

## Re-using Qualitative Research Data: A Personal Account

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**Abstract:** This paper describes three experiences of reanalysing data. The first is the re-use of data I have collected myself, primarily the much-used dataset "Family and Work Conditions before 1918", collected in the early 1970s, and the extent to which this has created difficulties and gains. The second is a more recent project, published as *Growing Up in Stepfamilies*, in which we tried to interweave the potentially complementary strengths of life story interviews with data from a quantitative longitudinal cohort followed since 1958 by the National Child Development Study. The third is my current project on transnational Jamaican families, where I have been using parallel interviews collected by Mary CHAMBERLAIN and Harry GOULBOURNE.

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### 1. Introduction: the Strange Silence

There is a strange silence close to the heart of the qualitative research community. There are a very large number of texts describing all the different styles in which you may choose to interview, how best to use them, their potential and their pitfalls. There also—although significantly fewer—good texts on how to analyse and interpret your interviews. When these are survey interviews, it is assumed that the researcher will be mainly interpreting interviews which were carried out by other researchers, and some of the textbooks explicitly discuss the issues involved in analysing survey material collected for other people's projects (DALE, ARBER & PROCTER 1988; PROCTER 1993). But for qualitative researchers, the traditional assumption has been absolutely the reverse. Surprisingly, even when a historical perspective is explicit, earlier qualitative research data is not considered as a source (MARWICK, 1970; BURGESS 1982). More typically, qualitative methods handbooks have simply ignored the possibilities altogether (PLUMMER 1983; BULMER 1984; SILVERMAN 1993, 2000; HOLLWAY & JEFFERSON 2000).<sup>1</sup> New researchers will normally be

<sup>1</sup> During the 1990s there has been some discussion of re-analysing earlier qualitative research data in the field of health research, but it must be said that this is still at a very preliminary level

advised that they need to immerse themselves in the field like embryo anthropologists, know their informants and their context, remember the gestures as well as the words of their interviews, savour them through the long process of transcribing, and out of that total immersion will emerge the original insights which they are seeking. [1]

Now I wouldn't dispute that such immersion is an essential part of the qualitative research experience. But because it is so time-consuming, it typically leads to interesting hypotheses which are difficult to prove. So if another researcher has already recorded interviews which cover a good deal of your topic, and would enable you to give a larger evidential base to your own interpretations, why not use them too? You will find it hard to find an answer to that question in the manuals. Nor are they likely to help you over the methods for reanalysing data qualitative created by other researchers. [2]

This is the silent space. As qualitative researchers, we love to meet and exchange ideas. But we are considerably less at ease about sharing our material. Quite often through our work at Qualidata we encounter sociologists who seem almost bonded to their own ethnographic fieldwork notebooks or interviews, feeling that nobody but themselves could interpret them sufficiently well or share the intimate understandings that they have of their informants. In terms of the archiving of qualitative research material, there have always been some notable exceptions, such as the ethnographic collections initiated in nineteenth century Scandinavia, or the work of Beatrice Webb on British trade unionism at the same time. And fortunately more widely attitudes among researchers have begun to shift substantially in recent years, in Britain stimulated especially through the Economic and Social Research Council's decision to set up Qualidata, the ESRC Qualitative Data Archival Resource Centre, in 1994. I have previously published my own account of the arguments for archiving and the establishing of Qualidata (CORTI & FOSTER & THOMPSON 1995; THOMPSON 1998; THOMPSON & CORTI 1998; THOMPSON 2000). [3]

Nevertheless, among qualitative sociologists—in contrast to social historians—there has been much less of a turn towards the re-use of data. Interestingly, a recent survey by Qualidata shows that only a small minority of qualitative researchers, 63 out of 542, state categorically that they would not use other people's data, mainly because they would not be happy with interpreting data without "having been there". The majority, by contrast, say that they are in principle willing to draw on other sources. Yet less than a third had attempted to do so. And equally interestingly, of those who taught qualitative methods over 90% were using their own material to do so, rather than drawing on alternative fieldwork material such as that created, for example, for a classic sociological study such as the interviews by GOLDTHORPE, LOCKWOOD and their team for *The Affluent Worker* (GOLDTHORPE, LOCKWOOD, BECHHOVER & PLATT 1968), or Stan COHEN's for *Folk Devils and Moral Panics* (COHEN 1972). In only illustrating method through data which they have collected themselves for their

