

Imagining the field

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Introduction

In most research projects there comes a moment when you must leave your own room, your literature review, your formulation – which will still be provisional – of the question, and ‘go out to encounter directly’ your ‘object of study’. Whether this latter is a sector of an economy, an archive, a social process or, indeed, a region, the activity frequently goes by the name of ‘doing your fieldwork’. The aim of this chapter is to coax you into reflecting upon your ‘field’, and your relationship to it, from a philosophical perspective and thereby to enrich this moment in the overall craft of doing research.

fieldwork How do you imagine (implicitly, in your mind’s eye) your field? What kind of engagement with your subject matter is involved in **fieldwork**, and is it any different from any other? What, precisely, are you up to when you ‘go out into the field’?

All the scare quotes around words and phrases in the preceding paragraphs are there to indicate that much of our habitual terminology of fieldwork deserves further investigation. Indeed, precisely, they raise philosophical issues.

The field vs the cabinet

The whole activity of doing research is frequently imagined in terms of ‘exploration’ and ‘discovery’. The language recalls an earlier age, of voyages and expeditions, and much of the imaginary of that period still frames our implicit conceptualizations of the process of investigation. The notion of ‘fieldwork’, and the complex of heterogeneous understandings of that term, are central to this. The mention of ‘fieldwork’ still evokes the idea of ‘going out there’ to address directly, ‘in the real world’, your chosen object of study. It is a distinct moment in the overall process of doing research. But even in those early days the relationship between work ‘in the field’ and the production of knowledge was the subject of fierce debate.

Debate

Alexander von Humboldt (1769–1859) was one of the most significant and thoughtful, as well as passionate, of ‘explorers’. His aim, in his extensive travels in Latin America, was both ‘exploration’ in a classic sense, and a wonder at the landscapes in which he found himself, and also precise scientific recording and measurement. Felix Driver writes of ‘Humboldt’s vision of scientific exploration as a sublime venture and his emphasis on geographical analysis as a means of scientific reasoning’ (Driver, 2000, p.35). He was also a man equally at home in his study, pursuing further his ‘scientific reasoning’ and philosophical enquiry. He had a ‘commitment to a synthesis between scientific observation and scholarly learning’ (2000, p.53) which, although exceptional, was influential. In particular, he influenced Charles Darwin, whose *On the Origin of Species* appeared in the year of Humboldt’s death.

Yet half a century before that book was published, an attack was launched on this approach to ‘doing science’. It came from Georges Cuvier (1769–1832), also a naturalist but one whose mission was to create a new science of ‘comparative anatomy’. Cuvier’s methods involved detailed anatomical investigation of the internal physiological structures of flora and fauna as specimens, and also of fossils. His workplace was the dissection rooms in the Museum of Natural History in Paris. In 1807 Cuvier vented his anger against the scientific claims of explorers in the field in a highly critical review of a report of Humboldt’s field research (Outram, 1996). It was a key moment in a debate which was to last for decades (and, I would argue, in some senses still goes on) and it wound issues of epistemology, and more generally the nature of science and what could be classified as science, together with spatiality, or the various geographies through which the scientific endeavour comes to be constructed.

At the very heart of this debate was a relation to ‘the field’ and ‘fieldwork’. As Dorinda Outram writes: ‘The concept of the field is a complex one, . . . the idea of “the field” is pivotal in its union of spatial metaphor and epistemological assumptions’ (1996, p.259). The challenge thrown down by Cuvier to men such as Humboldt raised crucial questions which still reverberate: ‘Where was their science located? Indoors or out? Were the systems of explanation created by the work of indoor anatomists superior to the intimate knowledge of living creatures in their habitats which was traditional field natural history?’ (Outram, 1996, pp.251–2).

Here is Cuvier’s opinion:

Usually, there is as much difference between the style and ideas of the field naturalist (*‘naturaliste-voyageur’*), and those of the sedentary naturalist, as there is between their talents and qualities. The field naturalist passes through, at greater or lesser speed, a great number of different areas, and is struck, one after the other, by a great number of interesting objects and living things. He observes them in their natural

surroundings, in relationship to their environment, and in the full vigour of life and activity. But he can only give a few instants of time to each of them, time which he often cannot prolong as long as he would like. He is thus deprived of the possibility of comparing each being with those like it, of rigorously describing its characteristics, and is often deprived even of books which would tell him who had seen the same thing before him. Thus his observations are broken and fleeting, even if he possesses not only the courage and energy which are necessary for this kind of life, but also the most reliable memory, as well as the high intelligence necessary rapidly to grasp the relationships between apparently distant things. The sedentary naturalist, it is true, only knows living beings from distant countries through reported information subject to greater or lesser degrees of error, and through samples which have suffered greater or lesser degrees of damage. The great scenery of nature cannot be experienced by him with the same vivid intensity as it can by those who witness it at first hand. A thousand little things escape him about the habits and customs of living things which would have struck him if he had been on the spot. Yet these drawbacks have also their corresponding compensations. If the sedentary naturalist does not see nature in action, he can yet survey all her products spread before him. He can compare them with each other as often as is necessary to reach reliable conclusions. He chooses and defines his own problems; he can examine them at his leisure. He can bring together the relevant facts from anywhere he needs to. The traveller can only travel one road; it is only really in one's study (*cabinet*) that one can roam freely throughout the universe, and for that a different sort of courage is needed, courage which comes from unlimited devotion to the truth, courage which does not allow its possessor to leave a subject until, by observation, by a wide range of knowledge, and connected thought, he has illuminated it with every ray of light possible in a given state of knowledge. (cited in Outram, 1996, pp.259–61)

