Triangulation in Social Research

Triangulation in Social Research: Qualitative and Quantitative Methods Can Really
Be Mixed

By Wendy Olsen

Abstract

For those who teach methodology within social science departments, notably
sociology, the mixing of quantitative and qualitative methods presents an ongoing
problem. Recent developments in the philosophy of science have argued that the two
traditions should not have a separate-but-equal status, and should instead interact. By
reviewing three positions about this issue ('empiricist', constructionist, and realist) the
chapter offers a review of the sociological approach now known as triangulation.
The chapter refers to several empirical examples that illustrate the realist position and
its strengths. The conclusion of the chapter is a more abstract review of the debate
over pluralism in methodology. Triangulation, I argue, is not aimed merely at
validation but at deepening and widening one's understanding. As a research aim, this
one can be achieved either by a person or by a research team or group. Triangulation
and pluralism both tend to support interdisciplinary research rather than a strongly
bounded discipline of sociology.

Biographical Note

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Wendy Olsen grew up in Indiana and moved at age 18 to Beloit College in Wisconsin,
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Triangulation in Social Research: Qualitative and Quantitative Methods Can Really Be Mixed

1. *Triangulation*

In social science triangulation is defined as the mixing of data or methods so that diverse viewpoints or standpoints cast light upon a topic. The mixing of data types, known as data triangulation, is often thought to help in validating the claims that might arise from an initial pilot study. The mixing of methodologies, e.g. mixing the use of survey data with interviews, is a more profound form of triangulation.

Denzin wrote a justification for triangulation in 1970 and is credited by some with initiating the move toward integrated research that mixes methods (Denzin, 1970, 1979, 1989; see Flick's review, 1992). However other authors in other contexts have used 'mixed methods research' both before and after Denzin's summary was written. For instance, Lenin used a mixture of quantitative data tables along with a political-economy analysis of charged words used in his classic research monograph, *The Development of Capitalism in Russia* (1898). Lenin, following Marx's 1840s-1860s writings, was a critic of bourgeois political economy and wanted to un-pick the discourse of capitalism in order to be a critic of the capitalist system. We would today say that his work used methodological triangulation of discourse analysis (a qualitative methodology), and survey data (a quantitative methodology), to study the end of the Russian peasantry and the early beginnings of working class conflict with employers in Russia.

For students today, then, it is important not to locate triangulation too late in the growth of sociological thinking. The discipline as it now exists has developed through a thriving interaction of quantitative and qualitative analysis. Textbooks help by providing a review of changing methodologies (Hughes et al. 2003). The main argument of this chapter is that there should not be a contradiction between these two modes of analysis, but rather that it should be possible to bring them together to shed light on any chosen social research topic.

The chapter will begin with some general points about how the philosophy of science has been moving toward this particular integration of methods, and how in the 1980s it was more common to perceive an antipathy of qualitative research to quantitative research. The more one perceives and teaches this kind of antipathy the worse the chasm gets. This antipathy is called an epistemological chasm, following work by Walby (2001) who noted that such chasms are usual between disciplines but that they are hard to justify philosophically. In the middle of the chapter I provide some research exemplars which bridge the chasm and succeed in integrating qualitative insights with the interpretation of quantitative survey data.

The chapter concludes with a rejection of pure constructionism, which is the idea that all social objects are merely socially constructed. This idea is rejected only in a guarded sense. Constructionism itself appears too simplistic to serve as a methodology in itself (as did positivism, and as does empiricism; see Fay, 1996). A realist alternative has been offered (Sayer, 1992). Realism argues that social objects are often affected by the way they are construed, but that they also have an ongoing real existence that is not constituted entirely by how today's researchers construe

them (Sayer, 2000). Whilst ultimately changeable, the 'real' has characteristics which are, in part, unresponsive to how we know about them. Realism is plural with respect to methodologies and with respect to theories, and therefore offers a good platform from which to embark on integrated mixed-methods research. This conclusion is consistent with the leading sociologists in this area such as Bryman, Sayer, Dex, and others who are cited in the chapter.

After working as a socio-economist using a wide range of methods and methodologies, my conclusion is that triangulation is something we do in order to generate a dialectic of learning. Triangulation means mixing approaches to get two or three viewpoints upon the things being studied. The resulting dialectic of learning thrives on the contrasts between what seems self-evident in interviews, what seems to underlie the lay discourses, what appears to be generally true in surveys, and what differences arise when comparing all these with official interpretations of the same thing. Valuing academic research careers is a way of valuing the ongoing dialectic of learning, and triangulation plays an important role in good social research. In making this argument I tend to undermine the separation of quantitative and qualitative methods teaching in colleges and universities. Instead, one might argue, it is valuable to teach *research design skills* and interpretive skills, which can be used with regard to a wide range of data types. Indeed, like some other critics of quantitative methods, I would argue that far too much time is spent by statisticians on statistics, when it would be interesting for them to spend more time on developing alternative interpretations (and triangulated re-interpretations) of the data they have.

