



CRESC Working Paper Series

Working Paper No. 95

The Double Social Life of Methods

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March 2011

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The Double Social Life of Methods

Introduction

How might we think about social research methods?

There's a standard answer to this question, which we dub the 'methodological complex'. It assumes that methods are tools for learning about the social world. That this is what they are. End of story. We see this in methods courses. Juxtaposed and differentiated both from theory, and from substantive courses, these tell us about techniques for knowing the world. Which to choose. How to use them. How to analyse data. And how to present it.

There's nothing wrong with this in certain senses: in social research indeed we need methods, and it's not a bad idea to use those methods properly. But to think of methods in this way – simply as appropriate tools – involves consequences, some of them unanticipated, which create a baggage which can be heavy, even burdensome. We can distil this as 'the methodological complex.' This has three elements.

First note the division of labour. Theory, substance, and method. These three realms are held separate from each other, so that research questions and issues are derived from theory ('as hypotheses' in more positivist approaches), and methods provide the tools that allow such questions to be tested, with respect to various 'substantive' areas. We see here a division of labour in the project of empirical social inquiry which imagines this in a particular and contestable way. Is theory really big? Does it actually do the methodological and empirical driving? And method? Is method really just technique? The appliance of science? The questions are rhetorical. The answer is: obviously not. And no doubt most social scientists would agree. The world of social inquiry is much messier than any such caricature.

Second, however, the resistance to this framing can actually reinforce the 'methodological complex'. Thus, it is hardly new to resist the instrumentalism involved in deeming methods to be tools: critical theory, notably that of the Frankfurt School, has made this a major plank of its interventions since the 1930s. This move finds its apogee in Habermas' distinction between instrumental, hermeneutic and critical modes of knowledge in *Knowledge and Human Interests*¹. However, this differentiation allowed theory and humanistic methods to be seen as 'the line of flight' (to use a Deleuzian term) as bearing the potential to resist instrumentalism. This actually further instantiates a rift between theory and method which can itself produce the kind of theoreticist orientation to knowledge which has been at least partially in evidence with aspects of the 'cultural turn' .

Third, a further differentiation in this instrumentalist rendering of methods as tools is that it buys into the idea that there's a world out there. It then assumes that this world has particular and definite features. And then it assumes that those features may be reported, and turned into data². But what's going on when we make these assumptions?

One answer is that we're setting up a binary divide. We're distinguishing between the world on the one hand, and representations of that world on the other. In this way of thinking it's methods that bridge the gap. If we get those methods right then our representations will match the realities of the world. Tools have a better or worse capacity to do the job at hand. They will, as the philosophers of science say correspond to it; or at least (this is what the pragmatists say) they will describe it sufficiently well to be treated as accurate. This means

¹ Habermas (1972).

² For discussion of some of these assumptions see Law (2004).

that they are tools for handling the world. If we get them wrong then our accounts of reality, our data, will be flawed.

Looking at this third point, sceptics may respond that in fact plenty of methodologists resist 'objectivism', and many methods textbooks also parade a variety of 'humanistic' methods which can define a more constructionist take on the world. The power of this current is evident from the work of Berger and Luckmann³, through Foucault's discourse theory⁴ and even Bourdieu's field analysis⁵. But in fact this 'line of flight' is also problematic. It instantiates questionable humanist assumptions about social relations into the research process (such as the phenomenological concern with experience) which are then signally out of kilter with post-humanist theorists such as those in science studies.

The problem, then is that social research oscillates between an objectivist concern with 'bias' and a humanist response which seeks refuge in an 'ineffable' human moment which somehow lies outside this purview of representational methods. It is this complex oscillation that we resist in this paper. But before proceeding, let us be clear about the attractions of the objectivist, instrumentalist framing. By reducing issues to questions of technique, it allows different parties to come together around some kind of shared project, whatever their goals, values, orientations and identities. If we need to create random samples, then this is because it is important to avoid undistorted samples. If it is dangerous to avoid recruiting so-called professional participants to our focus groups, then this is because we're looking for people who are naïve and untutored in appropriate ways. If the ethnographer needs to avoid the outsiders who flock to talk with her when she first arrives in the field, then this is because she's on the lookout for gatekeepers or people at the core of the community rather than people with grudges on the periphery. We learn all these things in a million different versions in the hope of reducing bias; in the hope of knowing and describing the world accurately. This search to avoid bias and to use our 'tools' more effectively is pervasive, indeed ubiquitous. We share it. But it then also leads to an automatic response, from even the most positivistic researcher, about 'what is left out' by any specific method. Hence, a concern with 'the margin for error' is built into the 'methodological complex'.

