

Editors' Introduction: Generating Data

Bruce Curtis and Cate Curtis

These four volumes explore processes and exemplars of generating data across a range of research approaches. 'Generating Data' was chosen for the title of this collection to stress the purposive, creative and context-bound actions of researchers in what might more narrowly be called data collection. Data in this respect is understood as information-capacitated material that must be discovered, mined, unearthed – or a near limitless range of similar adjectives – and that may or may not exist prior to the efforts of the researcher. Thus, exemplars of data generation range from Howard Becker's discussion of analytical induction in ethnography (see Becker, 1993), to Janet Heaton's (2008) acknowledgement of the 'reuse' of data in secondary analysis.

Generating data emphasises the primary and interactive aspects of generating data and as a corollary may, somewhat, extend the boundaries of what is considered empirical endeavour in the social sciences. While our focus is on the generative aspects of data collection, we also wish to contextualise these creative efforts by considering the location of the data sources, issues of access and factors that may impinge upon its analysis, including epistemological debates.

The four volumes are structured through seven groupings of research which provide the context for data generation:

1. Naturalistic research: Fieldwork, participant-observation, ethnographic research;
2. Interrogative research: Grounded theory, focus group, survey research;
3. Experimental research: Group research, remote instrumentation;
4. Material research: Artefacts, trace analysis, visual analysis and content analysis
5. De-centred research: Semiotics, discourse analysis and psychoanalysis;
6. Biographic research: Auto/biographic writing, narrative analysis, and auto-ethnography;
7. Secondary research: Secondary analysis, documentary research, meta-analysis.

Naturalistic Research: Fieldwork, Participant-Observation, Ethnographic Research

Naturalistic research refers to efforts to generate data in their normal, everyday contexts. Data generation in this tradition is marked by issues of entry and exit, the observer effect and issues of induction and translation. The approaches and terminology most associated with this grouping include fieldwork, participant-observational techniques and – most commonly – ethnographic research. Howard Becker (1993), Clifford Geertz (1973) and David Rosenhan (1973) reflect on three pivotal examples of fieldwork as ethnography. These are, of course, studies of their time and issues of risk to participants (and researchers) are treated as exciting technical issues rather than as epistemological or ethical dilemmas. Together the three articles provide a review of naturalistic research – from the traditions of sociology, anthropology and psychology – that are in the round, and at the same time must be contextualised by mid-20th century notions of middle range theory (see Merton, 1968). With this commitment came assumptions about the validity of a broadly anthropological approach (studying deviants and subcultures) and in reporting back on the consequences of labelling. This is surely the case for the two waves of Chicago School scholars. Further, Geertz's (see Geertz, 1995) later, post-fieldwork career, revisited issues of othering and anthropological angst that don't figure, at least explicitly, in the foundational troika. That said, these first three articles are exemplars of generating data, of creating accounts in natural settings and in the absence of a formal, theoretical framing.

Becker (1993) makes central the process of weaving, generating data from observations and conversations. In this respect, collecting data is twinned with the process of 'theoretical analysis' in understanding why medical students hated 'crocks'. Becker (see Becker, 1998) reuses this material in a broader discussion of analytical induction as a means of building theory in naturalistic research and research in general. What Becker downplays, Geertz (1973) highlights in his account of Balinese fieldwork. He provides an account of entry, observation and its impacts. Where Becker foregrounds the everyday back and forth of induction; Geertz makes exotic and therefore as needing translation. Rosenhan (1973) provides an additional experimental dimension, sending eight 'pseudopatients' (including himself) who feigned auditory hallucinations in an attempt to gain admission to psychiatric hospitals across the United States. All of the pseudopatients were treated as 'insane' by the psychiatric staff. Indeed, an unanticipated aspect around exiting the field was the extreme difficulty of convincing psychiatric staff that their patients were in fact sane/researchers.

Naturalistic research is located methodologically somewhere along a participant-observation spectrum. Rosenhan (participant), Becker and Geertz (observer) approximate the end- and mid-point of that spectrum. Hughson (1998) would associate all three with Chicago School approaches and his account of the then 'new ethnographies' in which ethnographers approximate insider or native accounts redrafts longstanding concerns around entry to and exit from the field. However, the ethical issues that such insider approaches raise are as longstanding as Laud Humphreys' (see Humphreys, 1970), *Tearoom trade: a study of homosexual encounters in public places*. Where Hughson and other new ethnographers see a break in naturalist research, Wacquant (2011: 87) sees continuity: "The boxing project is an ethnography in a classic mold in terms of its parameters, a sort of village study like the ones British anthropologists conducted in the 1940s, except that my village is the boxing gym and its extensions, and my tribe the fighters and their entourage." Wacquant's epistemological justification for insider researcher as a scholarly (as opposed to a journalistic) endeavour is made through the notion of habitus. In this Wacquant, like the protagonists championed by Hughson (1988), valorise claims to an at least partial insider status prior to undertaking research.

Blackman (2007) offers an account of alcohol and drug usage on the part of the researchers as well as the role of sexual attraction in generating data in natural settings. This is foregrounded and described as a 'hidden ethnography' that may or may not raise ethical issues. Blackman privileges negotiation, reflexivity and respect as the basis for his successful fieldwork, which suggests, among other things, that researchers drawn to this 'new' type of research are confident in their abilities and that much of the approach remains targeted at deviance and subcultures. Irwin (2006) provides something of a counter to Blackman's triumphalism. Her account of naturalistic research as an intimate endeavour is one of cost and failure. She also notes how Chicago School researchers attempted to maintain a balance between empathic participation and complete engagement with participants (field members). This effort at balance, at validity, was eroded by radical sociologists of the 1960s and 1970s well before the 'new' ethnographers of the sort Blackman (2007) and Hughson (1988) champion. Irwin doesn't reject such intimate research, but like Wacquant, she argues that the epistemological and ethical assessments around entering and exiting the field, of induction, of translation, of interpersonal negotiation and therefore of data generation requires a socio-structural underpinning: "We should locate how our relationships (intimate or distant), behaviors (norm-breaking or conventional), emotions (love or hate), writing (traditional or non-traditional), and other research choices are constrained by, work against, or reinforce social structures" (Irwin, 2006: 171).

