5
Generating materials

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> How to succeed in 'working together' . . . where phenomena continue . . . to speak in many voices; where they refuse to be reinvented as univocal witnesses. (Stengers, 1997, p.90)

Something solid to go on

What 'data' do I need, how much is enough and how should I go about obtaining it? These are the sorts of question that vie for your attention along with the pressing demands of refining a topic and formulating an approach to it as your research gathers pace. In the early career of a research project or thesis it is not uncommon to experience a kind of vertigo as theoretical ambitions heighten with the momentum of your reading, while their relation to the 'real world' seems to become increasingly remote. This is the moment in which the idea of data as something solid to go on is at its most seductive. Standard accounts of the research process suggest that all you have to do now is go out and 'collect' some of it. Indeed, for some types of research, such as statistical analyses of disease patterns or medical service use, the identification of a viable 'data set' is often treated as a prerequisite for defining a topic and the kinds of question you can ask. Taken at face value, the business of data 'collection' that abounds in introductory texts on research methods bears an uncanny resemblance to the activity of squirrels in the autumn, gathering up acorns and hoarding them as treasured stores of winter food. Whether interviewing actors *in situ*, manipulating the digital population of census returns, or trawling documentary archives for traces of past lives, data collection mimics this squirrel–acorn relationship as you scurry about after nuggets of 'evidence' just waiting to be picked up, brought home and feasted on at a later date. This rodent model of data collection has already been challenged by Doreen Massey's interrogation of the space–times of 'fieldwork' in the previous chapter. Moreover, in practice, I'm not sure that many social scientists would recognize this as a description of their own experiences of doing research. But its hold on our sense of what we *should* be doing is perpetuated to the extent that these experiences are

written out of, or as deviations from, this model in research accounts and methods manuals.

This chapter adopts the notion of 'generating materials' to further unsettle this stance towards the activity of doing research and its implicit distribution of energies, in which the researcher does all the acting while the researched are merely acted upon. This alternative formulation suggests that data, like questions, are produced, not found, and that the activity of producing them is not all vested in the researcher. I trace some of the consequences of this reformulation for 'doing' research. For a start, it trips up the apparently straightforward notion of research as an *investigation of the world* which positions the researcher at one remove from the world and renders 'it' a passive object of study. But the purpose of the chapter is not just to unsettle and trip up conventional ways of thinking about how research is, and should be, conducted. It also sets out to provide some way-markers and tactics for those of you who may want to pursue the consequences of these arguments as you set about **generating materials** for yourselves. In thinking through this process I draw on the writing of the contemporary philosopher Isabelle Stengers, which I will introduce in a little more detail in the section 'Stengers at work'. In particular, I work through some of the implications of her account of research as a process of knowledge production that is always, and unavoidably, an *intervention in the world* in which all those (humans and non-humans) enjoined in it can, and do, affect each other. This suggests a mode of conduct that, as she puts it in the quotation with which this chapter opens, demands a more rigorous sense of, and commitment to, research as a **co-fabrication** or 'working together' with those whom we are researching.

generating materials

co-fabrication

Towards a more-than-human social science

Some aspects of this line of argument may seem familiar in so far as they resonate with the well-established concerns of humanistic critiques of scientific methods and their empirical emphasis on the 'objective' measurement of observable phenomena and their interrelations. Such methods – so these critiques go – are inappropriate to social research, because people, unlike any other object of study, are purposeful agents whose own understandings of their actions in the world must be incorporated into, and even allowed to challenge, research accounts of them. Humanistic critiques have spawned a rich variety of social science research practices called qualitative research methods, from focus groups to discourse analysis, in which the spoken and written word constitute the primary form of 'data' (see Seale, 1998; Limb and Dwyer, 2001). These arguments have been well rehearsed in relation to one of the most widely used methods of generating data in the social science repertoire – the interview. For example, Holstein and Gubrium mobilize them against what they call the 'vessel-of-answers' approach to interviewing found in many research methods manuals,

particularly its emphasis on 'neutrality' as the ideal mode of conduct to prevent the interviewer from 'contaminating' what the interviewee has to say:

> In the vessel-of-answers approach, the image of the subject is epistemologically passive, not engaged in the production of knowledge. If the interviewing process goes 'by the book' and is non-directional and unbiased, respondents will validly give out what subjects are presumed to merely retain within them – the unadulterated facts and details of experience. Contamination emanates from the interview setting, its participants and their interaction, not the subject, who, under ideal conditions, serves up authentic reports when beckoned to do so. (Holstein and Gubrium, 1997, p.117)