Perhaps a fetishism of quantitative methods is the problem which now needs to be addressed in social science. Fifty years ago and thirty years ago it was positivism which was under attack (e.g. (Cicourel 1964); see (Crotty 1998) for a summary of changes over time). That attack has been very successful in making positivism out-dated, but these days there is still an empiricist mathematics-fetishism that can be very off-putting to qualitative researchers. For instance, if we look at the handbooks by Goldstein or by Snijders and Bosker on multi-level statistical modelling (Goldstein 2003; Snijders and Bosker 1999), we find letters of the greek alphabet on nearly every page. The maths is so dense as to be off-putting. Byrne has developed a critique of the multi-level modelling school which recognises the advantages of the technique but questions its entirely statistical methodology (Byrne 1998). Byrne notes that multi-level statistics are at present dependent upon the quintessential 'survey method' (Marsh, 1979, 1982). The survey method is defined by having its cases all comparable in the rows of a data table, and its variables all consistently measured in the columns. Byrne argues that we should focus on the rows of the survey data rather than the columns, ie researchers should look at the cases (e.g. individuals) and their histories – and context, e.g. their households, their culture, their city – rather than attempt to make universalistic statements about the variables (e.g. class, which is a variable in a column). The traditional column-wise approach, which looks at variables, tends to lead to overly generalised interpretations (Ragin, 1987). Using variables, say the critics of statistics, stops us from looking more closely at the complex, differentiated underlying social reality (Cilliers 1998). Let us look more closely at survey data and see whether it is correct to separate it so completely from qualitative methods.

2. *Survey Research and Triangulation*

The position taken by realists in the above debate is that methodological pluralism is an excellent starting point for empirical research. Methodological pluralism has been described in several works (e.g. (Carter 2003; Danermark 2002; Sayer 2000)). It refers to a pluralism of method that enables the researcher to use different techniques to get access to different facets of the same social phenomenon. For social class studies, for instance, a methodological pluralist would examine qualitative data on how it feels to be working-class; quantitative data on the flows of resources between classes or on their asset base; and policy documents in order to see how policies interact with or define social classes. The classic debates between Marshall, Goldthorpe, Skeggs, which have been reviewed in several works (Crompton 1998; Marshall 1989; Skeggs 1997) illustrate the methodological pluralist position. Each of these named authors takes the view that both qualitative and tabular data may be of use. Skeggs focuses more on ethnographic and interview data, whilst Goldthorpe concentrated more on quantitative data, but they recognise the value of the other type of data, too. This sort of pluralism is tolerant of the methodological choices of other researchers because it recognises that class is a multi-faceted phenomenon with personal and private aspects as well as publicly recorded aspects.

However, methodological pluralism is not evident in all the textbooks on methodology. For instance, texts by Sarantakos and Silverman suggest that there is always a clear distinction between the two main methodologies, and that these two methodologies are incompatible (Sarantakos 1993; Silverman 1993). The quantitative methods are often taught separately and distinctly from the qualitative methods (see for instance texts of purely qualitative methods (Alvesson 2000; Dey 1993; Holstein and Gubrium 1995; Kvale 1996; Miles and Huberman 1994; Puri 1996; Sapsford and Abbott 1992). Texts containing purely quantitative methods are too numerous even to list but see for instance how the publishers Sage keep their quantitative reference volumes separate from the qualitative reference volumes e.g. (Aldrich and Nelson 1984; Hardy 1993).

In this section two positions are contrasted. The first, traditional, position is that there is an epistemological chasm between quantitative and qualitative research. This view was put very firmly by Silverman (e.g. 1993: 94).

Figure 1: The Supposed Chasm Between Quantitative and Qualitative Research

Two Versions of the Interview Relationship

	Positivism	**Interactionism**
Interviewer	Object - following research protocol	Subject - creating interview context
Interviewee	Object - revealing items relevant to the research protocol	Subject - complying with or resisting definition of the situation

Source: Silverman (1993: 94)

Silverman argues that the advantage of qualitative research is that it recognise the inherently subjective nature of social relationships. People construe others' behaviour through their own subjective lens of perception, and the others' behaviour, too, is framed within their own subjective and discursive frame of reference. The act of interviewing, Silverman says, is a meeting of two subjectivities. A similar point was made by Gubrium and Holstein in a convincing rejection of the objective nature or un-biasedness of interview data (Holstein and Gubrium 1995). Similarly, Harre argues that since the knower is embedded in the social scene of the interview, they are not independent of the respondent's responses (Harre 1998). The impossibility of objectivity, for these authors, implies that subjectivity must be acceptable and must be understood in depth. Silverman's work is possibly too individualistic to allow properly for the social nature of human subjectivity, but apart from that he makes sound points about qualitative research. Silverman values the subjective aspects of social life. His table makes them seem to be poles apart from the objective or material aspects. Such a strong polar dualism is questioned by realists (e.g. Sayer, 1992, ch. 1). The claim of a dualism of object and subject caused Silverman to appear to reject statistical research completely.

Silverman was wrong is equating the techniques of quantitative analysis with the epistemology of positivism. Later in the same book he argues that 'positivism' routinely uses facts, random samples, standardised questions, and tabulations. He contrasts this positivist methodology with qualitative interactionism (his word), which uses 'authentic experiences', unstructured interviews, and open-ended questioning to gain knowledge. It seems that Silverman's position is that qualitatively obtained knowledge is more valuable than that obtained through quantitative data. However this dualism is far too simplistic. It merges positivism with empiricism; it merges empiricist epistemology (notions of fact and of impersonal information) with the techniques of tabulation. It is possible to take a more constructionist approach to tabular data (Bowker and Star, 2000). For instance one could argue that in so far as the respondents and the interviewers interpret the question to mean certain things, which are not necessarily identical, the pattern which emerges is X and a proper interpretation with due regard to theory is Z. This mode of analysis is not much different from qualitative analysis itself. We code the interview, we seek patterns, we summarise these in a one-page diagram or summary table, and we develop a line of argument which is rooted in the data and can be grounded in those data (Miles and Huberman 1984; Miles and Huberman 1994). Silverman rules out the use of mixed or tabular data, as well as survey data, but this is an unnecessarily restrictive methodological position.