To repeat, it is not wrong to treat methods as a matter of technique. But we are concerned to develop another methods agenda too, which is slowly being articulated. It's the agenda that animates many of the research concerns in CRESC. And it has to do with we're calling the social life of method.

So what does it mean to talk of the social life of method? We want to argue that it's helpful to think of the double social life of method. Here's a quick version of the argument. This starts from the view that we have begun to elaborate above, that it is unhelpful to separate out theory, method and substance, and so to generate an oscillation between instrumentalist and humanistic understandings of social research methods. Instead it starts from the recognition that methods are fully of the social world that they research; that they are fully imbued with theoretical renderings of the social world. This starting point leads us to two arguments. First, methods are social because they are constituted by the social world of which they are a part. This step is relatively easy. It is closely allied to cognate arguments made about scientific methods by researchers in STS. But we also take a second step: they are social because they also help to constitute that social world. In saying this we do not embrace a simple constructionism, for to do this depends on holding the world (acted upon, and constructed) separate from its tools and representations – which is precisely what we contest here. And, as we will show, there is no omnipotent 'methods machine' able to produce anything it wants. Despite considerable investment, some methods fail to produce hoped-for results. Rather, we

³ Berger and Luckmann (1967)

⁴ Foucault (1979)

⁵ Bourdieu (1984)

are concerned with the world as complex assemblage. Methods are fully of the world that they are also active in constituting.

Methods of the Social: I

Version number one. Methods are constituted by the social world of which they are a part.

In one way this is blindingly obvious. First, methods don't come into being without a purpose. And, second, they don't come into being without advocates, or more exactly without forms of patronage. Often the state has been the key player here.

In the eighteenth, and even more in the nineteenth century, states needed to know their territories. The result? Techniques of surveying and map-making; in the UK the Ordnance Survey⁶. And then the state also needed to know its populations. It wanted, it needed, to govern its people. Obviously people may be governed in endless kinds of groupings: as classes; communities; estates; and syndicates. But for states, population is the category of governing and involves states constituting people as individuals – and the subjectivities that go with this⁷. It involves classifying, categorising and enumerating people's specific characteristics, and aggregating those individuals and characteristics together to form a governable national population. It is through techniques such as censuses that states have come to locate and identify the people who inhabit their territories and make them into a population. The UK census started, as we know, in 1801, but as writers as diverse as Michel Foucault, Nikolas Rose and Timothy Mitchell have shown, this strategy for governmentality comes in many varieties⁸.

So methods have a purpose and they have advocates. They also build on whatever came before. So, for instance, while the census made people inhabiting a territory into a national population, subsequently that population reality could be further calibrated through the technique of sample statistics in the mid-twentieth century. So there's a history of method to be told here⁹. In the US, for instance, sample surveys grew up with the Gallup polls of the late 1930s¹⁰, and then with agencies such as the Department of Agriculture during the Second World War¹¹. Mike Savage traces their analogous though later rise in the UK¹². He shows, for instance, how during the Second World War, the Government Social Survey became a key instrument in generating knowledge of the circumstances and concerns of the British population. It proved popular in part because its methods of anonymous sampling avoided relying on known informants, in a way that had attracted popular opprobrium. The government proved a key player in promoting survey research into the 1960s, seeing it as part of a modernising form of government that no longer needed to rely on the views of the 'good and the great'. It proved important in shaping educational reform in the 1960s, with surveys being used to explain the social selectivity of grammar schools. As Hilton¹³ and Savage have shown, these methods were thus embraced by a technocratic middle class, seeking to distinguish themselves from older gentlemanly intellectuals. Through these means, academics – and perhaps especially sociologists – got in on the act too. The result was that the sample

⁶ Livingstone (1992).

⁷ Ruppert (2008).

