Researching family and social mobility with two eyes: some experiences of the interaction between qualitative and quantitative data

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Drawing on my own research experience, I argue in this paper for the advantages of a middle way in research which brings together the strengths of both qualitative and quantitative methods. This avoids the all too common tendencies in contemporary sociology to argue either from a very small number of individual biographies, or from sets of statistics not supported by in-depth research: research using one eye rather than two. This middle way has long been a familiar approach among historians, and also with a number of distinguished pioneer social researchers—including here Raymond T. Smith, Janet Finch and Peter Townsend—with whom I have recently recorded life story interviews. I also reflect on my own experience of some of the practical difficulties of using life stories drawn from large-scale samples and linked to quantitative studies, and in particular a recent study of stepfamilies based on the National Child Development Study cohort. I conclude that the philosophy of research needs to be changed, so that instead of a hard method and a soft method keeping their distance from each other except at the initial or concluding stages of research, the expectation should become a zigzag of mutual exploration: a sociology using both eyes to the full.

Introduction

Only a few sociologists would openly deny the logic of combining the strengths of both quantitative and qualitative methods in social research. It builds in triangulation to a research project, bringing additional hypotheses and standards of proof: it represents navigating with both eyes, rather than with one eye blindfolded. The combined approach has many distinguished precedents among pioneering researchers. It is also favoured in principle by many contemporary researchers. Daniel Bertaux and I have argued at length for it in the field of family and social mobility in our book *Pathways to Social Class* (1997).

In practice, however, despite such wider methodological aspirations in principle, social researchers have regrettably become increasingly divided

into two camps, many of whose members know little of each other even if they are not explicitly hostile. On the one hand, there are well-funded survey researchers who manipulate their statistics as ‘facts’, interpreting them often with the insights of little more than commonsense hunches. On the other hand are lone researchers who never have sufficient large numbers of interviews, or have drawn those interviews from sufficiently representative samples, to substantiate any of the hypotheses they may generate from the in-depth interviews which they carry out themselves, and who—in part reflecting a sense of impotence—often abandon any intention of interpreting society ‘as it really is’, instead shifting to post-modern or narrative approaches, in which the interview text replaces society as the focus of study. These two camps are reinforced by self-recruiting networks, and by occasional outbursts of mutual hostility. As John Scott suggested at a recent seminar on ‘Researching social mobility’, one might envisage the situation in terms of anthropological tribal divisions, with the qualitative/quantitative divide:

In the nature of a totem rather than a real distinction. . . There are bands who go around gathering their data in a qualitative way, and there are others who carry out large-scale hunting expeditions with their surveys. Each band is rather autonomous with very few links, apart from occasional periods of warfare and sporadic raids on one another’s cattle!

He suggested that the one important difference from the tribes studied by anthropologists might be their tendency to self-perpetuation rather than out-marriage:

The stratification bands don’t observe the taboo against incest. On the contrary, there’s a great deal of self-recruitment and very few people actually from the outside. And relatively few alliances. And these groups are patrilocal, and when people do make alliances, they have to go and live with their new father within the group!

Beyond such divergences of principle and of personal allegiance, their separate research cultures have also resulted in crucial differences of practice in the handling of data which have resulted in yet further obstacles to cooperation. My purpose in this brief article is to reflect on both the gains and the difficulties which I have experienced in thirty years of attempting a middle path in research on families and social mobility, striving throughout to keep both eyes open.

I want to emphasize to begin with that it is indeed for a middle path that I am arguing, rather than wishing to criticize one or another method. In this spirit, in the introduction to *Pathways to Social Class* (1997), Bertaux and I set out our ‘dream of a new study of social mobility’, which ‘must depend upon a broadening of methodological and theoretical perspectives’ as opposed to a research approach entirely defined by the technical requirements of survey reproach. The survey method, we urged, should be:

Used to do what it can do most efficiently, rather than aiming at reducing other methods, with different strengths, to its own ways of seeing. . . which is why the approach of the case study—which, conversely, has its own built-in limits—is vital for the full development of the field

(Bertaux and Thompson 1997: 1, 6)
Moreover, for both of us these are views embodied in our research practice. Thus Bertaux was trained in quantitative survey sociology and has always emphasized the importance of systematic sampling (Bertaux 1977, 1991). I myself first used in-depth life history/oral history interviews at the end of the 1960s, influenced by my sociological colleagues at Essex and especially by Peter Townsend, and for some of my projects I have also used a limited amount of participant observation, joining a Scottish fishing crew on the mackerel fishing, or staying with a family in a rural Jamaican village. But I have also always tried to use such qualitative approaches alongside quantitative methods and evidence. In all my projects I have used clear sample frames, ranging from the census-based national quota sample for the 444 interviews of *The Edwardians* (1975) to the subsample of a national longitudinal cohort which we used for *Growing Up in Stepfamilies* (Gorell Barnes *et al.* 1997). In *The Edwardians* I also include numerous tables and graphs of statistical information from other sources (Thompson 1992: 4, 6–7, 63, 159–160, 266–268, 275–278, etc.).