Bryman explored this dilemma in a thoughtful paper (Bryman 1998), pointing out that it is important in social science for us to avoid epistemological incoherence between our use of the two main types of data. If the theory of knowledge being employed perceives only one type of data as valid, then it would be incoherent to employ two types of data. Bryman's approach to mixed-methods research has been to suggest that for practical reasons one type of technique will usually be primary, but that all research is enriched by the addition of other, very different, techniques to the tool-basket (Bryman 2001). Bryman was an active proponent of the view that quantitative methods always rest upon a qualitative conceptual framework ever since he published his path-breaking book *Quantity and Quality in Social Research* (Bryman 1996 (orig. 1988)). His initiative in that book was based upon emergent findings shared by other

critical social scientists and published earlier than 1988. For instance, Fay (1975) argued similarly that empiricist sociology was a dead end (see also Fay 1987 and (Fay 1996). Mills (1959) argued that abstracted empiricism was one of the most awful parts of sociology in his time. Mills' notion of abstracted empiricism encased two habits at the same time – empiricism, ie a recourse to facts rather than to complex reality; and idealism, ie a focus on models of society which gave such a strong conceptual twist to research that the lay models of actors could be ignored. Choice models in labour markets, which see the operation of labour markets in terms of the choices made by individuals (Breen and Garcia-Penalosa 2002), and mathematical models of social class (e.g. Roemer 1982; Roemer 2001) perhaps illustrate what Mills and Fay were criticizing. Bryman's view was that quantitative analysis needed to respond to what was happening in the qualitative social research tradition.

The alternative position to Silverman's would integrate methods. Sayer presented this position in detail, but he also argued (on pragmatic grounds) that a single piece of research would always have to make a choice: either to be intensive (examining the topic in great depth) and qualitative OR to be extensive (examining a wide range of data) and hence quantitative. Sayer's view ((Sayer 1992 (orig. 1984)) is similar to that of Lawson who encourages descriptive statistics but nothing more of a quantitative kind (Lawson 1996a; Lawson 1996b; Lawson 1999). Similarly, many feminist economists have rejected social statistics for the reason that its apparent objectivity and its impersonal nature make it too authoritative and subject to the danger of oversimplification and untested universalism (Figart 1997; Nelson 1995; Ribbens and Edwards 1998; Truman et al. 1999). This viewpoint ignores the possibility that the interpretation of social statistics from a more thoughtful, critical and reflexive standpoint could reach a more acceptable version of objectivity than does the idea that statistics are facts that speak for themselves (Harding 1995; Harding 2001). The debate rages on, with Walby arguing the case against epistemological chasms (Walby 2001) and others arguing the case against statistics as if it were the same as the case against empiricism (Stanley and Wise 1993).

A classic statement of the empiricist viewpoint can be found in Nachmias & Frankfurt-Nachmias' book on research methodology. Figure 2 quotes from this empiricist source.

Figure 2: The Supposed Value Neutrality of Science

> **"Characteristics of Research Hypotheses:**
> - Hypotheses must be clear...
> - Hypotheses are specific...
> - Hypotheses are testable with available methods...
> - Scientific hypotheses are value-free."
> Source: From Frankfort-Nachmias and Nachmias
> (1996: 65).

The qualitative researcher or theorist who is horrified by these claims is the same one who, perhaps implicitly, has begun to question the 'scientific method' itself. The scientific method refers to the hypothetico-deductive method (for reviews, see (Kuhn 1970; Lacey 1999)). Hypotheses derived from good theory are tested. They are then either rejected or, if not falsified, retained for further investigation. Following the

works of Popper's mid-life output (e.g. (Popper 1963)), the hypothesis testing doesn't prove the claims, but it allows researchers to get on with their justified and non-rejected belief in their theories.

The interesting thing about Frankfort-Nachmias' statement is that it is meant to pertain to the hypotheses tested by qualitative researchers, as well. Therefore it assumes that the methodology and epistemology of qualitative research is very similar to the scientific method. This assumption is abhorrent to most qualitative researchers, and is not correct. The tendency however is for epistemologies (ie notions of what makes research findings valid) to be imperialistic and for their proponents to assume that their own epistemological foundations can and should be applied to other researchers' knowledge claims.

The problems with the scientific method are several. For instance, how do we know the theory is good enough to be used to generate the hypotheses? How do we know that deduction from one theory is applicable to a new region/time? If we are planning to learn from new data, but assume that the theory is true (or at least good enough) then are we really able to falsify the *theory*? We have disallowed its falsification before even starting the test. We test the hypothesis within the discursive terms set by the given theory. The discursive *a priori* is too strong to allow in depth testing.

