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From Methodology to Methodography: A Study of Qualitative and Quantitative Reasoning in Practice

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Abstract

Despite the huge literature on the methodology of the social sciences, relatively little interest has been shown in sociological description of social science research methods in practice, i.e., in the application of sociology to sociological work. The overwhelming (if not exhaustive) interest in research methods is an evaluative and prescriptive one. This is particularly surprising, since the sociology of science has in the past few decades scrutinised almost every aspect of natural science methodology. Ethnographic and historical case studies have moved from an analysis of the products of science to investigations of the processes of scientific work in the laboratory. Social scientists appear to have been rather reluctant to explore this aspect of their own work in any great depth.

In this paper, we report on a ‘methodography’, an empirical study of research methods in practice. This took the form of a small-scale investigation of the working practices of two groups of social scientists, one with a predominantly qualitative approach, the other involved in statistical modelling. The main part of the paper involves a comparison between two brief episodes taken from the work of each, one focussing on how two researchers analyse and draw conclusions from an interview transcript, the other on how collaborators work out an agreed final version of a statistical model for combining temporal and spatial data. Based on our analysis of these examples, we raise some questions about the way in which social scientists reason through their problems, and the role that characterisations of research, as research of a particular kind (e.g., qualitative or quantitative), play in actual research practice.

Keywords: Research practice; research methods; methodology; qualitative; quantitative

Introduction

The landscape of the contemporary social sciences can be divided up around methodological boundaries of many kinds. Depending on the circumstances, social researchers can and do point to specific features of the way in which they conceive and approach their tasks as a means of marking out a territory and of differentiating themselves from their peers, whether involved in related or quite unconnected forms of inquiry. While many of these lines of methodological demarcation are only of temporal and local significance, others have proven to be much more stable and enduring. One such line is that between ‘qualitative’ and ‘quantitative’ research, a methodological divide that cuts across the otherwise highly variable academic geography of the social sciences.
Exactly how the qualitative/quantitative divide ought to be drawn, what criteria it should be based on, what core epistemological, ontological and political debates it matches up with, and hence what its ultimate significance might be, are questions that have been hotly contested for decades. Nonetheless, despite recent attempts to defuse, blur, collapse or otherwise dispose of the distinction (as with the call for ‘mixed methods’), researchers are still commonly referred to – by themselves as well as others – as belonging to one or the other camp. The nature of the qualitative/quantitative divide is not simply a technical question, about the problems, instruments and materials a researcher happens to work with. One of the reasons why the debate has acquired and retained so much heat is because qualitative and quantitative research have been long been argued to involve incompatible ways of reasoning about the social world. Whether this incompatibility is linked to a clash between two cultures (Snow, 1959; Mahoney and Goertz, 2006) or, more modestly, the use of sets of non-overlapping procedures for formulating problems and arriving at conclusions about them (Hacking, 2002 [1992]), the presumption has been that qualitative and quantitative researchers think in identifiably different ways.

In what follows, we examine the idea that qualitative and quantitative researchers reason in basically different ways based on materials gathered in the course of fieldwork in two research units that can be largely placed on either side of the qualitative/quantitative divide. The aim of this study of research methods in practice – a ‘methodography’ as we will call it\(^2\), a category of study that joins a growing body of research into ‘the social life of methods’ (Law et al., 2011) – was to gain insight into how social scientists reasoned through their research problems and to assess the extent to which this aspect of their work could be broken down along qualitative or quantitative lines. We speak of ‘reasoning’ to emphasise that this process is not just about thinking ‘in your head’, but also, and perhaps more importantly, about talking, arguing, and working with various material objects (cf., Hacking, 2002 [1992], pp. 180-181).

**From methodology to methodography**

There are few direct precedents for a study of the working practices of social scientists. There is a large body of anthropological or ‘laboratory’ studies of the natural sciences, centred on detailed examinations of laboratory research in biology, chemistry, engineering, computer science and the like, and involving a number of influential figures in a range of social science disciplines (see, e.g., Knorr Cetina, 1985; Lynch, 1993; Doing, 2008). The number of studies of the equivalents of laboratory work in the social sciences is, however, far smaller.

