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Troubles with triangulation

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TROUBLES WITH TRIANGULATION

> 'Don't believe everything you hear, Nick', he advised me. I said lightly that I had heard nothing at all. They came to the door with me and stood side by side in a cheerful square of light. As I started my motor Daisy peremptorily called: 'Wait!
>
> 'I forgot to ask you something, and it's important. We heard you were engaged to a girl out West'.
>
> 'That's right,' corroborated Tom kindly. 'We heard you were engaged.'
>
> 'It's a libel. I'm too poor.'
>
> 'But we heard it,' insisted Daisy, […] 'We heard it from three people so it must be true.'
>
> Of course I knew what they were referring to, but I wasn't even vaguely engaged. The fact that gossip had published the banns was one of the reasons I had come East.
>
> (F. Scott Fitzgerald, *The Great Gatsby,* p22)

'Triangulation' is a term that is now very widely used. While it was originally developed by quantitative researchers, it has become one of only a small number of technical terms employed by qualitative researchers, and it has become central to much discussion of mixed methods research. Indeed, it is often treated as if its meaning were clear and its value universally accepted. Yet there are divergent interpretations, and fundamental questions have been raised about its value (Silverman 1985:105-6; Fielding and Fielding 1986:33; Flick 1992; Blaikie 1991; McPhee 1992; Massey 1999).[1]

Reviewing the literature, we can identify at least four meanings, and these point to distinctive purposes and are based on varying philosophical, or at least methodological, assumptions. In this paper I will outline, and consider the value of, these different forms of triangulation. I will suggest that at least two capture important elements of the research process, but I will also argue that these cannot be reduced to matters of technique, and do not necessarily imply the 'mixing' of quantitative and qualitative methods. At the same time, I will raise questions about arguments to the effect that different methods are based upon fundamentally discrepant epistemological or ontological assumptions, and therefore cannot be combined.

Triangulation as validity-checking

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[1] The first use of the term 'triangulation' in the field of social research methodology seems to have been in Campbell and Fiske's (1959:101) discussion of convergent and divergent validation of measurement instruments. It was later elaborated in Webb et al 1966, and was introduced into discussion of qualitative method by Denzin 1970. For useful brief accounts of triangulation, see Bryman 1988:131-4 and 2004:447. Kelle 2001 provides an account of different types or interpretations of triangulation that covers some of the same ground as my discussion here. Seale (1999:ch5) offers a good review of epistemological criticisms of triangulation.

The original usage of 'triangulation', within the literature of social science methodology, referred to checking the validity of an interpretation based on a single source of data by recourse to at least one further source that is of a strategically different type. It is worth noting that this does not necessarily involve combining different *methods* of data collection: for instance, it might require comparing interview data from several witnesses to an event (who played different roles within it or who have varying interests at stake in what is believed about it); or it could involve comparing observational data from various settings that bear on the same knowledge claim.[2] Of course, triangulation in this first sense *can* involve combining data produced by different methods; and these may (though they need not) span the qualitative-quantitative divide. Thus, postal questionnaire data may be used to check conclusions reached on the basis of semi-structured or unstructured interviews, or vice versa; while interpretations of interview data, produced in varying ways, might be checked through participant observation, or vice versa; and so on.

The idea behind this first concept of triangulation is that by drawing data from sources that have very different potential threats to validity it is possible reduce the chances of reaching false conclusions. For example, it might be argued that the tendency for people to give researchers socially desirable rather than honest responses is greater in face-to-face interviews than in anonymous postal questionnaires; so that, in this respect, the latter can be used to check the validity of conclusions drawn on the basis of the former kind of data.[3] If the data from contrasting sources confirm the original conclusion, then that conclusion can reasonably be held with more confidence than before; though there needs to be some assessment of the possibility that both sources of data were biased in the same direction (perhaps by different factors). If there is a discrepancy, then this requires interpretation in terms of the threats to validity likely to be involved in each type of data, and the direction and extent of error that these would tend to produce. Moreover, discrepancy will usually indicate a need for further investigation involving yet other sources of data, chosen to counter the effects of specific threats to validity.