But Stengers' philosophy of science is of a different order. Her imperative of 'working together' in the knowledge production process is not derived from any appeal to the uniquely human qualities of the research subjects with whom social scientists have predominantly concerned themselves. Rather, it amplifies the ways in which all manner of entities, non-human as well as human, assembled in the event of research affect its conduct, exceed their mobilization as compliant data and complicate taken-for-granted distinctions between social subjects and material objects reproduced through scientific divisions of labour. Thus, thinking through research in the company of Stengers challenges some of the methodological assumptions associated with the humanistic legacy of qualitative research practices in the social sciences, as well as those of the scientific methods that they critique. Such disputes have been staged for the most part in *epistemological* terms, that is in terms of the kinds of 'how can we know?' question that we saw in play in Chapter 1. This staging restricts the terms of any answer to the relationship between language, as the currency of human thinking and knowing, and matter, as the stuff of the world out there. Questions about 'how can we know the world?' hereby become reformulated as questions about 'how do we represent (or, in Rortyian terms, redescribe) it?' By contrast, Stengers picks up the argument in Chapter 2 that our disposition towards the world we study is better conceived as one of craft than discovery. If we are immersed in the world through bodily exchanges of various kinds, rather than at a distance from 'it' mediated only by language, the philosophical question is recast in *ontological* terms – 'how does the world make itself known?'

The philosophers and social theorists interrogated in Part I will have given you a sense of some of the many and varied ways in which this interval between word and world has been traversed. Gillian Rose's discussion in Chapter 3 of the discursively 'constructed' bodies that populate certain variants of feminist theory provides a useful example. To greater (e.g. Rorty) or lesser (e.g. Foucault) extents and with important exceptions (e.g. Deleuze), many of those whose ideas you have encountered thus far in this

book focus questions about the uncertainties of human knowing as if these uncertainties were confined to the properties of human cognition and language. Meanwhile, the stuff of the world remains 'out there', untroubled and untroubling, waiting impassively for us to make up our minds and making no difference to the knowledge production process. It is a stance captured in Rorty's claim that 'it is language all the way down' (discussed by John Allen in Chapter 1). By contrast, Stengers redirects attention to the uncertainties generated by the complexities and energies of the material world, including those of human embodiment. In this, Stengers' project is at odds with the terms of dispute set in train by the **word–world settlement** and stylized as a choice between two positions – 'constructionism' (what we know is an artefact of human thought) versus 'realism' (what we know is an artefact of the real world). Rather, her philosophical imperative of 'working together' challenges the intellectual entrenchment of this settlement and its hold on the terms of exchange between social scientists and natural scientists in the late twentieth century, illustrated with such venom in the so-called 'science wars' (Gross and Levitt, 1994).

word–world settlement

'Working together' in practice

Given their philosophical divergence, it is perhaps unsurprising that, with one important exception, Stengers' work has made relatively little impression on the English-speaking social sciences. For the most part, her philosophical project has not yet been 'domesticated', in the sense of having been made useful to social theory and research agendas. The exception is the research community and literature of science and technology studies that have flourished over the last two decades or so, particularly that in Europe. Her influence is most in evidence in that gathering of energies which gelled momentarily into actant network theory (ANT), associated with the work of such notable (if increasingly reluctant) intermediaries as Michel Callon, Bruno Latour, John Law and Annemarie Mol (see Law and Hassard, 1999). Here, Stengers' project finds resonance in substantive research concerns with the practices and artefacts of scientific knowledge production, and in theoretical commitments to rethinking the very idea of society as an exclusively human domain distinct from that of a material world of things. Science studies have long since over-spilled their early confines as an interdisciplinary niche through lively conversations with sociologists, geographers, anthropologists, historians, literary theorists and others (not to mention scientists). Stengers' philosophical influence has travelled through such conversations, most forcefully in the work of Latour. The interweaving of their projects is apparent from Latour's frequent references to her work, a compliment that is returned in her writing, and from his foreword to the English translation of her book *Power and Invention* (1997), a book which Stengers dedicates to him (and Felix Guattari). In light of this, I shall revisit Latour's essay, 'Circulating

reference' (1999), discussed in the last chapter, as a way of making connections between what is at stake in the approach you adopt to 'data' and the issues raised about constituting the 'field'.