Similarly, we can challenge the notion of value-free hypotheses. This challenge has been laid out carefully by Sayer (1992 (orig. 1984), pages 36-45). Sayer's comments on this question appear dotted throughout the book and I provide here a summary of his very important and influential argument. First, he says, everyday talk is not as coherent as a discourse. Texts such as dialogues are often deeply contradictory. Hermeneutics, the study of interpretation and meaning, is often confused with people feeling empathy for others. *Verstehen* (understanding meanings) is universal. It is common to all knowledge. Yet *verstehen* is not the same thing in all instances. How we understand or "take" a person's talk varies depending on the context. To understand something does not mean that you agree with it. People often assume it does. Hence empathy is the wrong word.

Sayer used the example of what it meant to understand *apartheid*. Groups of people agree on meanings. Misunderstandings occur, but we have to study them too, in order to fully grasp meanings. So understanding (*verstehen*) is often imperfect and we had better admit it. The social scientist can have a special ability to understand social and personal meanings. This area makes social science different from natural or physical science. Social science sometimes has to be critical of common sense. Therefore social science must have a critical relationship with its target. Be prepared to question what people tell you or give you as evidence. The "criticising" of apartheid, for example, doesn't refer to disapproving, but questioning the points that were claimed to be factual by the regime. Some may be false.

Criticism, says Sayer, would also encompass offering critiques of social practices, since practices, like knowledge claims, may have a false basis. Social scientists who claim to be value-free are not being honest, because all researchers exhibit at least restricted forms of evaluation – selection of the topic, group, and acceptable forms of research, given one's discipline, for example. Social objects are concept-dependent so it makes sense to criticise them. Social science therefore has a social role. It is a

process involving people. To pretend otherwise sometimes causes reification - making real that which is a mental construct, e.g. social capital or social class.

Sayer argues that many social scientists still don't realise how vast the effects are of concept-dependence. For instance, if we conceive of grandmothers' caring work as inherently loving work, and if we consider all loving to be best done unpaid, then we cannot conceive of grandmothers' babysitting as *work per se*. Research articles contribute to the social production of social objects' characteristics. Science will therefore often challenge common sense. Knowledge is embedded and developed within social practices. Researchers do not exist outside of the phenomena being researched. We are part of what we're studying. Sayer says that objectivity in social science is a false, unattainable aim. He calls scientists "naïve objectivists" if they advocate objectivity – which is impossible.

We can use a diagram to indicate the three polar positions that researchers may have to choose from. In Figure 3 the realist position is recommended, but sociologists can still draw upon the other two traditions (constructionism and empiricism) for selected insights and techniques. In the realist view, constructionism and empiricism are not adequate theories of what exists, but as bundles of techniques they can be utilized in a pragmatic and well-grounded way. Both theory and practical experience will help you to work out which techniques are going to be useful in a given context, e.g. discourse analysis from the constructivist tradition and tabulated matrices based upon qualitative categories, perhaps, from the empiricist tradition.

Figure 3: Drawing Upon Three Traditions for Triangulated Research
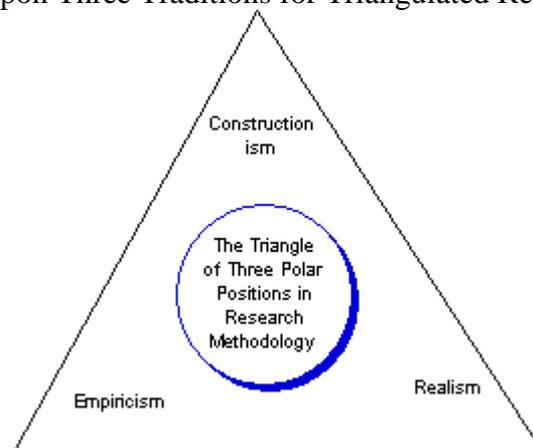
Sayer's argument provides a solid foundation for the integration of quantitative and qualitative method. Whilst this chapter is too short for a full exposition and defense of such an argument (see Olsen and Morgan, 2004, for details), the next sections will give some illustrative examples of workable triangulated strategies for research.

Section 3. *Integrating qualitative and quantitative approaches*

According to Bryman the combination of different methodologies will generally tend to have a leading strategy for starting out the research, and a follow-up strategy for rounding out and widening the enquiry (Bryman 2001). The sequencing of the primary and secondary strategies has also been central to the pragmatic approach taken by Kanbur (2002). Triangulated research may runs the risk of taking on too

many unfocused questions all at once unless it has sequencing and a sense of which technique is primary. Let us take two of the possibilities he sets out in his useful introductory texts, chs. 20-21. Notice that the same strategies can be found in other texts, e.g. (Creswell 2002; Kent 2001; Punch 1998; Robson, 2002; Sapsford and Abbott 1992). The first possibility is that survey methodology is used only after an in-depth qualitative enquiry. Three points must be made about this type of research design.

Firstly, all questionnaire surveys are inherently based upon an in-depth qualitative enquiry, known as the pilot survey (Blaikie 2000; De Vaus 1991; Harvey 1991; Layder 1993; Marsh 1979; Marsh 1982; Marsh 1988). Secondly, all questionnaire surveys are set up after a period of examining the relevant literature, ie conducting a literature review. This is true whether the survey is officially managed (Marsh 1982; (Bulmer and Warwick 1993; Marsh 1979; Martin et al. 1984) or privately run by a consultancy or academic research group. The literature review stage may perhaps be considered a qualitative technique, involving conceptual work and the analysis of meaning, prior to setting up the collection of further data. This was Bryman's view (1996, orig. 1988). Thirdly, survey data can and always do include categorical and qualitative data (Menard 1995; Mikkelsen 1995) which have separate 'nominal' categories. Survey data can thus interact with case-studies of individuals or life-histories of households, giving a rounded view of a limited number of cases alongside an extensive view of a wide range of cases.