My use of the sample of interviews in *The Edwardians* also deliberately combines quantitative and qualitative approaches, on the one hand for cross-sectional analysis, and on the other as individual illustrative stories. Thus in the first section of the book the whole set of interviews are used for a series of chapters on the life cycle, from childhood and childrearing through to youth, adulthood, marriage and old age. The fact that my analysis was based on a representative national sample gave unusual strength to my criticisms of some of the historical myths then current, such as the false assumptions that companionate role-sharing marriage was a new development since the 1950s, or that earlier generations of parents, in contrast to the gentleness and emotional intimacy of contemporary childrearing, were uniformly harsh and distant (on the latter point especially, in fact we found sharp regional differences in earlier patterns).

Following this life cycle analysis based on the whole sample, however, a second section of *The Edwardians* uses only 12 interviews to create a set of portraits of particular families, deliberately chosen to contrast different social classes from the daughter of a landed aristocrat to the children of unemployed labourers. One of these is the family of Harriet Vincent, daughter of a West Indian migrant, who represents a group not statistically significant at that time, but deliberately chosen as a symbol of a key change ahead. At the end of the book, to illustrate a section on the theory of social change, I use two more portraits, both of women, one a story of failure representing unconscious pressures for change and the other, the daughter of a London socialist artisan who became mayoress of an East End borough, representing articulate political action. This multi-method approach thus enables the same sample of interviews to be used both for quantifiable generalizations, and also for the illustration of the argument through carefully chosen strikingly contrasted examples.
Debating

For us, this kind of work is a midway method rather than standing on either extreme. We have not been willing to accept a sociology entirely based on unrepresentative individual biographies, however subtly analysed, as in the narrative method developed for some years by Gabriele Rosenthal and others and more recently espoused in Britain by sociologists such as Prue Chamberlayne and Michael Rustin (Rosenthal 1991, Chamberlayne 2000). But watching John Goldthorpe and his group dig themselves deeper and deeper into a pit of their own making, to the point of arguing that social mobility research is best conducted by excluding women—more than half of the population—because of the problems in categorizing them, we have been equally unhappy at the constraints brought by a statistically-driven research.

The result, perhaps not surprisingly, has been vehement criticisms from two fronts. Thus from one side there have been emotive exchanges in Biography and Society (1996–1997), the newsletter of the International Sociological Association’s Research Committee 38, with Gabriele Rosenthal graphically describing Bertaux as a giant fish tempted by ‘the fly of “naïve realism”’, who has ‘swallowed it, and like a huge salmon who has gotten hooked on the fishing line, is fighting for his life’ (Fischer-Rosenthal and Rosenthal 1997: 6).

On the other front, Goldthorpe has been so eager to defend his purely statistical approach that he has rejected the relevance of cultural or of political difference, asserting that social mobility in Japan and Britain and the Soviet Union have been essentially identical processes and that neither culture nor communism have any impact on the essential patterns of mobility. Stein Ringen (1998) in a recent review essay suggests that the methodological key to such implausible findings is a technical device, so that statistically allowance is made for fundamental structural changes such as the expansion of education:

If the approach is first to ‘allow’ away what changes, then, obviously what you subsequently see is no change… A scientific truth is one which stands when tested from different angles. A truth visible from only one angle, which disappears when one looks from a different angle, is not a scientific truth. It is at best a hypothesis, which should be tested from different angles (Marshall et al. 1998, Ringen 1998).

Nevertheless, with a similar determination Goldthorpe has continued to focus his research on men as relatively easily categorized representatives of households, and hence to deny that anything significant can be learned from the social mobility of women independent of men, or that variant family structures have any relevance to mobility—even though there is plenty of research evidence to the contrary (Goldthorpe 1983, and replies). It has been shown, for example, that women cluster in different parts of the occupational structure from men; that their mobility paths are different and cannot be predicted from their partner’s occupations; that a woman’s fertility is more influenced by her own occupation than her husband’s; that birth order is related to mobility; that certain family structures such as large families inhibit mobility, and that the impact of divorce on mobility is
almost opposite for men and for women (Gittins 1982, Abbott and Sapsford 1987: 47–87, Dex 1987, Marshall et al. 1988, Thompson 1997). Nevertheless, at least in seminars if not in writing, as I have myself experienced, Goldthorpe will go as far as to deny that there is any topic at all which can be investigated by qualitative research which cannot also be more effectively researched quantitatively.²

I have come increasingly to the contrary view: that neither quantitative nor qualitative research can reach its full potential without drawing on each other’s strengths. This is true both in terms of designing research questions, and of interpreting evidence. It is also often true of problems of sampling for which the two traditions have sought different solutions, on the one hand the problem of non-response in random sampling corrected by weighting, on the other the arbitrariness in the choice of in-depth informants structured through purposive sampling. These two branches of empirical social research even have to recognize a parallel weakness in their attempts to represent society. I believe they could and should help each other in all these ways.

