



## SYMPOSIUM "PERSONAL ENCOUNTERS WITH SERENDIPITIES"

# A Journey to the History of the Interview

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When I was 9 years old, my parents gave me a book about ancient Egypt as a birthday present. I read it avidly, although now the only thing I can remember about it is that it had a brown cover embossed with red and black Egyptian hieroglyphics. I was soon telling anyone who would listen that when I grew up I wanted to be an historian. It didn't quite work out that way; I became a sociologist instead. Entering a shrinking academic labour market after graduate study I became a 'research bum' to use Howie Becker's phrase (Molotch 2012), working over a number of years, mostly as a survey researcher, on a succession of short term research contracts. The focus here was relentlessly short-term and instrumental. Later, teaching research methods to undergraduate and post-graduate students I remained focused on the current and the technical. A turn towards studying the history of the interview would probably have struck me at this time as redundant and improbable. Yet, it was the topic that became a central focus for me as I moved towards the end of my career. Now *ar mhuin na muice* (literally 'on [the] back of the pig'), the Irish phrase that denotes that one is retired or living at one's ease, I can tell my 9 year old self that I fulfilled at least part of his ambition.

Having decided to pursue a PhD in sociology my first thought, for various reasons, was to do a thesis on Church-State relations in Poland<sup>1</sup>. In the end, however, that topic posed too many logistical and political difficulties to be feasible. I had to look for something else. The 'Troubles' in Northern Ireland had recently erupted and, since I was born there, I began to think that I should turn my attention to a topic related to the conflict. Unfortunately, I couldn't think of anything suitable. What caused the eventual topic to emerge was a serendipitous encounter. The morning after some riots in Belfast I encountered a small group of women on the edge of a Catholic area in the city who were surveying the remains of houses burnt down the previous evening. One of the women commented that the couple who had lived in one of the houses were in a 'mixed' (i.e. interreligious) marriage. According to the women, the house had been singled out by a Protestant mob intent on punishing the husband for taking the Catholic side in deference to his wife. To them, this revealed the iniquity of the mob since they reasoned that the husband couldn't possibly have sided with the Catholics since, after all, he *was* a Protestant. I began to think it might be interesting and enlightening to see how intermarried couples dealt with the tensions and conflicts inherent in a divided society.

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<sup>1</sup> My interest in Poland arose as a by-product of well-developed links I had to the jazz scene in Warsaw. These links were facilitated largely through the good offices of the now deceased music journalist Stefan Zondek.



I did my PhD on interreligious courtship and marriage in Northern Ireland at the University of Edinburgh. I won't dwell on the specific field experience to any great extent. (For more detail on the fieldwork and how I dealt with the immediate challenges involved, see Lee 1992.) What I will focus on instead are some of the lessons I drew, not always very consciously, from the research. I learnt many things in the course of producing the thesis. Most of them have probably been forgotten or faded in significance with the passage of time. A small number of issues have resurfaced, however, from time to time since, often in unexpected or serendipitous ways. One such issue relates to the analysis of data from unstructured interviews. I had interviewed in depth a sample of intermarried couples and transcribed the recordings of the interviews. Now I had to deal with a large volume of unstructured data. Over and over again I felt I was drowning in words, drowning in paper. I tried different ways of dealing with the problem, but nothing seemed to work. It was only, idly doodling one day, that I realised I could graphically represent the experiences the couples I studied had undergone as a set of linear or recursive trajectories. From this I was able finally to gain an analytic handle on the interview material, and was able to go back to the interview transcripts with something like a sense of direction. As have many researchers before and since, I came to think there "had to be a better way" to analyse qualitative data (Fielding and Lee 1998).

Probably not unrelated to this I began to question the centrality of interviewing as a research method within the social sciences. One reason for this, I suspect, is that I wasn't a very good interviewer. Often I would look at a transcript and think "Why didn't I ask the obvious follow-up question at that point" or "Why did I let [the interviewee] ramble on about that topic when it was unlikely to lead anywhere." I also suspected in some instances that I was being presented with a facade of harmony that hid a degree of conflict and difficulty within a couple's relationship that I could not dismantle. The upshot of this is that I developed what was to become something of a long-standing interest in the interactional dynamics of the interview but one embedded within a rather ambivalent view of the method. The interview in a way became for me something of a 'false friend'; useful and helpful on the surface, but potentially treacherous underneath. A further factor in my view of the interview related to its social acceptability in certain situations. In Northern Ireland at the time I was working there it could be hazardous to ask questions or to appear too inquisitive (see Lee 1995). In this situation it became necessary to find other routes to achieve one's research aims. Not surprisingly, then, I became very taken by some of the arguments in Webb et al's classic (1966) book *Unobtrusive Measures* about the deficiencies of self-report, the use of multiple methods, and the need to find creative solutions to research problems.