The strategy involved in this first kind of triangulation is quite closely determined by its goal – checking the validity of descriptive inferences – as well as by the means for doing this – comparing data sources carrying distinctive threats to validity. However, a number of questions have been raised about it.

Some of these relate to the source model for the triangulation metaphor. In navigation, 'triangulation' has a quite specific meaning: it involves taking bearings on two landmarks in order to locate one's position. The angle between the two bearings, plus knowledge of the distance between the landmarks, allows the navigator to plot

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[2] This relates to Denzin's distinction between within-method and between-method triangulation (Denzin 1970:301). In fact, there is considerable scope for within-method variation. As regards interviews, this may concern not just differences in degree of structure, but also in where interviews are carried out, who is interviewed, and in what manner (for example, single or group interview, face-to-face versus phone or internet, etc). In relation to observation, there can be variation in structure, but also in the role of the observer, whether it is overt or covert, and so on.

[3] It is worth noting that it could be argued that in some cases the reverse relationship will hold, and this points to the potentially problematic nature of assumptions about the validity threats associated with particular data sources.

his or her position on a map. This lies at the point of the triangle these lines create.[4] Erzberger and Kelle have argued that 'the transfer of the notion of triangulation from trigonometry to the realm of mixed methods of research [seems] to have transformed it into a somewhat fuzzy idea with a variety of possible meanings'. They continue: 'Whereas the term represents a straightforward concept in its initial frame of reference, it carries a systematic ambiguity when transferred to the domain of social research methods' (Erzberger and Kelle 2003:461-2). The problem, they argue, is that the meanings of the concept's components have not been defined within the new framework.

What Erzberger and Kelle are pointing to here is that the logic of methodological triangulation in social research is rather different from that of triangulation in navigation and surveying.[5] In the case of navigation, the second measurement does not provide verification or validation of the first, but rather is a necessary complement in order to identify relative location. By contrast, in methodological triangulation what is unknown, or at least sufficiently uncertain to need checking, is the validity of the first 'bearing', the first source of data. A complementary difference is that while in navigation a single bearing can tell us that we are on a line in a particular direction from the landmark, though not where we are located on that line, in the case of social research a single source of data can in principle tell us all we want to know: whether a particular knowledge claim is true. In short, potentially, it gives us the whole answer, we do not necessarily have to combine it with something else. Or alternatively, if it is wrong, it tells us nothing in itself. So, in this first social science version, we engage in triangulation in order to *check* our answer, not so as to gain further information in order to *produce* an answer. A third difference is that in navigational triangulation, assuming that the landmarks have been correctly identified and that the bearings have been taken correctly, the result is relatively certain; there is no need for further bearings. So, triangulation in navigation is not a device for detecting and discounting error; indeed, any error in identifying the landmarks or calculating the bearings will vitiate the triangulation process.

It is clear, then, that the meaning of the term 'triangulation' has been transformed in moving from navigation or surveying to social science. However, such transformation is characteristic of the use of metaphors. Moreover, it seems to me that the new meaning of the term has been clarified in relevant respects, through the idea that different data sources carry divergent threats to validity. Furthermore, opposition to this first version of triangulation often focuses on three features that are not essential to it: the idea that validating (rather than developing) interpretations is what is most important in research; the assumption that triangulation can provide absolute certainty; and the treatment of some sources of data as superior to others in general terms. It is worth noting that, in its original formulation, triangulation was associated with a fallibilistic conception of inquiry, which denies the possibility of absolute certainty and does not treat any source of data as having priority. The assumption was that only by comparing data from different sources could we try to determine what is a reliable basis for inference. Those who reject this kind of triangulation must either

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[4] A similar method is employed in surveying, though here the aim is not to discover one's location but to document the physical relations amongst various points on a site.
[5] Blaikie (1991:118-9) has also pointed to the discrepancies between triangulation in surveying and in social science.

insist that some single sources of data are always reliable, or deny that research involves the pursuit of empirically grounded knowledge.