This is, in many respects, surprising. The social sciences are known for their concern with their methods and there is a huge social scientific literature on the topic, covering all aspects of the research process from conception through design, data collection and analysis to writing up. Alongside the more conventional treatment methods work receives in textbooks, manuals, research reports and the many meta-level reflections which attempt to develop synoptic appraisals of the state-of-the-art, there is what might be termed a substantial ‘reflexive’ undercurrent in the literature. This encompasses confessional and exposé accounts (e.g., the infamous diaries of Malinowski [1967] or the accounts in the edited collection by Hammond [1964]), reports of academic politics (e.g., Williams, 1992), and critical-exegetical studies of social scientists’ literary, textual and rhetorical practices (e.g., Clifford and Marcus, 1986), including feminist scholarship concerned with reflexively studying and explicating researchers’ own forms of knowledge production (e.g., Smith, 1990).

However, despite this, there are very few studies that have instigated the social sciences with the same scrutiny as the investigations of the experimental sciences. This could be down to a fear of (self-)scrutiny: Goffman (1961, p. 152) has observed a “touching tendency to keep a part of the world safe from sociology” (in particular, sociology itself), while Coser (1971, p. xviii) has remarked that “sociologists find it still easier to study status superiors than to study themselves”. Only recently has there been an attempt to get a ‘sociology of social research’ (Leahey, 2008) off the ground, in particular, through studies of the interactive achievement of survey interviews (e.g., Suchman and Jordan, 1990; Houtkoop-Steenstra, 2000; Maynard et al., 2002), but also investigations of the practices employed in qualitative interviews (e.g., Hester and Francis, 1994; Rapley, 2001).
Whatever the reason for the paucity of studies, undertaking research of this kind presented us with a unique opportunity: the chance to study a rather elusive figure, the social scientist, in their ordinary working environment over an extended period of time. In terms of our starting point, we saw the study very much as a laboratory study of social scientists’ methods: an examination of research practice with an explicitly comparative dimension, that was oriented to the qualitative-quantitative divide without presupposing its relevance. We have chosen to call a study of this kind a ‘methodography’, i.e. an investigation of research methods in practice.

The study

The study was enabled by an award from a collaborative fund set up by the UK’s National Centre for Research Methods (NCRM). The NCRM, established by the Economic and Social Research Council (the principal funding agency for the social sciences in the UK), consists of a ‘hub’ and a series of ‘nodes’, and the bid was submitted by two of those ‘nodes’: Realities in the University of Manchester and BIAS in Imperial College London. Having come into contact through the NCRM, the proposed project represented an opportunity for each to learn more about the work of the other (and assess the potential for future collaboration) based on a study conducted by an independent research team with experience in ethnographic studies of expert work practices (ourselves). The money secured provided enough to fund a small-scale study that ran for five months on a half-time basis (with two-and-a-half months, then, of actual research time).

The NCRM represents a particularly interesting fieldwork site for investigating questions of method because, while it distributes funding on the basis of distinctions between qualitative and quantitative research, it does not take these distinctions entirely for granted. Instead it has provided opportunities for researchers to explore areas of overlap between research of different kinds, examine the possibilities for methodological cross-fertilisations, and initiate a relatively open dialogue about methods between practitioners who might not otherwise have much contact with each other. In this sense, the NCRM employs understandings of qualitative and quantitative methods as sets of differentiated technical repertoires but also topicalises them as a focus for investigation and discussion in their own right, with an emphasis on experimentation and innovation providing the bridge between the two. The kinds of experimentation and innovation being encouraged through the NCRM are exemplified in the case of the two nodes being discussed, Realities and BIAS.

Realities is connected to the Morgan Centre for the Study of Relationships and Personal Life in Sociology at the University of Manchester. Realities stands for ‘Real Life Methods for Researching Relationalities’ and its mission statement defines its purpose as being: “to pioneer the development, use and innovation of ‘real life methods’ for researching relationalities and personal lives in complex worlds”. ‘Relationalities’ is a key term because a concern with explicating different relationships and the ‘forms of connectedness’ they are grounded in, that is, relationalities, runs throughout the work Realities does.

BIAS is based in the Department of Epidemiology and Biostatistics in Imperial College London, and stands for ‘Bayesian Methods for Combining Multiple Individual and Aggregate Data Sources in Observational Studies’. Its mission statement is: “to provide social scientists with computational and methodological tools to combine and analyse heterogeneous data sources in a comprehensive and rigorous quantitative framework”. ‘Combining’ is a key term, as combining data sources is a major strand within BIAS’s work. The name is a deliberate play on ‘bias’, one of the chief analytical targets of their research.