Drug usage is also central to Bourgois (2011) but not for the reasons of fitting in as described by Blackman (2007) – who, in turn, echoes Howard Becker's justification for drug use in the 1950s. Rather, Bourgois (2011: 3–4) discusses his 12-year *Righteous Dopefiend* project: “The goal of *Righteous Dopefiend* is to render more visible from an analytical, emotional, and political perspective the human cost of the punitive version of neoliberalism that guides U.S. public policy. It is a narrative account documenting twelve years in the lives of a cast of characters who are bonded by a moral economy of sharing and mutual betrayal in the context of addiction.” The political character of the research, and arguably not fitting in as a participant-observer, mandated an extension of the methods of generating data deployed in much of naturalistic research: team ethnography primarily in the form of photo-elicitation. Harper (2003) goes beyond elicitation to argue for a visual sociology and anthropology, in which the image is central to ethnography.

This notion of extending methods of generating data is further addressed by Dicks, Soyinka and Coffey (2006). They provide a useful review of the developing methods of generating data in naturalistic research. Like Bourgois, their interest is to take multimedia and its co-created meanings into the field: “Data are the represented world as we know and experience it, rather than the ‘world in itself’ (Bauer et al., 2000). In other words, data are what we are able to perceive in the field. Further, we perceive them through all of our senses, including sight, hearing, touch, smell and even taste. Clearly, data in the field are by their very nature composed of diverse media (they are likely to include sounds, objects, visual designs, people's actions and bodies, etc.). Data, then, are necessarily composed of a diverse and shifting range of media” (Dicks, Soyinka and Coffey, 2006: 77). Extending the range of methods available to ethnography is undoubtedly a good in itself, but it often has ethical implications. The article from Jenness (2010) addresses this in passing while foregrounding the fraught circumstance that the participants/subjects of research find themselves in (in this case, transgender prison inmates).

Van Maanen (2015) provides his own reflections on naturalistic research that is both cynical in terms of academic careerism, and exuberant in terms of its prognosis. His focus is the researcher. He highlights how ethnography and fieldwork has changed from its anthropological/Chicago School origins becoming a less standardised and onerous approach for generating data (Van Maanen, 2015: 40): “as ethnographers, we spend, for instance, a few days in the field, meander about the scene, hang out, talk to a few people quite different from ourselves who hold ideas that in various ways differ – often spectacularly – from our own. We learn what we can and then alter the questions we ask or the way in which we ask them and spend a few more days in the field and meet with more people. And on and on (and on) it goes – and where it stops, nobody knows.”

Visconti (2010) addresses a substantial body of work on business ethnography, and his own fieldwork, in an effort to formalise a case study approach.

He draws on insights which Van Maanen (2015) also reflects on. That is, “that methods generate discovery, not only validation” (Visconti, 2010: 36). While his formalising (and legitimating in the context of business studies) approach to generating data stops short of co-created meanings, it suggests forms of cooperation between researchers and participants that underpin forms of theory-building associated with grounded theory.

Interrogative Research and Experimental Research

Interrogative research focuses on the interactive moment, primarily interviewing and analysis of the spoken words that eventuate. The naturalistic context of interviewing may retain significance (as with much grounded theory, where data collection occurs on-site or as part of a case study) but this is not a defining characteristic. In the case of focus group research, it is subordinated in part by an attempt to recreate the natural setting, or pre-existing relationships, within the researcher's preferred parameters (see Morgan and Krueger, 1993). A quasi-naturalistic setting is typically abandoned altogether in survey research. The core of interrogative research is that interview participants are accorded expert status which, as opposed to participant observation/fieldwork and ethnographic research, can be best captured in non-naturalistic settings. Furthermore, interrogative research eschews formulations of structuralism or an ontology of absence that are associated with all de-centred approaches (e.g., much of semiotics, discourse analysis and psychoanalysis). The main issues with interrogative data generation are those of framing the interview, question design and delivery and the interactions between researcher and interview participant.

Dexter (1956) addresses the issue of neutrality in interviewing and develops a nuanced position regarding subjectivity and rapport that confounds simplistic critiques of positivism. Dexter foregrounds the construction of the interview as being central to generating data (1956: 157): “This is not at all to argue for the establishment of rapport, if by that is meant a personally friendly relationship with the informant. Valid arguments against this have been advanced elsewhere. There are various obvious disadvantages in some cases, although advantages in others, in trying to establish rapport on the basis of organizational contacts in common; informants will fear that you hold the organizational view-point and may then familiarize themselves with what the organization thinks in self-protection. But it is to argue for the development of a sympathetic understanding, so that the interviewer can, without strain, talk the informant's language.” Dexter went on to author the classic textbook on interviewing the elite, *Elite and Specialized Interviewing* (1970) and the introduction is included here. He stresses the need for interviewers to do their homework in order to capture the frame of reference of the powerful and to appreciate the

subtleties of expertise. McIntosh (2011) reverses the dynamic somewhat, and confronts powerlessness and power, and most significantly – framing – in her account of Maori and cross-cultural research. She raises the point that indigenous forms of generating data and speaking to power differ from mainstream positivist and even relativist assumptions.