Geographical exploration and discovery were central in the development of empiricist methods of modern science and, as Livingstone (1990) argues, they continue to be an important background imagination shaping the practice of research in geography and other disciplines. In this view, we go out into the field to 'discover' things. Cuvier's response was that real scientific discovery can only take place away from the field, in the study. Why? There are three reasons given in that quotation which it is important to pull out here:

Study		Field
the possibility of comparison	vs	the specificity of the field
nature as specimens	vs	nature in action
distance from the fullness of the field	vs	embeddedness within the field

There are other (related) oppositions, implicit or explicit, within that passage by Cuvier which you might at this point just note: between mind

and body (an important issue which will be examined by Sarah Whatmore in the next chapter); and between specified genders (an issue which we shall return to in this chapter).

What is going on in this passage? There were clearly all sorts of oppositions in play: between theoretical speculation and confronting the empirical world; between a kind of isolated systematic logical clarity and an inevitable openness to the ‘thousand little things’ of the real world out there. The struggle here was over power and legitimacy between different kinds of scientist and different kinds of scientific practice. It is a debate, I think, which continues to reverberate; in some sciences in precisely this form (‘you can’t be a proper anthropologist/geographer/. . . if you haven’t been out in the field’), and also in other guises (see below).

But studying this debate from the vantage point of doing research today we might also read it differently. Much of our research will involve *both* the field *and* the cabinet (and, after all, Humboldt insisted on both and Cuvier had to have his ‘samples’ collected somewhere). So I would suggest that what we might understand as also going on here is a differentiation between two distinct ‘moments’ in the overall process of doing research; and one question consequently raised is how we think about the relation between these moments – crudely put, between going out and obtaining your ‘material’/‘data’ and what you do with it when you get back. Each ‘moment’ involves a distinct manner, or mode, of addressing our object of study. ‘Fieldwork’ is one such moment, and the focus of this chapter is, in part, on the nature of its relation to other steps in the research process.

Spatialities of knowledge

These are not only ‘moments’, a temporal differentiation; what was also crucial to the distinctions being contested was the spaces/places of the production of knowledge. The ‘geographies of knowledge’, in the most general sense of those words, have often been argued to be integral to the kind of knowledge which is produced, and to its subsequent status and reception. In this debate about fieldwork, indeed, there is a whole range of spatialities (some explicit, some implicit). What is more, they structure both the epistemological presuppositions and the practice of research. They are real **spatialities of knowledge-production**.

**spatialities of
knowledge-
production**

First, and most importantly, there is a key contrast in spatialities between the modes of investigation in play in Cuvier’s argument. At its starkest, it is a contrast between immersing oneself in the field and distancing oneself in the study or laboratory. Thus Outram argues that Cuvier is:

. . . saying that the knowledge of the order of nature comes not from the whole-body experience of crossing the terrain, but from the very fact of

the observer's *distance* from the actuality of nature. True observation of nature depends on not being there, on being anywhere which is an elsewhere. At bottom, Cuvier is fighting an epistemological battle. (Outram, 1996, p.262)

This establishing of *distance* is crucial. This was the period of the emerging hegemony of that geography of knowledge which insisted on a gap between observer and observed, between knower and known; and saw the production of the idea of objectivity. What this may develop into is the establishment of a gap in kind between known and knower: writes Cuvier, 'it is only really in one's study (*cabinet*) that one can roam freely throughout the universe'. This is not just an 'elsewhere'; it is a kind of nowhere. A gap which (it is supposed) lends placelessness, a lack of locatedness, objectivity. But, the reply might come, from those committed to 'being there', by doing one's thinking and one's science in the field itself, it is possible to capture the complexity and the ongoing movement of the world one is studying. Each position makes a different kind of claim to knowledge: the objectivity (supposedly) lent by distance; the verisimilitude (supposedly) lent by immersion. (And considering these different kinds of claim to knowledge may raise again the question of your question – in the continual back-and-forth between designing your fieldwork and refining your question you need also to consider the kinds of claim your research may propose to make.)

Traces of that opposition between objectivity/distance and immersion are still in play today. A questioning of the possibility of positionless objectivity (the so-called 'God trick') has led some to argue against distancing *tout court*. This kind of argument is implicit in some feminist approaches, which express a distrust of 'the view from above', or urge us to concentrate on 'local' investigations. It is mirrored in the opposition between structure and street – as in Michel de Certeau's (1984) exhortation that we abandon the view from the skyscraper to plunge into the real complexity of the lived life below. Sometimes, to the knowledge-claims being made by this argument is added the claim that such a position in and among (from the situation of) the 'objects' of one's research is also to be preferred on ethical or political grounds.