⁸ See Foucault (1979), Rose (1999) and Mitchell (2002).

⁹ Igo (2007).

¹⁰ Osborne and Rose (1999).

¹¹ Hyman (1991).

¹² Savage (2010).

¹³ Hilton (2003).

survey became – has become – a core tool for knowing the social in industrial societies. Perhaps, in an era of quantification, it became the pre-eminent tool.

But analogous stories may be told for other methods. Indeed for other disciplines. For instance anthropological ethnography insisted on the importance of studying colonised peoples qualitatively, and often holistically and functionally, ‘in their own terms’. Why? The caricatural response – but it is a caricature that also deserves some respect – is that it was important to understand the difference between the West and the uncivilised rest, but also important (and as a part of this) to understand the logics of the colonised. It’s a commonplace that this was an endeavour indissolubly linked with imperialism; with governing, with civilising, and with controlling empires. And the connection was inescapable, even for critical anthropologists. The position of privilege – and privileged access – came both literally and metaphorically with the colonial territory¹⁴.

So that’s the first argument. Social research methods need advocates and some kind of ecological context if they are to survive. They are tools for knowing the world that are of those advocates and ecological circumstances. And, as we have been hinting, in the twentieth century the relevant agendas often grew up in the relations between the state, the (largely state-funded) academy, and a range of private sector businesses as well as political groups and social movements. We mentioned Gallup a moment ago. But Gallup was simply at the front of the queue, historically at least. Here’s another example of the private sector at work in advocating methods: the focus group.

A version of the focus group was created in the space between the academy and the state in US media research early in the Second World War. Interestingly, it was Frankfurt School theorists such as Theodor Adorno who played an important role in developing an interest in focus groups, a striking example of the way that the gap between theory and methods was by no means as strong in this period as it was subsequently to become. Popularised by Paul Lazarsfeld and Robert K. Merton, it was used to explore how well radio programmes sold war bonds¹⁵. But then (to put it quickly) it largely disappeared from the academy. Why? Perhaps in the high era of American quantitative sociology it wasn’t scientific and statistical enough. In any case, whatever the reasons, it only reappeared in any serious way in the academy in the 1980s¹⁶. So where had it been? The answer is: in the private sector. Indeed, it had been transmuted into a core tool for marketing research. So the 1980s saw its transfer back to the academy. A process that sometimes, not always, meant that it was also re-theorised. Here was the question. What does the talk in a focus group actually tell us? Does it tell us about attitudes? That’s a common, and probably the predominant, view. Or does it (here comes the re-theorising) tell us something about how people negotiate and make positional arguments in contexts saturated by power relations? This is how it remerged in at least some of its academic versions¹⁷. That’s quite a difference. It’s a difference driven by the agendas of critical social science.

There are plenty of other examples of the way that methods come out of the complex tensions between different kinds of advocates during the course of the 20th century. We might see the competing visions of knowledge coming from the humanities (associated with traditional aristocratic, professional and gentlemanly interests) and the social sciences (linked to more technocratic currents) as fundamental. For many authors in the 20th century, writing novels was a form of social research, a means of elaborating social relations (see intriguingly, Marilyn Strathern on Jane Austen¹⁸). It was a new breed of social researchers, armed with interviews, sample surveys and questionnaires who made it clear (to themselves, at least) that

¹⁴ Lewis (1973).

¹⁵ Lunt and Livingstone (1996).

¹⁶ Kahan (2001).

¹⁷ Waterton and Wynne (1999).

¹⁸ Strathern (1992).

novelists were not doing social research, though they might be good entertainers or able to provide insight into psychological states.

These concerns straightforwardly illustrate our suggestion that methods are of the social. It shows how research methods embody the concerns of advocates and subsist in particular contexts or environments¹⁹. Second and as a part of this, it illustrates the importance of critical thinking about method, about what it is that methods are doing, and the status of the data that they're making. We'll return to this thought in due course. And then third, it tells us that methods do not necessarily reflect only the concerns of the academy. There are being constituted, and often on a large scale, by other advocates. And this is perhaps our biggest social science challenge: how to handle the challenge of methods that are of advocates or subsist in ecological contexts that have little or nothing to do with social science and its characteristically critical concerns.