A sophisticated positivism of the type epitomised by Dexter provided the baseline for grounded theory as first elucidated by Barney Glaser and Anselm Strauss in their book, *Discovery of Grounded Theory* (see Glaser and Strauss, 1967), and extended in the writings of the former. Hernandez (2009) rightly notes in her brief overview, that for Glaser grounded theory is an inductive process wherein theory emerges from the data, there are benefits for the researcher beginning with a blank slate, and this confidence in the process of coding reflects the positivist epistemology of Paul F. Lazarsfeld and Robert K. Merton. What is central in this variant of generating data, and hence theory, is the authentic commentary provided by participants and the expertise of the researcher. In contrast, Strauss and in collaboration with Juliet Corbin take the approach towards a deductivism and hypothesis-testing (see Corbin and Strauss, 1990). This is a form of data analysis that is probably better labelled as something other than grounded theory; part of the problem of course being that the champions of differing variants are separated by a common language. Breckenridge et al. (2012) flesh-out the observations made by Hernandez (2009) and highlight that in the realm of generating data and theory the movement from positivist verities to relativism and, even more so, to social constructivism creates fresh problematics. Glaser and Holton (2004) certainly think so in their effort to salvage ‘classic’ grounded theory from the constructivist turn. Charmaz (2014) provides an account of intensive interviewing, a Dexter-like gently guided one-sided conversation, from the perspective of a constructivist take on grounded theory.

Grounded theory is marked by a distinctive constructivist turn, or split, that expresses an epistemological angst about positivism. What is interesting is the surprising degree that champions of antagonist epistemologies have kept the debate in-house, that is, within a discussion of a methodology called ‘grounded theory.’ This reflects that the non-naturalistic aspects of this form of interrogative research are thoroughly subordinated by conceptual formulations of method. In other words, there isn’t discussion of non/unnatural aspects of data generation (e.g., interviewing people somewhere). Grounded theory is like focus group and survey research in that it lies off the participant-observation continuum. It differs from focus group and survey research in that its methods of generating data, at least their physicality, temporality and artificiality in contrast to everyday life, is not much acknowledged. As a result, the constructivist turn intensifies contestation around the methodology, whereas when the same angst played out around focus group and survey research the result was, primarily, their rejection by antipositivists (see

Oakley, 1998b). This isn't to say that the protagonists of both forms of research are *crass* positivists, but that they are confident at the level of methodology that their approach will generate valid and reliable data.

Merton (1987) provides a rambling account of the origins of the focus group that, among other things, bemoans the losses associated with the standardisation of forms of research/generating data. Morgan (1996) displays no such reservations or reflexivity. His article is a self-assured overview of focus groups research that, in passing, interrogates differences with survey research. However, even within a positivist bastion like focus group research, epistemological angst and its methodological response to generating data cannot be denied. Hence, Kamberelis and Dimitriadis's (2014) article remains textbook fodder, providing a set of instructions for researchers, but at the same time points to looming issues of framing the group interview.

De Leeuw (2013) offers a somewhat idiosyncratic account of where survey research has arrived. Her article suggests that the days of the probability sample may be numbered, not as the product of epistemological angst, but as the result of rising costs, burn-out on the part of potential participants and the possibilities for big data (see Savage and Burrows, 2007). However, a generation earlier these epistemological concerns about survey research were also manifest and Cicourel (1982: 15) placed the artificiality of interrogative research in the foreground of his discussion about ecological validity: "Current work on the analysis of discourse and textual materials can help us develop a theoretical foundation for understanding and improving interviews. The theoretical foundations of interviews and surveys must include the way that artificial circumstances necessary to guarantee adequate study designs can violate the ecological validity of findings vis-à-vis what takes place in daily life settings." His insights (which can be applied equally to focus groups) are revisited, in part, in terms of 'measurement errors' in Brenner's (2014) account of the over-reporting of prayer in surveys. Thus: "The survey question inquiring about salience primes the identity, allowing importance to bias measurement. This potentially biasing process may be further encouraged by the nature of the survey interview providing a measurement situation without the usual costs or constraints for identity enactment. ... The survey interview is a paradigmatic example of such a measurement situation, lacking many of the constraints of a 'normal' interaction" (Brenner, 2014: 1013). Such a formulation around methodology as opposed to epistemology, of measurement errors, constitutes a downplaying of Cicourel's concerns about interrogative research and validity *per se*. Indeed, Brenner's conclusion is decidedly upbeat and not that far removed from Dexter's oeuvre: "Once the behavior behind the error is understood, this knowledge can help address the cause of the error and improvement measurement" (Brenner, 2014: 1033).

Experimental Research: Group Research, Remote Instrumentation

Experimental research has at its core the randomised control group design, commonly called randomised control trials in the field of biomedical research, in which it dominates. Randomised control involves the allocation of participants into different groups or conditions, almost always including a control (or placebo) group in biomedical research. For positivist scholars in the biomedical and social sciences, randomised control trials are the zenith of scientific endeavour, at least in terms of reliability and validity (see Campbell and Stanley, 1966; Chapin, 1947). However, beyond the realm of medical (medicine) trials, randomised control is more problematic.

Experimental research (beyond medical trials) is most commonly associated with laboratory-based research, specifically investigations by social psychologists into/with small groups. We should note that the use of control/placebo groups in this type of research is highly contextual and arguably moot. This is certainly the case for the archetypes of social psychology, including Stanley Milgram's (1963) and Philip Zimbardo's (1973) studies of obedience and authority. Milgram's (1963) research took place in a classic laboratory setting, indeed simulated this vision of science as part of its rationale, whereas Zimbardo (1973) reconstituted the laboratory as a social laboratory, in this case a basement in a university building approximating a prison. Both archetypal studies generated a plethora of criticism, based on actual harm to its participants and the use of deception.