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(HEATON 2000; HINDS & VOGEL & CLARKE-STEFFEN 1997; SZABO & STRANG 1997; THORNE 1990).

own less well-known studies, these methods teachers would seem to be sending an important implicit message to students: that research material, rather than public and testable, is essentially private and personal. [4]

So what is the root of this reluctance to draw on material created by other researchers? Is it simply an unspoken inhibition, a feeling that we may be cheating when we are not creating our own data? Or are there likely to be such difficulties in using material created by other people that it is scarcely worth the time to try looking at it? It is for this reason that I have felt that it could be helpful to re-examine my own experience of re-using data—both my own, and material created by other researchers. [5]

## **2. Starting from Labour History**

I have worked in sociology for over thirty years, but my first research in the early 1960s was as a social historian, and I still keep some of the influence of that early experience. Historians were and are still essentially jackdaws, scavengers: they use other people's detritus rather than create their own data. At that time historians got no training of any kind in research methods, but two things were always clear. First, you should be willing to make the best use you could of whatever sources you could find, whether in public archives, or in business offices, or in private house attics or sheds. And second, you should search for unknown new sources, for finding a significant new source was the biggest scoop you could make. [6]

My thesis was about the labour movement in London, and I was also writing about architectural history as a sideline. I certainly had my own high moments of discovery. I still have vivid memories of an old socialist bringing out precious minute books from his garden shed in a North London suburb; or of cold winter days in the grimy unlit cellar of a Birmingham glassworks, reading packets of letters from the 1850s, unopened for a century, in the fading daylight. One certainly had to be prepared to suffer in the cause, of course much less drastically than an anthropologist, but I do remember eating squashed flies in a cut bread sandwich offered to me by a Yorkshire vicar whose son was keeping a chicken farm on the glebe (the parish priest's land) next to the vicarage—and it paid off, because within an hour he had lent me another precious bundle of forgotten correspondence. [7]

As it turned out, one of the best sets of already archived sources for my labour movement thesis turned out to be in the library of the London School of Economics. The core was the deposit by LSE's founders, Sidney and Beatrice WEBB, pioneers of British social science, of their own papers covering their lifetime's personal activity, politics and research work. Beatrice WEBB had the foresight to keep her handwritten notes of the large number of interviews which she carried out with union leaders from the 1880s for their classic pioneering study of British trade unionism (WEBB 1894, 1920). These notes provided me with unique information, by far the best source on the earlier development of trade unionism in the capital. I did not realise the full significance of this

experience at the time, or recognise how the systematic quality of this material and the thoroughness of its scope were consequences of its being research data: for me, it was simply a very good source. But I did not forget Beatrice WEBB's example, and it was one of the early influences which set me on the path to Qualidata. [8]

### **3. First Experiences of Archiving and Re-using Interviews: Early Twentieth Century Family Relationships**

Most of my research in the last thirty years has focussed on family relationships, how they change, and how they relate to the economy and the community. Soon after I came to Essex, initially as a social historian, Peter TOWNSEND, the first professor of sociology at Essex, with whom I taught a course on social policy and social change, suggested to me that it might be worthwhile to interview older people for my research. And to help convince me, he showed me bunches of extracts from the interviews he had recorded himself for his first major book, *The Family Life of Old People* (TOWNSEND 1957). I remember in particular the vivid material on changing funeral customs. So here was a second experience of re-use. And it is good to know that those same interviews, along with the whole data from Peter TOWNSEND's lifetime of research, very likely the most in-depth documentation which will ever be collected of the conditions and experience of old age and poverty in Britain, a unique and unrepeatable set of qualitative research material, have now been archived by Qualidata in the National Archive of Social Policy and Social Change at the University of Essex. [9]

Initially, however, the influence of these interviews led me towards my first important experience of creating my own research material through interviewing. Commissioned to write a social history of Britain in the early 20th century, I wanted to write about the themes which a sociologist would have then highlighted for a portrait of contemporary Britain, including gender issues, childhood, youth culture, the informal culture of work and family leisure, sexuality and marriage. As a result of receiving a generous grant from the new Social Science Research Council in 1970, our research team were able to carry out some 450 interviews right across Britain with a quota sample of men and women born between the 1870s and 1906. [10]

