The Double Social Life of Method[1]

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Introduction

How to think about social research methods?

There's a standard answer to this question. It assumes that methods are techniques for learning about the social world; that the social world is out there; and that we can describe it, more or less, and for practical purposes.

This isn't wrong. In social research we <u>need</u> methods, we <u>use</u> them to discover features of the social world, and we also need to use them <u>properly</u>. There is therefore plenty of room for <u>technique</u>. But to think of methods in this way, and this way alone, is to frame them in a particular and technical manner. In terms of their own internal adequacy.

Of course in practice we all know that social research is more than this. We all know that our methods and our findings are messy in practice. That they are more or less embedded in the world. That they can't be easily separated from it. This, of course, is why we're here at a meeting on 'the Social Life of Method'. We're here because methods aren't just matters of technique. We're here because we know that they have a life of their own.

But how to think about this life? And what to make of it?

Over the next few days this is what we'll be debating. But I want to start our meeting by making a proposal. I want to argue that it's helpful to think of methods as having a <u>double</u> social life. I want to suggest that

- one: methods are social because they are shaped <u>by</u> the social world in which they are located.
- and two: they are <u>also</u> social because they in turn help to <u>shape</u> that social world.

This is what I mean by the <u>double social life of method</u>. A word, then, on each of these.

The Social Shaping of Research Method

Version number one. <u>Methods are shaped by their social circumstances</u>.

In one way this is blindingly obvious. The practicalities may be complex and messy, but the basic argument is stunningly simple.

- First, it's obvious that methods don't come into being without a <u>purpose</u>.
- Second, it's clear that they don't come into being without <u>sponsors</u>.
- And, third, its obvious, again, that they draw upon or are adaptations of, <u>existing resources</u>, methodological, cultural, and/or social.

Examples.

One.

From the late eighteenth century states wanted to know their populations. They wanted to govern their people. One way of doing this was to classify and categorise the characteristics of individuals, and then to aggregate these[2]. As a range of social science authors have shown, this strategy of governmentality came in <u>various</u> flavours[3]. But, and most obviously, there were <u>censuses</u>. From 1801 in the UK, then, for certain purposes, the country <u>became</u> a population.

Two.

In the course of the twentieth century this notion of 'population' was aligned with (much cheaper!) techniques of sample statistics. I won't tell the story here. But if it were told it would show, in part, how survey research came to prominence after World War Two in the Western democracies. Again in ways that linked it to the state. So, for instance (as Mike Savage shows), in the UK in the 1960s sample surveys on educational inequality became crucial to the policymaking that led to the growth of comprehensive education and the new universities[4]. But this is just an example. In one way or another, by the 1970s sample surveys had become a core tool for <u>knowing</u> the social in industrial societies. A tool that was indeed <u>shaped</u> <u>by</u> the social.

And where censuses, sample surveys, or, for that matter, careful household observation weren't possible? This takes me to example number three.

<u>Anthropological ethnography</u> insisted on the importance of studying colonised peoples qualitatively, and 'in their own terms'. Why? The quick response is that it was important to understand the difference between the West and the rest. But it was also important (and as a part of this) to understand the <u>logics</u> of the colonised. It's a commonplace that this endeavour was indissolubly linked with imperialism; with governing, with civilising, and with controlling empires. And the connection was inescapable, even for <u>critical</u> anthropologists. The position of privilege – and privileged access – <u>both</u> came with the colonial territory[5].
More social shaping.

Example number four.

The focus group. In one crucial version this was explored by Paul Lazarsfeld and Robert K. Merton during World War Two to discover whether radio programmes sold war bonds or not[6]. Interestingly the technique then subsequently disappeared from the academy – indeed for thirty years. But it didn't die out. Instead it was converted into a private sector tool, where it was developed as a core technique for marketing research. The 1980s saw its transfer back to the academy – where, however, it was also re-theorised.

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[2] Ruppert (2008).
[3] See Foucault (1979), Rose (1999), Mitchell (2002) and Savage (2010).
[4] Savage (2010).
[5] Lewis (1973).
[6] Lunt and Livingstone (1996).

Here was the question. What does the talk in a focus group actually <u>tell</u> us? Does it tell us about <u>attitudes</u>? This was (and is) a common, and probably the predominant, view. Or does it (here comes the re-theorising) alternatively tell us something about how people <u>negotiate</u> and make <u>positional arguments</u> in contexts saturated by power relations? This is how it re-emerged in at least some of its academic versions[7]. That's quite a <u>difference</u>; it's a difference between the <u>functional</u> concerns of marketing and the <u>critical</u> agendas of social science.