Without being directly derivative of them, ANT can be seen as giving methodological expression to Stengerian principles through its trademark adaptation of *ethnographic* research methods to the study of scientific conduct. Originating in anthropology, and now well established throughout the social sciences, **ethnography** is distinctive in its approach to what constitutes 'data', paying as close attention to social practices (what people do) as to social discourses (what people say). It also attaches particular weight to 'doing fieldwork', requiring the researcher to spend significant periods of time working with those whom they are studying, engaging in their everyday routines and exchanges – a process formalized as 'participant observation' (Cook and Crang, 1995). In this sense, ethnography can be argued to come closest to the notion of 'generating materials', as opposed to 'collecting data', of any method in the social sciences. ANT amplifies two currents in this body of research practices. The first concerns the spaces of fieldwork or the question of 'where' to engage in generating materials discussed in Chapter 4. Here, emphasis is shifted from working in single locales, such as a laboratory, to 'multi-sited' fieldwork that traces networks of association connecting several (Marcus, 1995). The second concerns the objects of study or 'what' to count as relevant material. Here, a 'symmetrical' approach is adopted that redistributes attention from exclusively human actors, what scientists say and do, to the host of non-human devices, codes, bodies and instruments that are active parties in 'doing' or practising science (Callon, 1986).

ethnography

In this chapter, I want to outline some key elements of Stengers' philosophy of science and illustrate their implications for generating materials should you want to follow them through. In particular, I will elaborate three related elements in Stengers' philosophical vocabulary, which, as you will expect of philosophers by now, is uniquely her own. The first is the idea of '**mapping into knowledge**', an approach to knowledge production that by-passes the word–world settlement, and the constructionist/realist choices it sets in train, by positing research as an event co-fabricated between researcher and researched. The second element is her criteria for what constitutes good research, which centre on researchers placing themselves 'at risk' in terms of entertaining, and even inviting, the non-compliance of those whom they are studying. The third element is her commitment to what she calls 'cosmopolitics', a politics of knowledge in which the admission of non-humans into the company of what counts invites new alignments of scientific and political practices and more democratic distributions of expertise. In the closing section of the chapter, I will return to the still pressing anxieties of 'what data do I need, how much is enough and how should I go about obtaining it?' with which I began, to outline the lessons and pitfalls of reworking these anxieties through Stengers.

mapping into knowledge

Stengers at work

As a philosopher of science based at the Free University of Brussels, Stengers has worked closely with scientists, including an early and influential collaboration with the Nobel prize-winning chemist Ilya Prigogine in a book published in English under the title *Order Out of Chaos* (Prigogine and Stengers, 1984). Her subsequent work, which is only now being translated, exposes more directly her philosophical allegiances. The first of these is *Power and Invention* (1997), a collection of essays, most of which appeared in French during the 1980s, and the most recent is *The Invention of Modern Science* (2000). Here, she refers directly to the philosophers whose thinking has most inspired her own. These include historical figures such as Lucretius, Leibniz and Whitehead, about whose work she has written (see Prigogine and Stengers, 1982; Stengers, 1994), and older contemporaries such as Michel Serres and Gilles Deleuze, with whom she has engaged. The common thread she identifies in their work is: '. . . the attempt to speak of the world without passing through the Kantian tribunal [see below], without putting the human subject defined by his or her intellectual categories at the centre of their system' (Stengers, 1997, p.55).

However, Stengers is anything but an 'ivory tower' philosopher and her writing is alive with a political militancy and scientific passion that make her a public figure in her native Belgium. These interconnections are most apparent in her untranslated work, notably the series of essays published under the umbrella title *Cosmopolitiques* (1996).

The movement of Stengerian energies through the social sciences that I sketched in the previous section maps my own journeys into her work too. ANT furnished a provisional opening that was intensified for me by a research collaboration with Belgian colleagues on food scares (Stassart and Whatmore, 2003). I find her writing daunting and compelling in equal measure. There is something relentless in the rigour of her thinking, combined with a style that obstinately refuses to be read lightly such that, if you persist (and it can be tempting not to), you find yourself forced to follow arguments past the comfort zone of your own habits of thought. Her main philosophical protagonists are not post-structuralists but less fashionable philosophers of science, such as Kuhn and Popper, and the science establishment that clings to the authority of the Scientific method. In this sense, she is not a philosopher who is readily made to serve the purposes and problems that social science readers bring with them to her texts. Stengers is also difficult to read for other reasons, in part because something of the tenor and wit of her writing is lost in translation and in part because she anticipates a familiarity in her readers with the intricacies of scientific, philosophical and science studies literatures that is quite formidable. Nevertheless, if you stick with it, I think her work can be instructive for connecting enduring debates in the philosophy of science to the growing theoretical and methodological emphasis in social research on

knowledge *practices* as the currency of 'non-representational' approaches to the study of social life (Thrift, 2002).