I developed two particular interests in relation to unobtrusive methods. One had to do with the use of 'institutional discovery' methods (Marx 1984), that is research based on documents that become available through investigative, judicial, or legislative procedures such as investigative commissions and tribunals, court cases, and material obtained under freedom of information legislation (see, e.g., Lee 2001; 2005). My second interest in this field related to what can be called the 'generative problem' in studies using unobtrusive methods (Lee 2000). Beyond the injunction to be 'creative', Webb et al gave little guidance on how sources of unobtrusive measures could be identified for particular research problems. I began to wonder if there might not be a range of heuristic strategies that could be used in such circumstances. What I had in mind has some affinities with aspects of Becker's (1998) work on 'tricks of the trade' and probably has some relevance to the creation of fruitful environments for serendipitous discovery (Merton and Barber 2004, 200-2), but for one reason or another the idea is one I have never been in a position to develop further.

While doing my thesis I obviously read the existing literature on interreligious marriage, although I found it not to be overly useful. This was because most of the then existing literature tended to take intermarriage as an indicator of the assimilation of immigrant groups. However, assimilation was hardly an issue in Northern Ireland. Since intermarriage was relatively uncommon and generally frowned upon, I turned instead to the literature on deviance. One of the books I read was David Matza's *Becoming Deviant* (1969). The book is written in two parts and, although it is one of my favourite books, I must confess I've only ever skimmed through the second part. What I was struck by was how in Part I Matza picks apart the assumptions behind a number of different sociological conceptions of deviance. In particular, I was taken by his comments on the romanticising impulse in the Chicago tradition. Later I read Alvin Gouldner's much neglected article on romanticism and classicalism in sociology (1973), and later still Colin Campbell's *The Romantic Ethic and the Spirit of Modern Consumerism* (1987). In a strange way, reading this material helped me make sense of something that I found slightly puzzling. On visits back from the field I became aware that people often thought that doing research in Northern Ireland was a brave thing to do. But I never felt brave, quite the opposite. I often felt scared or apprehensive, as did most other people in the same situation. Talking to field researchers then and since and reading a number of first-person accounts of the fieldwork process, I seemed to detect in the way some researchers described themselves an implicit image of the fieldworker as an existential hero going out to do battle with the world. Far from ubiquitous but frequent enough to be noticed and often a matter of *tone* as much as anything else, this made me wonder about how fieldworkers' constructed their own self-images, and in particular how far such self-images might be suffused by romanticism and heroicism. While, again, this is not something I have pursued in any detail since, at the time it did encourage me towards an interest in the sociology of sociology.

In the 1980s, a number of scholars in the United Kingdom embarked on work that re-evaluated the Chicago tradition in sociology. Martin Bulmer (1984) marshalled an impressive range of material to produce an institutionally-oriented account that emphasised both the breadth and depth of Chicago sociology. More polemically, perhaps, Harvey (1987) set out to undermine what he saw as the myths that had grown up around the Chicago School. In a number of articles Jennifer Platt (1983; 1985; 1994) challenged the assumption that participant observation as a method *understood in the modern sense* was one associated with the urban studies conducted at Chicago by Robert Park and his colleagues, and questioned the extent to which Max Weber had been an important influence on Chicago sociologists. Bulmer's work, in particular, broadened and deepened my knowledge of Chicago but it was Platt's writing that I experienced as serendipitous<sup>2</sup>.