I'm saying, then, that all of these techniques – censuses, surveys, ethnography and focus groups – have been socially shaped. All reflect the concerns of <u>sponsors</u>. And the ways in which they work all illustrate the importance of critical thinking about method as it is shaped. The focus group story is particularly instructive in the present context because it tells us that methods may be shaped in ways that don't reflect the concerns of the academy. For this is perhaps our biggest contemporary social science challenge: how to handle methods being shaped in places that don't share the critical concerns of social science. And how, in particular, to think about the proliferation of <u>digital data and methods</u>.

A brief word, then, on the digital.

It's easy to make big stories out of this. That we're living in a new <u>information</u> society; or that we're living in a <u>surveillance</u> society; or that we're suffering from a <u>data deluge</u>; or, alternatively, that we're living in an internet era in which methods are being <u>democratised</u>; or, perhaps more modestly, that the proliferation of methods outside the academy is leading to more pluralist, tolerant, and/or <u>heterodox</u> approaches to method and its construction.

You pays your money and you takes your choice. But I'm a student of STS, and I'm cautious about large claims to do with epochal change. The printing press, the railways, electrification – all were the object of analogous hype in their own day[8]. But hype (utopian <u>and</u> dystopian) makes it difficult to think clearly and critically. What <u>is</u> clear, however, is that the focus group story should be treated as a cautionary warning. For there's a whole range of new methods out there, and those methods are being shaped by concerns that have little to do with critical academic interests. Surely the message is clear: unless social science keeps in touch with these changes, it will <u>not</u> be competent to participate in much of the new social science action. The sponsorships and locations that are shaping social science method will start to pass us by.

Of course I know that I'm pushing at an open door. As so many of the contributions to this conference show, there is much good and critical social science work doing just this!

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[7] Waterton and Wynne (1999).

[8] 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)

Methods Help to Shape the Social World

So that's the social life of method, version one: the idea that methods are <u>shaped</u> by the social world. But what about, version two: the idea that methods in turn help to <u>shape</u> the world? Or, to use the jargon, the idea that they don't just represent reality out there; but that they are also <u>performative</u>.

There's a straightforward way of thinking about this. It's to say that methodologically speaking, <u>what you see is what you get</u>. This is because once you're <u>inside</u> a method and you're 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 works first by sampling <u>people</u>. And then it works by asking them questions about <u>facts</u> (age or gender) and <u>opinions</u> (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 many virtues. It tells those who sponsor it something important about facts or attitudes. It's useful because the basic methodological thinking has been routinised and tested. All in all, if you've got the resources it's simply a matter of farming research out and getting the results in.

At the same time, like all package deals, it <u>is</u> indeed standardised. You get to see parts of social reality in particular ways, while you <u>don't</u> see things that <u>don't</u> fit the package. For instance, as Mike Savage shows, you tend to get to see social abstractions and individuals rather than social landscapes and households. And more strongly it may be that you get to <u>perform</u> certain kinds of social realities whilst not performing others. It may be that you're actually <u>bringing certain kinds of reality into being</u> while shutting others down.

The basic logic here is scarcely new. In what was to become a cliché, W.I. Thomas famously told his readers: 'If men define situations as real, they are real in their consequences.'[9] Robert K. Merton elevated the principle into what he called the 'reign of error'. Banks fail, he said, because people first wrongly think that they will, but then this definition of the situation become true[10]. STS writers Donald MacKenzie and Barry Barnes have shown how this may happen, for instance in finance. But I also think the point needs to be reworked. Methods, it seems to me, are potentially more profoundly self-fulfilling than Merton's talk of the 'reign of error' might suggest. To show why, I return yet again to the social survey. My example is a 2007 Eurobarometer survey on people's attitudes to farm animal welfare and their meat-buying habits[11].

If you look at them these Eurobarometer findings are instructive. It turns out that lots of people in the EU are concerned about farm animal welfare and quite a lot (though fewer)

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[9] Thomas (1928, 572).
[10] Merton (1948).
[11] For details, see Law (2009). For the survey itself, see European Commission (2007).

also say that they think about this when they're buying meat. The first general point, then, is that research methods such as surveys indeed teach us about the social world: in the first instance they <u>aren't</u> self-fulfilling prophecies. But if we press the argument, a different picture starts to emerge. At this point it's useful to think in terms of archaeological layers. Perhaps we might imagine three.