A quick version of the story runs so. Social research methods are proliferating.²⁰ The commonplace is that this has partly, perhaps substantially, to do with the digitisation of everyday life and the growth of digital transactional data²¹. We know that the flows of people, of goods, of vehicles, of information and of money are all being recorded. And we know, too, that they're being analysed using digital methods advocated by a variety of individuals and organisations that cannot be easily summarised as being of the state, private sector or the academy. These methods do things such as identify patterns and associations that are of interest to those who develop and/or use them, patterns that often take the form of visualisations. And those interests and purposes are multiple. A sample list might include: state security, targeted marketing, personal health tracking, wikis and open source analytics, crime control, child protection, firm patenting strategies, disease control, supply-chain logistics, public issues and concerns, military battlefield tactics, changing linguistic usages, Web 2.0 social analytics, and population genomics.

It's easy to make big stories out of this: that we're living in a new information society; or that we're living in a surveillance society; or that we're suffering from a data deluge; or (slightly at odds with the idea that we're living in a surveillance society), that actually we're living in an era in which methods and their data are being democratised by the supposed ubiquity of the internet and the tools that this makes available. Though you pay your money and you take your choice, we want to be cautious when we come across large claims about epochal change, especially when they take forms reminiscent of technological determinism. The printing press, the creation of the railways, the development of electricity supply – all of these and many more have been treated as epoch-bending moments by some of those who lived through them.²² It's difficult to think in ways that are critical and grounded in fields where hype (whether utopian and dystopian) is so prevalent. What is clear, however, is that there is a whole range of new methods out there, and that those methods exhibit concerns that have little to do with academic – and in some cases state and commercial – interests. So the message is clear. It is that unless social science keeps in touch with these changes, it will no longer be competent to contribute to new modes of social research. The advocates and the ecologies of social science method will start to pass us by. Of course we know that we're

¹⁹ For discussion of what we are calling 'contexts or environments' in a somewhat different idiom, see Law (2011).

²⁰ Thrift (2005).

²¹ Savage, Ruppert and Law (2010).

²² On electricity supply, see Hughes (1983). For a splendidly nuanced account of both the importance and the complexity of a series of technologies including, especially, the railways and refrigeration, see Cronon (1991).

pushing at an open door here. As recent academic work illustrates, much good and critical social science work is doing just that²³.

Methods of the Social: II

That, then is the social life of method, version one. The claim is that methods are of the social world; that they are tools that tend to reflect the concerns of those who advocate them, and that they subsist in particular ecologies. But then there's the social life of method, version two; the idea that methods are in turn implicated in the social world. They are thus also of the social in the sense that they constitute and organise it. Or, to use the jargon, that they don't just represent reality out there; but that they are also performative of the social.

There's a straightforward and matter-of-fact way of thinking about this. This is to say that methodologically speaking, what you see is what you get. This is because once you're inside a method and you're actually using it there isn't much room for anything else. The point is that that which is invisible for all intents and purposes doesn't exist, at least in practice, and at least for the moment.

Take an example: the sample survey. This is not any example, because the sample survey is one of the most legitimated methods in use today. In the UK University teaching is assessed by the scores academic departments achieve in the National Student Survey. Our measures of crime are dependent on the British Crime Survey, which 'corrects' for the under-reporting of much crime to the police. The inflation rate is determined by responses to the Family Expenditure Survey, and so on.

The survey works by first sampling people. And then it works by asking them questions about matters of fact (like age, gender, income bracket, religious affiliation, lifestyle choices) and matters of opinion (such as the performance of the government, attitudes to abortion, or meat-eating). We might think of a survey as a bit like a methodological package deal. Like all package deals it has great virtues. It tells those who advocate it something about how people are planning to vote, or, say about their attitudes to global climate change. It's also exceedingly useful because the basic methodological thinking doesn't have to be done again. Surveying, after all, is a bog-standard method that has been industrialised and routinised. Its standards of quality control have been set, are widely agreed, and ethical guidelines are in place to police them.

At the same time, like all package deals, it is indeed standardised. You get to see parts of social reality in particular ways, while you don't see things that escape the package. Or more strongly (and now we're getting to the point we want to make about constituting), it may be that you get to perform certain kinds of social realities whilst not performing others. You're actually bringing realities into being while you're shutting down others.