At the same time, we should note that there are some practical difficulties in operationalising this first form of triangulation. One of these concerns the grounds on which we attribute potential validity threats to particular data sources; often, this process is largely speculative. A second problem is that it may be difficult to meet the requirement that the different sources of information are independent of one another: for example, interview accounts produced by different informants may not be independent (see van den Berg 1996:28). There is also the question of how to respond to conflicting results: given that continuing the process of triangulation until multiple sources of information agree could be a lengthy, possibly a never-ending, process (see Perlesz and Lindsay 2003 and Ribbens McCarthy et al 2003).

The most fundamental question that has been raised about this interpretation of 'triangulation' concerns its assumption that there is a single reality whose characteristics can come to be known via the use of different data sources, methods, approaches, etc. This has led to advocacy of other forms of triangulation.

Indefinite triangulation

A second interpretation of 'triangulation' involves a different purpose and, on some formulations, abandons belief in a single reality. Aaron Cicourel proposed what he called 'indefinite triangulation', which requires collecting accounts of the same event from several people with a view to documenting how these accounts were 'assembled from different physical, temporal, and biographically provided perspectives [...]'. Referring to his research on school classrooms, Cicourel reports that: 'Comparing the teacher's account of the lesson before and after it was presented, and comparing the teacher's version with those of the children, produced different accounts of the "same" scene'. And he adds that: 'the children seemed to receive and organise the lesson in terms of their own orientation at the time of the event, and these conceptions do not always match the teacher's account of the lesson's purpose and conduct' (Cicourel et al 1974:4). The use of scare quotes around the word 'same' here indicates that for Cicourel what is involved is not an attempt to identify the truth about the scene witnessed, and therefore to assess the accounts produced by different participants in terms of how well they represent what went on. Rather, the approach adopted is closer to the sociology of knowledge: the interest is in why participants' accounts take the varying forms they do, or rather in how they have been put together. In another place, Cicourel describes indefinite triangulation as designed to 'make visible the practicality and inherent reflexivity of everyday accounts', in other words to show that accounts are always formulated for a purpose and in a way that is sensitive to a particular occasion, rather than simply being reflections of the world (Cicourel 1974:124). Built into the ethnomethodological position drawn on here is a denial that there can be only one true statement about relevant features of the situation to which various accounts relate, and (even more significantly) a rejection of the idea that social science can or should adjudicate amongst informants' accounts in terms of their truth.

A slightly different version of this second interpretation of 'triangulation' was provided by Clem Adelman and his colleagues on the Ford Teaching Project. They were influenced by Cicourel, but their purpose was a more practical educational one. They elicited different perspectives about teaching situations, and then communicated these to the participants, this then producing *second-order* accounts of the first-level accounts, and so on, potentially without end. Adelman writes: 'The underlying idea here is that no action is self-contained; people can have intended actions which are constrained by context. No actions are untrammelled, all actions in the social world are interactions. Interaction necessarily involves a reciprocation and thus a reciprocal viewpoint. Triangulation, then, does not treat the speech act as self-contained action. A speech act is seen as incomplete, needing reciprocal interpretations to complete its meaning in a social context' (Adelman 1981:79-80).[6] What is involved is a kind of educational development work: the aim seems to be to induce all parties to the interaction to overcome the restraints of politeness and to say honestly what they thought about what had taken place, and to take notice of the honest accounts of others; thereby illuminating the meanings which were involved in the original events, and enhancing mutual understanding and future practice.

Both versions of this second interpretation of 'triangulation' treat it as a device for generating divergent interpretations, rather than for checking the validity of inferences from data. Moreover, in each case the research concerned was governed by a distinctive orientation. With Cicourel and his colleagues, the approach was one in which the focus of inquiry has been transformed from that which governs most social science: the concern is entirely with how accounts of social phenomena are constructed differently by different participants, in the belief that the social world is constituted in and through such accounting practices. How one evaluates this notion of triangulation depends upon an assessment of the sociological approach involved, either ethnomethodology in general or Cicourel's particular interpretation of it.[7] In the case of the Ford Teaching Project, this second interpretation of 'triangulation' is also linked to an approach that is very different from most social science, this time geared very closely to educational development. Here an assessment would have to take into account not just what knowledge about the social world is produced by this approach but also its value in educational terms.