These interviews provided the basis for a large part of my book *The Edwardians* (THOMPSON 1975). In practice it proved impossible to use the whole set, because there was much too much information to absorb. Nor was there appropriate software to help us: we sorted out the transcripts into themes using scissors and staples. I used the interviews not only in detail for cross-analysis, for example of childrearing practises such as punishment by parents, but also as a set of in-depth family portraits to illustrate the whole range of the social spectrum, from a landowning family at one extreme to a Welsh West Indian boarding house or an impoverished London labourer's home at the other; and two final portraits of women, a London socialist and a struggling Stoke pottery worker, as examples of the contrasting dynamics of social change, conscious and unconscious. [11]

We developed our interviewing method for this project initially from sociological influences. Right from the start we always used an interview guide, but equally importantly, we used it with a light touch, flexibly, giving as much space as possible to the informant to talk freely. It was only later on that we learnt that a comparable practice using the memories of the old—but without samples or interview guides, and then mainly focussing on "great men"—had evolved in the USA under the title "oral history". But by the later 1970s there had sprung up not only an international oral history movement, but also a parallel movement among life story sociologists launched by Daniel BERTAUX from France. This first project for *The Edwardians* was closer to oral history, in that we only collected detailed evidence from informants about their lives up to 1918, which was the period I needed for my book. In retrospect, I feel that this was a serious mistake, because the potential value of the interviews, for example for studying social mobility or intergenerational influences, would have been far greater if they had also covered the missing fifty years up to the present. It would have been better to have done fewer but fuller interviews. All my later projects have used full life story interviews, and I believe that this is the form not only most likely to reveal insights to the original researcher, but also for subsequent users. [12]

It was however clear to us from very early on that we were collecting a unique set of interviews of great potential value to others, and so we set up an informal archive in the department. The interviews were read over the next twenty years by large numbers of both students and outside researchers, and the result was a far larger number of publications by others than we could have ever achieved on our own—at least five times the output from the research. These include, for example, Standish MEACHAM's, *A Life Apart* and David CROUCH and Colin WARD's *The Allotment* on working class culture; substantial parts of Charles MORE, *Skill and the English Working Class* and Michael CHILDS, *Labour's Apprentices* on work; and John GILLIS, *For Better, For Worse* on marriage, as well as articles on class by Patrick JOYCE, on social mobility by David VINCENT, on education by Jonathan ROSE, on religion by Hugh McLEOD, on stepfamilies by Natasha BURCHARDT, and on women and the family by Ellen ROSS. Currently there is also important ongoing work by James HAMMERTON on marriage and by Richard TRAINOR on the British middle class.<sup>2</sup> [13]

- 2 Standish MEACHAM, *A Life Apart: the English Working Class, 1879-1914* (1977); Thea VIGNE and Alan HOWKINS, The small shopkeeper in industrial and market towns, in G. CROSSNICK (Ed.), *The Lower Middle Class in Britain*, (1977); Charles MORE, *Skill and the English Working Class* (London, 1980); Thea THOMPSON, *Edwardian Childhoods* (1981); Patrick JOYCE, *Work, Society and Politics: the Culture of the Factory in Later Victorian England* (1982); Ellen ROSS, "Fierce Questions and Taunts": Married Life in Working-class London, 1870-1914, *Feminist Studies* 8 (1982-3); Ellen ROSS, Survival Networks: Women's Neighbourhood Sharing in London before World War One, *History Workshop* 15 (1983), 4-27; John GILLIS, *For Better, For Worse: British Marriages 1600 to the Present* (1985); Hugh McLEOD, Religion: the Oral Evidence, *Oral History*, 14 (1986), 31-49; John SPRINGHALL, *Youth, Empire and Society: British Youth Movements 1883-1940* (1987); Natasha BURCHARDT, Stepchildren's Memories: Myth, Understanding and Forgiveness, in R. SAMUEL and P. THOMPSON (Eds.), *The Myths We Live By* (London, 1990), 239-51; Michael CHILDS, *Labour's Apprentices: Working Class Lads in Late Victorian and Edwardian England* (1992); Colin WARD, Allotments; Jonathan ROSE, Willingly to School: The Working-Class Response to Elementary Education in Britain, 1875-1918, *Journal of British Studies* 32 (1993) 114-38; David VINCENT, Mobility, Bureaucracy and Careers in Early Twentieth Century Britain, in A. MILES and D. VINCENT (Eds.), *Building European Society* (Manchester, 1993); Anna DAVIN, Growing Up Poor: Home and Street in London 1870-1914 (1996); David VINCENT, Shadow and Reality in Occupational History, in