Three constructive pioneers

There have of course long been outstanding researchers who have shown how qualitative and quantitative methods can be effectively combined. They include among the founding fathers Marx, and among early pioneering researchers Charles Booth and Seebohm Rowntree in Britain, and Robert Park and the Chicago school of urban sociology in the inter-war years in the USA. For more recent examples, I would like to highlight the work of three researchers whom I have recently recorded as part of the ‘Pioneers of Social Research’ project launched by Qualidata.³

The first is the anthropologist and demographer Raymond T. Smith, whose work culminates in his magisterial book *Kinship and Class in the West Indies* (1988). Smith’s own researches in Guyana and Jamaica are based on a combination of statistical demography, participant observation, and over 170 extended in-depth family interviews. He opens his book with an evaluation of all the research work on kinship and class in the Caribbean which has been carried out since around 1945. He has one section called ‘Quantitative analysis and the refinement of error’, in itself a telling phrase, in which he demonstrates how once you get locked into a one-eyed perspective, you can go on proving and re-proving the same finding, but if it is based on false assumptions, the further calculations simply replicate the original error in new forms.⁴ Thus in terms of his own work on Caribbean families, a fundamental problem has been that until recently the concepts of family and household used by demographers and the census have been based on European assumptions—notably reinforced through the international demographic research promoted by the Cambridge Group. Thus it has been assumed that normally on forming a sexual union a man and woman will become married and set up house together; and that normally households constitute families. But in the Caribbean in the last 150 years marriage has never been more than a minority practice,
and usually relatively late in life; while because of the prevalence of visiting
unions, migrating long distances to work and informal child fostering by
kin, the boundaries of households and families can never be taken as
identical. These cultural differences could only be understood by in-depth
fieldwork; and failure to take them into account had meant that the wrong
categories were being used for interviewing and analysis in quantitative
studies, and had thus vitiated decades of serious demographic research.

A second example is the work of Janet Finch, one of the pioneers of a
feminist approach to sociology in Britain, on family obligations, kinship
and inheritance. Her earlier work for *Married to the Job* (1983) had been
entirely qualitative. She explains how she now aimed at a mixture of
approaches, and—as a vivid illustration of the inhibiting effect of the ‘two
camps’ in methodology—how she feared being accused of betrayal:

The Family obligations study had two parts: it had a survey and a qualitative study. And I
actually can remember, very vividly, the moment when I decided that that had to be. I originally
assumed that it would be based on qualitative interviews, (as in) all the previous research that I’d
done...I was very much committed to qualitative research, both as a sociologist and as
somebody who might potentially contribute to social policy. And I can remember the moment
when I realized, with something of horror, that the research questions I was asking could not be
fully answered unless I had some quantitative data to answer them...You know, how frequent,
how many, how common, etc.... And so with some anxiety that my friends would disown me, as
it were, I planned a project which was a mixture of a survey and the qualitative interviews...
Most of my friends were qualitative researchers, and heavily committed to promoting the
importance of qualitative research. It was the survey side that I thought my friends might disown
me, yes, for doing a survey. But...in fact, my friends didn’t disown me, I’m happy to say!
(Finch, interview, 2001: 65–66)

In the event, she used a subtle fusion of the two research approaches. First,
in order to define the questions more precisely, she took a year surveying
the literature and writing *Family Obligations and Social Change* (1990). In
the subsequent fieldwork, on which *Negotiating Family Responsibilities*
(Finch and Mason 1993) is based, qualitative and quantitative methods
were intertwined.

The survey sought to show what were the most typical attitudes
towards kin in need, but it the interview took an unusual form—‘it’s a
survey designed by a qualitative researcher, if you like’—by replacing the
normal attitudinal questions, asking what the interviewee would think
about a situation, with vignettes about other people: ‘Joe Bloggs is in his
fifties...What should he do in the circumstances?’ (Finch 1986). The
survey also provided a sampling frame for the qualitative interviews, from
which a ‘theoretically driven’ sub-sample was selected. This consisted of
interviewees who were most likely to be in the process of negotiating with
their families, such as young people, or those who had been divorced or
separated, so that concepts of family responsibilities would be ‘nearer the
surface’. In some of these they went on to further interviews with up to a
dozen other family members, and again these cases were chosen on a
theoretical basis, and in particular to test an emerging hypothesis by trying
to disprove it (Finch, interview, 2001: 67–77).

The third example is that of Peter Townsend’s 50 years of research on
poverty, families and ageing, primarily in Britain but also internationally. He
has been a key inspiration for my work ever since I came to work with him at Essex in 1964. More recently, through Qualidata we have been able to archive his lifetime’s research material in the National Archive of Family and Social Change at Essex, so that it is now available to all future researchers. At the same time I carried out a 20 hour interview with him (my longest ever!) about both his personal life story and his development as a researcher. One of the strongest themes in this interview is his continuing belief in the need to combine a broad quantitative perspective, while at the same time always seeking to refine his statistical approaches through in-depth fieldwork.