These two nodes could be (and were, albeit with qualification) described as ‘qualitative’ and ‘quantitative’ respectively, with research at Realities for the most part centred on the analysis of the spoken and written word, as well as visual materials, while BIAS for the most part worked on large-scale numerical data sets. However, these labels are also slightly misleading. For one thing, Realities was also engaged in mixed method research (combining qualitative data of various sorts with quantitative data of various sorts) and BIAS’s focus was not so much ‘quantitative’ as it was ‘statistical’. Moreover, neither employed tried-and-trusted methods but deliberately set out to design, trial and evaluate a range of new techniques in each of the areas their research focussed on. As a result of the licence to experiment and innovate provided by being part of the NCRM, neither could be seen, therefore, as entirely orthodox research units. Nonetheless, while both had in an interest in re-thinking existing methodological boundaries, each was engaged in recognisably distinct types
of research and occupied quite distant positions in the landscape of contemporary social research. For all of these reasons, they represented excellent case studies for a comparative examination of the salience of the qualitative-quantitative divide at the level of research practice.

Over the five months of the project (November 2009-March 2010), we conducted several interviews with individual researchers from both units (17 with Realities, 10 with BIAS), attended workshops and talks (2 and 3), sat in on a variety of group meetings (12 and 2), and observed analysis sessions in which researchers worked on data together (3 and 6). We also had a variety of different kinds of written accounts to work with as well (working papers, websites, official documentation and the like). While the analysis sessions provided our focus, our capacity to make sense of what was going on within them was directly informed by what we learned in these other ways. All those who participated gave their consent, and were free to decide the level of involvement they felt most comfortable with.

We turn now to our examples, two brief episodes taken from meetings between researchers working on different projects within each node. The first centres on two researchers involved in analysing and drawing conclusions from an interview transcript, the second centres on two researchers in the process of checking a statistical model built to combine temporal and spatial data at the small-area level. The researchers at Realities were at the beginning of data analysis (they had collected interviews and were starting to read through and discuss them), while the researchers at BIAS were almost at the end of a particular project (and were now working up to a final choice between three – marginally – different models). We will go through each in turn, before broadening out to a discussion centred on what we can take from examples such as these.

Example one: analysing interviews

Our first example is taken from one of the two major investigative strands within Realities work. Both strands were designed to explore forms of connectedness with place, with others, and with the physical/sensual world. Starting off from the recognition that our everyday lives are ‘peopled’ in all manner of ways, the aim of Realities as a whole was to explore both the positive and negative aspects of the ways in which relationships with others come to permeate particular aspects of everyday lives in particular places and at particular times. As a research focus, this involved a series of methodologically novel (but topically interconnected) investigations of the forms of connectedness between, for example, friends, generations or colleagues. An important aim of these investigations was to unsettle or ‘trouble’ existing sociological theorising about relationships, an analytical concern visible in the data we shall present in what follows.

The episode we examine relates to a facet of Realities programme that looked specifically at ‘associations in place’. This particular part of the study came about when, during a chance encounter, one of the researchers on the team had been shown a ‘memory book’ documenting the personal histories of people who had made their lives in a working class housing estate (which we will refer to as ‘Treetown’), following municipal slum-clearances in the late-1950s and early 1960s. The research team then decided to conduct interviews with eight people (some couples) who had contributed to the memory book. The interviews took place in people’s homes and incorporated discussions of the memory book itself alongside other participant produced artefacts like photographs.

We observed an analysis session in which two researchers (one of whom, L, had conducted the interviews) worked through the transcripts of these interviews (having read through them prior to the meeting).
In this excerpt, L and C begin to discuss one interview (with participants we have chosen to refer to as Graham and Jane), having just finished their discussion of another one. In the previous case, L led the discussion, providing an analysis of its significance; here the roles reverse and C leads.

The purpose of reviewing the transcripts together in this setting was to talk about what each had found of interest in them and to come to an agreement on the most promising lines to develop analyses around. What is
in the transcripts and on the tapes is a starting point for this. The researchers remark that, on close inspection, there is ‘a lot in there’, perhaps even more ‘than it seems’. But what is it that is ‘in there’? Although both the interviewing itself (by one of the researchers) and the reading of the interviews (by both of them) were carried out with certain ideas in mind, until they talked through the data, they would not know what might be found ‘in’ their materials (what, in effect, they could say about them).