First, there's the surface layer that I've just mentioned: attitudes. Here, with consumer attitudes to farm animal welfare. This is where we make <u>discoveries</u> about the social world.

Then, two, there's a layer beneath this that has to do with the <u>characteristics</u> of people: with <u>subjectivities</u>. Very briefly it's like this. You can't phone people up and ask them about animal welfare unless you also make a whole lot of assumptions. You need to assume that people indeed have more or less long term <u>attitudes</u>. You need to assume that when they go to the shops they are capable of making <u>choices</u>. You need to assume that they are capable of making <u>rational</u> choices (because they'll make use of information if it's given to them). And finally, you need to assume that they are more or less <u>ethical</u> beings, and that notions of moral right and wrong may influence their choices.

That's a lot of assumptions about <u>subjectivities</u>. But they are <u>built</u> into the survey. They're taken for granted. Though this is not the end of the story. For the Eurobarometer also works by making assumptions about <u>collectivities</u>. This is the third layer. To say it very quickly, it only works as a sensible exercise if Europe can be imagined and re-enacted as a <u>container of individuals</u>; as a bunch of citizen-consumers whose opinions and actions may be aggregated to form something called 'the European citizen and/or consumer'. In short, I'm suggesting that a version of Europe (and the nation state too) is being <u>done</u> in the survey results. For those who read it, for a moment at least, that is what Europe actually <u>is</u>. It's been turned into a stratified population of individuals endowed with attitudes and behaviours. That's <u>performativity</u> at work.

There is much more that might be said on this, but the important point is that in order to work and to generate empirical findings, <u>all</u> social research methods (the Eurobarometer survey is just an example) have to pick and choose between different individual and collective realities. More strongly, they take it for granted that those realities exist. And yet more strongly still (this is really the core point), they tend to <u>reproduce these</u> in one form or another. People are done as rational-ethical discretionary subjects, whose actions are partially shaped by relatively stable attitudes. And collectivities are done as statistically derived collections of particular person-subjectivities.

Don't misread what I'm saying as an attack on quantitative social science. Everything I'm saying applies just as much to qualitative methods as well. What I'm doing isn't an attempt at critique. Rather, I'm rehearsing an argument about what it is that methods, all methods, actually <u>do</u>. First I'm saying that they make <u>discoveries</u> about the world, and that those discoveries may surprise us. But then, and counterintuitively, I'm saying that they <u>also</u> make

more or less <u>self-fulfilling assumptions</u> about the character of the social world. And that in so doing they tend to shape it, so to speak, below the radar in ways that we scarcely notice. In short, that they tend to produce what I sometimes think of as <u>collateral realities</u>: 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 social[12].

This, then, is the second part of the argument about the double social life of methods. If methods are shaped <u>by</u> the social, then methods <u>also</u> help to <u>create</u> social worlds, to make them <u>current</u>, and to circulate them. In short, <u>methods are actively engaged in doing the social</u>.

Reflections

So what follows?

We can discuss and debate. That's what we're here for. But let me offer a few thoughts.

<u>First</u>, this agenda suggests that methods are too important to be left exclusively to those who work on their technical specificities. To say this is <u>not</u> a put-down or a sneer. Technique is important. But the social life of method is different and <u>it's</u> important too. This is because it tries to put method back into its social context. It tries to treat method as a social phenomenon in its own right[13]. Note, before we move on, that if methods help to make the social, it might also be useful to think of them as <u>social theories in practice</u>, as theories that are powerful because they go in under the radar and help to <u>form</u> the social by stealth.

<u>Second</u>, it suggests that Merton's account of 'the reign of error' is both right and wrong.

> 'The self-fulfilling prophecy' (wrote Merton), is, in the beginning a false definition of the situation evoking a new behavior which makes the originally false conception come true.'[14]

That which was false becomes true if enough people believe and act on it. Obviously this isn't stupid so what's the problem?

The answer is: it's the black and white distinction between definitions of the situation that are true, and those that are false. As if, somehow or other, those definitions were always <u>clear</u>. For, as I've suggested for the Eurobarometer, some definitions of the situation may indeed be <u>explicit</u> (as in 'the bank is going to fail') but others are <u>implicit</u> because they're deeply embedded in practices. No-one knows about them. So, for instance, I'm certainly not saying that those involved in the Eurobarometer survey actually sat there and said to

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[12] Law (2010, forthcoming)

[13] 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.