But as Meaghan Morris (1992) points out, this is a false opposition. On the one hand, distance, or height, or standing on top of skyscrapers, cannot lend 'objectivity'; it is still a view from somewhere. However high you climb, however much distance you put between yourself and your object of study, you will still be located somewhere. You cannot pull off the God trick. Objectivity in that sense is not possible. On the other hand, there is no such thing as total immersion; there will always, still, be a perspective, some things will be missed. You will still be producing a particular knowledge. So maybe that opposition of extremes (between total removal and total immersion) is itself unhelpful. Moreover, abandoning that opposition opens up other, perhaps more productive, questions. If

some ‘distance’ is inevitable between knower and known, how do we conceptualize it and how is it to be negotiated? And if ‘total immersion’ is impossible, how do we negotiate our engagement? These questions will run through the later sections of this chapter. But, before that, there are other spatialities of knowledge-production to consider.

material geographies The second spatiality concerns the symbolic significance of the **material geographies** of knowledge. In many cases, the material spaces/ places of the production of knowledge are both constructed and conceptualized as reflecting the nature of the knowledge-production with which they are associated. Cuvier’s museum was conceived as a heavenly place of order ‘outside’ the real world. Over the ages in the western world there has been a tradition of certain forms of knowledge being produced in places ‘set apart’ from the world – in monasteries, on science parks, in ivory towers. And it can be argued that such locations both reflect the epistemological relation of distancing and use this isolationist spatiality as an adjunct to the legitimation of this form of knowledge and as a reinforcement of the status of its producers. (Chapter 9 will consider in more detail this issue of legitimation and status.) The very place of research can be one of the sources of its authority. Being aware of the locations of your research, and of their social meaning, can itself induce reflection on the nature of the process in which you are involved. Indeed, one of the points you might pull out of this chapter, and use to reflect upon your own research process, is this relation between particular activities of research, and types of knowledge, and their geographical location.

Finally, our imaginaries of ‘fieldwork’ itself are often very strongly spatialized. (The notion of the field as being ‘out there’ is essential to the construction of Cuvier’s argument.) And each of these spatial imaginaries will encapsulate a relationship, maybe only implicit, of inquiry and of power. Johannes Fabian (1983) has analysed what might be called this ‘epistemological positioning’ of the field within anthropology. He argues that for anthropologists the field is not only (classically) geographically distant, it is also usually imagined as temporally distant too; that anthropologists imagine the societies they are studying as ‘further back’ in historical time than the scientist themselves. This manoeuvre of the imagination has significant effects, most obviously in that it increases the supposed distance between observer and observed (and thus, on the model above, increases objectivity). This, as Fabian (1983) notes, is only anthropology’s way of doing what all other sciences do. (It is, of course, also internally contradictory, for the anthropologist’s actual practice ‘in the field’ is to engage with, talk to, these people whom he or she has imaginatively placed in another time.) The imagination of the field is thus a significant element in the articulation of the relationship between the anthropologist and the peoples being studied. It substantially affects, recursively, the nature of the encounter. It is for this reason that addressing the spatio-temporal imaginary within which ‘the field’ is placed is an important part of doing research.

For this to be true, it is not necessary that your fieldwork takes place in some distant part of the planet; it may rather involve studying other texts, or archives, or your own home neighbourhood. Strongly accented spatialities may nonetheless be in play. The East End of London, and many other working-class areas, have frequently been figured as the Heart of Darkness, for example, into which ventures the intrepid researcher. Or again, perhaps more likely these days, one's field may be imagined as 'exotic' or as 'peripheral' or, even worse, as titillating or eye-catching. All these ways-of-imagining are mechanisms of distancing researcher from researched, and thereby – even if inadvertently – of establishing a particular relation of power.

Discovery/Construction/Transformation

The constraints of discourse

Those debates which began in the latter half of the eighteenth century continue today and have significant influence upon the way in which western scientific practice is structured. But, as is common in fierce debates, the early protagonists, as well as disagreeing strongly on major questions, also shared some significant assumptions. For them the aim of science was to find out about the world. The vocabulary of discovery was strong. Cuvier writes of 'truth' and of 'reliable conclusions'. The assumption is that the aim and the possibility of research are to produce an accurate representation of 'the world out there'. At this point in your research this becomes a critical assumption to confront. After all, the whole burden of connotation with which the very term 'fieldwork' has come down to us through the centuries is that this is the moment of going out into that world to investigate it.

And yet, in Chapter 1 you have encountered philosophers who in various ways would challenge this view that our language is, or can demonstrably be, 'a mirror of nature'. The argument of Richard Rorty is that we cannot connect with a world of experience outside language; that what we have available to us, as researchers, is language 'all the way down'. On this view, then, we cannot plunge into the truth of the real (the background imaginary which so much of the history of fieldwork has bequeathed to us); there will always be a gap which we cannot cross. Rather, our task as researchers is to produce the new through the process of inventive rearticulation of language. Here is a strong challenge: 'the field' is not out there waiting to be discovered; rather, it is already linguistically constructed and the researcher's aim must be imaginatively to reformulate this construction in such a way that new avenues can be opened up, new ideas and practices can flow. Discovery: construction. Indeed, we have already begun to recognize the power of 'construction' in the last section, though without commenting upon it in this form. Fabian's

argument about anthropology, for instance, is precisely concerned with how we do not just ‘encounter’ the field but construct it, imaginatively, linguistically. Rorty is arguing for re-imagination in productive ways.

