities and appearances. There are processes for deleting the unfolding and uncertain nature of practices in favour of apparently stable and separate objects. There are methods for keeping different realities separate and distributing them across time and space. There are methods, we might say, for deferring multiplicity, for keeping it at arm's length, for effecting its disappearance. And, all the while, there is the practical business of reconciling multiplicities, of making the endless and complex ramifications of out-thereness look as if they were much more straightforward.

And such is the context and the character of Euro-American conditions of possibility. These talk of the necessity of singularity, but they also, and at the same time, *enact* multiplicity while erasing it, pushing it into invisibility. Atherosclerosis (or the decision to cancel an aircraft) are said to be single things but they are also being made multiply, or fractionally. So what should we make of this?

One argument is that the insistence on singularity is productive: that this *enables* invisibly multiple practices to craft invisibly multiple realities out-there. For instance, it may be that the idea that there is a single atherosclerosis makes it easier to create many different versions of the disease because it allows participants to assume that they are talking about (and making) a single condition. Or again, it may be that the idea that there is a single decision to be made about whether or not to cancel the TSR2 aircraft allows different participants to make multiple and different decisions. And then (for the argument can be looped back) it may be that it is the fact that multiple decisions are made which makes it possible to arrive at a single decision (for if everyone thought they were making different decisions, then it might be difficult or impossible to arrive at a single decision).

So there are arguments to be made for the current conditions of possibility for Euro-American out-thereness. Visible singularity, and invisible multiplicity. Perhaps this allows us the best of both worlds. Certainly Bruno Latour has made analogous arguments about the seeming (but only seeming) purity of modernity.[53] But there are counter-arguments too. In particular, it can be argued that presupposing singularity and deferring multiplicity into invisibility also makes it impossible to think about partial connections: to make visible the possibilities offered by what we might think of as *the discovery of fractionality*.

The possibilities? Yes. For the discovery of fractionality opens out the possibility that realities might be otherwise. This is like a deeper or broader version of the argument against the notion that biology is destiny. Notwithstanding its continued re-enactment in the popular and esoteric press, a large body of feminist-inspired writing demonstrates that biology is not destiny. And this is not simply because biological entities such as genes do not code for social behaviour. It is also, and much more profoundly, because biological entities are not themselves irrevocably fixed. Anatomical, endocrinological, genetic and expressive bodies are produced in different practices whose consistency – and indeed whose internal consistency – like atherosclerosis, is an uncertain product of moment-by-moment practice.[54] Biology is not fixed except in theory. It is enacted.[55] The problem, then, is that the commitment to visible singularity directs us away from the possibility that realities might in some measure *be made in other ways*. Or, to put it more generally, the presupposition of singularity not only hides the practice that enacts it, but also conceals the possibility that different constellations of practice and their hinterlands might make it possible to enact realities in different ways.

One way of putting this is to say that ‘truth’ is not and cannot be the only arbiter. In multiplicity or fractionality there are *varieties of truths*. But this then means that other values, concerns, and goods are also in play, one way or another, acknowledged or otherwise:

> The reality of atherosclerosis does not precede medical technology and the organization of health care, but is intertwined with them. This implies that the impairments of the body and the politics of crafting tools and organizing health care are intertwined as well. If this is so then reality, the physicalities or the psychology of a disease, cannot be the standard by which to assess treatments. The very advantages and disadvantages, the goods and bads, of *performing* reality in one way or another are themselves open for debate.
>
> (Mol 2000, 96–97)

Following in the footsteps of Foucault, Mol offers a provisional way of grasping at what we are after. Perhaps, in this alternative metaphysics of enacted fractionality we might think of what is made and what is told as an *ontological politics* (Mol 1999). Realities are real enough. These may take the form of in-here statements and the visible out-there realities they describe. This is what we learned from Latour and Woolgar. They may, as we have now learned from Mol, take the form of in-here objects or processes, and out-there contexts of one kind or another that go along visibly with those objects. Method assemblage, we are learning, needs to be about more than representations. But either way – whether we are talking about representations or objects, it becomes clear that truths are not the only arbiters. In an ontological politics we might hope, instead, to interfere, to make some realities realer, others less so. The good of making a difference will live alongside – and sometimes displace – that of enacting truth.