Platt suggests that anachronistic readings of the history of Chicago sociology provide contemporary researchers with an origin myth linking their methodological preferences to those of a revered, but mythical, ancestor. Some years earlier I had chanced upon a used copy of the *Introduction to the Science of Sociology*, the 'Green Bible' authored by Park and Burgess (1921), and had been struck when reading it by what seemed like a lack of congruence between it and contemporary methodological preoccupations conventionally presented as a legacy of Chicago. While thinking this was odd, I had never followed up the idea and tended to assume that if I couldn't see a connection between past and present it was because I was too stupid to do so. Platt's work was gratifying in the

<sup>2</sup> According to Merton and Barber (2004), the term 'serendipity' was coined by Horace Walpole in 1754. In a nice coincidence, I read Platt's article on participant observation within sight of Strawberry Hill House, the house Walpole built that is now regarded as one of the finest examples of Gothic revival architecture in Britain. Recently restored, it is now open to the public. <http://www.strawberryhillhouse.org.uk/about.php>



sense of rescuing me from this assumption, but it did more than this. It had never really struck me before that research methods were not timeless givens but practices embedded in specific historical contexts. Nor had it occurred to me that disciplinary histories could be seen as being socially constructed. The sense I had taken away from my thesis that methodological self-images were important could begin to be seen in a more interesting light as something that fed off and into the kind of origin myth Platt had identified. This was the point at which I began to take seriously the history of social research as an area of study.

That was a road I didn't take, however. I did produce a number of books inspired by my thesis experience, *Doing Research on Sensitive Topics* (1993) and *Dangerous Fieldwork* (1995), but I also became involved in work on the use of computers in qualitative research. My original interest in this had been sparked by another serendipitous encounter. At a meeting of the American Sociological Association I encountered Renate Tesch. Originally from Germany but settled in California, Renate (who also called herself Renata) had committed herself to popularising the use of computer software packages as tools for analysing qualitative data. Given the problems I had analysing my thesis data I was rather receptive to some of her ideas and, although initially I didn't take matters much further, this was soon to become highly consequential. Nigel Fielding, a well-known qualitative researcher at the University of Surrey had been asked to organise one of an annual series of methodology conferences at Surrey. When in the course of a conversation with him I mentioned Tesch's work, the idea of a conference on qualitative computing soon emerged. Our fears that the conference would be of marginal interest were unfounded by the huge amount of interest it generated. Out of this was born the CAQDAS Networking Project. Funded over the years by a variety of grants from the UK's Economic and Social Research Council, the project provides courses and workshops on computers in qualitative research as well as maintaining a highly popular online forum, the qual-software list. (CAQDAS stands for Computer-Assisted Qualitative Data Analysis. The pun on 'cactus' is deliberate. In the beginning, for many qualitative researchers computer use was a 'thorny' issue.)

The CAQDAS Networking Project was in a sense a very forward-looking enterprise, which was open to new ways of doing things especially as the opportunities and challenges opened by the Internet became increasingly apparent. However, a basic tenet of the project was that researchers should not simply embrace or reject the use of new technologies in research but explicitly and critically assess their methodological and epistemological implications. As a result I became quite attuned to the sometimes subtle ways in which technological affordances affected research practice. In this context, it was probably not surprising that my eye was drawn to an observation made in another paper by Jennifer Platt. In an article on the history of the interview Jennifer observed that "research on the consequences for practice of changing techniques and technologies for the recording of free answers is strikingly absent" (2001: 41). When a conference at the University of Sussex on 'The History and Practice of Sociology and Social Research' was announced to mark Jennifer's retirement, I decided that it would be appropriate and a fitting personal tribute to Jennifer's contribution to the field to try to fill in the gap she had identified.

There is a fascinating interplay between interview practice in the social sciences and methods for sound capture that extend from the use of wax cylinders through to solid-state digital recorders, not to mention in earlier times the use of (usually concealed) stenographers (see Lee 2004). If I thought of it at all, I probably assumed that the paper I wrote on recording technologies and the interview was a one-off exercise. However, as I delved into the topic, all sorts of wider issues and questions began to emerge. Why did the interview seem to have low status as a method in Chicago



sociology? How was it that social workers rather than sociologists were regarded as innovators in interview practice in the 1920s? How had nondirective interviewing moved from being a clinical practice to one widely used by social scientists? Why did theorists like David Riesman and Robert Merton take an interest in interview methodology? There was nothing for it but to try to find some answers (Lee 2004; 2008a; 2008b; 2010; 2011). There is no overarching plan to this work. The topic simply took on a life of its own. Nor is the work intended to be explicitly revisionist in tone. It is, however, broadly in line with the view that the history of qualitative research in sociology tends to over-emphasise historical continuities with what is in effect a mythologised past. This in its turn reflects my conviction that the commitment to reflexivity generally found in contemporary work inspired by Chicago sociology should imply a willingness to examine how that tradition has been socially constructed. That it often does not is implicitly connected I suspect to the kinds of romanticised and heroicised self-images I mentioned earlier.