Now, it’s all very well to agree with Rorty as one reads him (he does, appropriately, given his philosophical position, have immense powers of rhetorical persuasion) or to argue his case in a seminar. But what does this position mean for the craft of research? Most tellingly of all, what does it mean when you come to the moment of ‘fieldwork’?

For me, there are a number of things that Rorty argues that can have an important impact upon both how we conceptualize and how we practise that element of research which we call fieldwork. First of all, it emphasizes the need to be aware of prior linguistic construction. This is significant, whether or not we agree that there is an unbridgeable gap between language and something else ‘beyond’. But if you are a strict Rortyan your engagement in fieldwork cannot lead to claims of discovery, or about how things really are. Rather, you will seek to persuade your audience to understand differently, to articulate the linguistic constructions in such a way that they make a different kind of sense. This will mean, perhaps even more strongly than is usually the case in research, that you are self-consciously engaging a debate, an already constituted understanding (academic or popular or political). There is an emphasis (though again this need not by any means be confined to Rortyians) on conceptual experimentation. This does not, even in Rorty’s insistence on linguistic construction, mean that anything goes. There must still be rigour, consistency and relation to purpose. And, finally, that sense of purpose is also very important in a Rortyan approach: you want to redescribe *in order* to disrupt the hegemonic imagination, open up new ways of thinking, remove blockages to potential new forms of practice.

Those who do not accept Rorty’s philosophical position may respond that they agree with, and value, many of these things (the significance of reconceptualization, the importance of a sense of purpose), but query, at this moment of fieldwork, the role of ‘the world out there’ in all this. Does it not have the capacity to surprise us? To force our reconceptualizations? In Rorty’s pragmatist universe it is the researcher who seems responsible for all the surprises, who is the only active agent in this process. What about all the arguments in Chapter 2 about the need to go outside ourselves, to break out of the prison house of language, to stop seeing ourselves as the centre of everything? The notion of an ambulant science, maybe even the notion of surprise, implies the possibility of an unknown into which we may venture. But if our encounter is language all the way down, even the unknown (if there can strictly be said to be such a thing) will come to us immediately framed by the concepts we already have available to us.

That latter point is, of course, even more strongly made by Foucault (see Chapter 1), particularly in his earlier work. While Rorty is pretty ebullient about our freedom to redescribe, Foucault points to the power in

and the powers behind dominant discursive practices. In Chapter 1 the concern was with how the questions we ask are constrained by the discursive rules and conventions already available to us. In fieldwork, this same argument points to the limits upon our freedom to re-imagine, to reconceptualize.

This is especially to be recognized to the extent that fieldwork is thought of, as it so often is, as a voyage ‘into the unknown’ (Driver, 2000, p.268). And once again the terminology of discovery can provide food for thought. Much has been written about how the Europeans who first landed upon the shores of what was to become the Americas came to terms with what they found. They were indeed faced with what was, to them, the unknown. On the one hand, all they had at their disposal, linguistically and conceptually, was what they had brought from Europe. So, both in order to make some sense of what they found, and in order to be able to communicate it back home to an expectant European public, as well as demanding European paymasters, they had to struggle to arrange this new reality into the terms which they already knew. The discursive constraints were very real. Wayne Franklin (1979, 2001) has analysed this struggle as it faced Hernán Cortés. He writes of how this rebellious Spanish conquistador had to communicate back to Charles V in ‘canons of allowable speech [which] shaped the manner in which he perceived and acted in the world of Mexico’ (Franklin, 2001, p.120). In other words, the discursive regime, outside which he could not think, moulded the reality he confronted. But also, for himself, he had to struggle to make sense. Franklin writes of Cortés undergoing ‘a formidable cognitive test’ (2001, p.120) and argues that ‘we can . . . see his literary efforts as an . . . attempt to fill the almost aggressive silence of the West with words, to convert “noise” into meaningful sound’ (2001, p.120). Yet that last sentence gives a clue also to an opposing process: that, in Franklin’s interpretation, Cortés was not the only agent in action. There were tensions between word and thing: ‘the voyager found himself so far beyond the bounds of his known world that knowledge and words alike were threatened with a severe breakdown’ (2001, p.125).

It is in this context that we can appreciate why, as pointed out in the last section, such voyages were so significant in the establishment of the importance of empirical enquiry – and why in its day this was a liberatory, even revolutionary, move. For the philosopher Francis Bacon (1561–1626 – in other words, two centuries before Cuvier), the voyager, by the very fact of discovering new things not immediately capturable within the old, set ways of European thinking, was a brilliant exemplar of the possibility of breaking free from the ancient established authority of book-bound scholasticism. For him, ‘the library as a symbolic enclosure of authority stood opposite to that “road” which he urged his readers to pursue. By breaking through the enclosures of traditional space, the American traveller also was breaking the bonds of received language’ (Franklin, 2001, p.125).