Peter Townsend was originally trained as an anthropologist with Meyer Fortes in Cambridge, and some of the lessons that he learnt there have stayed with him all his life. But at the same time few social researchers have been more committed to advancing quantitative methods, and his achievements in this respect are a particular point of pride. For instance, for *The Last Refuge* (1962) he developed new sampling techniques for institutions, and key quantitative measures of poverty and for disability—the latter originally called the ‘measure of incapacity for self-care’—which remain the basis for those which are currently used internationally: ‘I regard this as one of the most original features of the work’. He was also innovatory, at a time when the potential of quantitative analysis with computers was far more primitive and restricted than today, in his belief in exploring the potential of quantitative analysis of his data through a ‘dynamic interchange with the programmer’, and the need, after the basic categorizing and counting of the data, for a third stage of creative quantitative analysis. ‘The third stage is the really creative stage where you are checking your conclusions...using your statistics to the hilt’ (Townsend, interview, 1997–1998: 89, 105, 122).

Yet always, alongside his quantitative research, he has carried on a parallel ethnographic or anthropological approach. In his first work in Bethnal Green, for *The Family Life of Old People* (1957) he became a friend and regular visitor to many of the families whom he was studying, participating in their holiday outings and their weddings and funerals of the people. For *The Last Refuge* he not only personally visited large numbers of Old Peoples’ Homes, but in one case took a temporary job as a bath attendant at Newholme in Manchester.

The research team had already completed its typical three-day research investigation, looking at every part of the buildings, and interviewing some dozen of the residents. But Peter was able to stay on ‘like the good social anthropologist’ and ‘witness the place not being conducted round it at times chosen by the management’—a chance he thinks would be much rarer today. So he was able to become an attendant, ‘looking after the bathing of the old men’. And this had a powerful impact on his understanding of the inmates. He was particularly struck by:

The extraordinary abject passivity of some of these elderly men. It was almost as if the process of institutionalization had forced upon them how to behave, and how to preserve their integrity only within...We have an inside life and an external life, and some of them communicated this by talking a bit about their past and about their feelings for their loved ones.

But mostly it was feelings of being bereft and being abandoned. It was also feelings of being neglected by staff, who didn’t want to know, and didn’t have any forms of communication. It was
almost as if they were... in a prison where the doors were locked at such-and-such hours, and you were only let out at such-and-such hours. They led a routine which was amazingly limited...

But it was also physically seeing these very thin, many thin characters, how gentle one had to be in using one's elbow to make sure the water wasn't too hot. And how little they had in the way of personal belongings, how the underpants they had, the kind of combinations they were wearing, had six, seven, eight, ten laundry marks, or tabs attached, which almost underlined their loss of identity. They were just numbers in an institution, where they didn't even possess underpants of their own...

Yes, of course there were those characters, some of them had enormous resources in terms of personality, but they'd turned it inwards, in order to—oh, I don't know, make sure that they got another slice of toast at breakfast. They knew that conformity was the order of the day... It was the old tradition of the workhouse...

(Townsend, interview, 1997–1998: 97)

Peter Townsend still continues with this two-pronged research strategy today. In a current national project he has been working with the social geographer David Gordon, statistically mapping wards with the highest concentrations of poverty, but then focusing in to a local area and individual families with a qualitative investigation. He sees this in-depth work as essential before any social policy conclusions can be suggested, because the causes of poverty can differ drastically between areas: ‘indeed, a qualitative study is absolutely necessary, not just as a complement, but as a necessary part of the investigation’, in order to relate the local and ‘individual experience, and the general structure. To move between the two is almost an inescapable principle of the research exercise’ (Townsend, interview, 1997–1998: 153).

The collaborative evaluation of sampling

The two types of method are interactive in Townsend’s work in another crucial way, in the evaluation of sampling. He argues that there are serious research pitfalls in the standard evaluations of survey samples:

Generally speaking, people engaged in research tend to fall into the conventional trap of finding that if they get 10% or 20% refusals... this doesn’t matter too much because the great majority have answered. And if they check on some very crude criteria of the distribution of age, or the distribution of occupation, that if it roughly reflects the distribution in the population, that’s all right.

But I learned, to my cost, first of all in the studies of the elderly, that you (could be) missing, even among a small percentage of refusals or non-contacts, people who really mattered when it came to generalization. In the case of the elderly... there were people who were confused, or so severely disabled, or sick on the particular days, that it was impossible to arrange an interview. It was quite clear to me that we were under-estimating the severely disabled, and especially the mentally disabled among them. And therefore I tried to counterbalance that to a certain extent.

(Townsend, interview, 1997–1998: 127)

He tried two devices for this counterbalancing. One was full ‘proxy interviews’ with people such as kin or carers who knew the non-respondant well. The other tactic, which was used for the Poverty Survey, was to try to gather some key information at the time of refusal:
That led me to believe that when we carry out interviews and sampling, it's extremely valuable to obtain as much information about those who are not present when you call at an address, or those who refuse, or more likely, people who refuse on their behalf. Because it's usually in those cases a husband or a wife, or a mother or a father, or another relative who deters you from meeting the informant you want to address. So I began to develop...a mini-questionnaire for the interviewer to complete. This obviously covered straight forward issues like the type of house, and then ownership of the house...how many people were there, what kind of age they were, and anything...clearly not controversial or disputable...roughly, that could be described about them, even to the extent of an occupation, (that) could easily be an extra item among the responsibilities of the interviewer to carry out. (Townsend, interview, 1997–1998: 127–128)

The question of what is missing from survey samples is clearly a crucial issue which can only be effectively researched with a combination of methods. Townsend is well aware of the need for continual vigilance in his studies of deprivation, for it is precisely the most vulnerable groups—such as, today, the transient young, the homeless, the disabled, the very old, or illegal migrants, who are most likely to be missing from a survey. Nor is there any way of quantitatively 'correcting' for their absence when survey researchers do not have enough information about what the missing interviews would have been like.