Let us give an example from CRESC research. We have been involved in a project mapping people's cultural tastes and practices, using an unusually comprehensive survey asking about respondents' interests in reading, the visual arts, music, eating out, television and film, and sport²⁴. The survey response shows a systematic difference between those who appear to be engaged in myriad activities: going to the opera, eating out in French restaurants, going to night clubs, and so on, as opposed to those who mainly appear to be inactive. However, our qualitative interviews give us cause for second thoughts. It turns out that the inactive are

²³ For indicative samples biased in the direction of technoscience, see Marres (2009), McNally (2005), Oldham and Cutter (2006) and Rogers (2009).

²⁴ Bennett et al. (2008).

actually often heavily engaged with their families and neighbours in ways that are not amenable to ready survey responses. And when one of the authors gave a presentation of these findings to market researchers, he found them nodding their heads and pointing out all the activities which often ‘slip through the net’ of the survey: walking the dog, hanging around on street corners, visiting the allotment, and so forth. And yet, these complexities can be forgotten about as powerful institutional agents seek to operate on the world in the name of the survey.

The basic logic here is scarcely new. In the late 1920s US sociologist W.I. Thomas famously said: ‘If men define situations as real, they are real in their consequences.’²⁵ Robert K. Merton elevated the principle into what he called the ‘reign of error’. His point was that a bank may fail because people first wrongly think that it is going to. But then this definition of the situation starts to become true²⁶. And Merton’s argument has been explored in the sociology of scientific knowledge and in finance, by Barry Barnes²⁷ and Donald MacKenzie respectively²⁸, and in governing by Ian Hacking.²⁹ It’s obvious that these writers are onto something important. But we also suggest that the point needs to be reworked. Methods, it seems to us, are potentially more profoundly self-fulfilling than Merton’s ‘reign of error’ might suggest. To show why this might be the case, let us give another example from the social survey. And a particular social survey: a 2007 Eurobarometer on people’s attitudes to farm animal welfare and their habits when buying (or not buying) meat³⁰.

First let us say that the findings are instructive. For instance it tells us that many people in the EU are concerned about farm animal welfare (on a scale from 1-10 the average score is 7.8 which seems pretty high). Quite a lot (though many fewer) also say that they think about the issue when they go to buy meat. The first general point, then, is that research methods such as surveys indeed teach us more about the social world. What we learn may or may not be stunning, but in the most straightforward way, their findings don’t count as self-fulfilling prophecies, and it would be stupid to pretend that they do. But if we press the argument a stage further, a different picture starts to emerge. At this point it’s useful to think in terms of dimensions. Perhaps we might imagine three.

First, there’s the surface dimension that we’ve already touched upon: this has to do with consumer attitudes to farm animal welfare. This is where we make discoveries about the social world.

Then, two, there’s a dimension that has to do with the character of people: that is, with particular subjectivities.³¹ Very briefly it’s like this. You can’t phone people up and ask them about the welfare of farm animals unless you also make a series of assumptions about the character of people and their competences. For instance, people are being endowed with attitudes: that is, with more or less long-term orientations or sentiments. The questions about what they do when they go to the shops then assume that they are choosers. The questions about information on products additionally assume that they are rational choosers because they may be influenced by that information. Finally, the questionnaire also assumes that people are more or less ethical beings, and that notions of moral right and wrong may influence their choices.

²⁵ Thomas (1928).

²⁶ Merton (1948).

²⁷ Barnes (1983).

²⁸ MacKenzie (2001).

²⁹ Hacking (2007).

³⁰ For details, see Law (2009). For the survey itself, see European Commission (2007).

³¹ See Osborne and Rose (1999) on the formation of the survey subject and Ruppert (2007) on the census subject.