I wanted to come back, full circle, to Bacon here, and to the dawn of the age of empiricism and of ‘the field out there’, for one particular reason. It indicates how philosophical shifts are themselves historically embedded. And this in turn is a caution to us: both not too scornfully to deride ‘past’ positions nor to be too confident of the ‘truth’ of the newest arrival.

Of course, in your own fieldwork there may be rather less of this journeying into the pure unknown. But you may well be attracted precisely by an element of the not (yet?) understood. Sometimes the aim of research may be to disrupt the reassurance of the apparently familiar (where, you suspect, the very familiarity can be obscuring) precisely by *rendering* it strange. You may have posed a question, as discussed in the last chapter, which precisely tries to remain open to the unfamiliar, even to ‘hover on the edge between the known and unknown’. Indeed, it can be argued that much of the writing of Alexander von Humboldt, which embraced the minute documentation of ‘scientific data’ alongside the expression of a sensuous exhilaration in the landscape, was his way, precisely, of maintaining this position. More simply, you may just wish to insist upon an element, at least, of ‘finding out’ (note your position here in relation to those who insist upon the prison-house of language). If so, all the foregoing arguments would urge upon you an acute sensitivity to the fact that your field, and much that you find therein, will come to you already organized into a frame of reference. One can never be totally questioning (partly because it is likely to become circular, and partly because you do need to finish your research at some time) but do question, be aware, as much as you can. (It is also the case, of course, that one often cannot be aware of all the constraints and confinements.) On the other hand, the tale of Cortés may enable you to open up a space of engagement, where you may become aware that maybe you are forcing well-worn categories, or categories and concepts to which you are committed, upon recalcitrant material, where the world speaks back. And once again, pondering all this will give you another opportunity to refine your question further.

Bringing the world back in

Let us pause for a moment and consider again an issue which was raised in the previous section: the need to be explicit, and reflective and critical, about the spatiality of knowledge within which one is working. At this point I am thinking particularly about the imaginary spatialities through which we express epistemological positions. Thus Outram argued that in Cuvier’s day the ideal positioning for the achievement of objectivity was in a ‘heavenly’ location removed from the particularities of the world one was studying, a location which was intended precisely to obviate the ‘problem’ of locatedness. There was a gap in kind between the scientist and the field. Rorty also imagines a gap, and again it is a gap in kind, but this time it is between reality and representation, between ‘the world out there’ and

language. (In developing this line of thought in second-stage pragmatism, Rorty was part of a wider movement called the ‘linguistic turn’.)

This is a very general epistemological position, in that it concerns the whole of our relationship, as linguistically able (indeed often linguistically defined) human beings, to the world beyond us. What we are exploring in this chapter, however, is ‘fieldwork’ and ‘the field’. Two points immediately arise. First, the term ‘fieldwork’ has greatly extended in its meaning from those days when Humboldt set out for Latin America. Today it is often used in a more general way, to indicate original empirical work. Your field may be an economic sector, a set of people, a group of social processes, or an archive or other texts. Nonetheless, fieldwork is still a specific activity within the wider research process. And that indicates the second point: are not discourses and texts, books and tables and diagrams just as much of ‘the real world’, and are not other stages of your research (your literature search perhaps) also engagements with that world?

One approach which takes this position is perhaps best exemplified in the work of Bruno Latour. Latour is a philosopher and social scientist and his writing, and his intellectual contribution, span a huge range. By asking awkward questions, and by maintaining a steady focus on practices, he has attempted to overturn a number of ways of thinking which have often been taken for granted. He has stressed both the **multiplicities** involved in all practices and processes (often using terms like ‘collectives’) and the **effectivity** (the ‘actant’ status) of things other than human beings. He has become particularly known for his contributions in the spheres of actant network theory (ANT) and science studies. The wider philosophy of Latour will be explored in later chapters. Here, however, I want to take advantage of the fact that on occasions Latour has addressed specifically the question of fieldwork and its relation to (its setting within) a wider practice of research. Indeed, I want to focus on one chapter of his book, *Pandora’s Hope* (Latour, 1999) – Chapter 2, ‘Circulating reference’ – in order to interrogate Latour in a particularly focused way, and in relation to just a part of his work. Doing it this way, however, allows some important issues to emerge concerning fieldwork. Later you can put them in the context of his wider work.

In this chapter, Latour does a very Latourian thing: he pays ‘close attention to the details of scientific practice’ (1999, p.24). He does his own field research on a group of scientists doing *their* field research, which concerns the shift of the border between forest and savannah at Boa Vista in Amazonia. For Latour, it is ‘a chance to study empirically the epistemological question of scientific reference’ (1999, p.26). It is a detailed study, documented in detail.

And what emerge are a picture and a proposal. Latour jumps into that supposed gap between the field and the written-up research to investigate the practices which he argues it in fact entails. He points out the numerous distinct operations which it involves (we might think of operations such as: deciding how to sample, collating information under different headings in

your filing system, fixing on the key questions for an interview or series of interviews). Latour argues that each of these distinct stages in the research process involves a *transformation*. You turn the object before you into something different. You make it *mean* something which will feed into the next stage of research, for which it will in turn become an object, to be worked on further. At each stage of research, in other words, what you have before you (whether it be, for example, an interviewee or a set of interview notes) has characteristics both of being a ‘thing’ and of being a ‘sign’ (1999, p.60). At each stage you take the thing created at the previous stage (say, your interview notes) and work on them to produce a new sign – maybe a redistribution of the transcript under a sequence of headings. At each stage, says Latour, something is lost (locality, particularity, materiality, multiplicity, continuity) and others things are gained (compatibility, standardization, text, calculation, circulation, relative universality). In his terms, there is both ‘reduction’ and ‘amplification’. At each stage there is an engagement, a transformation, a process of creation. One should never speak of ‘data’ as something given, argues Latour, but of ‘achievements’.