Rather more recently, an argument very similar to that of Cicourel has been central to what has been referred to as the 'radical critique of interviewing' (Murphy et al 1998). For example, Silverman has claimed that 'counterposing different contexts [triangulation, in the first sense discussed above], ignores the context-bound and skilful character of social interaction and assumes that members are "cultural dopes", who need a sociologist to dispel their illusions […]' (Silverman 1993:158). The inference drawn here is that as researchers we should not be concerned with

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[6] This seems to be at odds with Garfinkel's (1967) account of meaning-in-social-interaction, since he treats asking for clarification when there is no interactionally obvious need for it as both socially disruptive and as endless (because there is no possibility of 'completing' the meaning of an event or action). See Heritage 1984.

[7] I have attempted a general assessment of ethnomethodology elsewhere, specifically in relation to conversation analysis: see Hammersley 2003a. It is worth noting that there is no need to reject the idea of a single reality in order to study how people put together accounts. All that is required in studying this important research topic is to suspend any concern with evaluating the validity of the accounts in order to understand how they were constructed, and perhaps also why they were constructed in the ways that they were. This need not be a competing orientation.

assessing the validity of the accounts that informants provide, but rather with analysing how they produce these accounts and what functions are served by them. This argument usefully emphasises the situated nature of all accounts, and offers an important caution against assuming that one source of data is always superior to others (whether a researcher's observations as against informants' own accounts, or the views of people in official positions versus the opinions of those at the bottom of the credibility hierarchy, or vice versa, and so on). However, there is little sign of most social scientists abandoning assessments of the validity of informants' accounts. More importantly, it is not clear why the fact that accounts occur in contexts, are skilfully produced, and may serve various functions should be taken to mean that they cannot be valid, or should be treated as expressions of multiple realities or of situational variation. The fact that informants' accounts can be analysed for the interpretative work they involve, and do, does not disqualify them as sources of information on which social scientists can draw (see Hammersley 2003b). And, contrary to what Silverman claims, following Garfinkel, using them in this way does not turn social scientists into 'ironists' any more than a concern with the validity of competing accounts in a court of law has this effect on jury members.

Triangulation as seeking complementary information

A third interpretation of 'triangulation' has been outlined by Erzberger and Kelle, amongst others, and is perhaps today the most common meaning of the term routinely employed by researchers. These authors comment that: 'the use of different methods to investigate a certain domain of social reality can be compared with the examination of a physical object from two different viewpoints or angles. Both viewpoints provide different pictures of this object that might not be useful to validate each other but that might yield a fuller and more complete picture of the phenomenon concerned if brought together'. And they add a further metaphor to clarify what they have in mind: '[…] Empirical research results obtained with different methods are like the pieces of a jigsaw puzzle that provide a full image of a certain object if put together in the correct way' (Erzberger and Kelle 2003:461).[8]

This interpretation of 'triangulation' taps into older discussions about the strengths and weaknesses of different research methods and the value of combining them (for example Zelditch 1962 and Sieber 1973), and about how complementary data can be derived from diverse informants (Dean et al 1967). In early responses to these arguments, questions were raised about whether data sources should be judged in terms of their 'fitness for purpose' or in terms of 'completeness' (Becker and Geer 1957; see also Trow 1957 and Becker and Geer 1958). Here, there is a parallel to the problem (in the case of the first version of triangulation) of identifying the biases characteristic of particular sources of data: how do we know which data sources will provide the most desirable kinds of complementary information? There are also problems with the idea, sometimes drawn on here, that we can have complete knowledge of a phenomenon.

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[8] For other versions of this notion of complementarity, drawing on the metaphors of a mosaic and of binocular vision see Becker 1970 and Gorard and Taylor 2004:44, respectively. The latter authors insist that triangulation can *only* be about providing complementary information not about validation, appealing to what they refer to as the 'true meaning' of the metaphor (p45).