Although in recent years the most important users have been other researchers, I have also had the experience of re-using my own material from a new perspective for a later project. This was the research on the experience of ageing, funded as part of the Economic and Social Research Council's Ageing Initiative which I carried out in the late 1980s, leading to the publication of *I Don't Feel Old* (THOMPSON, ITZIN & ABENDSTERN 2000). We also collected a substantial new dataset for this, interviewing up to three generations in a national sample of 'The Hundred Families'—a very rich source which, to my surprise, has not attracted re-users in the way that the set for *The Edwardians* does, although providing just the same type of information for the period since 1920. However, combining information from the earlier dataset with the new evidence undoubtedly gave us added strengths. [14]

A key issue for the new project was how relationships between grandparents and grandchildren were changing over the longer timespan. So the first advantage of the older material was that it went back twenty or more years further, with recollections of grandparents by informants born as far back as the 1880s. This meant that I was able to write a whole chapter in *I Don't Feel Old*, "At the Edge of Living Memory", on the experience of ageing in an earlier generation, through the point of view of grandchildren. A second advantage was more accidental. [15]

When I was researching for *The Edwardians*, I had not been interested in grandparenting as such, so that the interview guide did not include any questions about this topic. But the consequence was that the material on grandparents which did appear in the interviews was spontaneous, because people just wanted to tell us about them. With the later research, by contrast, we deliberately drew them out. So we could compare how far the two forms of interview brought in similar material—and reassuringly, it was indeed broadly similar. We were also able to evaluate from the earlier interviews, I think more clearly because they were more spontaneous, for how many people was a grandparent really significant? [16]

Being able to estimate the earlier significance of grandparents was one important gain from having an alternative dataset to re-use. We were also able to draw a good picture of older people's occupations and other sources of income in the late 19th century, a point on which contemporary statistics are not at all reliable. And we gained a lot of information too about mutual help, exchanges of help each way between the generations, the extent of influence of grandparents, and the varying ways in which grandchildren helped them in later life. None of this would have been possible if we had not kept the material, and we undoubtedly learnt a lot from it in new ways. So that was a very encouraging experience of re-use. [17]

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Daniel BERTAUX and Paul THOMPSON, *Pathways to Social Class* (Oxford, 1997), 98-123. Important forthcoming books drawing include James HAMMERTON, *Marriage and Masculinity in the Edwardian and Victorian Lower Classes*, and Richard TRAINOR, *The Urban Middle Class and Stratification in Britain*.

#### 4. Transnational Families: What Kind of Confidentiality?

I have stayed interested in intergenerational research, but in increasingly complex family situations. My most recent projects have been on stepfamilies, and currently on transnational Jamaican families. Both have involved the re-use of data created by other researchers. Let me take the Jamaican project first, because its implications in this context are more straightforward. [18]

The method of transgenerational life story interviewing which we had used for the Hundred Families was subsequently taken up by Mary CHAMBERLAIN for her study *Narratives of Exile and Return* (1997) of migration from Barbados, and then more recently she and Harry GOULBOURNE undertook a wider project on West Indian families in Jamaica, Trinidad and Barbados, using a similar approach (GOULBOURNE 1999). I became interested in these families myself because normally they were not only transnational, with the interesting consequences that geographical separation had for kin networks, but also intrinsically complex in structure, and very often multiple stepfamilies. I therefore thought that it would be very revealing to try to extend the research to include relatives in North America, where social conditions and constraints were different, and also to interview rather more members of each family, thus studying fewer families but in greater depth. We would also combine interviewing with participant observation. So currently we are recording members of twenty transnational Jamaicans in this way (THOMPSON & BAUER 2001). [19]