'Mapping into knowledge'

For Stengers, knowledge production is not about translating between the pre-constituted and self-evident constituencies of word and world, mind and matter, subjects and objects, in which the act of knowing is always an act of mastery. Rather than taking these divisions as given, she sees them as particular outcomes of philosophical interventions by eighteenth-century thinkers such as Kant and the ways in which these were harnessed in the methods of inquiry institutionalized by professional science as it emerged in the nineteenth century. Stengers describes this Scientific method (with a capital 'S') as a stance towards knowledge that 'unilaterally' makes it possible 'to subject anything and anyone at all to quantitative measurements' (2000, p.23). Such measurement procedures presuppose and reinforce what kinds of knowledge count, what kinds are forbidden and what is authorized to be mutilated 'in the name of science', like the 'innumerable animals [that] have been vivisected, decerebrated [brain removal] and tortured in order to produce ''objective data''' (2000, p.22). Thus Stengers' objection to the 'Kantian tribunal', a term which she invests with Stalinesque overtones, is not just a philosophical nicety but a concern with the abusive consequences of the word–world settlement. Her alternative to a knowledge production process engaged in filtering the indifferent stuff of the world through human ideas, theories and categories is one not of mastery but of *modification*, in which all these components are mutually reconfigured.

Stengers' approach can be located in the very different philosophical traditions identified above. In particular, she adopts a Deleuzean term to describe the way in which all the parties assembled in the research process, researcher and researched, bodies and texts, instruments and fields, condition each other and collectively constitute the knowledge 'event'. On this account, 'evidence' does not pre-exist scientific inquiry (Stengers, 1997, pp.85–6), both the scientist and his/her object of study are (re)constituted through the activity of research. Thus, the philosophical choice posed by Stengers is, as Latour puts it, between those philosophies that hold the real and the constructed to be opposites, like fact and fiction, and those that hold them to be synonymous aspects of fabrication (Latour, 1997, p.xiv). In this vein, the business of 'generating materials' becomes one of how to 'map phenomena into knowledge' (Stengers, 1977, p.117). Here she contrasts the mappings of science-in-practice, the routines and crafts of scientific work which she characterizes as 'labyrinthine', with those reproduced in (and as) *the* Scientific method, which she characterizes as 'triumphant'. Both are in the business of making connections but, where Science is looking for 'interconnections . . . between [already] separated

populations of phenomena', science-in-practice is more concerned with configurations that 'string together at once all the phenomena and those who study them without distributing *a priori* . . . what is significant and interesting, and what . . . can be ignored' (1997, p.117).

Latour's essay 'Circulating reference' (1999) provides a vivid illustration of what Stengers means by 'mapping into knowledge', emphasizing, among other things, the complex space–times of the **research event** discussed in Chapter 4. In it, he gives an account of a scientific expedition to the edge of the Amazon rainforest. Describing his own part in the expedition as that of a 'participant-observer', he reminds us of the debt that his methodological approach to studying this expedition owes to ethnography. He details in words and images (black-and-white photographs) the many small but consequential displacements through which the soils that the scientists are studying are transformed into samples, charts, numerical and textual records of observations. Each displacement involves a mobilization of the world, like the maps and aerial photographs from which a field-site is discerned; the grid squares and markers that organize the collection of 'samples'; and the specimen boxes and classificatory schemas that carry these field-materials away. On this account, the exchanges set in motion in the research event never seem to separate out into words (signs) and things as neatly or thoroughly as they are supposed to, or to begin or end in 'the field', but rather constitute what Latour calls a 'circulating reference'. As he suggests,

research event

> . . . we never detect the rupture between things and signs, and we never face the imposition of arbitrary and discrete signs on shapeless and continuous matter. We see only an unbroken series of well-nested elements, each of which plays the role of sign for the previous one and of thing for the succeeding one. (Latour, 1999, p.56)

Latour's essay makes the consequences of Stengers' notion of 'mapping into knowledge' for the treatment of research 'data' more tangible. Data emerge here not as nuggets of the 'real world', or as so many 'discursive constructs', but rather as intermediaries or 'third parties' between researchers and researched that are as material as they are meaningful. What difference might this stance towards 'data' make to, say, the practicalities of interviewing? Among other things, it could enable you to be explicit about the displacements involved in your own mobilizations of the talk generated by interviewing as 'data'. Consider, for example, the displacements between the interview encounter rich with bodily habits and cues; the tape-recording that transports its sounds alone; the transcription process that distils these sounds into words on a page; and the quotations from the transcript that make an interviewee 'present' in your research account. In place of 'raw data' that, so to speak, takes the words out of an informant's mouth, the interview/tape/transcript/quotation emerge as intermediaries constituted *between* the researcher and researched; talk

and text; devices and codes that take on a life of their own as they travel through the knowledge production process.