It is also worth emphasising that using triangulation to produce complementary data and using it to serve validation are not incompatible. Indeed, further information about a phenomenon could lead us to change the category into which we originally placed it, on the grounds that it no longer looks like an X but appears to be a Y. Here, while the purpose for which the new data were collected was not validation, what has resulted is a correction of the initial interpretation that is analogous to what may occur in triangulation for checking validity. This reflects the fact that this third interpretation, like the first, assumes a single reality.

More recently, it has been argued that particular methods involve divergent assumptions about the very nature of the social world (ontology) and about how it can be understood (epistemology) (Blaikie 1991; Flick 1992). These arguments challenge not just the first interpretation of 'triangulation' but also this third one; and they point to some further interpretations of the term.

Triangulation as epistemological dialogue or juxtaposition

Flick has put forward a formulation that might, at first sight, seem to be an example of the third type of triangulation, just discussed. However, it suggests a significant new element. He writes: 'Triangulation was first conceptualized as a strategy for validating results obtained with the individual methods. The focus, however, has shifted increasingly toward further enriching and completing knowledge and towards transgressing the (always limited) epistemological potentials of the individual method' (Flick 1998:230; see also Flick 1992; Sale et al 2002). Flick argues that different methods do not simply provide varying kinds of information about the same object, but constitute the world in different ways.[9] And the shift he is reporting here has been associated with the growing influence of constructionism and postmodernism, with their focus on the way in which social phenomena are created in and through social interaction or discourse. Crucially, if we apply this idea to the research process itself, we are led to conclude that different methods construct the social world in divergent ways, so that combining them may not lead either to validation or to increasing the completeness of the picture.

This shift could imply, not so much a reinterpretation of 'triangulation', as an abandonment of it. The triangle is, after all, a modernist image, and drawing metaphors from technical occupations like navigation and surveying might be rejected for the same reason. More generally, if the data sources to be combined involve conflicting epistemological assumptions, then issues emerge about what 'combination' could mean, and whether it is legitimate. Perhaps research should

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[9] Much the same position is advocated by Fielding and Fielding 1986. It is worth noting that Denzin partially anticipated his critics in 1970: 'I have attempted to indicate that […] research methods represent different means of acting on the environment of the scientist. Surveys, for example, dictate a stance toward the invariant and stable features of this reality, while participant observation assumes a reality continually in change and flux. […] Each research method reveals peculiar elements of symbolic reality' (Denzin 1970:298). He goes on to use the metaphor of a kaleidoscope, but argues that: 'this is not to imply that reality has the shifting qualities of the colored prism, but that it too is an object that moves and that will not permit one interpretation to be stamped upon it' (pp298-9). This connects with his later championing of Richardson's metaphor of a crystal: Denzin and Lincoln 2005:6.

operate strictly within the confines of a single paradigm? Some qualitative researchers would advocate this (see for instance Lincoln 1990).

However, other responses are possible. It might be argued that we need to set up some form of dialogue between the epistemological positions built into various research methods, interpreting data from different sources with a view to resolving or transcending epistemological divides. A source here might be philosophical hermeneutics (see Warnke 1987). This is what we might call the dialogical strategy; and it is perhaps what Flick had in mind.

Alternatively, it could be argued that data produced by methods having different epistemological assumptions must simply be juxtaposed. For example Denzin and Lincoln adopt Richardson's argument that the model for 'mixed-genre texts in the postexperimental moment' should be a crystal not a triangle, they write: 'Like crystals, Eisenstein's montage, the jazz solo, or the pieces in a quilt, the mixed-genre text "combines symmetry and substance with an infinite variety of shapes, substances, transmutations….Crystals grow, change, alter….Crystals are prisms that reflect externalities and refract within themselves, creating different colors, patterns, arrays, casting off in different directions" (Richardson, 2000, p.934)' (Denzin and Lincoln 2005:6). This position perhaps reflects a refusal to choose among epistemological paradigms, or to let the reader do this easily. Instead, the goal is to put, and to keep, methods and epistemologies both in tension and in question, along with throwing doubt on any idea that one or other approach is correct, or that the differences between them can be overcome. We might call this 'postmodernist triangulation'.