C starts off with a general observation about what C took from the interview as a whole: Graham and Jane’s relationships are different, and are framed differently, to how sociologists who write on this subject ordinarily conceive of ‘relationships’ (L and C included). Rather than continue the discussion in a general register, however, C goes on to concentrate on a particularly illuminating example, that of Graham’s relationship with his neighbour. Most of us would, at a common sense level, note something odd about someone who says he goes to the pub with a neighbour, but where the two of them do not make their way to the pub together, sit together when there, or even speak to each other. Being ‘in the same room’ as someone else is not usually enough for us to claim a personal relationship with that person, but this appears to be what Graham is doing. This is certainly strange, but why has C taken an interest in it?

It is important to note that C is not merely marking a striking passage, something that ‘jumped out’ from the transcript. Instead, C is engaged in a form of analytic noticing with the transcript as the focus. C treats this as an analytically significant detail because it supports the more general reading of the transcript (a reading which led C to pick out details such as this in the first place). This is, in other words, a discovery: before conducting the interviews, it would not have occurred to the researchers to seek out this particular way of ‘associating in place’. It is not a possibility that the interview was specifically designed to produce. Now they do know about this relationship, however, it tells them something about relationships on the estate more broadly. The general and particular are linked and used to support each other. That said, what makes this a discovery requires further elaboration. In the rest of the excerpts, we see C attempt to build a specifically sociological analysis of what makes this detail so revealing.

Transcript 2

1 C: It’s clear that Jane and Graham are really
2 L: [Mm]
3 C: Connected to the people that they kind of live with in
4 L: [Yeah]
5 C: This kind of estate, or whatever. But also they’re highly ambivalent, you know
6 L: [Mm]
7 C: They’re ambivalent about these relationships. And the people. Y’know, the neighbour guy that he
8 ([ed: went to the pub with]), you know, he could barely remember his name I think, heh, heh
9 L: Yeah
10 C: He was like, after forty years

In this excerpt, L and C continue to unpack what a detail such as this has to tell them more generally about relationships. They note a distinction that emerges in the stories being told by Graham and Jane. This is between, on the one hand, being connected to people (and the importance of that) and, on the other, the ways of that connection (in terms of knowledge, mode and frequency of contact). Graham and Jane know people, and people are important in their lives. However, it is in the way that they know them that this particular detail about their next door neighbour acquires its significance. More specifically, it is the ambivalence associated with this ‘critical association’ that is of note, an ambivalence that characterises other relationships they have been told about too.

Not only, then, does this detail “jump out” as an isolated, singular utterance, but building outwards from it allows for inferences to be made to the way Graham and Jane interact with the people around them based on what their remarks reveal about the character of those relationships. By working in this way, the sociological relevance of this remark can be slowly spelled out.
Transcript 3

1 C: So this idea of being a regular at the pub
2 L: Mm
3 C: It's a category of relationship itself
4 L: Yeah
5 C: Isn't it, it's like. So it's not a friend, it's not heh someone ((ed: you're particularly close to)),
6 but in its, it's a very important relationship
7 L: Hmm
8 C: And, y'know, it has certain norms that go with it
9 L: Mm
10 C: And certain, knowing how to be
11 L: Mm
12 C: So you wouldn't sit on someone's chair which y'know
13 L: Mm
14 C: So there's all of that
15 L: Yeah

The discussion here turns to what to make of “being a regular at the pub” and it involves, we suggest, a normalisation of what has initially been a peculiar kind of ‘noticeable’, creating a description that covers both Graham, his neighbour, and how they relate to one another. Going to the same pub is not an aspect of a relationship but is described as “a category of relationship itself”, one imbued with some significance in this context, at once ambivalent but nonetheless important. C goes on to say what kind of (minimal) relationship this is: it is not a friend, not someone you are particular close to. However, it is still an “important relationship”, which is bound up with certain normative requirements and rules of conduct, ways of “knowing how to be”, that define it and make it stand out as a category.