Randomised control means that deception is the handmaiden to much experimental research. Although institutional ethics committees and public queasiness have corralled the worst excesses of experimenting on participants, deception remains its leitmotif. The imperative for deception in generating data reflects more than the need for good statistical analysis around the null hypothesis; it embodies a sceptical take on the potential for interrogative research, for example, to reveal social truths (by asking questions of participants). From this perspective, experimental research has intrinsic benefits over interviewing (where truth is revealed by asking), and participant-observation (where truth is revealed by watching), and arguably even the study of material traces (where truth is revealed by measuring residues of action, see Webb et al., 1966). It is a bastion for a positivistic science, whose aim is value-freedom and objectivity. Certainly, the article from Carnagey, Anderson and Bushman (2007), confirm that 'traditional' laboratory-based research continues to thrive. Williams and Jarvis (2006) demonstrate how experimental researchers continue to innovate, how deception remains the core to this approach, and suggest that the margins of the laboratory are increasingly plastic thanks to advances in technology and programming in particular. Their programme, Cyberball, simulated a ball throwing game

which allowed them to conduct small group studies into ostracism. It is also a method in search of an application. As they note: "It portrays a relatively decontextualized social situation into which can be added all sorts of context, either through variations in the cover story, through information provided about the other players, or through what information is ostensibly given to the other players about the participant. We encourage others to find new uses for Cyberball and hope this article will expose a wide variety of researchers to its possibilities" (Williams and Jarvis, 2006: 177).

This is not to say the deception itself can secure the randomised control trial. Time, repetition and learnt expectations on the part of participants are less hard to control for (or as the champions of survey research would have it, to treat as measurement errors). Veroff, Hatchett and Douvan (1992) discuss the nonrandom impacts of a longitudinal study, using a questionnaire in experimental research. They conclude that participants might be more reactive to research design than researchers may think. Certainly there seems little doubt that the principals in the most famous of the social psychology experiments, mentioned above, were surprised by the reactions of participants.

While experimental research in the social sciences belongs primarily to the realm of social psychology, it has long been seen as offering advances to other disciplines. Indeed Ann Oakley (1998) notes that sociology has an experimental tradition that dates back to the 1930s, and that the post-WW2 period in the United States was nicknamed the "golden age of evaluation." Furthermore "It was one in which there was an enormous burst of activity in applying the randomised controlled trial design to the evaluation of public policy" (Oakley, 1998: 1240). The experimental tradition in sociological policy analysis declined primarily because of concerns for the participants, and because the results were often not favourable for its by-and-large conservative sponsors. Druckman et al. (2006) charts a similar story for political science, and borders on describing the social world as a social laboratory: "This trend has been accompanied by a growing interest in the statistical underpinnings of causal inference and increasing awareness that even non-experimental research borrows its logic and assumptions from experimental analogs" (Druckman et al., 2006; 633–634).

Experimental research design outside the laboratory frequently involved door knocking, as Druckman et al. (2006) attest. And in this respect, Milgram, Mann and Harter's (1965) use of the 'lost letter technique' heralded the use of remote instrumentation in experimental research. Remote instrumentation has a specific, technical meaning in terms of disaggregated or virtual laboratories (see Davoli et al., 2010); it is used here as a play on the notion of the positivistic 'research instrument', like the survey questionnaire. Milgram's innovation was to extend the experimental research instrument beyond the laboratory, and the digital realm appears ripe for this. The near-replication of Milgram et al.'s (1965) 'Lost letter' experiment into prejudice,

more recently conducted via email by sociologists, provides a neat example (e.g., Bushman and Bonacci, 2004). Of course, the digital world isn't the only way experimental researchers can leave the laboratory. Cialdini et al. (1990) describe an investigation of littering in which carparks figured prominently. Their approach suggests that there are no real limits to the social laboratory and experimentation in 'different natural settings.' Yet deception and randomised control are set to remain the basis of the experiment. The article from Jones et al. (2004) extends this tradition albeit in a playful and very innovative manner.

Deception, randomised control and distinctions between laboratory and social world fall away in Bell's (2012) post-positivist account of '*Colliding human–animal trajectories (road kill!) on a Tasmanian journey.*' In this account of a road trip, the remote instrumentation is the automobile, or more correctly the windshield as screen (see Verhoeff, 2012), and the experiment is decidedly autoethnographic. An autoethnographic engagement with experimental research seems non sequitur, if only because it denies the possibility of the null hypothesis; however, this type of work is burgeoning and surely embodies experimentality as a form of thought experiment.

Material Research and De-centred Research

Material research is characterised by a supplementary aspect, as a source of triangulation for other mainstream forms. It is in this vein that Webb, Campbell, Schwartz and Sechrest (see Webb et al., 1966) provide a playful introduction to material research, which they labelled unobtrusive and nonreactive. Nonreactive research frees the researcher from the problems of capture or bias or other errors associated with interviews and questionnaires, on the one hand; and from the need for random control and deception, on the other. However, material research is generative; the interaction between researcher (as observer, or coder, or forensic scientist) creates something new to world and to the social sciences. The main issues in data generation include sample and data biases such as the predominance of material produced by one gender over the other, or by one social class, or by a favoured school of thought.