[14] Merton (1948).

themselves: 'yes, nation states are collections of individuals, that's what they <u>are</u>.' No-one <u>made</u> explicit theoretical judgements about the characteristics of nation states. But that is what the method was <u>doing</u>, even so. Implicitly. It was generating and propagating a <u>working</u> definition of the nation state.

Why's this important?

The answer is that social realities are being shaped by social research methods <u>covertly</u>, and in ways independent 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. Which suggests that it's important to excavate the versions of the social hidden in our methods and then to debate these. Do we actually <u>want</u> the kinds of collectivities implied by ethnographies, by surveys, by focus groups, or by collations of transactional digital data? Do we even know what they <u>are</u>? And what kinds of subjectivities and collectivities they are propagating? These are the kinds of issues that we need to be discussing.

As you'll see, when we start talking like this we're no longer dealing simply with methodological questions. We're also trading in politics, in questions about the <u>kinds</u> of social worlds and subjectivities we want to help to make more real – to <u>realise</u> – in and through our methods. We're dealing with what Annemarie Mol calls an <u>ontological politics</u>.[15]

But if methods <u>shape</u> the social then it also becomes urgent to think <u>structurally</u> or <u>institutionally</u> about where and how they work. It's difficult to say this well – for 'structures' or 'institutions' are themselves performative effects. But 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 <u>implied</u>, what's being <u>assumed</u>, in the practice of that method? What goes <u>with</u> it? What else <u>necessarily</u> goes into the space? Any answer to these questions would include the following.

- One, there are the <u>researchers</u>, those that <u>do</u> the knowing, so to speak. A set of <u>subject-positions</u> constituted, <u>inter alia</u>, by the descriptions and the <u>representations</u> being produced by the research.
- Then, two, there are the putative <u>realities</u> being described. Unless the research is complete nonsense, there <u>will</u> be people out there with the relevant attitudes (for instance about farm animal welfare) at least when they're confronted with an appropriate questionnaire. And then, alongside the realities like these that are being made manifest, there are the <u>implicit</u> realities too, the collateral realities tacitly embedded in the method, in (for instance) the form of rational <u>subjectivities</u> and

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[15] Mol (, 2002 #246}.

statistical <u>collectivities</u>. This is the performative argument about the shaping the social that I've just been rehearsing.

- And then, third, to use these terms that I'd prefer to avoid, there's an enacted <u>institutional</u> context which includes the <u>sponsors</u> (such as the European Commission) but also, something that I've not talked about, the <u>circuits</u> through which the findings flow. The circuits probably include but extend beyond the sponsors (they'd include the European meat trade in the case of the Eurobarometer).

Here's the problem. Typically we think of

(one) <u>representations and findings</u>

(two) the <u>realities</u> that relate to these and (I'm saying) are generated by them, and

(three) the <u>institutional contexts</u> in which everything takes place

as being quite <u>separate</u>. But it's becoming clear that if we really want to understand the social life of method and its force then we will need to break this separatist habit. We will need to understand that methods inhabit and help to reproduce a complex ecology of representations, realities <u>and</u> institutions. So (for instance) survey methods 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. It is very, very, difficult: to <u>know</u> differently, to shape new <u>realities</u>, and to imagine different <u>institutional circuits</u> for knowing because <u>all of these have to be shifted together and at the same time</u>.

This thought needs to be tempered by the recognition that there are diverse methods at work within the standard institutions and circuits – and all sorts of methods at work within the new institutions of the digital. Even so, however, I think that it catches the predicament of those brave souls – for instance in post-colonialism, STS, and feminism[16] – who have been seeking to simultaneously reground expertise and forms of knowledge, versions of the real, <u>and</u> institutional structures. The importance of spirits in road-building; the Aboriginal need to dream the land in order to keep it alive; the refusal of scientific expertise in debates about nuclear power; all of these are realities that don't find space in the standard methodological and institutional ecologies. The realities that these alternatives shape are literally <u>unthinkable</u>. But, here's the bottom line, until we <u>can</u> find ways of rethinking knowledges, realities and institutions together in the same breath, we won't have the tools that we need to understand the work done by our methods, or to shape these and their social worlds in ways that are different and better.

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

Perhaps I have pushed the argument too far. But whether or not this is the case, I look forward to our discussions of the social life of method!

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