Most surveys do of course collect and analyse information on the implications of their response rates, and many do look at incomplete interviews or the interviewer's notes on the house and the context of the interview, in order to give some picture of non-response homes. Longitudinal cohort surveys can also examine the earlier records of those who later drop out, in some cases noting their lack of cooperativeness at the interviews as an advance warning. Most typically, the dataset is compared with other large-scale surveys such as the Census. But while re-weighting the data on the basis of comparison with other surveys has clearly proved an important advance, it is an illusion to presume that it can eliminate the problem of non-response. You can correct the statistically balance for missing something you know about, but not for something you have to imagine. For example, unless you have some well-based knowledge of the homeless, or of illegal immigrants, you cannot represent them through weighting. Nor can a longitudinal cohort assume that those who have dropped out of the project have comparable lives to those who, at a similar occupational level, have remained in it: indeed, it is likely that the very fact that they have not kept in contact may be symptomatic of fundamental differences, foreshadowed earlier by their having been 'difficult' interviewees. I would suggest that surveys could greatly add to their understanding of such marginal non-response groups through regular in-depth interviewing based on purposive sampling.

For example, there are numerous American studies using the US census statistics to plot patterns of migration, and the economic and familial state of immigrants. But these census figures give no indication at all of the numbers or situations of illegal migrants. In the case of anglophone West Indians in New York, for instance, many observers believe that the Census includes no more than half of the total number of migrants. Yet we have good reasons—as indeed some of my own life story interviews with Jamaicans there have suggested—
for supposing that in terms of occupation, and therefore of housing and family, such illegal immigrants are additionally disadvantaged, with many well-educated immigrants spending years in unskilled work such as cleaning and caring. In short, in this instance the figures which so many researchers continue to use are not much better than mere guesses. A much better way for a researcher to get a convincing sense of what is really going on would be to live for a period in Brooklyn or the Bronx, building up a house-to-house picture of their neighbourhood.

I have sometimes been surprised by how fundamental problems about the representativeness of data can be ignored by surveys. In a project comparing car workers in Coventry and Turin which I carried out in the early 1980s, when designing our sample base I found that in the annual official figures the proportion of skilled workers were totally different in the Census, which, of course, is based on self-reporting, and from the Ministry of Labour, whose figures are based on employers’ returns. Thus the 1971 Census, based on self-report, gave 42% of the entire Coventry population, and 57% of manual workers, as skilled. By contrast the Employment Gazette gave figures from employers which showed the skilled as only 24% of the engineering workforce including management, and 35% of manual workers only in the vehicle industry. Thus the Census figures are almost double those of the employers’ returns (Thompson 1988). One might ask why is it that these two groups of people collecting these figures for the public are not addressing these differences and trying to explain them in public. I could find no discussion of these startling differences.

There are some quantitative sociologists who are publicly candid about the difficulties with their figures: a notable early example were Peter Blau and Otis Dudley Duncan, who included a fascinating appendix to their pioneering path analysis study of *The American Occupational Structure* (1967), discussing the many discrepancies between their own results and that of the census. When they took individual cases they were able to broadly match less than a third. Blau and Duncan also point out that a post-enumeration survey carried out by the Bureau of the Census to check its own data found that 17% of men needed to be re-classified differently (Blau and Duncan 1967).

The best surveys now publish their resulting analysis of their sample in much detail, as is the case with the British Household Panel Study (Taylor 1994). Unfortunately such openness is not universal: indeed a survey with a low response rate may well wish to avoid public discussion of the problem. I found considerable difficulties myself in working with a national longitudinal study, which does not publish detailed analyses of their achieved samples and who has dropped out from them: and I want to return to that point. In such situations all that a puzzled researcher can do is to resort to a private enquiry about such details. Yet despite such difficulties, I remain convinced that the evaluation of samples and response rates is a neglected yet potentially very fruitful point for collaboration between the quantitative and qualitative approaches: for using both eyes.
**Encountering obstacles in the middle way**

But is it so easy for researchers, even when convinced that it is best to keep both eyes open, to work in that spirit? I want to say something of the positive experiences and the difficulties which I have experienced in two projects since the mid-1980s. The first was the research for *Pathways to Social Class* (1997), which was a comparative Anglo-French study carried out with Daniel Bertaux.