Webb et al. (1981) provide a revision to their earlier monograph and in this extracted article identify the great potential for material research, its continuing marginal status and some problems of bias. They note that "Any single class of physical evidence is likely to have a strong population restriction, and all physical evidence data are troubled by populations in general. It is not, for example, easy to get descriptive access to the characteristics of a possible population restriction. One has the remnant of past behaviour – a groove or a pile – and it says nothing by itself of those who produced the change" (Webb et al., 1981: 32). If this archaeological dilemma is a major

downside to material research, it is offset by the innovative techniques used by its protagonists, looking at datasets with fresh eyes or reframing aspects of the material world as being generative of data.

Webb and Weick (1979) combine unobtrusive measures and organisational investigations in a reflexive and instructive account. There is of course more to material research than its playfulness – the authors identify six dimensions – but here a methodological justification is made: “Unobtrusive measures have come to be associated with a light-hearted, playful stance toward the world in data collection. This stance furthers science in several ways. If the same event, for example, is regarded as both absurd and serious then more of it is likely to be seen because, in fact, it contains both qualities. Foolish interludes generate novel inputs and permit people to recognize and break the singular focus toward a problem in which they had persisted” (Webb and Weick, 1979: 652).

Alison et al. (2001) are not playful; they further develop Webb et al.'s (1966) insights in the domain of forensic science. They emphasise physical, archival and observational sources of generating data. Their contention is that such material research is supplementary which is noted in Webb et al.'s (1966) modest contention. In contrast to this supplementary focus, Abel and Kruger (2010) epitomise the rounded aspects of material research; innovatively combining a new dataset, found visuals (in the form of Major League Baseball cards) with a confident, nomothetic theorising. Klofas and Cutshall (1985) explore yet another dimension of material research. They deliver a moving account of life in an institution for juvenile delinquents through a study of the graffiti on its walls. The article demonstrates how material research, especially trace analysis, can be used to give voice to marginalised or subaltern populations.

As noted, material research deals mainly with the study of artefacts and trace analysis. We include in this section other approaches to stored information, including recent developments in visual analysis, excluding the art history tradition and focusing mainly on the digital realm (see Van Leeuwen and Jewitt, 2001). Pauwels (2010) provides a review of visual analysis and a particularly interesting discussion of found visuals. Whereas Webb et al. (1981) make a virtue of the unstandardised/playful nature of trace analysis, Pauwels (2010) seeks the opposite. In this case, a drive for standardisation.

Standardisation is arguably one of the key themes of content analysis. The approach is thoroughly informed by positivism as a technique of/for social progress, and was initially championed by ‘muckraking journalists’ to combat the racism and vested interests of newspaper oligarchs in the US (see Berelson, 1952). In this respect, proto-content analysis and unobtrusive research are not so far apart, although the former could never be mistaken for a playful framing. Neuendorf (2011) is one of the key methodologists of content analysis, and this article is one of many overviews she has produced. As a primer it is slightly more reflexive about generating data than most

because its audience, gender researchers, are typically drawn from an anti-positivist tradition: "This article provides an overview of guidelines and recommendations for reviewers and researchers in the field of gender studies, in an attempt to provide support for increased rigor in the execution of content analyses" (Neuendorf, 2011: 286). Catanescu and Tom (2001) present a worked example of content analysis using traditional media. Rightler-McDaniels and Hendrickson (2014) extend the approach into digital media and the seemingly ephemeral domain of Twitter. Both demonstrate the legitimating function of standardisation to which Neuendorf (2011) and Pauwels (2010) allude and which Webb and Weick (1979) try to subvert.

De-centred Research: Semiotics, Discourse Analysis and Psychoanalysis

De-centred research refers to forms of data generation that occur within decentred framings of the social sphere: semiotics, discourse analysis and psychoanalysis being three variants. What is decentred, or at least defocused, in each is human agency and the intention is to focus on structure. Here the focus is over-and-above the individual or even collectivities: the study of sign systems, deconstruction of texts, structuration of the unconscious. What might be called therapeutic psychoanalysis has as its clinical method the in-depth interview, which sociologists have long since repurposed (see Savage, 2010). Decentred research includes a variety of data collection techniques including interviews, ethnographic work and the purposive or found selection of texts, but decentred research isn't particularly interested in the assembling aspects of data generation (see Deacon et al., 2007). Indeed, Derrida (see Derrida, 1995: 200) only reluctantly accepts the possibility of methodology while rejecting method outright in deconstruction. More prosaically, this reluctance means that the success of discourse analysis (see Leiss et al., 1990: 214), we would say decentred research in general, is dependent on the inherent (unlearnable) skills of the practitioner.

This ambivalence around method reflects that decentred research approaches the limits of empirical endeavour, and eschews evidence in justifying its capacity to make commentary. It is characterised rather by philosophical notions of absence-absent signifiers, aporia, an ontology of lack. Indeed this fascination with absence is the common denominator of decentred research. Such philosophical formulations are essentially Platonic and tend to free their protagonists from some of the concerns of empirical research, especially in regard to evidence. On the other hand, decentred research can be understood as a form of synthetic inquiry that is strongly determined by the (pre)existence of a grammar or formulation of subjective predispositions. Such is the methodological line pursued in this section. The

key issues in data generation relate then to the exceptionality of texts (see Scott, 2006) and the development of logical and plausible principles, an analytical grammar, to underpin the analysis.

Roland Barthes (1957/1972), Jacques Derrida (1985) and Jacques Lacan (1977) provide three foundational accounts of decentred research. None of the triumvirate describe a method (a reproducible set of procedures) but all three at least countenance methodology as a logic for shaping research and thus for making commentary. Barthes is gentler to potential practitioners in this regard, with his location of the famous image from *Paris Match* in a semiological system. The two shorter articles reflect self-referential aspects of decentred assumptions and the liminal place of instruction in what are esoteric contexts. The article from Derrida, in which he deconstructs deconstruction, is possibly also his clearest rejection of method as learnable. Lacan's instructional oeuvre is arguably as dense as Derrida's, but his description of both the fantasy of self/I and the limits of psychoanalysis in accounting for the mirror stage, is a rationale for the approach as a form of commentary, as social science.