For our own project we have been able throughout to share ideas very constructively with Harry GOULBOURNE and Mary CHAMBERLAIN. Originally I had hoped that we might work even more closely together, by taking some of their own Jamaican families and adding new interviews with relatives in the USA and Canada. Unfortunately this proved impossible, since the original interviewees had been offered a guarantee that they would never be contacted again. While I fully sympathise with the wish to protect informants from disturbance which led to this promise, I feel that it was unnecessary and unfortunate to take such a drastic step. Effectively it fossilises the sample. Sometimes it is only later that one spots a crucial piece of information missing in an interview, which could be easily filled by a five minute telephone call. And there are also many instances in which further insights could be easily gained, either by the original team or by other researchers, through recontacting a sample or part of it, and expanding the information in the interviews on a particular theme. Indeed, this is precisely what some researchers want most of all when they are seeking to re-use previous research material. [20]

Clearly as a researcher one does have an obligation not to pass on the names of informants without their consent. Additionally, when one knows informants personally through interviewing, there are some whom one might decide to protect from any possible contacts by other researchers. But in principle, it is wise to leave oneself the freedom to recontact on one's own behalf, or to ask informants whether they would be willing to be contacted for a different project. They can then make their own decision. It is important to remember that for many

people in-depth interviewing, with the experience of being listened to seriously, are very positive experiences. [21]

I would add that the concerns over confidentiality with a research project of this kind raise parallel issues. It is not really possible to completely conceal individual life stories, and still more whole family dynasties, without changing not only the names but also so many of the details that they lose their intrinsic validity as evidence. It has to be accepted that individuals and their intimate kin would sometimes be able to recognise themselves. Hence the protection which can be offered is somewhat different from the conventions of a large-scale quantitative survey. Firstly, the researcher has to accept an ethical obligation to write about informants with an appropriate sensitivity, avoiding disparaging terms, and when information is particularly sensitive—for example, about crime or sexual abuse—disconnecting it from the main citations of the case. [22]

Secondly, in fieldwork we avoid offering more than we can deliver. We explain to informants, when offering them confidentiality at the start of an interview, that this means that we will not use their name or identify them in any publication from the research. Then at the end of the interview the release form which we ask them to sign is worded:

I agree that my interview recorded by ... on (dates) ...

(a) may be used by the "Transnational Jamaican Families Project";

(b) may be offered to an archive for the use of future researchers and writers.

I do not wish my name to be used in any publication. [23]

It should be noted that the last sentence is an option, and some informants choose to delete it. They rightly regard their life stories as their own, feel a pride in them, and want their authorship acknowledged. I feel strongly that they should be able to make that choice. [24]

## 5. Re-analysis as Piloting

Because the GOULBOURNE-CHAMBERLAIN informants had been given a more draconian protection, we could not build on their families for our own research. Nevertheless they did prove very useful, by offering a kind of pilot material. I therefore spent two days reading the interviews from their Jamaican families. I found this very encouraging, because I could quickly see the richness of information which would be available. And there were immediately fascinating details, which gave me ideas for what we might find ourselves. [25]

One instance was of the sons of a Jamaican carpenter who had migrated to England. Sigurd, the son who was interviewed in Britain, said that he had little to do with one brother living nearby in London, but by contrast was in close contact by phone and letters with the other brother who was over in North America, describing how "we've been like twins". And I was very struck by how strong the

bonds between members of a transnational family could be, for two brothers so far apart to express the feeling of being so close. [26]

There was another family, where the interviews included a marvellous description of the grand family reunions which they held every couple of years. They had started these reunions in Jamaica, and subsequently continued in North America. On the Sunday, they all go to church, and then on the next day, they set out thirty tables out on the beach, and cook a great feast of Caribbean delicacies. That again fascinated me, and made me hope to find a family which would welcome me to participate in an occasion like that! [27]

It has turned out that large family reunions are not uncommon, but a third instance is of a rarer detail. In Jamaican families, as in many others, one often finds that there is a particular person who acts as guardian and holder of the family memory, an anchor figure. But that is a less easy role in a transnational family, and in one of these CHAMBERLAIN-GOULBOURNE interviews I found that there is a grandmother who lives in Jamaica, who acts as a kind of transnational anchor, if that is conceivable. To quote,

She travels quite regularly, though. She's got kids living all over America—Florida, New York, California—so she travels to see them quite often. And she'll travel about two or three times a year, every year, to different parts of America, visiting California, Florida and New York. Sometimes all three in one go, visiting her brothers and sisters, nieces, nephews, or children and grandchildren. [28]

So here, with the geographical stretching of the family network, the anchor has evolved into a frequent-flyer granny! [29]