The exemplars listed in the next section give several examples of innovative questionnaire surveys which arose only after a qualitative stage of research.

More problematic is the case where one first conducts secondary research, using survey data, and follows this up with qualitative research. This possibility (sequencing quantitative then qualitative; priority on the quantitative research) would require that the researcher(s) have both sets of skills, and that they actively seek out qualitative research questions from the first stage of the research. An example would be if one sought reasons for the patterns found in the survey, e.g. a pattern of labour-force participation rising with age or being in an inverted U-curve with wealth, ie. participation is highest among the middle income households, but lowest among the very poor and very rich. The 'cause' is not simply age, or wealth. Instead, the pattern gives us reason to wish to explore further the real causes. Variables are not the same as causes, and I have argued elsewhere that to conflate variables with real causal mechanisms is to over-simplify the real world underlying our very crude data-collection and data-creation activities (Olsen 2003a; Olsen 2003c; Olsen 2003d). This viewpoint is shared by all the realist researchers whom I have already cited, notably being argued by authors who advocate integrated research methods (Blaikie 2000; Blaikie 1993; Devereux and Hoddinott 1993; Dow 2002; Lawson 2003).

These integrated research designs imply methodological pluralism. We now have a position in which the empiricism of which statisticians have been accused is purely optional. Empiricism is an epistemology in which facts are thought to 'speak for themselves'. Its difficulty is that empiricists assume the world consists of 'facts'; this is unrealistic. The real world is very complex. The data we record about reality is only a rough, partial, and incomplete record. The critics of empiricism note explicitly that some things are difficult to observe. Empiricism and realism stand poles apart.

As Sayer described (see earlier summary), naïve realism is not the form of realism to which social scientists should refer. In criticizing realism, naïve realism is frequently confused with scientific realism. For instance, a critique by Baert argued that realism cannot allow for social constructionism (Baert 1996). Baert misunderstood what other authors meant by realism.

By way of contrast, though, what realists mean to propose is not a naïve objectivist exploration using a foundationalist ontology, but an expansion of the range of activities of social scientists beyond induction and deduction. Induction, developing theories though examining data, it turns out, is unable to provide a solid basis for true statements. Instead its truths rest upon our acceptance *a priori* that the measurement or record-keeping methods are acceptable. Deduction, in which hypotheses derived from theories are tested against empirical data, also has its weaknesses, as pointed out earlier in criticizing the scientific method of hypothesis-testing. Realists have advocated (a) retroduction, and (b) abduction as the logic of analysis arising from/with/after data creation processes. (Retroduction means working out what might have caused the observations we have in our data; and abduction means grasping the inner meaning of a phenomenon.) Realists argue that many 'things' in the social world are unobservable. Relationships, power, status, etc. have this inherent quality. However, we make observations, record them as empirical data, and try to work out what the world *must be like* or what it may be like, such that our claims are consistent with the recorded data. The logic of retroduction, as described here, is common to both the quantitative and qualitative data-analysis activities.

Abduction, which can be translated as kidnapping (abducting) the subject, refers to something rather different. Abduction refers to the phenomenological attempt to get inside the thing which is being researched. Understanding, when abduction is being done, grows in pre-discursive ways. Understanding is later integrated into verbal descriptions that are rich, rooted in the locality and in the lay discourses, and phenomenologically accurate although perhaps not always easy to translate back into scientific or theoretical discourse. One can see that studying the experience of disability, the experience of sexual pleasure, or the state of being gay probably all require abductive research techniques. I will not treat abduction further here but can recommend the work of other authors in this area (Blaikie 1993; Danermark 2002; Outhwaite 1987). Abduction is perhaps the mode of analysis least well suited to any integration with quantitative or survey-data based methods. However, abduction in itself disallows communication with other researchers, and does not provide access to general social structural features of society or to those emergent features of society which are not personally experienced but rather which arise at higher levels, e.g. globalisation, the spread of a discourse, the rise of a wave of riots, and other macro topics. Abduction about one thing can usefully be combined with other techniques suited to the study of the macro and meso, ie large-scale and intermediate-scale, phenomena.

Section 4. *Exemplars of Mixed-Methods Research*

Mixed methods research has been used by research teams, as well as individuals, in many research areas. Illustrations from sociology include the study of unpaid caring work and of consumption. Illustrations from socio-economics include the study of flexible working on non-standard work contracts. Since the first two topics illustrate

sociology in its narrow interpretation, and the latter area treads on the fields of political economy and hence reflects the wider range of studies which can today count as 'sociology', I will use these three areas to illustrate triangulated research. Of course when using mixed-methods research one may not respect the usual boundaries of disciplines, but that is a somewhat different issue (Harriss 2002; Kanbur 2002; Olsen 2003b).

a) Grandmothering and Other Unpaid Care Work.