Transcript 4

1 C: Is there anything else about Graham and Jane? I mean there's lots of the specifics of their story,
2 which is interesting, ehm () But I think kind of just capturing that, kind of, very much () through
3 this interview Jane, and Graham, it's just this is a very, doesn't seem to be a very knowing
4 community, it's a much more a kind of, or a community based on knowledge of each other, of
5 L: Mm
6 C: But very much a kind of community that's lived with
7 L: Yeah
8 C: That's the narrative on the page
9 L: Yeah
10 C: The community on the page that emerges here is a community that's lived with
11 L: Mm
12 C: It involves ambivalences, and it involves other (things), but it certainly doesn't involve any kind of
13 dialogical knowing of, y'know, the other. One's neighbour, one's friend, one's, eh what's, the
14 regular in the pub
15 L: Mm
16 C: Kind of, so that's
17 L: Mm
18 C: That's a really strong story that comes, and it's, as you say, really situated in a particular historical
19 time
20 L: Mm
21 C: And a particular location. It's very spatialised

In this our final excerpt, C begins to draw more general conclusions from the example. The interest is not simply in these particular remarks made by these particular people (Graham and Jane) but in how those remarks resonate with what else these people have to say about relationships ‘in place’. C is also interested in how it relates to what other interviewees have said and how they have said it. What data such as these provide
is more than an account of specific relationships ‘in place’, they also reveal something about ‘the place’, the kind of community, in which relationships like this can be spoken of in these terms. In the closing remarks on this particular transcript, we come full circle. Having begun with a loose general description of the nature of relationships on the estate (that they were not what the literature would lead us to expect them to be) and an example of that divergence, Graham’s minimal relationship with his neighbour through the local pub, we arrive at a much more developed account which draws these elements together in a more tightly-knit way. ‘Being a regular in the pub’ has become a category of relationship through which it is possible to ground the claim that this is not a “knowing community”. At the same time, it also helps them deepen their claims about the limitations of existing sociological accounts: accounts which are limited because they do not consider the ‘non-dialogical’ ways of knowing that are central to life in particular places.

In this sense, what we see over the course of the four extracts is a glimpse of an analysis-in-the-making, the transformation of an odd comment in response to a question in an interview into one of the keystones of a sociological analysis of ‘associations in place’ through the alchemy of social scientific reasoning practices. What makes this episode interesting is precisely the way those reasoning practices are displayed in the above. The finding instances happy serendipity, the researchers appreciating that their method has only fortuitously yielded this interesting finding, and, as is remarked, that it could so easily have been missed (the exigencies of the interviewing processes might have led away from, rather than toward this point). It is interesting, because it is used to establish a counter-point for them, that it does not fit with the conceptions that the researchers and their professional colleagues would themselves have called a ‘relationship’ – the interviewee’s account is used to question the sociological practice not vice versa.

Example two: building models

Our second example is taken from one of the four projects that formed BIAS, a project in which researchers were attempting to develop ways of modelling spatial and temporal data together. The main aim of this project was to create generic models able to capture the degree of local variation among small geographical areas over time, establishing both the common trend across areas as well as those areas which departed from or ‘bucked’ that trend. As in the rest of BIAS’s work, an important programmatic aim was to demonstrate how Bayesian statistical techniques make it possible to link together different datasets to generate greater inferential power. In this case, the work had two important aspects: (a) the construction of generic models and (b) the testing of those models against real data (crime rates, income levels and COPD mortality figures) to see whether they worked. The schematic diagram in Figure 5 captures the logic of the modelling process. The important thing to take away from it is that the common and area-specific trends are modelled in different ways.

Figure 1
models, three different ‘takes’ on modelling the common and local trends (Figure 2), which formed the focus of their discussion. The excerpts that follow centre on the first of the three models.

**Figure 2**

![Three models](image)

\[
\log(\mu_{i,t}) = \begin{cases} 
\alpha + z_i \cdot (\xi_i + v_t) + (1 - z_i) \cdot \eta_{i,t} & \text{Model 1} \\
\alpha + \xi_i + z_i \cdot v_t + (1 - z_i) \cdot \eta_{i,t} & \text{Model 2} \\
z_i \cdot (\alpha_1 + \xi_i + v_t) + (1 - z_i) \cdot (\alpha_2 + \eta_{i,t}) & \text{Model 3}
\end{cases}
\]

The two alphas do not have the same interpretation when using the AR(1) structure for the area-specific trends.