It was especially valuable to look through these interviews at the start of our own project because we originally modelled our own interview guide quite closely on that used for the CHAMBERLAIN-GOULBOURNE project. Effectively, it saved us a pilot stage. And we hope that when we get further with analysing our own material, we can come back to look at their interviews again, because on many issues that will almost double number of cases which we can look at. [30]

I would want to emphasise, however, that starting from the seedbed of another project is in no way a straightjacket. We quickly modified the interview guide as our own ideas evolved. We decided, for example, that we needed to interview some people—typically poorer relatives still in Jamaica—who had kin in other countries but were out of contact with them, to see how they might be significantly different. We have also become interested in the possibility of investigating in depth one or two instances the "mega-families" of cousins which can be found in many Jamaican villages, to try to understand the different fates of different branches. [31]

Perhaps most fundamentally of all, it was only when I got into the field that I fully realised that the very complexity of these families made the three-generational method which I had previously used impossible to use straightforwardly. These

Jamaican kin networks are not "families" in the British sense at all: they slide off much more rapidly in all directions. Partly because they are often held together by women rather than men, the members may all have different family names. And while with every family tree, the ego is the centre, and depending on where you put the ego, you get a different pattern, the shifts here are much more extreme. For instance, in one of the first families that I got to know about, there were four sisters, each with different fathers. If you try to draw a family tree like that on a piece of paper, you can't do it. And then one of the sisters will say about the relatives of her sister's father, "They're not my relatives". And another will discover her lost father in mid-life: so is this father, who has shared none of her childhood or her adult formation, never been a significant other or an influence of any kind, someone who ought to be interviewed for her family? [32]

So this project is proving very revealing, not just about transnational Jamaican families, but more generally about who belongs to families and the patterns which are evolving in a world in which remarriage and transnationalism are more and more common. But in all this I am in no doubt that we have gained immensely from being able to share both ideas and research material with Mary CHAMBERLAIN and Harry GOULBOURNE. [33]

## **6. Interacting with Longitudinal Quantitative Data: Stepfamilies**

The last experience which I want to discuss is with the research which I carried out with Gill GORELL BARNES, Gwyn DANIEL and Natasha BURCHARDT for *Growing Up in Stepfamilies* (1997). For this project we were particularly anxious to secure a reliable sample base, because although a considerable amount of research has been carried out on stepfamilies in Britain, with very few exceptions this has been based on unreliable sampling methods, such as snowballs or clinic-based samples. It was therefore a crucial assistance to be able to base our work, through the help in particular of John BYNNER, Peter SHEPERD and Elsa FERRI of the Social Statistics Research Unit, City University, to base our work on re-interviewing a subsample of the cohort, all born in March 1958, which has been followed longitudinally by the National Child Development Studies people. We also hoped that this would bring the added strength of being able to check our hypothesis against some of the evidence which they had collected earlier, both for our own informants and for the wider sample. So we had a double hope from this co-operation. [34]

As it turned out, our main success was in securing an unusually good sample base for our own fifty in-depth interviews with men and women, by then in their thirties, who had grown up in stepfamilies. It was, moreover, particularly reassuring for this type of co-operation that NCDS were subsequently able to compare the response rates for their own next wave of interviews between those we had interviewed and those we had not, and this showed that, in the next wave, the people we had interviewed were much more likely to say "Yes" to being interviewed again, than people we hadn't. Thus some of the resistance, which as a qualitative researcher I have encountered from survey researchers when suggesting in-depth re-interviewing, is based on a false assumption. These

NCDS informants had never, over thirty years, been given the chance to tell their full life stories, and most of them valued the chance to do so. Because we had taken an interest in them, and listened to them, it made them more likely to want to go on being connected with the main cohort survey. [35]

A second interesting issue which arose, and which also suggests to issues on which qualitative and quantitative researchers surely ought to combine more often, was that our interviews revealed that one fifth of the informants had been seriously misclassified in terms of the timing of their membership of a stepfamily. We had wanted all of them to have entered a stepfamily between the ages of seven and sixteen, so that we could hope for a good account of the transition which this entailed. But in fact it turned out that ten out of the fifty were not what we had requested. Most of these them had already been stepchildren way before the age of seven, typically since infancy. It seems clear enough that they are misclassified because their parents were misleading NCDS as well as the children themselves, and it was only when the child was older, so that for example when he or she needed a passport, that the truth emerged, because there was a different name on the Birth Certificate. We found they were an interesting category of stepfamily, for typically relationships were excellent when they were children, but then there was huge and sometimes highly damaging explosion in late teenage, when they discovered that that they had been deceived all their lives. There was also one remarkable instance of a woman who was not a stepchild at all: she was growing up with her natural parents. She had been conceived outside marriage by her mother, and on subsequently, when her then husband died, she was able to re marry to this child's true father, but she never admitted that this was what she had done. So this child was brought up with her mother saying that her father was the first husband, while the new husband, who was her real father, always hinting in a rather covert way that it was him. The informant even remembers as a child her mother prompting her during NCDS interviews. [36]