Now, for Latour the implication of the elaboration of all these steps in the research process (this chain of transformations) is that we must challenge that spatiality of knowledge which envisages an uncrossable gap between two polar extremes, of ‘real world’ on the one hand and ‘representation’ on the other. Thus, he argues:

The philosophy of language makes it seem as if there exist two disjointed spheres separated by a unique and radical gap that must be reduced through the search for correspondence, for reference, between words and the world. . . . While following the expedition to Boa Vista, I arrived at a quite different solution . . .

. . . Phenomena . . . are not found at the *meeting point* between things and the forms of the human mind; phenomena are what *circulates* all along the . . . chain of transformations. (Latour, 1999, pp.69, 71; emphasis in original)

Not only is every object both ‘thing’ and ‘sign’, depending on its positioning within the process of research but, insists Latour, ‘There is nothing privileged about the passage to words’ (1999, p.64). This, then, is a radically different spatiality of knowledge from Rorty’s: ‘at every stage, each element belongs to matter by its origin and to form by its determination; it is abstracted from a too-concrete domain before it becomes, at the next stage, too concrete again. *We never detect the rupture between things and signs*’ (1999, p.56, emphasis added).

This view also alters the way in which ‘the field’ itself is spatialized. Latour is very clear that there is a difference, for his scientists, between the field and the room in the university to which the information will be taken. Indeed, in his characterization of reduction and amplification he makes some of the same distinctions that Cuvier makes. He writes also of the

room in terms of the advantages of comfort, in terms of being the place where all the ‘achievements’ (recordings, interviews, documents, for instance) can be brought together for the unifying gaze, and where they can be shuffled around while the researcher thinks (1999, pp.36–8). Elsewhere again he writes of ‘disciplining’ the field. What Latour adds, though, is an emphasis on each stage as a distinct kind of engagement, where a different mixture of things, signs and activities is enrolled, and also an emphasis on each stage as being open, both through its position in a chain of transformations and because each operation (through its artefacts and categories) is produced through and therefore connects out to a wider world of research and scientific production. ‘The field’, then, begins to seem less like a space which one goes to and subsequently leaves. Rather it is a much more complex structure which one transforms; it is still present, in transformed form, in your written report (1999, pp.70–1), and the processes of transforming it are present, too, in every operation ‘within’ the field. The field and the cabinet, then, are distinct certainly, but also are utterly linked through a chain of your own production.

There is much here that can enrich the way we go about fieldwork. It encourages an awareness of each operation. It points to the need to consider what each operation is really doing. (As you collate notes from interviews, or records of observations, for instance, you are transforming them into a particular distillation: creating something, engaging with the object to produce a new sign.) You need therefore to be aware of both what you are gaining and what you are losing and aware, too, of the collectivity and materiality of each operation. It is in the next chapter that these stages (what is sometimes lumped together under the term ‘data collection’) will be considered. Here, what is important is to note that in this view there is no huge uncrossable gap between you at your desk reading ‘the literature’ and a field ‘out there’.

An adherent of the linguistic turn might want to respond to this onslaught, and it is important that we give them some right of reply. First of all, one could argue that what has happened here is that a big gap has been reduced to a lot of little ones (within each transformation). Even if we recognize the constitution of phenomena as inevitably hybrids of thing and sign, there remains the question of where the ‘sign’ aspect derives from. This study of Amazonian fieldwork is very much an empirical inquiry. Latour gives full recognition to the necessary dependence on concepts and categories inherited by the researchers from the earlier studies and from a range of fields. But here those categories (elements of wider discursive frameworks) are taken as given (as indeed in practice they often *are* taken). But what about a piece of research that aimed at reconceptualization? What of re-signing, of ‘re-describing lots and lots’? The lack of attention here is ironic given Latour’s own conceptually innovative record. Indeed, on the first page of this chapter he tells us that he is going off with this bunch of scientists because ‘I want to show that there is neither correspondence, nor gaps, nor even two distinct ontological domains, but an

entirely different phenomenon' (1999, p.24). In other words, he goes off with a real purpose: to redescribe.

Yet, and to circle round again, in his *own* field research *into* field research Latour is disarmingly unreflective. By simply describing, by 'paying close attention to', by examining in detail (1999, p.24), he will give us a more realistic picture. What of the concepts and categories, the discursive regimes, which he brings to this close paying of attention? Later, he acknowledges that he is posing as 'a simple spectator' (1999, p.72). Nevertheless it is important to recognize that the injunction just to look at what researchers do is also, itself, an epistemological position.