In relation to these arguments, however, we should ask whether it is true that different sources of data, or even different methods, do involve conflicting ontological or epistemological assumptions. A number of writers assert this; for example, Blaikie (1991) identifies empiricism, interpretivism, and realism as fundamentally different philosophical orientations that underpin various social research methods. And he and other critics ascribe 'ignorance or misunderstanding' on the part of those who fail to 'recognise' the ontological and epistemological differences built into different methods, thereby perpetuating 'confused' claims (Blaikie 1991:126 and 128; see also Massey 1999:183). However, these authors do not effectively establish that different epistemological and ontological assumptions are necessarily built into the use of specific methods.

One of the problems with many discussions of triangulation is that distinctions are not drawn between combining data from different sources, using different methods, and integrating different methodological approaches.[10] And, in part, this reflects the fact that discussion of triangulation has been caught up in debates about the relationship between quantitative and qualitative research traditions, as well as in disputes among competing qualitative traditions.

The relationships between philosophy and method are much more complex and open to change than these discussions suggest (see Hammersley 1992:ch9 and

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[10] Indeed, there are problems about how we should differentiate among sources, among methods, and among approaches. The nature of the 'combination' or 'integration' is also often obscure: Bryman 2006; Moran-Ellis et al 2006.

1996). There are not just three philosophical positions that have been influential in social science. Moreover, particular philosophical ideas are almost always open to divergent interpretations, and are combined with others to shape the influence they have. For instance, positivism is by no means univocal (Halfpenny 1982; Hammersley 1995:ch1), and while a positivist conception of science has often encouraged the use of highly structured methods, it has not always done so. For example, in anthropology, it led Malinowski to rely on participant observation (see Leach 1957 and Strenski 1982) and the 'naturalism' that informed the thinking of some qualitative sociologists in the twentieth century drew on nineteenth-century positivism, with its inductivist notion of science (Hammersley and Atkinson 1995:ch1). Similar diversity is revealed if we examine the methodological implications that have been derived from phenomenology: this has encouraged both primary reliance on introspection and in-depth interviews and the restriction of data to transcripts of 'naturally occurring conversation' (see Frank 1979; Maso 2001; Heritage 1984).

In specific terms, we need to ask, for example: to what extent is it the case that in combining data from interviews with multiple witnesses - in order either to validate inferences about events they observed or to provide a more complete picture of those events – we are conflating divergent ontological or epistemological assumptions? While people may have different perspectives on the world, and we do of course need to take this into account, in practice they will rarely act on fundamentally discrepant assumptions about the nature of what exists in the relevant domain, or about how we can gain knowledge of it. Rather, what we generally find are sets of accounting practices that involve overlapping as well as discrepant assumptions, the degree of overlap and discrepancy varying considerably across cases.

Nor is any such difference in fundamental assumptions automatically involved when researchers combine data from different methods, even when we combine data from methods associated with qualitative and quantitative approaches, for example participant observation and structured observation. While one of these methods seeks to avoid making prior judgements about what is likely to be observed, whereas the other relies on previously defined categories, there is a continuum between the two. Furthermore, there is no fundamental epistemological or ontological discrepancy here. Both methods depend upon looking and listening. Both usually emphasise the need to make the data as concrete as possible, so as to minimise the inferences built into it. And while structured observation is often seen as reducing the danger of idiosyncratic reporting, the use audio- and video-recording in much contemporary qualitative research largely reflects the same concern. Of course, we could dress up these two methods as deriving from contrasting epistemological positions - a methodism which treats following procedures as the way to truth and an intuitionism that insists on the priority of researcher insight - but it is doubtful that use of these methods usually reflects commitment to these positions. Much the same point could be made about diverse kinds of interviewing.

If we take the difference between a life history interview and the administration of an attitude scale, while there are certainly differences in assumption here about the nature of attitudes and how they can be identified, it is not clear that these are either epistemological or ontological in any strict sense. To contrast attitudes as unique complexes of evaluations and orientations built up by a person over the course of life with the view that they are more specific in character and can be

categorised in terms of a standard typology is to outline different *psychological* theories rather than different philosophical positions.[11] While the boundary between psychological theory and philosophical viewpoint is not a clear-cut one, it is important nonetheless.