Two classic studies of unpaid caring work among women in the USA, France and other countries used a mixture of tabulated data and qualitative data (Delphy and Leonard 1992; Folbre 1994). The first of these studies suffers from a tendency to verify its own hypotheses, which is a danger of methodologies that avoid falsification practices. In other words, by seeking to find evidence of exploitation within families, notably exploitation of unpaid working women, Delphy and Leonard tended to verify their a priori hypothesis. They were not sufficiently sceptical of their initial thoughts. However as a comparative study of how families negotiate self-employment and farming work, the study is path-breaking. Folbre's study is more about public policy and it uses a comparative method as well as detailed case study material. Folbre showed that women are constrained by policy and by social norms from engaging in labour markets on the same terms as men. Hence, she argues, they tend to come to terms with their own socially-imposed limitations and to internalise those limitations. Combined with other studies of housewifisation ((Mellor 2001; Mellor 2003; Mies 1998), the Folbre study offers a way out of the reification of 'women liking their housewife role'. However, it must be admitted that Folbre, Mies, Mellor and Delphy and Leonard all share an *a priori* structuralism that would have difficulties disproving its own presuppositions. In other words, relations between genders are structured, either by patriarchy or by the gender regime (depending on which work we refer to) and these structures are taken for granted by these authors.

Recent research in the UK in this area includes a qualitative study by Wheelock and Jones (2002) which argues that grandparenting is a newly growing form of unpaid caring labour. Wheelock and Jones appear to be less sexist than other feminists, in the sense that she presupposes that men, as well as women, may work unpaid as grandparents. The approach taken by Whatmore (1990) makes explicit the dialectical, changing nature of gender structures which are the subject of these feminists' research (Waldfogel et al. 1998; Westwood 1984; Whatmore 1990; Wheelock and Jones 2002) Since structures are changing, one can assume they *may* exist whilst empirically examining their actual effects as evidenced in recorded data. As work by Waldfogel, by Warren, and by Fagan illustrates, the resulting research methodology can easily integrate quantitative data (especially survey data over time) with qualitative research data (Waldfogel et al., 1998; Fagan 2003; Fagan and Rubery 1996; Warren 2000). The issue of reification, ie of making structures appear real which were merely assumed to exist *a priori*, has been more explicit in anthropology than in sociology but now offers an area for urgent exploration (e.g. see Warner, 1993; Webster, 1996; and Yeung, 1997 for discussions).

In summary, the area of unpaid caring work has been a rich ground for mixed-method research. Some new issues have arisen for methodological debate.

b) Eating Out and Consumption Behaviour

A major preoccupation of the literature on consumption is whether consumer habits are determined by individual assets and resources, or whether instead people shape their identity rather freely using displays and expenditures to mark their social status. According to the first view many people are constrained to limit their consumer habits (and social class is a major correlate, though not completely a determinant of consumption patterns (Edgell et al. 1996; Warde 1997; Warde 2000; Warde 1999). According to the second view the post-modern consumer is able to differentiate themselves from others, or to constitute themselves as part of sub-cultures or trends or movements, rather freely (Featherstone 1991). A research programme funded by the Economic and Social Research Council (ESRC) on cultures of consumption includes a range of projects which each fall somewhere between these two extremes. (http://www.consume.bbk.ac.uk/research.html).

The research on consumption has convincingly shown that both social class and region are strongly associated with large differences in levels and styles of consumption. For instance, eating out in London takes different forms than in the north, and people position themselves differently toward eating out in different parts of the country (Olsen et al. 1998; Warde 1999). Lifestyles show strong class differentiation, which is arguably caused by the limited income levels of households in the working classes (Tomlinson 2003). A full discussion of the class effect using a wide array of secondary data, primary survey data and qualitative data for England can be found in (Warde 2000) and a theoretical and empirical review in (Warde 1997).

Since such studies routinely use triangulation and find survey data invaluable for unpicking complex interacting causal mechanisms, several of the empirical researchers on consumption have shown an explicit awareness of epistemological issues. For instance, De Vault - author of a classic study of notions of propriety in Chicago residents eating habits (DeVault 1991) has published a review of the data interpretation and reflexivity issues that arise (DeVault 1999).
Research on consumption illustrates the triangulation debate since it has an economistic, individualistic, rather simplified strand which studies expenditure data (Burton et al. 1994; Sefton and Veld 1999) as a well as a rich strand of triangulated studies which have a more dialectical view of changing practices, referred to earlier. The triangulated studies have a pluralist approach to theory, allowing for individualist consumer theories which have constrained people making choices, under constraints, as well as for sociological theories of the construction of the consumer and the identity-formation of the socialised individual. Validation and falsification activities are typical of the first strand, whilst theoretically informed and ontically complex interpretations are typical of the second. A theoretically informed interpretation is one which explicitly asserts that an appropriate set of theoretically derived categories (names of things; verbs representing what they do; named relationships and institutions) can be used in the interpretation. Ontically complex interpretations refer to a set of 'things', each one different, rather than being limited to one type of actor such as the individual. In economics, for instance, the orthodox approach treats firms as if they were individuals and is thus ontologically simplified, but the latest innovative approaches has people, firms, social institutions, and social structures all interacting. The latter is ontologically complex (for a review, see Layder, 1998).