Relating to the field

'The field' itself is a spatial concept with material, practical, effects. Whatever imaginary you operate within (and it would be difficult to manage without one), it will have implications. It will have effects on your relationship to the field, on the nature of your own identity as a researcher, and on the range of practices and behaviours which are thereby enabled. It will also raise questions of power and responsibility. We have already touched upon this, particularly in the discussion of spatial imaginaries of fieldwork: anthropologists displacing their field to the past; the imagining of the field as 'exotic', and so forth. Those imaginations stand at one extreme, perhaps. In them, the field is at some distance; it is a bounded space separated from the academy where other stages of research are performed; you the researcher are not implicated in it; you just go there and, even more significantly, you leave. Such an imagination is likely to induce, or to reflect, an assumption of power on the part of the researcher. This may not be at all deliberate, but imagining the field as 'exotic', for instance, raises all kinds of questions, about objectification, about the assumption of a right to investigate, about the centrality of the imagination of the researcher, for instance. At the other extreme, Katz (1994), writing of ethnography and of the difficulty of drawing boundaries 'between "the research" and everyday life; . . . between "the field" and not; between "the scholar" and subject' (1994, p.67), argues that she is 'always, everywhere, in "the field"' (1994, p.72) and she explores the issues of relationships and of power which necessarily have to be faced.

The work of Bruno Latour, stressing the myriad of small but crucial transformations which connect field and study, so that the moment of study is in the field and the Amazon forest and savannah were brought back (transformed) to his study, has already begun to raise questions about that 'here-there cartography' of doing fieldwork. What his work clearly

territorial
cartography

does is to challenge that **territorial cartography** where the field is a bounded space. Here it is open and porous, and connected by a chain of practices (and also by the complex networks, human and non-human, within which those practices are set) to the rest of the research process.

Here spaces are constructed through relations. And once the question of 'relations' is on the agenda, then not so far behind should come questions concerning the nature of power within those relations. How, then, can we relate the disruption of the settled territorial cartography to questions of power? Of particular importance here have been some strands of feminist philosophy.

It is often indeed argued that *fieldwork* is classically characterized as a masculine activity, while the *field* itself is positioned as feminine. There are all kinds of source for this, including the frequent historical associations between going into the field and military endeavour, on the one hand (see, for instance, Driver, 2000), and the counterposed connoting of the field itself as passive and available for entry on the other (see, for instance, Clifford, 1990). Matters are, however, also more complicated than this, and go deeper philosophically.

Thus Georges Cuvier was clearly all too aware of the prevailing heroic, manly image of the fieldworker and feels he is obliged to struggle, to assert, in competition, the 'courage' required of intellectual labour, and the different kind of manliness that characterizes sedentary scientific production. Whatever these scientists are doing, it has to be understood as masculine. And indeed the distancing, universalizing, procedures of the cabinet have subsequently been taken to task for their 'masculine' structurings. One might reflect that what is at issue is representational power rather than any essential masculinity or femininity. One of the things most evident, here, about 'masculinity' is its mutability. Similarly, within the discipline of geography, while there is an extraordinarily strong tradition of characterizing fieldwork as a manly rite of passage, more recently other geographers, including many feminists, have used 'fieldwork' precisely to challenge some of the existing orthodoxies (see Hyndman, 1995; Sparke, 1996). (Shades here of Francis Bacon.) So the means and mechanisms of gendering are by no means simple.

Nevertheless there is a consistency, although of a different kind. In the opening paragraphs of this section, a distinction was made between the field conceptualized as a separate and enclosed entity and the field as more clearly constructed in relation to the researcher and to other stages in research. The first conceptualization is characteristic of a way of thinking which was introduced in Chapter 3, where the world is imagined as consisting of 'atomistic singularities' (Cheah and Grosz, 1998, p.6). Things are what they are, and only then may they come into contact, interact. It is a way of thinking which has been much challenged by feminism. Moreover, in this particular matter of field and fieldwork, as has also been pointed out by feminists, not only is the field a separate place, already given, but the relation between field and fieldworker has often been viewed in dualistic terms. The field is everything that the fieldworker is not, and vice versa. The fieldworker is active, thinking, part of culture. The field is passive; it is the real world; it is nature. Field and fieldworker, in other words, are counterpositionally characterized through some of the classic

dualisms of western thought: dualisms that counterpose mind and body, culture and nature (and, indeed, possibly, the real and representation). As the last chapter pointed out, to this way of thinking what is not natural is understood as cultural; and nature is passive while culture is active. The imagination of the field outlined at the beginning of this section depends upon such dualisms.

One of the reasons why it is important to be aware of the form of this classic imagination is that it has significant implications for the distribution and nature of power between fieldworker and field. The fieldworker is the only active agent. The field itself actively contributes nothing; it only offers up. Neither field nor fieldworker are (imagined to be) changed by the encounter.

Moreover, the complex concatenation of dualisms structuring much of western philosophy has positioned 'the feminine' as the passive pole, along with body and nature, as against the active masculinity associated with culture and mind. For a whole variety of reasons, therefore, feminist philosophers have been at the forefront of challenging these presuppositions (see, for example, Lloyd, 1984, and the collection edited by Nicholson, 1990). As the last chapter pointed out, just about all strands of feminist philosophy 'think about relations between things'. Indeed, one of the most significant lines of argument is that we should think about things as constituted *through* relations.