But it is also worth taking a little time to think about what work the photographs do in Latour's account of 'fieldwork' (see also Mike Crang's discussion of photographs in Chapter 7). Among other things, they provide snapshots of science-in-practice that show various members of the scientific team and a variety of devices (like the soil-corer) and documents (like maps) working together in the making of measurements. The photographs have the effect of making the 'doing' of research present in the text, emissaries of the energetic exchanges between bodies and instruments, soils and plants that are set in motion in the research event. In other words, they extend the register of what it means to 'generate materials' from one in which only human talk counts, to one in which bodies, technologies and codes all come into play. In direct contrast to Rorty's insistence that 'the world does not speak, only we speak' (see Chapter 1), Stengers and Latour are adamant that 'good' research practice is

> . . . actually a matter of constituting phenomena as actors in the discussion, that is, not only of letting them speak, but of letting them speak in a way that other scientists recognise as reliable. . . . The real issue is . . . the invention and production of . . . reliable witnesses. (Stengers, 1997, p.85, emphasis in original)

I now want to look more closely at how Stengers conducts this shift from 'data' as passive evidence in the hands of the researcher to active witnesses in the collective research event.

'Being at risk'

being at risk

Stengers' principle of '**being at risk**' provides a litmus test for distinguishing between well and badly constructed propositions, a term she derives from Whitehead (1978), as opposed to true and false theories. If you recall from the first section, theories premised on the word–world settlement bridge the interval in representational terms. By contrast, propositions admit many different kinds of element into the company of the research event (gestures, devices, bodies, sites, etc., and words too) and seek to establish practical relations between them in terms of the articulations afforded by their different properties in combination. To pass Stengers' 'test', a scientist/researcher must demonstrate that 'the questions raised by [their] experiment/research are at risk of being redefined by the phenomena mobilized by the laboratory or theory' (Latour, 1997, p.xvi). In other words, the production of questions discussed in Part I is recursively linked to the business of generating materials. This Stengerian principle applies equally to natural and social sciences, human and non-human objects of study, marking out 'bad' scientific practice as that which does not give the

researched a chance to answer back. This might be giving people being asked by sociologists to complete a questionnaire an opportunity to redefine the terms of what it is that is being interrogated, or the bacterium under the microbiologist's microscope the opportunity to demonstrate other capabilities than the one under scrutiny. In either case, the crucial criterion is that the researched are permitted, by the way the research is conducted, to resist being aligned to only one scientific 'truth', as if this exhausted their potential as either agents or evidence. This is what Stengers means by 'univocal' witnesses (1997, p.87).

One of the examples that Stengers gives is that of the work of the Nobel-prize-winning biologist Barbara McClintock, who was engaged in the 1950s' revolution in microbiology, working on the singularity of the genetic material of corn. She uses this example to address the question posed in a short essay entitled 'Is there a women's science?' McClintock's working practice was not to commence her research as a means to make the world fit her models, but rather to search for ways of permitting the world to contradict the theories that biologists brought to bear on it. Stengers describes the delight recorded in McClintock's laboratory journal, and detailed in Evelyn Fox-Keller's biography of her (1983), 'when she knew that the corn had, if I can put it this way, ''intervened'' between her and her ideas' (Stengers, 1997, p.111). For Stengers this joy or passion (she uses the French term *jouissance*) of the scientific craft occurs when the materials the scientist is working with force an unexpected possibility into the exchange. It is a joy less of knowing than of *not* knowing that she argues is a defining feature of scientific knowledge practices, but one that invariably gets written out of scientific literature and education. This happens, Stengers suggests, by the unexpected being retrospectively accounted for as the consequence of an ultimately rational method or correct theory (1997, p.88). The joy of *not* knowing is disciplined out of Science by training and, more specifically,

> . . . learning never to say I but we, never to present research methods as the expression of choices but the expression of unanimous and impersonal consensus; never to admit that an article's object is contingent not . . . the result of what was being aimed at from the beginning. (Stengers, 1997, p.113)

The question for Stengers becomes one of how to hold on after the event to those moments in which researchers find themselves lost for words in the face of some unexpected possibility that bodies forth in the knowledge production process. Her answer lies in shifting the onus of what it means to 'know', such that 'to understand means to create a language that opens up the possibility of ''encountering'' different sensible forms, of reproducing them, without for all that subjugating them to a general law that would give them ''reasons'' and allow them to be manipulated' (Stengers, 2000, p.157).