For this we were greatly helped on the British side by Howard Newby and David Rose, who generously allowed us to use as our initial sample base their own national random sample used for their project ‘Stagflation and social change’, for which informants were locally clustered by wards (Marshall *et al.* 1988). We decided to choose our initial sub-sample of middle generation informants of our hundred three-generation families from a smaller cluster of wards. Basically we adhered to this strategy, but not without difficulties. We found that there was a class bias in refusal rates, and eventually we had to introduce a class stratification to achieve a balanced sample. But in addition, we found that an unexpectedly high number of informants—especially in inner city polling districts—had moved and were untraceable. And more puzzlingly, where contact was made, we found that some people claimed never to have been interviewed for the earlier survey—and in some cases, given the discrepancy between the basic survey data and themselves, this seemed likely.

Nevertheless these three-generation family interviews proved extremely revealing of patterns of social mobility. To give one example, they made manifest the separate patterns, experiences and influences of men and women. Thus in most families there were parallel traditions of men’s occupations and women’s occupations handed down between generations, so that children were often offered a choice of models to follow. And such traditions were not at all necessarily handed down between parents and children: the transmission might equally be between an aunt and a niece to whom she became a model. It required an extended in-depth interview to capture such complexities.

Or again, the men who were upwardly socially mobile contrasted sharply with women risers. Put simply, men who were risers almost always rose with a helpmeet, with a wife who was there in the background, and remained at the time of interview in a continuing marriage. But by contrast the women either rose with a rather reluctant man in tow, or almost equally often, in our cases, rose after they had freed themselves from their husband.

This produced the interesting possibility that at least for women divorce was a catalyst for rising, as well as for falling. This later tied up with our work on stepfamilies, whose overall disadvantage has always been stressed by researchers, but among whom we again found a surprising amount of upward social mobility. Certainly historically, elite groups include in terms of creative artists and thinkers over a long time-span a surprisingly high proportion of stepchildren—among them Leonardo da Vinci, Michelangelo and Isaac Newton; or more recently, Bob Marley. We were led to think that the normal assumption that family break-up is likely lead to social disadvantage is a simplification concealing wide variations in
fortune, and that in fact a break-up will encourage family members to go both up or down, and that the presence of the risers may be as socially important to notice as that of the fallers.

A last point was that once mothers as well as fathers were introduced to the picture of transmission, and still more when grandparents, aunts and uncles were added, the picture became too complicated to analyse usefully through available quantitative techniques. This was particularly true since the complexity of most family occupational patterns meant that individuals were presented not so much with a clear tradition as with a choice. Furthermore, the interviews showed clearly how some people—while still influenced by parental example—were making their life choices in negative reaction to parents or kin, while others tried to follow them in positive emulation. Thus in analysing transmission, it becomes as important to look not only for positive models, but also for what people want to get away from in their family context. And that, of course, is quite difficult to deal with quantitatively.

There is a parallel with this in the second project which I want to discuss, the research carried out with Gill Gorell Barnes, Gwyn Daniel and Natasha Burchardt for *Growing Up in Stepfamilies* (1997). In this project we again found that it was crucial to explore the role of the extended family: indeed, one of our findings was that the most crucial support which children had when their parents split was, after the parent with whom they stayed, came from—not the absent parent, or from professional agencies, but from the extended family: and especially from grandmothers.

One of our main hopes in this stepfamily research was to compare different ways in which separating parents can handle the transition to stepfamily life, in order to evaluate the long-term impact of different approaches on the child. We therefore needed to evaluate which grown-up stepchildren were relatively successful, and which were not. There were great difficulties in such measurement. As one instrument we employed a self-administered questionnaire on mental health, which has been widely-used in psychiatric research, but we found that while the results were credible for most informants, there was a minority of men who seemed from the interview experience to be in deep psychological crisis, but who denied any symptoms at all. It was thus only possible to use this self-administered quantitative measure by fine tuning it through the interactive qualitative evidence of the interview. I was later interested to discover how George Brown’s pioneering research on the social origins of depression is based, in a much more sustained way, on developing statistical measures of each individual case through the evidence of his team’s in-depth interviews (Brown and Harris 1978; Brown, interview, 2001).

Another problem was in the defining of success. This was relatively easy when categorizing work, but very difficult for relationships. A broken marriage or an illegitimate child would have been seen in terms of failure by most people in the 1960s, how could this be so in the 1990s when over a third of births were outside marriage and half of marriages expected to end in divorce. And how could one form of success be balanced against another? For example, we found that many of our interviewees could be successful at work, but very unsuccessful in relationships. This was particularly true of
men: we found a number of men who were upwardly mobile, but who had had a whole series of unsatisfactory relationships or, in some cases, did not seem to want relationships at all. A few had created quasi-families at work, for example commenting, ‘The railway is my family’. At the other end of the spectrum, there were women who were determined to succeed with relationships, and were just not interested in work. Women more generally, even if they were upwardly mobile, tended to under-rate their work achievement. Hence, while we did persist in measuring in order to test our own hypotheses, we were forced to recognize that such measurement rested on highly ambiguous foundations.

For the stepfamily 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 Shepherd and Elsa Ferri of the Social Statistics Research Unit, City University, on re-interviewing a subsample of the cohort, all born in March 1958, which has been followed longitudinally by the National Child Development Study project. 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 cooperation.