Instances such as this made me think a lot about the NCDS sample, and how it could be very valuable to carry out more in-depth to understand not only some of the misclassifications in the early data, but also, perhaps more importantly the social processes involved in longitudinal interviewing over many years, and why some people dropped out and then stayed out or returned to the study. We were struck by how sometimes, when it proved impossible to trace one of the interviewees from the sample, it seemed clearly related to their way of life: a man, for example, who was a lorry driver rarely at home, or another man who we were able to trace up to the moment when he entered a drug rehabilitation unit, but no further. Could it be that in order to maintain membership of a longitudinal study, or a panel study, you have to have a coherent life? This seems an extremely important issue for longitudinal studies to address, but one which they seem to have usually shied away from. [37]

The other hope of this co-operation proved wholly unsuccessful. NCDS had collected very little earlier qualitative material. The main exception was when they asked the children at the age of eleven to write essays on what they expected

they would become as adults. I was able to look at this material. But apart from that, there seems to be nothing except the figures. We had hoped to be able to go back to some of the earlier questionnaires, but it does not seem that these have been kept, so that any notes scribbled by the original interviewers which might have illuminated our more dubious cases were lost. [38]

Nor were we able to make any headway in testing out our hypotheses on the wider group of stepchildren which they had interviewed. Perhaps this is partly because of the form in which this information is held. Being used to discussing and analysing individual cases, we were surprised to find it was not possible to print out the coded information for each informant over the successive surveys. On reflection, I have begun to see that this has been in part because of different professional assumptions about confidentiality and evidence. The only cast-iron guarantee of anonymity is to destroy the evidence of fieldwork interview schedules and scramble all the coded information so that it becomes impossible to link up all the details obtained from a single individual. But for good qualitative research it is absolutely essential to be able to consider each case as a whole. [39]

This sense of an unacknowledged gap in professional expectations was reinforced when we considered depositing our interviews with NCDS, so that future researchers could analyse the two types of material side by side. I regret that they should very little interest. They wrote to suggest that the interviews should be totally anonymised like quantitative material. We explained that this would effectively destroy the interview evidence, but suggesting various other options to protect confidentiality, but sadly, we never heard again. As a result, our interviews have been archived independently. So this last experience of re-use has been a mixed story. [40]

## **7. Looking Ahead: Creating Systematic Qualitative Resources for the Future**

There are, in short, many very important gains from re-analysis. At the start of a research project, it can be invaluable in providing a sense of the topics which can be successfully covered in interviewing, and therefore make the pilot stage of the new project both more effective and also much swifter. At a later stage a comparable interview set may also provide a crucial wider sample base for testing the interpretations which are emerging. Finally, by making your research data available to re-analysis by others, you may strikingly multiply the outcomes from your research through the publications of others from the same material which you have created. [41]

It is curious therefore that while reanalysis is a normal process for survey sociologists, and despite the existence of important precedents for the archiving of qualitative research material—as in most Scandinavian countries, or by pioneering social science researchers in Britain such as the WEBBs—there is an extraordinary silence in the literature on qualitative research methodology about

the issues involved in re-analysis and the types of dataset which have proved most valuable. [42]

To sum up my own experience hitherto, I would argue that the most valuable qualitative datasets for future re-analysis are likely to have three qualities: firstly, the interviewees have been chosen on a convincing sample bases; secondly, the interviews are free-flowing but follow a life story form, rather than focussing narrowly on the researcher's immediate themes; and thirdly, when practicable re-contact is not ruled out. [43]