At this point the idea of data as 'third parties' in the relationship between researcher and researched discussed above becomes critical to acknowledging that, as intermediaries, they are only ever partial and incomplete mobilizations of the phenomena enjoined as the 'object' of research. Rather than a mute world being rendered compliant evidence of theory 'x' or 'y', Stengers insists on the capacity of worldly phenomena to exceed their alignment in the knowledge production process. It is in this sense that 'good' science should allow 'phenomena [to] continue to speak in many voices; [to] refuse to be reinvented as . . . objects in the Kantian sense' (1997, p.90). Here, Stengers' criteria for discerning 'good' research propositions from 'bad' ones come very close to the characteristics of the 'ambulant science' advocated by Deleuze and Guattari in *A Thousand Plateaus* (1987/1980), as discussed by Nigel Clark in Chapter 2. There are also interesting parallels, though not ones that Stengers herself makes, with Irigaray's use of the term *jouissance* and her celebration of an 'openness to the new' discussed in Chapter 3.

While she makes her argument in relation to, and through, natural science examples, unlearning the conventions of 'writing out' the unexpected from research accounts is no less significant a challenge for social scientists. What difference might the principle of 'being at risk' make to how you conduct and use research interviews? One of the implications of Stengers' argument, to which she repeatedly returns, is writing research differently, developing a style that better holds on to the open-endedness of what is said and done in the research event and the multiplicity of sometimes incommensurable 'truths' that it admits. But this is getting ahead of ourselves and trespasses into the domain of Part III. In terms of interviewing itself, it might encourage you to experiment with practices like cumulative interviews with the same person or collective encounters like focus groups, which amplify the frictions, discrepancies and silences in the talk generated between researcher and researched. Such variations on the standard one-off individual interview also permit more opportunities for research subjects to engage with, and object to, transcripts of the talk generated from previous encounters and your analysis of them, thereby making these intermediaries more 'reliable' in Stengerian terms.

If Rorty's argument that it is 'language all the way down' removes any substantive basis for lending more or less credence to scientific as against other kinds of knowledge-claim and leads him to resort to irony, Stengers' philosophy invokes substantiation as an evaluative criterion but appeals to humour as a bulwark against any scientific claim to a monopoly of the 'truth'. By humour she means,

> . . . learning to laugh at reductionist strategies which in impressing research institutes and sponsors turn the judgements they permit themselves into brutal facts; learning to recount histories in which there are no defeated, to cherish truths that become entangled without denying each other. (Stengers, 1997, p.90)

The important point here is that the imperative and exercise of such 'humorous' tactics is not restricted in Stengers' account to practitioners of science alone. It is not a matter of self-regulation within the research community. Rather, as Latour notes (1997, p.xvii), she looks everywhere for the conditions where the power of scientific knowledge-claims is counterbalanced by the intervention of those whom scientists speak for and about – the 'lay public', in whose name science is conducted, and the objects of scientific study (including people) themselves. This takes us to the third element in Stengers' philosophical vocabulary that I want to outline here.

'Cosmopolitics'

Stengers' philosophy of science is bound up with a politics of knowledge that spills beyond the communities of science and through the wider fabric of civil society and governance. It is for this strange mix of science, philosophy and politics that she contrived the 'beautiful name' – '*cosmopolitics*' (Latour, 1997, p.xi). As the literary theorist William Paulson notes of her project:

> . . . it is not enough to decide to include nonhumans in collectives or to acknowledge that societies live in a physical and biological world as useful as these steps may be. The crucial point is to learn how new types of encounter (and conviviality) with nonhumans, which emerge in the practice of the sciences over the course of their history, can give rise to new modes of relation with humans, i.e. to new political practices. (Paulson, 2001, p.112)

cosmopolitics

As a 'learning process', Stengers' **cosmopolitics** are thoroughly collaborative. On the one hand, they have been elaborated through ongoing conversations with science and technology studies, and with Latour in particular, as is evident in the extensive cross-referencing to each other's work. Both share a concern with developing a politics of knowledge that is not restricted to an exclusively human constituency but rather involves 'the management, diplomacy, combination, and negotiation of human and nonhuman agencies' (Latour, 1999, p.290). On the other hand, the political in Stengers' cosmo*political* project is also manifestly informed by her involvement in activist campaigns, for example, on the politics of drug (ab)use and the treatment of AIDS, an activism that marks her out from Latour.