And after subjectivities? The survey also works by making a series of assumptions about the character of collectivities – and then reproduces these. This, then, is the third dimension. Individuals are abstracted from a place and then taken as representatives of that spatially delimited place. A particularly important example is the modern nation. There is no independent nation which does not appropriate to itself the ability to conduct censuses and surveys on its national population delimited by its sovereign boundaries. It is not incidental that new and developing states see the capacity to conduct national censuses and surveys as central parts of their ‘statehood’

It is this which makes the Eurobarometer so interesting. It works as a composite of different national surveys, but it stretches this national formation and begins to hint at the possibility that a coherent ‘Europe’ can be imagined and re-enacted as a place and container of individuals; that is, as a bunch of citizen-consumers whose opinions and actions may be aggregated to form something called ‘the European citizen and/or consumer’; or as a set of 27 different country collections with their aggregate ‘national citizens and/or consumers’. The point, then, is that a version of Europe and of its nations is being done in the survey results. For those who read it, for a moment at least, that is what Europe actually is. A stratified population of individuals endowed with attitudes and behaviours.

A final example is from Ruppert’s work on censuses and how the category ‘Canadian’ became the fastest growing ‘ethnic’ origin group in Canada by the end of the twentieth century³². Yet, until 1986, the census discouraged and advised against ‘Canadian’ in the classification of racial or ethnic origin³³. Indeed from 1871 – 1971 ethnic Canadians simply did not exist, according to the Canadian census. Yet, in that period numerous respondents persistently and consistently wrote ‘Canadian’ as their answer such that by 1971 Statistics Canada was compelled for the first time to report their numbers at over 71,000³⁴. By 1986, when multiple responses to the ethnic origin question were permitted, 0.5 per cent of respondents wrote ‘Canadian’; in 1991 this rose to 4 per cent and by 1996, when ‘Canadian’ was added to the list of possible categories the percentage increased to 31 per cent. Where did all the Canadians come from? Many explanations have been put forward such as the role of political campaigns and a rising nationalist sentiment. In our words, these explanations are saying that the census category is of the social. However, Ruppert argues that the reporting and inclusion of the category itself on census forms was active in inventing or making up ethnic Canadians. The category was part of bringing into being a new kind of person that had not been self-conscious before through a process Ian Hacking calls ‘dynamic nominalism’³⁵.

The question here is not whether people identified themselves as ethnically ‘Canadian’ prior to being reported by the statistical authority in 1971. Some people may have identified as such while others many only have considered it a possible identification after seeing it beside a tick box on a census form. Rather, it concerns how methods can be active in making up people. Certainly, researchers have long acknowledged that how questions are posed and the categories listed can affect survey and census results. What they are suggesting but which usually gets turned into technical issues and debates about wording, is that methods are active in the making of the social.

There is much more to be said, but we’ll stop. The important point is that in order to work and to generate empirical findings, all social research methods (the Eurobarometer, surveys or the census are just examples) have to pick and choose between different individual and collective realities. More strongly, they necessarily take it for granted that those realities exist ‘out there’ and separate from methods. And yet more strongly still (and this is really the core point), they tend to reproduce these in one form or another. As rational-ethical and (we might add)

³² Ruppert (2007).

³³ Boyd and Norris (2001).

³⁴ Boyd (1999).

³⁵ Hacking (2007).

verbally competent discretionary subjects, whose actions are partially shaped by relatively stable attitudes. That's what the survey schedule does to people. That's what they are turned into when they start to respond to the questions posed to them by the telephone interviewers. Or, when we stand back and look at the collectivity, statistically derived collections of particular person-subjectivities. ('Canadians'. Or 'the UK'. Or 'The European Union').

Now a health warning. What we are not saying is that surveys are flawed and that we should really be doing (say) ethnographies, or focus groups. We are not rehearsing the classical anti-positivist critique of standardised methods in order to seek refuge in humanistic methods. We are not saying that alternative methods discover subtle truths that elude techniques such as surveys. Don't misread what we're saying as an attack on quantitative social science. An analogous job could be done on any social science method (the qualitative included³⁶), and we're not in the slightest bit interested in making a contribution to technical methodological criticism. What we are doing, rather, is rehearsing an argument about what it is that methods, all methods, actually do. First we're saying that they make discoveries about the world, and that those discoveries may surprise us. That's why we conduct interviews and surveys and all the rest. But also, and counterintuitively, we're saying that they also make more or less self-fulfilling assumptions about the character of the social world. And that in so doing they tend to constitute it, so to speak, below the radar in ways that we scarcely notice. In short, that they tend to produce what John Law calls collateral realities: that is, realities that we don't think about very much but that we're all busy reproducing as we go about the daily methodological work of gathering and analysing data 'about' the social³⁷.