If these challenges are applied to our imagination of field and fieldwork, then all kinds of further questions arise, questions which pertain to the formation of the identities of each term and questions about power and responsible behaviour. Indeed, further questioning can problematize (or enrich) the situation even more. For there is another characteristic of what we might call the 'classic' imagination of the field which deserves attention and which relates back to our earlier discussion about discovery/construction/transformation. If you take a position that the world out there, or more specifically your object of study, can speak back, that it too is an active agent in this process of research, then what is at issue is a real *two-way* engagement. Many imaginations of the field have pictured it as static, as synchronic. A revision of that imaginary would make the field itself dynamic; and it would make *fieldwork* into a relation between two active agents. It would recognize it as a two-way *encounter*.

Now the question of 'the ethics of the encounter' has been the subject of much philosophical attention which will be addressed directly in Chapter 6. But some initial points arise already from the discussion in this chapter.

fieldwork as an
engagement

Thus, as a first point, this encounter, in the actual practice of doing fieldwork, may take a huge variety of forms. In this chapter, the issue has emerged out of a very general discussion about the nature of **fieldwork as an engagement**. We have also seen, however, that there are debates even about the initiating terms of that engagement (whether one can have direct access to something called the real world, and so forth). Moreover, the

nature of the fieldwork varies dramatically between disciplines and between individual research projects. Much will depend on the nature of your research question. The encounter may be focused through interviews with other human beings (but then the latter may be among the most powerless people on the planet or they may be the power-brokers of major corporations or international institutions). Or the encounter may be through already constituted statistical sources, or through an archive. Or, and this is important to stress, the encounter may not be with human beings or their representatives/representations at all: there is also an ethics to the encounter between human and non-human. The immediate point is that what constitutes an encounter will vary, and thus very different ethical questions will be raised.

Secondly, although I have stressed in this chapter the significance of the implicit, but powerful, spatialities of our imaginations of this practice of fieldwork, I personally am wary of attempts to address the problem through a spatial response alone. For instance, a common response across the social sciences to, say, the gap between field and academy is to claim that one stands in-between. ‘Between-ness’ and a whole set of associated tropes has become very popular. In my own opinion, such metaphorical ‘re-spatialization’ alone will solve nothing. It leaves unaddressed the issue of the character of the social relations constituting that space. ‘Between what?’, one might ask (between two separate and still not mutually implicated atomistic entities?). And what role is this ‘between-ness’ enabling? (It could be mediator, translator or powerful orchestrator.) In other words, the ‘political’ questions concerning ethics and relative power remain unspecified. All spaces are constituted in and through **power relations** and it is this co-constitution which must be addressed (imagining spaces as relational poses the question of the nature of the relations): it is this which so much of feminist philosophy has been trying to stress.

Thirdly, the ethical issues of the encounter are not easily resolved. This is true in two senses. One is in a rather practical way: it is often, though not always, going to be the case that it is the researcher who has the initiating power to define a field in the first place. The aim is not to ‘remove’ power from the situation (which is impossible given its constitutive nature in social relations; and power is enabling as well as constraining) but to work on its nature and distribution and to recognize the inequalities which will almost inevitably remain. But, in another sense, these questions are not simply resolvable precisely because they occur in practical, particularized situations. On the one hand, there may be an ideal, an absolute imperative, against which you would like to behave; on the other hand, there are the real constraints and particularities of this specific situation. Jacques Derrida has written of this kind of structure and argued that what it involves is a necessarily double or contradictory imperative (see, for instance, Derrida, 2001/1997). There is no ‘resolution’ to this situation in the sense of being able to have recourse to a foundational rule or an eternal truth. Rather, the truly ethical or political element

consists precisely in being forced to negotiate between these imperatives. Your ‘resolution’ of this negotiation is unlikely to be amenable to assessment as ‘correct’ or ‘incorrect’. Rather, on Derrida’s argument, what will be at issue will be appropriateness to the particular situation.

judgement And this raises a final point, which will be taken up more fully in Chapter 6. What is involved here is **judgement** and the sense of responsibility of you, the researcher. The lack of single, correct answers does not mean you have to plunge into an endless vortex of self-doubt. (The same point was made in a previous section about reflexivity.) There will, moreover, be others to talk to, other work to read and consider, and established sets of guiding conventions (we might imagine these last as temporally congealed forms of society’s thinking-so-far on the question). In the end, however, this is one of many occasions on which considered, informed judgement is a crucial element in the craft of being a researcher.

Further reading

In *Geography Militant: Cultures of Exploration and Empire* (Blackwell, 2001) Felix Driver explores in great detail the history of fieldwork within geography. Johannes Fabian’s *Time and the Other: How Anthropology Makes its Object* (Columbia University Press, 1983) provides a particular example of how anthropology imagines the field and positions us in power relations to it. In *Pandora’s Hope: Essays on the Reality of Science Studies* (Harvard University Press, 1999) Bruno Latour follows the practices of scientists ‘in the field’, ‘back in the study’ and the journey between them, as a basis for wider arguments about the nature of the research process. In relation to this chapter you might like to focus on Chapter 2, ‘Circulating reference’ (pp.24–79).