As noted above, data generation in decentred research skirts the nonempirical and is captured here in articles that foreground principles which underpin the intended analysis (we think of this as a grammar). The selected articles are drawn from the less esoteric interweavings of Barthes, Derrida and Lacan. Williamson's (1978) primer on decoding advertisements is a hugely influential attempt to operationalise the insights of Barthes, Lacan and both the cultural and structural Marxism of the day. The conclusion to her primer is provided, as well as a contemporary instance of her synthesis (Williamson, 2010). The latter is undertaken without commentary but stands as an exemplar of commentary grounded in decentred assumptions. Gill's (2009) analysis of text – while being somewhat amnesiac in terms of its intellectual origins – provides a sound restatement of decentred research: “its focus is on discourse itself, rather than seeing this as a means of ‘getting at’ some reality which is deemed to lie behind or beyond the text – whether social, psychological or material” (p. 351). Williamson's and Gill's commentaries are based on selected texts, whereas Barley (1983) is engaged in fieldwork in the American tradition of Becker, Geertz and Goffman. The decentred, semiotic concerns are entanglements with Barthes and Eco. Like the Williamson and Gill ones, this article displays in practice the centrality of a grammar or formulation of subjective predispositions to analysis.

Movahedi (2014) makes central the problem of “how to translate or connect the realm of the psyche/individual to the realm of the social or structural” (p. 153). In answer, he cites Lacan and Slavoj Žižek, the latter being the chief, contemporary protagonist of a decentred analytical and populist commentary. As with the other articles in this section Movahedi provides here an exemplar, adducing and deploying an analytical grammar, while addressing a substantive issue – the conflicted relationship between sociology

and psychology. Similarly Daly (1999), as an exemplar of methodology, interweaves Barthes, Lacan and Žižek while presenting a homage to the coming man. Cremin (2010) and Movahedi and Homayounpour (2012) provide a brace of articles; approaching a synthesis of sociology and psychoanalysis, indeed what Freud called 'social psychology', from contesting start points.

Biographic Research and Secondary Research

Biographic research in the social sciences focuses on accounts that explore the polarity of agency and structure, especially the limits of human action. This Millsian approach to biography sometimes sits at odds with the reflexive focus of accounts that deal with the self. The main approaches are auto/biographic writing, narrative analysis, and autoethnography. Subjectivity, researcher reflexivity/visibility and interpretation are recurring dilemmas which centre on the capacity of biographic research to operate as a social science.

In a seminal article, Allport (1942) argues that personal documents, including auto/biographic accounts, cannot be fully utilised within the context of nomothetic, lawful, generalising accounts largely because that approach to science must focus on a narrow range of variables for, effectively, inferential purposes. This is simultaneously a critique of a strictly nomothetic take on positivistic science and of its measures of rigour. This broader critique is sometime overlooked when personal documents are foregrounded at the expense of epistemological considerations: "Although personal documents have been profitably employed in mass investigations, it is not possible to evaluate fully their contribution to social and to psychological science so long as an exclusively nomothetic outlook prevails. Not until we are prepared to dwell upon the unique patterning of personality, and to concede that lawfulness need not be synonymous with frequency of occurrence in a population, and to admit that prediction, understanding and control are scientific goals attainable in the handling of one case and of one case alone not until then are we in a position to assess the full value of personal documents. Nomothetic studies are all to the good, but by no means do they exhaust the usefulness of first-person documents for a science that will admit the idiographic as well as the nomothetic perspective" (Allport, 1942: 64).

Hookway (2008) eschews any epistemological angst about the lawfulness or not of auto/biographical accounts in his pragmatic account of research using blogs. Similarly, the online/digital sphere, across which auto/biographic writing has exploded is not seen as fundamentally different to that of the physical world. As a result, Hookway (2008) produces an account of doing online research which updates the more prosaic aspects of

method, and downplays more abstracted concerns. The article reflects a useful start-point or how-to-do for researchers interested in this type of auto/biographic writing. As noted, it is intentionally concrete in its scope: "While 1999 may well be remembered as the year blogging exploded, it is yet to be seen how this newest addition to online life can be utilized as a qualitative social research technique. The aim of this article then was to make an important first step in building this knowledge base. Alongside indicating the research potential of weblogs, it has provided a researchers' guide to accessing the blogosphere, looked at what blogs are, where they are hosted and how to use blog search tools for sampling. It has also identified some of the methodological and ethical issues associated with blog research" (Hookway, 2008: 106).

A focus on method at the expense of methodology is in direct contrast with Polkinghorne (1995) who critiques narrative analysis, of which life stories are an important component, through the opposition of paradigmatic and narrative accounts: "I have advocated the importance of identifying two types of narrative inquiry. Both share the general principles of qualitative research such as working with data in the form of natural language and the use of noncomputational analytic procedures. Although both types of narrative inquiry are concerned with stories, they have significant differences. The paradigmatic type collects storied accounts for its data; the narrative type collects descriptions of events, happenings, and actions. The paradigmatic type uses an analytic process that identifies aspects of the data as instances of categories; the narrative type uses an analytic process that produces storied accounts. The paradigmatic type is based in . . . paradigmatic reasoning; the narrative type is based in narrative reasoning. Narrative inquiry of the paradigmatic type produces knowledge of concepts; the narrative type produces knowledge of particular situations" (Polkinghorne, 1995: 21). Bamberg (1997, 2010) reconnects the methodological and method aspects of narrative analysis of storied accounts. His first article provides a brief overview of narrative positioning of 'I and the other'. That is: (1) How are the characters positioned in relation to one another within the reported events?; (2) How does the speaker position him- or herself to the audience? and (3) How do narrators position themselves to themselves? (Bamberg, 1997: 337). His latter article (Bamberg, 2010) is a sustained account of how narrative analysis can make sense of the sense of a life story. Similarly, Kenten (2010) and Elizabeth (2008) explore in depth how solicited biographical writing can be used to explore life stories, show the limits to human action in the form of discourse, and provide scope for reflexivity on part of both the researcher and research participant.