**Transcript 5**

1 M: I’m only thinking that um, I mean really here we’ve, kinda this all started because we’ve been
2 thinking it’s, it’s, not odd, but it, we’ve got a, a CAR on the common temporal trend, then you’ve
3 got an AR on the area specific temporal trend
4 J: \(\text{Mm (.) mm}\)
5 M: It’s kind of like (.) Well is there kind of a good substantive, or methodological reason for that or
6 was it kind of just because of the way it was implemented in BUGS with the sum-to-zero
7 constraint
8 J: Yeah
9 M: Eh (.) So I suppose it’s kind of a case that (.) if it’s really, y’know we either want the CAR or
10 an AR I prior, but basically that is just a prior on the temporal part=
11 J: \(=\text{Yes=}\)
12 M: =And the rest of it should be the same, so if, it you put in, if you have a CAR prior on the
13 temporal bit, so you need an extra intercept, then if you put a spatial prior on that, that would
14 be then different (.) that would be quite a substantively different model compared to this one,
15 with the AR prior because you’ve got no
16 J: \(\text{Mm}\)
17 M: Borrowing of information
18 J: \(\text{Right}\)
19 M: Across space
20 J: Right, right, okay
21 M: So I think that’s all I was, well, I suppose at least, I’d like to get that thing resolved in terms of
22 the difference between the AR and the CAR just on the temporal part, with:out, sort of, other
23 differences as well (“inaudible”)
24 J: Right, okay, I understand what you mean
This is a difficult exchange to follow. It is centred on technical matters, and requires an advanced level of mathematical and statistical understanding. Nevertheless, certain aspects stand out. It opens with something noticed by the second researcher M, who expresses some unease, some worry (‘not odd, but’) about the way they have set up the models. M’s unease stems from a possible asymmetry in the first one: there is a ‘CAR’ on the common trend, but an ‘AR(1)’ on the area-specific trends (lines 2-3) when the two should perhaps align.

That there is an imbalance is not a question of a simple ‘mistake’. M has not spotted a miscalculation (like 2 + 2 = 5), which now needs to be corrected. It is rather that M notices an asymmetry and wonders whether there is ‘a good substantive, or methodological reason’ for it. M is attempting to see the logic embodied in the model and the worry is that there is no reason, that this asymmetry is simply a result of the statistical software they are using rather than technical, substantive or methodological decisions. The researchers start to think about possible ways to correct this asymmetry, but note that the most obvious ways of proceeding (i.e. introduce symmetry) would result in ‘quite a substantively different model’.

The discussion continues:

Transcript 6

1 M: I mean it may still, well I don’t know, eh, it, it sort of seems, given the sort of, the actual idea of
2 the model, you’ve got a common model for most areas then some areas have got their own
3 L: Mm
4 M: Sort of trend
5 L: = Trend, trend pattern, yes
6 M: = But it sort of seems that if you then start to try and smooth that spatially it, eh I think
7 it’s pretty, it sort of seems neater just to allow it, it’s either totally different, so essentially comes
8 from a totally different model
9 L: Mm
10 M: Where there’s no shared parameters with any other areas or
11 L: Right
12 M: Just from the common one, it sort of seems a sort of clearer structure
13 L: So, for the area specific trend model it’s not fair to, to, to smooth out, well, to borrow strength
14 from each other. In that case we are saying, and there is a similarity between areas
15 M: Yes, I mean it’s not, I suppose it’s not that it’s not fair but it’s, I suppose, is that what we want, do
16 we actually, do want to be doing any sort of smoothing
17 L: Mm
18 M: Or do we want it just to be completely
19 L: Yeah
20 M: Sort of different from everything else
21 L: Sure. Yeah. If I mean all, all
22 M: So yeah both could be plausible but again we need to be really clear as to actually
23 what assumptions we’re actually making and whether that sort of does actually make, makes
24 sense
25 L: Yeah
26 M: (That) interpretation
27 L: Right, right. I mean all these differences are very subtle
28 M: Yeah, I’m sure actually, at the end of the day
29 L: (inaudible)
30 M: It doesn’t make an awful lot of difference ((laughs))
31 L: Yeah ((laughs)) in terms of the, the performance in simulation they, they are all the same
32 M: Right
33 L: To be honest ((laughs))

The second researcher M continues to formulate why the spotted asymmetry might be a problem: it seems to go against ‘the actual idea of the model’, i.e., the separation into ‘general’ and ‘area-specific’ parts.
So in going through the model again, the researchers are asking: “Is that what we want?” (line 15). Is it possible to have a different, ‘neater’ (line 7) model with a ‘clear structure’ (line 12)? Calculating the common trend involves borrowing. The question is: should calculating the area-specific trend involve smoothing as well? There was no clear answer; it was a genuine puzzle. There were good reasons either way, and discussion about which path to take continued over the next month (with other collaborators).