INTERLUDE:
Notes on interferences and cyborgs

Visions of science. Donna Haraway offers an important feminist version of method-and-politics that is also an ontological politics.

What, asks Haraway, would it be to be 'objective'? In standard practice the answer is usually detachment. Disentanglement from location. This is the kind of response offered by Merton, and by the empiricists and the positivists whom he follows. But, says Haraway, detachment is never possible. As we produce knowledges we are all located somewhere, in our practices and in our bodies. We are caught up, as she puts it, in a dense *material–semiotic network*. That is, we are caught up in sets of relations that simultaneously have to do with meanings and with materials.[56] We are entangled in our flesh, in our versions of vision, and in relations of power that pass through and are articulated by us. So detachment is impossible. At best a self-delusion, more often it is also a form of irresponsibility. It is irresponsible because it attempts what she calls the 'god-trick'. That is, it pretends to see 'everything from nowhere' (Haraway 1991b, 189). Whereas it is, indeed, somewhere. And it makes and remakes the textures of the material–semiotic networks.

How, then, to imagine objectivity? The answers vary (Daston 1999). But it seems that we cannot step outside, either to be neutral, or to find some special place – for instance a feminist or women's standpoint that sees further than the alternatives. But if we are always a part of what we explore then, writes Haraway:

> only partial perspective promises objective vision. This is an objective vision that initiates, rather than closes off, the problem of responsibility for the generativity of all visual practices. Partial perspective can be held accountable for both its promising and its destructive monsters.
>
> (Haraway 1991b, 190)

But why *partial* perspective? We have touched on the answer above. Subjects, people, are not coherent.

> The topography of subjectivity is multi-dimensional; so, therefore, is vision. The knowing self is partial in all its guises, never finished, whole, simply there and original; it is always constructed and stitched together imperfectly, and therefore able to join with another, to see together without claiming to be another. Here is the promise of objectivity: a scientific knower seeks the subject position not of identity, but of objectivity; that is, partial connection.
>
> (Haraway 1991b, 193)

We are sets of partial connections. We are, to use the language that I am proposing, both in-here, as subjects, and out-there, as networks of meaningful and material relations. Or, to put it differently, people are, or form a part of, methods assemblages. In their envisioning practices they, we, bundle together not very coherent but nevertheless structured hinterlands. And objectivity, in the way Haraway redefines it, is possible if we acknowledge and take responsibility both for our necessary situatedness, and for the recognition that we are located in and produced by sets of partial connections.

Partial connection. This is the metaphor that lies behind Haraway's trope of the cyborg. Haraway:

> A cyborg is a cybernetic organism, a hybrid of machine and organism, a creature of social reality as well as a creature of fiction. Social reality is lived social relations, our most important political construction, a world-changing fiction.
>
> (Haraway 1991a, 149)

Cyborgs, then, are sets of partial connections. These may present themselves as political. They may present themselves as material (between machine and human, or between human and animal). And, as the citation suggests, they may present themselves as lying somewhere between reality and fiction. For, another visual metaphor, cyborgs are about interfering in the distributions between reality and fiction. Which is why Haraway picks up and plays with the metaphor of the cyborg. A product of the space age, a rarefied achievement of the military-industrial complex, Haraway seeks to tear the metaphor from its location of birth and bend it to interfere, to make interference patterns, in the unjust material–semiotic networks of what she calls 'the current disorder'. Indeed 'a world-changing fiction'. Reality and fiction relate to one another. They are included in one another. But they cannot and should not be reduced to one another.[57] So

> Feminist cyborg stories have the task of recoding communication and intelligence to subvert command and control.
>
> (Haraway 1991a, 175)

Multiplicity and partial connection. There is no gold standard. No single reality. Realities may be made and remade. They are made and remade. This is a version of ontological politics.