There is a tendency to assume that because, at a particular point in time, some philosophical ideas and research methods have been associated with one another this indicates a logical connection between them; whereas usually the connection is much looser and less stable. This is not to deny that particular epistemological and ontological assumptions can be taken to have significant methodological implications. For example, a commitment to standpoint epistemology implies an asymmetrical approach to understanding the accounts of informants from different social classes or genders. Similarly, some kinds of relativism or scepticism perhaps imply that research findings cannot or should not be assessed in terms of whether they correspond with reality, but must rather be judged in political, moral or aesthetic terms (Smith and Hodkinson 2005). But by no means all philosophical differences have major methodological implications, and it is rare for these to be entirely determinate in character.

In fact, it is hard to know how to interpret some claims about epistemological/ontological differences. The main failing that is ascribed to the first interpretation of 'triangulation' (though it also applies to the third) is that it assumes that there is one reality and that it is knowable. Some commentators deny these assumptions, on the grounds that people have different perspectives on the world, and that social researchers need to document these, and not simply judge those perspectives in terms of whether they correspond to reality (as determined by researchers). Yet this still involves describing a single world, albeit one in which there are multiple perspectives, and researchers are still claiming validity for their own accounts of those perspectives.

Conclusion

The case of triangulation illustrates how relatively straightforward practical research strategies can become caught up in the philosophical debates that now plague social inquiry. Checking other sources of information – both for the purposes of testing the validity of one's initial interpretation and to provide complementary information – is a routinely used practice in everyday life; and one that was incorporated into scholarly work in history and the human sciences long before the triangulation metaphor was developed.[12] Given this, we should hesitate to reject it on philosophical grounds.

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[11] This reflects wider slippage in the use of 'epistemological' and 'ontological' to refer to what would more appropriately be identified as methodological and theoretical differences.

[12] For example, writing in 1898, Langlois and Seignobos declare that: 'It is a principle common to all sciences of observation not to base a scientific conclusion on a single observation; the fact must have been corroborated by several independent observations before it is affirmed categorically. History, with its imperfect modes of acquiring information, has less right than any other science to claim exemption from this principle.' (Langlois and Seignobos 1898:196). It is clear that what the authors are referring to here is the first interpretation of 'triangulation' outlined above. Seale also notes that the idea of comparing data from different sources was common in the writings of Becker and others before this was explicitly labelled 'triangulation' within the qualitative tradition by Denzin (Seale 1999:55).

This is not to say that there are no problems with how 'triangulation' has often been interpreted. One reason for opposition to it is that it has been treated in some of the methodological literature as a validation *technique*. While Campbell's own position was fallibilist, there has long been a tendency within some parts of the methodological literature, especially that dealing with quantitative methods, to reduce the social research process to the application of techniques or the following of rules. On this interpretation, triangulation comes to be treated as a feature of research design that can be included in checklists designed to evaluate the quality of studies (Seale 1999:56). To some degree, qualitative researchers' criticisms of triangulation are a negative reaction to this technicism, an insistence on the interpretative judgment necessarily involved in the research process.

Yet, as we have seen, these criticisms often go well beyond challenging a technical orientation, apparently rejecting the idea that there is a single reality which it is the aim of social research to understand. Embedded in what I referred to as the postmodernist version of triangulation, for instance, is the belief that there are multiple realities or forms of life, and that research is itself necessarily implicated in these, able at best only to draw attention to their incommensurability. While this line of argument highlights some difficult philosophical problems, I am not convinced that these have much significance for the practice of social research. Indeed, it seems to me that deciding to engage in research of any kind necessarily assumes that there is a single reality and that aspects of it can be known. It is difficult to see what other distinctive goal inquiry could have than the production of knowledge, and in everyday usage 'knowledge' implies true understanding of something, where truth (though not relevance) is independent of perspective. While we must certainly recognise that there are variations among people and groups in what is taken to be true, and that all knowledge is fallible, we need not and should not reduce 'truth' to 'what is believed to be true'.[13] In fact, it has long been recognised that any sustained effort to use the concepts of truth or knowledge (and their synonyms) in this fashion ends in contradiction; and also that any attempt to avoid using those concepts fails. Moreover, these constitutive assumptions of inquiry, far from being restricted to one epistemological perspective, such as positivism, are shared by almost all of them, including those that have most shaped social research: for example, empiricism, Kantianism, Hegelianism, and pragmatism. The only philosophical positions that reject these assumptions are strong forms of relativism or scepticism; and there are few philosophers who have advocated these, or claimed that this can be done consistently.