As it turned out, our main success was indeed in securing an unusually good sample base for our own 50 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 cooperation that NCDS were subsequently able to compare the response rates for their own next wave of interviews between those respondents we had interviewed and those we had not. This showed that those whom we had interviewed were much more likely to say ‘Yes’ in the next NCDS wave to being interviewed again, than were those we had not interviewed for our own study. This suggested to me that some of the resistance, which as a qualitative researcher I have encountered from survey researchers, based on the belief that carrying out in-depth interviews with their sample will jeopardize their own future response rates in re-interviewing, is probably based on a false assumption. These NCDS informants had never, over 30 years, been given the chance to tell their full life stories, and most of them valued the chance to do so. Indeed typically they accepted our invitation to be interviewed with an alacrity such as I have not experienced in any other project. And 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.

Once again, however, in-depth investigation raised serious doubts about the reliability of the sub-sample which we had been given. We had asked for a sub-sample in which all of our informants would have entered a stepfamily between the ages of seven and 16, so that we could hope for a good account of the transition which this entailed. But in fact it turned out that one fifth of the informants had been seriously misclassified in terms of
the timing of their membership of a stepfamily. Ten out of the 50 were not what we had requested. Most of these had already been stepchildren way before the age of seven, typically since infancy. Although some of these variations may be due to coding, it seems more likely that they were misclassified because their parents were misleading NCDS as well as the children themselves, through social shame at a time when remarriage and births out of marriage were much rarer than today. And indeed in most cases 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 that these misclassified informants represented a special and revealing 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 although subsequently, when her then husband died, she was able to re-marry to this child’s true father, 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, was always hinting in a rather covert way that it was him. This informant even remembers how, when she was a child, during NCDS interviews her mother would prompt her responses.

We had originally hoped to be able to investigate some of these instances of discrepancies through going back to the earlier NCDS data collected in childhood, but this did not prove possible in practice. In fact NCDS had collected very little earlier qualitative material. The main exception was when they asked the children at the age of 11 to write essays on what kind of person they expected they would become as adults. I was able to look at this material. But apart from that, there seems to be nothing surviving from the earlier waves of fieldwork except the figures derived from the interviews. 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. The disappearance of the original evidence meant that we had no way at all, for instance, of guessing what had happened when those people who were born into step-families were being categorized. I had dreamt that, somewhere locked away, there would be the original sheets, and you could see the interviewer’s notes, which might say, ‘Suspicious of the story this mother told me’, or something like that! But there was no evidence of that kind at all. We found that particularly disappointing.

Subsequently I was very surprised to come across an article in the *Journal of Child Psychology and Psychiatry* (Rodgers et al. 1997), which is also based on the NCDS sample, but includes the results of the re-interviewing sweep at age 33 in 1991. These figures were still not available when our own book went to press. These new figures suggested extraordinary discrepancies between the most recent and earlier figures
from the same cohort. Although only 69% of the original participants were now interviewed, it was reported that 'the adult samples showed little bias compared to the original cohort, but disadvantaged groups were slightly under-represented'. However, in terms of parental separation and divorce before the age of 16, while 1046 now reported such an experience, only 435 of these could be shown to have separated parents from the original contemporaneous childhood data: less than half. While possibly some of these adult men and women may have developed fantasies of former stepfamily childhoods, it seems most likely with these that the real truth had only emerged later. This is in itself also a striking confirmation of the validity and usefulness of retrospective interviewing. More unexpectedly, a further 97 who were recorded as having separated parents in childhood did not so report at 33 (although in half of these the discrepancy was about the date rather than the fact of separation). All these disconcerting results have to be put in the context of a cohort of whom nearly a third is not being interviewed at all, and no published detail is available as to what kinds of participants, for example among former stepchildren, may have dropped out, or why. This began to seem to be to be an ideal ground for mutually supportive qualitative and quantitative interviewing.

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 research to understand not only some of the variations between contemporary and retrospective 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 whom 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 relatively stable and coherent life? This seems an extremely important qualitative-quantitative issue for longitudinal studies to address.

Our other hope for this cooperation proved wholly unsuccessful. We had a list of some twenty hypotheses which we hoped to test not only against our own life story interviews, but also against the questionnaires collected from the wider sample. But we were unable to make any headway in testing out our hypotheses on the wider group of stepchildren. To some extent this reflected the sheer pressure on NCDS staff to produce quick results under which surveys operate. But I realized that another reason is that retrieving the information which qualitative researchers need is often technically difficult or even impossible because of the form in which information is held. Being used to discussing and analysing individual cases, we were surprised to find that NCDS were not willing to print out the coded information for each informant over the successive surveys.

On reflection, I have understood that this may have been in part because of different professional assumptions about confidentiality and evidence. While qualitative researchers must always struggle with the ambiguous ethical demands of confidentiality, for the very depth of detail
in their information, which constitutes its professional validity, is impossible to totally anonymize, survey researchers can aspire to cast-iron guarantees of anonymity if they either destroy or refuse access to all the original evidence of their fieldwork interview schedules and then scramble the coded information, so that it becomes impossible for an outside researcher to link up all the details obtained from a single individual. But for good qualitative research it is absolutely essential to be able to re-consider each case as a whole. The lack of access to the primary evidence therefore becomes a crucial barrier to any reanalysis.