Such capacity for reflexivity is arguably the *entrée* for autoethnography and as a break with older traditions of ethnographic research. It is moot whether Ellis et al. (2011) see autoethnography as truly lying within the realm of social science – in a way that might be extrapolated from Allport

(1942) – or merely as being encumbered by various technical demands: “When researchers do *autoethnography*, they retrospectively and selectively write about epiphanies that stem from, or are made possible by, being part of a culture and/or by possessing a particular cultural identity. However, in addition to telling about experiences, autoethnographers often are required by social science publishing conventions to analyse these experiences” (Ellis et al., 2011: 3). Getting published in scholarly journals is of course fraught, and if reflexivity and therapy is a *raison d'être*, as Ellis and company suggest, it is probably unnecessary. However, Anderson (2006) cites other reasons for autoethnography and his accounts return this form of data generation to the traditions of C. Wright Mills and the Chicago School. Wall (2006) offers a non-polemical account of autoethnography, in the form of a beginner doing an autoethnography, while Longhurst (2012) demonstrates how reflexivity and therapy are in themselves not barriers to theoretical engagements with substantive social problems.

Secondary Research: Secondary Analysis, Documentary Research, Meta-Analysis

Secondary research is sometimes called desk-top research as the researcher never has to leave his or her office. The initial research process, data collection, remains a creative and generative one, however, in that the researcher confronts some of the issues associated with material research, as well as those more discursive ones in contextualising and analysing the data. The innovative dilemmas in data generation deserve attention, and Heaton (2008) makes the crucial point that even when dealing with secondary, precollected data, the researcher typically reuses it. Heaton makes a useful distinction between secondary analysis, documentary research and meta-analysis: “Secondary analysis involves the reuse of pre-existing qualitative data derived from previous research studies. These data include material such as semi-structured interviews, responses to open-ended questions in questionnaires, field notes and research diaries ... this focus on *non-naturalistic* qualitative data distinguishes secondary analysis from documentary analysis ... which involves working with *naturalistic* or ‘found’ materials, such as auto-biographies, personal diaries and photographs. However, some types of qualitative material, such as life stories and diaries, may be subject to either secondary analysis or documentary analysis, depending on to what extent the material was solicited and shaped by researchers’ involvement in collecting the material. In revisiting the actual data, secondary analysis is also distinct from meta-analysis and systematic reviews of quantitative and qualitative research because these approaches usually involve going back over the published findings

of previous studies and not revisiting and reworking the data" (Heaton, 2008: 34–35).

Heaton's overview is short and as a result is perhaps somewhat formalistic. The distinctions between naturalistic and non-naturalistic sources of data, the reification of qualitative and quantitative research and between shared and collected data provides a conceptual scaffolding rather than insights to be operationalised. In particular, she somewhat understates the extent to which documentary research and meta-analysis involves the reuse and consequent creative generation of (new) data. Murray (2010) details this reuse of data, from semistructured interviews, and in so doing relaxes some of the categorical claims made by Heaton (2008). The article is an exemplar of secondary analysis as a creative process.

Murray makes the issue of fit in the reuse/generation of data of central importance in her reconceptualisation of resistance to youth offending as being 'active resilience': "The challenge of the 'fit' between the purpose of the analysis and the nature and quality of the original data is identified by Thorne (1994) [see Thorne, 1994] as 'the most pivotal issue in deciding whether secondary analysis is a viable option in any inquiry' (Thorne, 1994, p. 270), although, as Thorne notes, this is less problematic in respect of semi-structured interviews, as they are likely to produce rich and varied data. However, there are also advantages to utilizing already existing data-sets, not least the enormous savings in cost and time" (Murray, 2010: 119).

Platt (1981a,b) provides a remarkable overview of data generation in the realm of documentary research. Her first of two linked articles addresses issues of authenticity and problems around sampling and concludes with a statement we fully endorse: "We conclude, therefore, that there are important senses in which documentary research has problems which are not significantly different from those of research using other data sources. A more general moral might be suggested, which is that the distinctions commonly made among 'methods' of research in terms of their data sources may be analytically unhelpful... Nonetheless, each data source gives rise to its own special technical problems" (Platt, 1981a: 49). In her second article (Platt, 1981b) turns her attention to more general issues of interpretation and rigour. These relate primarily to the veracity of inference in this form of secondary research (in effect, based on nonprobability sampling).

Atkinson and Coffey (1997) discuss the possibilities for analysing organisations via the enormous amount of paperwork they generate. Their approach to documents is decidedly semiotic, in that they frame the approach in terms of the production and consumption, and circulation of texts. The article is an interesting companion piece to the combination of unobtrusive measures and organisational investigations suggested by Webb and Weick (1979), as discussed above. Paperwork/documentation has changed substantially since 1997 and Robert Smith extends document research into the digital age. Smith's (2015) account of criminal activity synthesises a range of

documentary research techniques and is an exemplar of a consciously mixed, qualitative approach to contemporary organisational inquiry. Smith and McElwee (2015) concurrently revisit documentary research as a methodology that centres their research on illegal entrepreneurial activity. While the review is somewhat limited as a methodological statement, largely because of its focus on the substantive topic and some name-checking, rather than on the approach to generating data, it nevertheless stands as an operationalisation of the sort of issues raised by Platt (1981a,b). That is, documentary research tends to blur or synthesise with other forms of secondary research and qualitative endeavours.