If the aim of research is to produce knowledge of the social world, and specifically of the kind that most social scientists have traditionally pursued, then the most fruitful interpretations of the term 'triangulation' are the first and third ones distinguished above. Furthermore, as already noted, these interpretations are complementary rather than in competition. In other words, using data of different types can help us both to determine what interpretations of phenomena are more and less likely to be valid *and* to provide complementary information that illuminates different aspects of what we are studying. Triangulation of these sorts also helps us to

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[13] For a recent argument against this tendency from someone who previously was open to the accusation of encouraging it, see Habermas 2003:Introduction. And for challenges, from very different perspectives, to the idea that the work of Nietzsche, commonly appealed to in this context, provides a justification for this kind of perspectivism, see Clark 1990 and Sadler 1995.

recognise the limits to what any particular type of data can provide. Of course, even if we concentrate on these two forms of triangulation, there are still important questions to be asked about what 'combining' data from different sources means, and how we should go about it. In relation to 'triangulation-as-validity-checking', these concern how we should seek to identify predominant threats to validity associated with various data sources, and how far we should pursue the process of triangulation. In relation to triangulation-as-seeking-complementary-information, one problem is how we should decide what additional information is and is not relevant to our study. Fortunately, though, these are questions that, in themselves, do not raise fundamental philosophical problems. It *is* important to remember, though, that these forms of triangulation are investigative strategies that offer evidence to inform judgments, not techniques that provide guaranteed truth or completeness.

Let me turn, finally, to the question of the role of triangulation in so-called mixed methods research. The position I have taken on the nature and value of triangulation here is similar to that of many advocates of this approach. However, I want to caution against conflating triangulation with the combining of quantitative and qualitative approaches. One obvious reason not to do this is that, as I emphasised earlier, triangulation may involve using different qualitative sources of data, or various quantitative methods, rather than crossing the divide between the two. A more fundamental concern, however, is that the very notion of mixed methods research preserves the quantitative-qualitative division even while seeking to bridge it.

The problem is not that this distinction does not refer to significant variation in how researchers go about their work. To the contrary, the problem is that it refers to very many sorts of variation. And just as it is best to see triangulation operating at a more micro level than the combining of different broad approaches - for example using data from interviews with different people or observations in different settings, combining different forms of interviewing or observation, and so on – so too it is better to see the differences between qualitative and quantitative as operating at a more specific level. The qualitative-quantitative distinction can refer to variation in at least the following: the specification and development of research problems (a more 'inductive' versus a more hypothetico-deductive approach), the planning of research (more emphasis on initial or on recurrent planning), the collection of data (more versus less structured approaches), data analysis (use of counting, tables, statistical techniques versus reliance on qualitative and discourse analysis), and writing research reports (standard format versus a flexible format depending upon what is being reported, what audience is being addressed, etc). Now, the point is that there is no automatic link between most of the choices made about each of these aspects of the research process and the choices that are made about others. It is possible to combine a relatively inductive approach with using quantitative data; to collect unstructured data and then turn it into quantitative form; to report qualitative research in terms of the standard format, and so on. The spirit of much advocacy of mixed methods research, which I applaud, is to undermine the tendency to assume that there are impermeable boundaries between the quantitative and the qualitative. The danger of such advocacy is that it nevertheless treats the distinction between quantitative and qualitative methods as if it were more uniform, stable, and meaningful than it is.

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