Thus, if Stengers' philosophy of science is at odds with the Rortyian inference that scientific knowledge-claims are no more or less intrinsically compelling than any other, she is no less critical of the stratagems of scientists who would bolster their authority by exempting their knowledge-claims from political dispute. As she puts it: 'Because we now know the connivance of . . . scientists with all forms of power capable of extending

the scope of their judgements, . . . new constraints have to condition the legitimacy of inventions in ''the name of science''' (Stengers, 2000, p.158).

Cosmopolitics, then, is precisely a project about recasting the intellectual and social terms of engagement between science and politics. The shift it seeks to make is from a problematic that presumes a gulf between science and politics even as it sets about bridging it, to one that takes their entanglement as given and redirects attention to the democratization of expertise (Stengers, 2000, p.160). Here,

> . . . it is a question of inventing apparatuses such that the citizens of whom scientific experts speak can be effectively present, in order to pose questions to which their interest makes them sensible, to demand explanations, to posit conditions, to suggest modalities, in short, to participate in the invention. (Stengers, 2000, p.160)

An example of cosmopolitics in action can, I suggest, be found in the work of the economic sociologist Michel Callon (1998), whose substantive research interests lie in the organization of markets.

Callon has been closely associated with ANT (as well as being a colleague of Latour's), but has turned its distinctive methodological energies to the study of economic knowledges and processes. In his recent work he develops the notion of 'hybrid forums' to describe the proliferation of public spaces in which scientific expertise, and the commercial and regulatory practices that it underpins, are becoming the subject of intense dispute. These forums are 'hybrid' both because the questions raised mix economic, political, ethical, legal and technological concerns in new and complex ways and because of the variety and heterogeneity of social interests engaged in them (lay persons and experts; parents and consumers; pressure groups and civil servants). Take the case of the vigorous public resistance to genetically modified (GM) foods in Europe in the late 1990s. Callon (1998) suggests that amidst the many counter-currents in play, it was less the health and environmental 'risks' of this technology *per se* that fuelled public dispute, than their association with monopolistic corporate markets and the impoverishment of producer practices and consumer choices that they entailed. Such forums exposed both the contested nature of the science informing the assessment of the risks and benefits of GM, and the relevance of other kinds of knowledge to the terms of dispute. In so doing, they forced a redistribution of competencies and rights in the politics of knowledge-making, expressed not least through the explosion of new market practices like organic, animal welfare and other certificated 'quality' food networks that variously proscribed GM (Callon et al., 2002, p.195).

But Stengers' cosmopolitics do not just place scientific knowledge practices on trial while those of other members of the polity are left untouched, but rather require that concerned citizens also put at risk their own opinions and convictions (Stengers, 2000, p.160). What difference

might this stance towards the politics of knowledge production make to the conduct of social science research? One such difference concerns the distribution of powers and affects *between* researcher and researched in the research event. By way of illustration, you might work through the consequences of reframing the question raised earlier about the methodological importance of the photographs in Latour's essay, 'Circulating reference' (1999), in cosmopolitical terms. In making the 'doing' of research present in the text, such a framing should encourage you to interrogate more closely whose eye is behind the camera lens, whether the picture-taker is singular or plural and to what extent, if at all, they figure in the images of the research account.

In the case of Latour's essay, we learn on the first page that the camera is 'his' and it is his eye behind the lens in all twenty-odd black-and-white photographs (1999, p.24). While he is at pains to position himself within the research event by constant reference to the collective 'we', it is nonetheless the case that he never appears in front of the lens. Thus, for example, he notes that as he 'snaps the picture' of the scientific team, the pedologist René is enlisting him as an 'alignment pole' to take a topographic bearing with an instrument that can be seen pointing directly at the camera in the photograph (1999, p.41). By the same token, none of the photographs in the essay is witness to any of the other scientists in the party assuming the role of photographer. Stengers' cosmopolitics should encourage you to work more reflexively with such visual methods (Pink, 2000). This might include harnessing the skills associated with the social usage of camcorders and disposable cameras by inviting research subjects to position themselves behind the lens, and by subjecting yourself to their picturing of the research event. In this, her emphasis on inventing apparatuses to democratize participation in the production of knowledge finds resonance in Nigel Thrift's discussion of the ethics of Spinoza in the next chapter, with its emphasis on the affective relationships between manifold beings. For both of them, ethics (and politics) are better understood as relational activities and practical accomplishments, rather than as individual stances or universal rubrics.