The sociological school seems more well founded. As described by Flick (1992), there are both structural aspects and subjective meanings to consumption (page 187). Whilst triangulation may lead to convergent perspectives, it is also possible for different data sources to lead us interpreters toward divergent perspectives (Flick, 1992: page 189). The role of the sociologist is to some extent a meta-role. The sociologist analyses data, but also exposes and comments on the competing perspectives, ie how others might interpret the same data, and why those interpretations are not as strong or good as one's own analysis.

c) Flexible Labouring

A recent flowering of studies on the employment experiences of flexible workers also illustrates triangulation. Dex and McCulloch (1997) laid some groundwork for studies of flexible labour by using secondary data to assess its frequency and specific modalities. For instance, most largescale data sets reduce piecework contracts to their equivalent hourly wage-rate. A cottage garment worker, who has a contract for 2000 items at £x, would report their hours worked and thus an equivalent hourly wage could be (falsely) applied to the work. Dex's approach might be to create new variables and categories that adequately represent piecework in its own terms. Piecework then becomes noticeable and is no longer comparable with waged labour nor will it be capable of being averaged into the person's monthly average wage rate. If every flexible-labour contract were represented in detail, we would have at least eight categories: fixed-term contracts, labourers' piecework, seasonal work, temporary work, self-employed people's work done after a 'quote', and self-employment itself, as well as waged labour and salaried jobs.

Dex et al (2000) presented a study based on an eight-wave panel study of 436 TV production workers in Britain 1994-97. This particular study, which has also been reported on by Paterson (Paterson 2001a; Paterson 2001b), and for which background is provided by Paterson (1990, 1993) and Hood, ed. (1994), offers an integrated mixture of qualitative and quantitative data. Other studies such as (Fagan 2003) use a range of types of data, including qualitative data, policy documents, and secondary data.

Dex *et al.* analyse the qualitative commentaries made by the respondents (Dex et al. 2000). In other words, although the questionnaire method was used, there was open space for comments from respondents, giving a triangulated flavour to the data-collection method in this panel study. The themes of workers' risk-taking, uncertainty, networking and job exit repeatedly arose in this study. The research indicates clearly that a multi-perspective triangulation illuminates aspects of work that have been hidden in standard survey-based studies. Anthropological methods offer another source of insight about flexible working, including short-term contracts and insecure tenure of jobs. Research on Mexican women working in the manufacturing sector illustrates the ways in which employers keep open the option of shedding staff, whilst workers have to construct their identity in sexist ways (Salzinger 2002). Such research is complemented deliberately by researchers using the questionnaire method (Fussell 2000). In this case both strands of literature argue the same conclusion: that women are coerced and persuaded into yielding themselves up as flexible workers in order to maintain their relationship with the employer. Qualitative research throws

light on aspects of the workers' relationship with the employer and other potential employers, whereas standard employment research can easily mis-construe important dimensions as either irrelevant or as components of an agreed wage bargain. See also related works for more details in the UK context (Casey et al. 1997; Gallie 1998).

Taking a more general overview of flexible working, a similar scepticism about the existing statistical data sources emerges. Beynon, Grimshaw, Rubery, and Ward (Beynon et al. 2002) use a survey and interviews inside organisations alongside the case study method. They conclude that the labour market is rapidly changing its qualitative character because of the growth of contingent (i.e. non-standard) labour contracts. This conclusion implies that radical changes in government "employment" data collection procedures are needed. Since there is now so much sub-contracting, causal self-employment, piecework and part time, often un-registered or un-taxed work, the concept of employment needs to be widened to include non-employee workers. A discussion of the legal concepts of 'worker' and 'employee', and their respective rights, shows the depth and wide implications of this transition to a newly de-standardised mode of work (Davies and Freedland 2000).

For those of us who routinely study the informal sector (Beneria 2003), caring work (Waring 1988), self-employment (Wheelock 1998), micro- enterprise (Ehlers and Main, 1998) or farming work (Whatmore 1990), these discoveries offer a welcome return to complexity after the relative simplicity of studies of wages *per se* which assume standardised employment practices. Both academic and official sources offer discussions; see (Rubery 1998; Walby and Olsen 2002); (European Commission 2002; European Commission 2003).

Qualitative studies are highly regarded among labour researchers. Examples include studies of caring work among airline stewards and stewardesses (Bolton and Boyd 2003; Taylor and Tyler 2000; Williams 2003) and studies of the work done in call centres (Taylor et al. 2002); and studies of homeless people's daily lives (Gaetz and O'Grady 2002). Some such studies inherently cannot be reproduced at a national level because the people concerned cannot be approached easily (e.g. homeless people). However, some lessons can be generalised.

One lesson is that government surveys attempt to respond to both internal and external qualitative research. As an example of internal research, view the website of the Department for Work and Pensions, Social Research division, www.dwp.gov.uk. This department uses a range of qualitative as well as survey methods in order to gauge the uptake of government schemes; a major aim is to capture evidence about people who are usually hard to reach. The creation of new government surveys, and their revision, is best seen as a dialectical process, which involves paying attention to qualitative research. In the case of the Department for Work and Pensions, for instance, the 'local area labour force survey' is modified from time to time to allow for new benefits schemes, recognition of new forms of work (e.g. the Modern Apprenticeships), and other changes. Figure 4 illustrates this process of change.