Meta-analysis has clearer boundaries than documentary research, and because its centre of gravity remains in the realm of positivism, clearer understandings and measures around the issues of 'fit' raised by Murray (2010) in qualitative contexts (through the use of semi-structured interviews). The overview by Rosenthal and DiMatteo (2001) squarely addresses the statistical testing associated with positivist conceptions of data generation and inference (see Oakley, 1998b). Their final statement echoes that quoted from Platt (1981a: 49) and underscores meta-analysis as a form of secondary research in which data is creatively generated: "Meta-analysis is not inherently different from primary data analysis; it requires the same basic tools, thought processes, and cautions ... A desire to impose order on chaos may be helpful given the challenges of certain research fields, and patience with the limitations of some research write-ups will certainly help. Intimate communing with the data, particularly within the context of the theory guiding the work, is an essential and rewarding requirement" (Rosenthal and DiMatteo, 2001: 78–80).

Pratt (2010) argues that the discussion of meta-analysis is somewhat decontextualised and hence overly technical. He interrogates statistical measures of meta-analysis with some appreciation of the 'art' and 'science' of scholarly research, while arguing strongly for the statistical measures associated with the latter as a means of building the credibility of criminology, as an emergent discipline. There are no doubt echoes here of the initial efforts by the protagonists of content analysis to gain credibility through standardisation (see Berelson, 1952).

In the final article, Dixon-Woods et al. (2006) revisit the same epistemological ground covered by Allport (1942), discussed in the section above on secondary research, about differences between nomothetic and idiographic uses of data. For Dixon-Woods et al., the conundrum centres on the synthesis of qualitative data in the context of meta-analysis. Qualitative data is pitched broadly in this discussion and in contrast with randomised control trials, which they contest is the privileged source of data, at least within health studies. They highlight the enormous difficulties in synthesising qualitative research within the context of meta-analysis, when there are no agreed upon measures for comparison between the different sources of data in terms

reliability and validity (see Campbell and Stanley, 1966; Chapin, 1947). The article is therefore more aspirational than operationalisable.

References

- Becker, H. (1998). *Tricks of the Trade: How to Think about Your Research While You're Doing It*, Chicago: University of Chicago Press.
- Berelson, B. (1952). *Content Analysis in Communication Research*, New York: Free Press.
- Campbell, D. T. and Stanley, J. C. (1966). *Experimental and Quasi-experimental Designs for Research*, Boston: Houghton Mifflin.
- Chapin, F. S. (1947). *Experimental Designs in Sociological Research*, New York: Harper and Row.
- Corbin, J., and Strauss, A. (1990). Grounded theory research: Procedures, canons, and evaluative criteria. *Qualitative Sociology*, 13(1): 3–21.
- Davoli, F., Meyer, N., Pugliese, R. and Zappatore, S. (2010). *Remote Instrumentation and Virtual Laboratories Service Architecture and Networking*, Boston: Springer.
- Deacon, D., Pickering, M. and Golding, P. (2007). *Researching Communications: A Practical Guide to Methods in Media and Cultural Analysis*, London: Hodder Arnold.
- Derrida, J. (1995). *Points . . . : Interviews, 1974–1994*, Stanford: Stanford University Press.
- Geertz, C. (1995). *After the Fact: Two Countries, Four Decades, One Anthropologist*, Cambridge: Harvard University Press.
- Glaser, B. and Strauss, A. (1967), *Discovery of Grounded Theory: Strategies for Qualitative Research*, Chicago: Aldine.
- Humphreys, L. (1970). *Tearoom Trade: A Study of Homosexual Encounters in Public Places*, London: Duckworth.
- Leiss, W., Kline, S. and Jhally, S. (1990). *Social Communication in Advertising: Persons, Products and Images of Well-being*, London: Routledge.
- Merton, R. K. (1968), *Social Theory and Social Structure*, New York: Free Press.
- Morgan, D. L., and Krueger, R. A. (1993). When to use focus groups and why, in Morgan, D. L. (Ed.), *Successful Focus Groups: Advancing the State of the Art*, Newbury Park, San Francisco: Sage: 3–19.
- Oakley, A. (1998b). Gender, methodology and people's ways of knowing: Some problems with feminism and the paradigm debate in social science. *Sociology*, 32:707–731.
- Savage, M. (2010). *Identities and Social Change in Britain since 1940: The Politics of Method*, Oxford: Oxford University Press.
- Savage, M and Burrow, R. (2007). The coming crisis of empirical sociology. *Sociology* 41(5): 885–899.
- Scott, J. (2006). Assessing documentary sources, in Scott, J. (Ed.) *Documentary Research*, London: Sage: 23–42.
- Thorne, S. (1994) 'Secondary analysis: Issues and implications', in Morse, J. (Ed.), *Critical Issues in Qualitative Research Methods*, London, Sage.
- Van Leeuwen, T. and Jewitt, C. (2001). *Handbook of Visual Analysis*, London: Sage.
- Verhoeff, N. (2012). *Mobile Screens: The Visual Regime of Navigation*, Amsterdam: Amsterdam University Press.
- Webb, E. J., Campbell, D. T., Schwartz, R. D. and Sechrest, L. I. (1966). *Unobtrusive Measures: Nonreactive Research in the Social Sciences*, Chicago: Rand McNally.