This reveals something about this kind of work: the researchers started this project with a rough idea about the model (captured in Figure 5), but that idea was highly underspecified. It told the researchers about the kinds of components that should be in the model, but not about how to build them or fit them together. Here the schematic does not tell them whether the components should be fully isolated from each other. Both possibilities are ‘plausible’ (line 22), but the second researcher thinks that they need to be more explicit about the assumptions that they are making in order to see what “makes sense”. While the researchers started with a general idea, then, they are now faced with a mathematical model and are searching for an interpretation of it. As M puts it later in this meeting: ‘I’m just trying to think about actually the, the plausibility from the interpretation point of view’.

Note that this discussion is not centred on ‘measurable’ differences. M acknowledges that “it doesn’t make an awful lot of difference” (line 30) and L agrees that the performance of the three models is “all the same” (line 31). The issue at this point is thus not about a model that ‘fits’ the data better than another. Rather, the second researcher is contemplating whether a model, which does fit the data reasonably well, has a ‘good’ or ‘reasonable’ interpretation. Being in a position to see the sense of the methodological choices made in the model is an important aspect of reasoning such questions through. As M puts it later in the meeting: “it’s just kind of making sure that we can actually, the model we end up writing down as the model we’re using does have a sensible interpretation”.

Over the course of this exchange it becomes clear that the model is not quite there yet.

One of the most interesting things about these exchanges is that they give us access to that unfolding history, a glimpse of the model-in-the-making. The end-products of statistical research are typically designed to stand-alone, to wear their logical structure on their sleeve, but here we are looking at something that is not a finished product; it is still a live issue. In these exchanges, we come to see that the researchers are actively engaged in working the model through. Models do not build themselves any more than they interpret themselves; it is neither a predominantly mechanical nor purely deductive process. Of course, some standard techniques are involved; they are not starting from scratch. But choices still have to be made, and these are frequently based on intuitions, hunches and ideas of what is needed that have not yet been fully rationalized. The researchers are not following a pre-specified template, this is not the not the ‘beginning of […] rails invisibly laid to infinity’ (Wittgenstein, 1953, §218) along which the modelling process glides without effort.

We also get to see the work that goes into interpreting the model. Models have to ‘fit’ the data, to be ‘sound’, and there was a lot of testing and checking going on ‘in the background’ of this exchange, involving lots of graphs and plots in order to check this. What this exchange shows is that this is not enough. The researchers have to be able to see a clear logic in the model – a ‘neat’, ‘clear structure’ that ‘makes sense’. For that reason, models have to go through continuous modification before they can reach a settled form even if it makes no measurable ‘performative’ difference.

**Discussion**

In relation to the theme of this special issue, that of crossing methodological boundaries, our concern has been less with cross-border travel than with questions about where the border between ‘quantitative’ and ‘qualitative’ research runs and, mainly, with what falls on its different sides, focussing on two examples of qualitative and quantitative research. Our study, however, does itself ‘cross boundaries’ even though it involves only qualitative research because it (a) applies qualitative methods to the use of qualitative methods, thus crossing the divide between sociology as a study of social practice and sociology as a form of social practice and (b) it applies qualitative methods to the study of quantitative social science work (unfortunately it does not round out by applying quantitative methods to qualitative research).
Our examples and observations are thus not intended to address methodological questions such as: Is the method free standing? Can it be used in conjunction with other methods? Does it adequately represent the data it captures? Or: Does it embody epistemological preconceptions? As announced at the beginning, this descriptive study was interested in how researchers implemented their methodological preferences in research practice. We pay attention, borrowing a phrase from studies of computer-supported cooperative work (CSCW), to ‘the work that makes the methods work’, to the things that members have to do to – as far as they are concerned – adequately deploy their elected method, but which are not normally articulated as part of the explication of how the method is followed. We might say that the activities we report in the two examples are ones that are ‘not on the record’ (which they assuredly are not – the occurrence of these exchanges will not be recorded amongst the study materials nor referenced in published reports). However, we prefer our slightly more cumbersome expression to ‘off the record’, since the latter may suggest a sort of self-censorship, that our examples reveal something that is kept out of the record because it is at odds with official policies and requirements, something that would render the forms of reasoning involved questionable were they to become known. The latter is, we think, an impression generated by many studies of ‘situated practice’ in the natural sciences, which tend to argue that the way science is presented in textbooks or research articles creates a ‘misleading’ picture of it (cf., Medawar, 1963; Greiffenhagen and Sharrock, 2011).