Figure 4: The Integrated Research Cycle
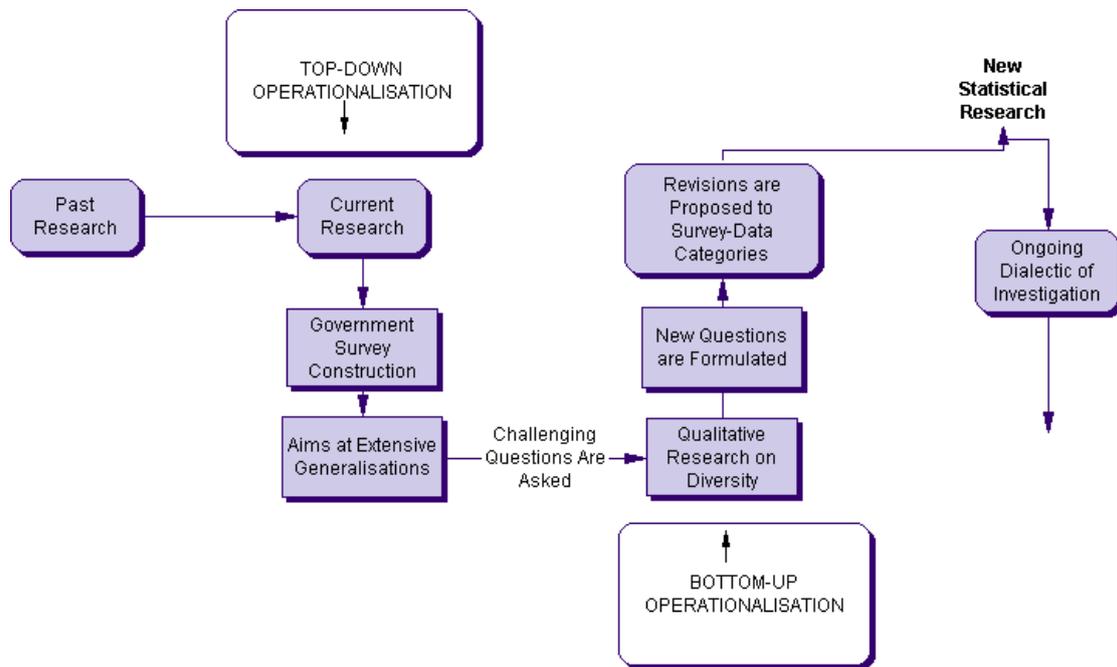
In this section I have described three substantive areas where both qualitative and quantitative research have been combined to good effect. I argued that triangulation in these cases did not merely aim to validate findings. Instead, following the suggestions of Flick (1992), it was used to achieve innovation of conceptual frameworks. It often led to multi-perspective meta-interpretations. The political, social and economic aspects of each phenomenon were given attention, making the research multi-disciplinary. In this context, a meta-interpretation might be that the economic behaviour has social/political aspects and that standard economic theory does not adequately expose or explain that behaviour. Triangulation assisted in making sure that research was interdisciplinary and holistic. Triangulation has played a useful role in the socially situated process of government data collection, too.

Conclusion

Triangulation can cut across the qualitative-quantitative divide. This article described an unsupportable empiricist position that qualitative and quantitative techniques are antithetical. The empiricist position was illustrated through textbook presentations about 'value-free science' and those which assume a dualism of qualitative epistemology versus quantitative epistemology (Silverman, 1993). The empiricist

position is somewhat confused and confusing, since we need a more integrated epistemology for social science instead of two competing epistemological schools.

The article has, secondly, described how triangulation can work, with individuals using it in their mixed-methods research. Both government and academic research teams use it to explore and improve their knowledge of the real world. The realist position was posed as a methodological pluralist approach to research. A certain pluralism of theorising is needed to accompany pluralism of method. Therefore the methodological pluralist approach is relatively challenging and does not easily allow research topics to be simplified. Parsimonious models are unlikely to result from this approach. Since a parsimonious model would have only a few variables in it, it would be likely to be monocausal rather than holistic. Such models might suffer from over-simplification.

A third possible position is the constructionist viewpoint. If constructionism were taken to be a set of assumptions about society, as well as an epistemology, then it would be inconsistent with realism. In Burr's introduction to constructionism we see the standard approach that argues that all social objects have a transitive element (ie they are socially constructed; Burr 1995). That is, their definition and even visibility depend upon the lenses we wear when viewing them. In realist philosophy, and in mixed-methods research that has a structuralist angle of any kind, there is a different set of assumptions. Besides the transitive element there is also an intransitive, enduring, stubbornly un-malleable element to societies. We refer to structures as the enduring element that emerges from persistent patterns of social relations. Poverty, for instance, emerges from an unequal class structure (Byrne, 1999). We also refer to persistent institutions, such as marriage or monogamy, as normed behaviour patterns even whilst we know these institutions are being challenged and changed. These 'structures' or 'institutions' are not simply changed when we re-vision them. They independent of the observer, they are real, and they can constrain social action whether they are perceived to be real or not by members of society. They are somewhat resistant to change (as explained elsewhere, e.g. (Fleetwood 1999)). The thing being pointed to, described, observed and recorded has, to some extent, got a life of its own. Post-structuralists at times forget this reality. Their argument is implicitly that all social objects are entirely socially constituted.

Thus the approach I have outlined here places empiricism, realism, and constructionism at different edges of a triangle of viewpoints. Each can offer a philosophical starting-point for research, and the realist approach is the one that best fits a mixed-methods research methodology. Triangulation across the qualitative-quantitative divide is only consistent with a pluralist theoretical viewpoint. Numerous examples were provided of mixed-methods research arising from such a viewpoint. Whilst more could be said about the methodological debate, it is established through these exemplars that a wide range of topics are currently being covered by triangulated, qual-quant research.

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