This, then, is the second part of the argument about the social life of methods. If methods are of the social, then methods also help to create social worlds, to make them current, and to circulate them. In short, the argument is that methods are fully part of the social world, embedded in and constitutive of it. This means that they are active agents of the social, and may be used to intervene in its constitution. Thus methods and the social are co-constituting and cannot easily be disentangled and separated. For this reason we can think of 'methods of the social' as having a life and liveliness where neither the social nor methods are settled, but are rather always in formation. In other words, methods have relations, circulate and (re)produce realities and have genealogies – of problems, interests, purposes – that are mutually implicated.

Reflections

So what follows?

First, the agenda we've set ourselves suggests that methods are too important to be left exclusively to those who work on their technical specificities. We say this not as a put-down or a sneer. Technique is important. But what tends to happen is that boundaries get put round 'technical' (and indeed 'theoretical') concerns, which is precisely what we are problematising. To attend to the social life of method is to put method back into the social. It is to assume that methods are of the social. It is to treat method as a phenomenon in its own right³⁸. It is to work on the assumption that the binary divide between reality and representation that we touched on earlier doesn't hold. It is to imagine, as feminist technoscience writer Donna Haraway puts it, that methods and their knowledges are

³⁶ Though perhaps with more difficulty, since the assumptions built into qualitative methods and so their forms of performativity are often less clearly defined.

³⁷ Law (2011, forthcoming).

³⁸ One might suggest that it would be a strange social science that reflected on and sought to characterise everything in the social world with the exception of its own methods.

situated³⁹. It also to imply that the division between theory, method and substantive empirical field that we called the ‘methodological complex’, doesn’t make much sense either. Methods and substantive findings embed social theories. Indeed, they might be thought of as social theories in practice, theories that go in under the radar to form the social world by generating and reproducing collateral realities. But if this is right, then it also suggests that methods may be far more powerful theoretical tools than anything that we call Theory with a capital T.

Second, and to loop back, it seems that when Merton talked of self-fulfilling prophecies he got it both right, and at the same time profoundly wrong. The problem is condensed by his catchy phrase, ‘the reign of error’.

‘The self-fulfilling prophecy’ (he wrote), is, in the beginning a false definition of the situation evoking a new behavior which makes the originally false conception come true.⁴⁰

That which was false becomes true if enough people believe and act on it. Obviously this isn’t stupid. As we mentioned, it works well enough for a run on a bank. (Merton talked about a fictitious ‘Last National Bank of Millingville’, but for British readers the real case of Northern Rock probably comes more readily to mind). But there’s an inconvenience in his argument. This is the black and white distinction that he erects between those definitions of the situation that are true, and those that are false. This is because we need to think hard about what we mean by a ‘definition of the situation’ – which is what we were trying to do in the brief accounts of the workings of surveys and censuses. For definitions of the situation may be explicit, as in: ‘the bank is going to fail’. But they may also be deeply embedded in practices. This means that they may also be implicit. So we don’t imagine for one moment that people involved in the Eurobarometer survey actually sat there and said to themselves: ‘yes, nation states are collections of individuals, that’s what they are.’ No-one made explicit theoretical judgements about the characteristics of nation states. But that is what the method was doing, even so. Implicitly. And once the Eurobarometer findings went into circulation, so too did a working definition of the nation state: that it’s a collection of individual rationally-choosing subjects.

Why is this important?

The answer is that social realities are being constituted by social research methods way below the radar, and quite independently of what we think we are doing when we undertake social research. ‘Definitions of the situation’ prevail – and are enacted – even when we don’t make them explicit. But if this is right then it becomes important to excavate the versions of the social embedded in our methods, to bring them into the light, and to debate them. Do we actually want the kind of collectivities implied by ethnographies, by surveys, by focus groups, or by collations of transactional data? Do we even know what they are? And what kind of subjectivities and collectivities are they propagating? As you’ll see, we are no longer dealing only with methodological questions. We’re also trading in politics, in questions about the kinds of social worlds and subjectivities we want to help to make more real – to realise – in and through our methods. We’re dealing with what Annemarie Mol, STS empirical philosopher, calls ontological politics.⁴¹

So methods are too important to be left to methodologists alone. They are fully of the social, which means that we need to characterise and debate what it is that they do. But this allows us to move to a third and final point. For we want to suggest that it’s also becoming urgent to think structurally or institutionally about our methods, though in the present paper we will only gesture at this point.