We are not ruling out the possibility that forms of sharp practice are to be found in the work of social science investigators, but the examples we have given are not of that kind. While they are, for those engaged in them, quite natural aspects of carrying out the work in hand, the work involved is not of interest in and for itself. It is a means-to-an-end, and having delivered materials for further consideration, its particular specifics are thereafter forgettable. Harold Garfinkel made a useful distinction between ‘virtual’ and ‘actual’ production accounts (cf., Francis and Hart, 1997, p. 150), where the latter portray the nuts and bolts of how things get done, how end products specifically acquire the features they possess, whilst the virtual production accounts are those which provide an appropriate understanding for users of what the product amounts to. This is, we suggest, a function of methods reports, to serve as ‘virtual production accounts’ designed to show how the delivered results relate significantly to the chosen method.

Slightly more controversially, and just a little more connected to methodological issues, we can observe that the reasoning in which the researchers are engaged in both cases involves ‘interpretation’. Insofar as there are tendencies to differentiate ‘interpretive social science’ from ‘non-interpretive’ kinds, where this is also meant to track the qualitative/ quantitative divide, then these instances would suggest there is nothing definitive about such a demarcation. In line with our approach, ‘interpretation’ is treated not as an a priori form of understanding generic to a certain sort of phenomena but as a distinctive, practically occasioned form of everyday activity. Interpretation is involved in each of our cases because it is appropriate to what the participants are currently attempting to do.

The main difference between the two nodes, then, was not to be found in the way researchers reasoned their problems through, as though one was entirely logical and systematic (or mechanical) whilst the other was entirely intuitive and interpretive (or undisciplined) – the picture according to the worst stereotypes in the literature. Each element was present and indeed necessary in both cases. Nonetheless, the inferences the researchers made in the course of their exchanges began at different points, were leveraged in different ways and led them in very different directions because they belonged to distinct lines of inquiry into quite distinct problems each with their own local and disciplinary histories. Embedded in and addressed to distinctive ‘problem situations’, it was their differences that provided the researchers purchase on their particular problems and gave their research its character as research of a particular kind. It was these diffuse differences, and not a generic separation down qualitative and quantitative lines, that mattered to doing the work.

We must not be misread. There is no intention to match the practices of either group against anyone’s ideals of method so as to attribute methodological failings (or successes) to them. Acknowledging that these were specific moments in ongoing projects, what we have attempted to bring out is that the described practices are constitutive aspects of producing sound research for all practical social scientific purposes (cf. Anderson et al., p. 136). In other words, social scientists’ capacity to hold to methodological prescriptions relies upon them being able to display heterogeneous, but largely unspecified, practical competencies. As a result, locally adequate ways of reasoning problems through occupy an important place in ordinary social scientific work.
Because we have not said anything about the macroclimate of methodology, the cross-border relations between quantitative and qualitative research, we do not want to leave the impression that such relations are generally relaxed or conciliatory – open to the idea of commonalities across ‘separate’ lines of inquiry. Although we were not soliciting views on the quantitative/qualitative divide, our research showed that, even amongst a small number of people, the invidious division between quantitative and qualitative had not entirely disappeared from mind and there was a generally perceived need to hold some view on their relation. Across even that small number, attitudes were varied, ranging from enthusiasm for warmer and closer relationships between quantitative and qualitative researchers to quite sceptical or hostile views of The Other. Whatever the nature of cross-border exchanges, invoking the divide will continue to play an active role in articulating the state, tasks and problems of sociological inquiry even if the divide itself does not, or at least not straightforwardly, at the level of research practice.

1 While we could indicate the problematic character of these designations (see, e.g., Hammersley, 1992; Bergman, 2008) through the use of apostrophes, we will continue on the assumption that our bracketing of them is taken as read.

2 The term ‘methodography’ has only rarely been used in the literature. Our use is slightly different from that of Buchler (1961) who distinguishes methodography from methodology as “the practitioner’s discrimination of his methodic process and its aspects” (p.128)

References


Biographies

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