Let me conclude by looking forward to the future. I have no doubt of the immense potential for re-use in many existing qualitative research datasets which have already been archived. The continuing demand for access to the original interviews recorded for *The Edwardians* over a period of well over twenty years has been a particularly compelling instance. But I am sure that a major reason for this is that those interviews are based on a national sample, are rich in detail, and are thematically organised. This is the kind of dataset which is always most likely to be useful to others. It is of course very important not to throw away unique data gathered by more specialised projects, and the new ESRC datasets policy now ensures that much of this is archived. But this inevitably results in an arbitrary spread of material. I believe that we ought to include among our basic research strategies the systematic collection of sample-based life story data. And there are indeed some precedents for this, both in Britain and abroad. [44]

In Britain, for example, there is a striking recent instance in the very large set of some 6,000 in-depth recorded interviews collected by the BBC for their millennial year local history programmes. These were selected on the basis of a rough quota sample of age, gender and social class in all parts of Britain, based on a thematic interview guide, and included the collection of basic social information and a summary of the interview for each informant. Qualidata have helped in the writing of the interview guide and also in the indexing of this important new dataset, which became available in May 2000 at the British Library National Sound Archive. [45]

A more sustained instance is provided from Sweden. In Stockholm the Nordic Museum Archive, with a staff of 250, provides a national service for museums encompassing libraries, photographs, exhibitions and objects. It has a separate "Memory" section, led by Stefan BOHMANN, with a staff of ten. The archive has been collecting material resulting from the researches of academic ethnographers—who in the Scandinavian case have filled the role more typical of sociologists in Britain—for almost a hundred years: substantial in quantity, well kept and indexed, and regularly used in an attractive reading room. [46]

Significantly, I was informed that the archive's experience is that such research material was most likely to be useful when the original words of informants were recorded; rather than merely summarised. In the latter case, they were too much reshaped by the academic preoccupations of the time and the needs of the particular project to be of much research interest to contemporary ethnographers. [47]

Still more strikingly, the archive has been organising regular autobiographical competitions since 1945, and since 1928 it has been collecting special thematic essays from a panel of four-hundred correspondents right across Sweden. The themes have gradually shifted from earlier historical preoccupations to encompass all aspects of contemporary everyday life, including even computing. The archive also has a notable collection of diaries. All the basic thematic and autobiographical data is well indexed and this has now been computerised, so that very full information on the collection in Stockholm can be available at educational and library centres throughout the country. [48]

This Swedish system offers an impressive example of what could be achieved in most countries if research money were to be put into the creation of general qualitative research resources as well as towards specific new projects. I believe that we need to give a much higher priority to such initiatives for the creation of basic in-depth datasets for the general use of qualitative researchers for the future. Indeed, I see such a structural initiative, which would enable any new qualitative research to take places alongside "re-use", to be a fundamental need, not only for the qualitative approach in sociology, but more generally for the continuing vitality of the discipline itself. [49]

Given that very large sums of money are dedicated to collecting national longitudinal and panel study survey material as general resources for researchers, and also that the in-depth interviewing of sub-samples can be shown to be supportive of the survey and its interpretation, it is difficult to see any convincing intellectual or methodological justification for not attaching, as a regular practice, sub-samples of qualitative interviews to such surveys. My experience has suggested that it will need a shift of attitudes on both sides, and in particular a greater mutual respect and consideration for different research traditions. But the potential would be enormous. It would allow quantitative sociology to become surer of its sample base and its interpretations of informants' behaviour, and also far richer in its power of illustration. It would give qualitative sociologists the chance to make controlled comparisons outside their own group of interviewees and to test their hypotheses on convincing samples. And it would also help to bridge the increasingly disastrous split down the centre of sociology between policy-orientated statistical survey research on the one hand, and on the other a qualitative sociology which, I would suggest partly because it no longer feels the power to do so, has less and less interest in engagement with the "real" world.<sup>3</sup> While the danger in quantitative research has always been to impose meanings of social behaviour without the evidence which comes from listening sufficiently to informants, the failing of much qualitative work in the postmodern or narrative modes has been to make the interactive research process the centre of study in itself, and forget what can be learnt from the stories which are told. If we could convince both traditions of the value of re-use, and then move forward towards creating the kinds of linked data which would be of the greatest mutual value, I believe that we would release a powerful reinvigorating new force in social research. And that is my hope for the future. [50]

3 Increasingly abandoning the home field of sociology to more enthusiastic fieldworkers from other disciplines: social geographers especially, but also anthropologists.

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