Until the advent of audio and video recording it was extremely difficult to study the interview in actuality and in real time. Information about how interviews were conducted in the past has had to be recovered for the most part from prescriptive or programmatic sources, such as textbooks and training manuals, likely to present a partial or idealised view of actual practice (Platt 2001). The development of the interview has also involved a great deal of borrowing from one discipline to another, much of it unacknowledged or unattributed. Bearing these caveats in mind, patterns of methodological innovation and diffusion relating to the interview can be tracked to a degree through the journal literature. As a result, I have made much use of resources like the Social Science Citation Index, JStor and, for older material, sites such as the Internet Archive (<https://archive.org/details/texts>) and the HathiTrust Digital Library (<http://www.hathitrust.org/>). There are, of course, clear limits of this approach, something that became readily apparent when I began to look at the history of the focus group.

In a paper published in 1987 Robert Merton mused on the relationship between his wartime work on focused interviewing (see, e.g., Merton and Kendall 1946) and the growth of focus groups in consumer research, but noted that the topic had not been studied in any detail. I decided to try to trace the connections. This proved difficult because both the journal literature itself and norms relating to referencing and citation developed relatively late in the field of consumer research. Merton's work, it seemed, had left no trail. I was frustrated by this until, idly searching the Web one day, I happened across a blog post that claimed the Edsel, a notorious and disastrous flop introduced by the Ford Motor Company in the 1950s, had been the first car to have been designed by focus group<sup>3</sup>. In fact, this appears to be a myth (which might say more about public perceptions of focus groups than their historical development). It is the case that the Bureau of Applied Social Research conducted studies for Ford during the Edsel's design phase (Brooks 2014), but these involved conventional survey and depth interviews with individual consumers rather than group interviews. What had seemed like an interesting and promising lead now looked like a dead-end until it occurred to me that it could be useful to look for other companies that might have commissioned work involving focus groups. This led me to a classic example of materials produced by institutional discovery, the Tobacco Industry Documents. Now totalling over 5 million documents, most of which are available online, the Tobacco Industry Documents came into the public domain as the result of litigation against tobacco companies once the link between smoking and cancer became

<sup>3</sup> <http://fourwheeldrift.wordpress.com/2008/06/19/car-names-that-can-never-be-used-again/>



well-established<sup>4</sup>. These documents have been widely used by public health researchers (Bero 2003), and although for various reasons difficult to use, proved to have much material relevant to the marketing of cigarettes in the 1950s and 60s when companies tried to shift away from their traditional market, working-class males, and towards women, young people, and ethnic minorities. The Tobacco Industry Documents included material documenting the introduction and growing use of focus groups, bridging the gap left by the absence of citation data.

The use of online resources raises quite broad issues for research on the history of the social sciences. Will we see, for example, a shift towards use of large-scale digitised archives at the expense of small-scale local archives? Or to put this more dramatically, towards the broad sweep at the expense of the detailed investigation of particular cases. Might the availability of citation indices and the ability easily to search long runs of journals encourage the production of more bibliometric studies? There are implications here for resource distribution, training, infrastructure, and what counts as expertise. (For an examination of how the growing use of textual databanks changed the nature of expertise in classical studies, see Ruhleder 1995.) More specifically, it is interesting to note that researchers in the digital humanities have for some time been worried about the extent to which existing tools and methods for searching online resources inhibit serendipity (see, e.g., Foster and Ford 2003). That concern in turn has encouraged information scientists towards the empirical study of serendipity and investigation of ways in which it might be encouraged, rather than inhibited, through the use of online tools (Martin & Quan-Haase 2014). What solutions will emerge and how useful they will be remains for the moment an open question.

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<sup>4</sup> The major online repository for the Tobacco Industry Documents, is based at the University of California, San Francisco: <http://legacy.library.ucsf.edu/>



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