³⁹ Haraway (1991). See also her (1997).

⁴⁰ Merton (1948).

⁴¹ Mol (2002).

Imagine a space, and imagine the practice of a method (a survey, an ethnography, it doesn't matter) at the centre of that space. Then ask what's implied, what's being assumed, in the practice of that method? What goes with it? What else necessarily goes into the space? Any answer would include the following.

- One, there are the researchers, those that do the knowing, so to speak; IPSOS-Mori or whoever. And then it would include the descriptions, the representations, being produced by the research. (The accounts of attitudes to animal welfare, or whatever it may be.)
- Then, two, there are the putative realities being described. Unless the research is complete nonsense, there will be people out there with the relevant attitudes about farm animal welfare, at least when they're confronted with an appropriate questionnaire. And then, alongside the realities being made manifest, there are the implicit realities too, the collateral realities tacitly embedded in the method, in (for instance) the form of rational subjectivities and statistical collectivities. This is the performative argument about how methods are of and constitute the social that we've just been rehearsing.
- Then third, there's what we might think of as an institutional context which includes the advocates (in the case of the Eurobarometer the DG 5 in the European Commission) but also, something that we've not talked about, the circuits through which the findings flow. The circuits probably include but extend beyond the advocates (For the Eurobarometer, these included the DG 5, the animal welfare NGOs, parts of the European retail and meat trade, and academics interested in farm animal welfare).

Here's the problem. Typically we think of (a) representations and findings, (b) the realities that relate to these and (we're saying) are of them, and (c) their advocates and institutional contexts as being quite separate. But if we really want to understand the social life of method and its force then we need to break this separatist habit. We will need to understand that methods inhabit and help to reproduce a complex ecology of representations, realities and advocacies, arrangements and circuits. So survey methods (we pick on these but once again the point is general) inhabit and reproduce ecological forms that fit more or less comfortably together. And, this is important, these are patterns that don't take kindly to being disrupted. The implication is that there's a kind of triple lock at work here. And this, if it's right, makes it very, very, difficult to know differently, to shape new realities, or to imagine different 'methods assemblages' or modes of knowing⁴². For all of these have to be shifted together.

Of course there are straws in the wind. The move to the digital that we discussed above is certainly significant – though epochal arguments about the character of these changes need to be resisted.⁴³ In addition, it's important not to lose sight of the fact that there are diverse methods at work within the standard assemblages that make up method. Even so, however, we think that the difficulties entailed in the triple lock that we've just mentioned catch what has been happening to the brave souls in, for instance, post-colonialism, STS, and feminism⁴⁴. These are writers and activists who simultaneously seek to reground expertises and forms of knowledge, versions of the real, their advocates, and the larger ecologies of knowledge and expertise in ways that contest those that subsist most comfortably within the standard arrangements for knowing. The importance of spirits in road-building; the need to dream the land in order to keep it alive; the refusal of scientific expertise in debates about nuclear power; the idea that animals may be known in non-standard ways⁴⁵, all of these are realities or modes of knowing that don't find space in the standard methodological ecologies. To talk of

⁴² The notion of 'method assemblage' is developed in Law (2004).

⁴³ See Savage, Ruppert and Law (2010).

⁴⁴ An indicative list of writers here would include Blaser (2010), Blaser *et al.* (2004), Chakrabarty (2000), Escobar (2008), Haraway (2007), Harvey and Knox (2008), Henare *et al.* (2007), Mol (2002), Moser (2008), Thompson (2002), Tsing (2005), Waterton and Wynne (1999) and Verran (1998; 2001).

⁴⁵ Raffles (2010).

spirits makes little sense in those assemblages. The realities that these alternatives constitute are literally unthinkable. But, here's the bottom line, until we can find ways of rethinking knowledges, realities and methods together in the same breath, we won't have the tools that we need to understand the work being done by our methods. Neither will we be able to imagine a social that is radically different.

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