Conclusion

The urgencies and dilemmas of questions about the kinds and quantities of research material you need and how best to generate them, do not disappear with the wave of a philosophical wand. But neither is it possible to abstain from situating the activities of data generation in philosophical terms – there is no 'philosophy-free' option even in this seemingly most practical aspect of research conduct. Different philosophical resources are consequential for 'doing research' and for the ways in which you formulate and address these questions. Working these consequences through the particularities of Isabelle Stengers' philosophy of science holds both lessons

and pitfalls for social scientists, not least because her own scientific reference points are characteristically those of thermodynamics or psychiatry rather than society. In other words, her's is not a ready-to-wear philosophy that fits the questions that social researchers are predisposed to bring to it. Rather, her work might best be approached as a rigorous attempt to articulate some principles for 'good' research conduct in terms of the generation and treatment of 'evidence' in any field of inquiry. These principles, notably those of 'working together' and 'being at risk', have now been transposed to numerous social research contexts, including literary studies (Paulson, 2001), economic sociology (Callon, 1998) and political science (Barry, 2002), as well as science studies, and provide useful intermediaries for engaging with her work. Revisiting the questions about 'generating data' posed at the start of this chapter in turn, where might Stengers take us?

The first question – 'What data do I need?' – is clearly one that is directly linked back to Part I of this book and the 'kinds of question' you want to ask. What Stengers offers here is a way of keeping these questions open through the research process by allying them to an insistence on the produced-ness of 'data' and the creative and sometimes contrary possibilities generated in and by exchanges between researcher and researched. Her work has been taken up in the social sciences to emphasize the importance of non-human witnesses in the research event and to inform methodologies that extend the register of what counts beyond both the human and the said. While it is not antithetical to taking language and cognition seriously as human competences that afford a vital site or mode of engagement with the world (see Paulson, 2001, p.118), neither does it privilege them over the bodily repertoire of senses and practices that make us human. For this reason, many social scientists will always find this a philosophical pill that is hard to take.

The second question – 'How much is enough?' – is in no small measure a logistical question of how much time you have to spend on generating materials and 'being in the field', given the time and resource constraints of your research. These, too, are very much part of practising science, even if they figure nowhere in the rarefied conventions of the Scientific method or the 'big questions' of the philosophy of science. These constraints might be the schedule and/or budget for the production of a research report commissioned by government; the institutional regulations on the maximum allowable period of registration as a student before a thesis has to be submitted; or the duration of a research grant to support your activities. But however long or short the time you have to spend on such activities, you will not be alone if you find yourself feeling overwhelmed by the sheer volume of materials generated or its recalcitrance in the face of your efforts to fashion it into some kind of order. But, in Stengers' terms, this is not an entirely unhealthy state of affairs, in the sense that the research objects mobilized in your research should be troublesome intermediaries in the research process. As Nick Bingham

elaborates in Chapter 8, it is just such intermediaries that prompt you in new and unexpected directions and keep your analysis 'at risk' as you engage in what commonly passes for 'writing up' your research.

And, finally, the third question – 'How should I go about obtaining data?' – has shifted through this encounter with Stengers from a rodent activity of 'collecting' bits of the world and bringing them home, to one of generating materials in and through the research event. This has been a recurrent theme through the whole of Part II. This process of what Stengers calls 'mapping into knowledge' involves, as we have seen through Latour's example of 'circulating reference', precarious displacements between matter and meaning, things and signs generated by and through relations between researcher and researched. It is a process that entails rethinking the space–times of research in important ways, not least those of 'the field' interrogated in Chapter 4. But it is also a process that you might want to think about more reflexively than is evident in Stengers' (or Latour's) own writing about this process, in terms of how you situate yourself in the research interventions you describe and the ethical implications and possibilities of so doing. It is these ethical considerations that are brought into focus in the next chapter.

Further reading

For those interested in a taste of Stengers' philosophical writing, her essay, 'Is there a women's science?', in *Power and Invention: Situating Science* (Minnesota University Press, 1997, pp.123-32) is a useful starting point. Latour's essay, 'Circulating reference', in *Pandora's Hope: Essays on the Reality of Science Studies* (Harvard University Press, 1999, pp.24–79) provides a highly readable exposition of the notion of research as a process of 'working together' through an ethnography of a scientific expedition. For an economic illustration of the politics of knowledge associated with Stengers' approach it is worth looking at the article by Callon et al., 'The economy of qualities' (2002).