The difficulties which can be caused by these contrasts in professional expectations were again brought home 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 showed very little interest. We offered to archive the interviews with access controlled by a joint committee to ensure the maintenance of confidentiality by users. They replied to suggest that the interviews should be totally anonymized like quantitative material. We explained that this would effectively destroy the interview evidence, and suggested various other options to protect confidentiality, but sadly, we never heard again. As a result, our interviews have been archived independently. 6

Cooperation, in short, is easier to espouse in principle than it is to realize in practice. The dream of a middle way in research, drawing on both quantitative and qualitative skills, could not be widely fulfilled without changes not only in attitudes, both by researchers and fundors, but also in specific research procedures.

Building joint practice

What then may we hope for in the coming years in terms of this cooperation between different methodological traditions?

I believe that the crucial need is a building of joint practice: in fieldwork, in data archiving and in analysis. In particular, it would be an immense step forward if some of the numerous national surveys, panel studies and longitudinal cohorts were to regularly include sub-samples with in-depth life story interviews. I recognize that there are problems in this. There is the fear that additional interviewing would result in worse subsequent response rates, although our own experience suggests that the reverse may be true: clearly more research is needed to clarify whether this is indeed a danger. There are also structural problems, particularly for annual panel surveys, which need to strictly limit the time they demand of their interviewees, and therefore have little space for experimenting within their own interviews: longitudinal cohort studies, because their interviews are at much greater intervals, could provide more space for an interactive methodological relationship, with qualitative fieldwork raising hypothesis from sub-samples of the cohort which could be subsequently tested in an interview sweep of the main cohort. Lastly, such an approach would certainly add a small proportion to costs—although in the case of the longitudinal studies it would be worth considering recouping this by
trimming the unnecessarily large basic samples. But I believe that such joint practice would be richly rewarded in terms of the quality of the findings and their interpretation. There are moreover some precedents for precisely this type of combined work.

One example is of the oldest longitudinal cohorts which are still running, the Oakland cohorts in California. They were used by Glen Elder for his *Children of the Great Depression* (1974) and by a series of other notable researchers including Erik Erikson, Arlene Skolnick and Tamara Hareven. From the start, the documentation of informants was both quantitative—from physical measurement to questionnaire assessments—and qualitative, including highly detailed reports of social workers’ visits to each family. In the 1980s a further dimension was added with the recording of life retrospective life story interviews with all the surviving participants, brought together by John Clausen in his *American Lives* (1993). This cohort has been somewhat neglected by researchers in the last five years, but it constitutes an exceptionally long-lived and powerful precedent for systematically collecting qualitative and quantitative data from the same sample. It is also one of the richest long-term fieldwork resources I have ever encountered.

An alternative and still more longstanding model 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 special autobiographical section led by Stefan Bohman, with a staff of ten. The archive has been organizing regular autobiographical competitions—a research approach originally popularized in Polish sociology by Znaniecki—since 1945, and since 1928 it has been collecting special thematic essays from a panel of 400 correspondents right across Sweden. The themes have gradually shifted from earlier retrospective preoccupations to encompass all aspects of contemporary everyday life, including even computing.

All the basic thematic and autobiographical data is well indexed and this has now been computerized, so that very full information on the collection in Stockholm can be available not only in the strikingly busy research room in the capital but also at educational and library centres throughout the country. The notably high level of usage of these qualitative resources, built up through the cumulative practice of many decades, is a measure of how much we are missing in our own more individualistic research culture.

Perhaps the nearest British equivalent to the Swedish autobiographies is the Mass Observation Archive at the University of Sussex, which again has a strikingly busy reading room. But, as with the Swedish archive, the weakness is that the essays do not emanate from an even roughly representative sample. In a sense, such regular essay writers cannot ever be wholly typical. But it would be worth attempting, for example, to reduce the great predominance of middle class women contributors, and to seek out more working class women in their place. Thus a more deliberate quota sample was used for the 150 amateur diarists and video recorders of BBC’s *Video Nation* series, whose well-indexed manuscript diaries and 5000 tapes make it potentially an exceptionally valuable research resource. A third
resource of comparable potential importance, which is again based on a rough quota sample in terms of locality, occupation and gender, is constituted by the 6000 interviews of the BBC Millenium Oral History Project, which are already available for researchers at the British Library National Sound Archive. These interviews have been broadly thematically indexed. However, they have not been transcribed, which for the moment seriously impedes their use.

Both the Oakland cohorts and the Swedish autobiographies offer impressive examples of what could be achieved 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 sample-based in-depth datasets for the general use of both quantitative and qualitative researchers for the future. I see such a structural initiative, which would enable any new qualitative research to take place alongside the re-use of both types of existing data, to be a fundamental need, not only for the qualitative approach in sociology, but more generally for the continuing vitality of the discipline itself.

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 may well be supportive of the survey and its interpretation, it is difficult to see a convincing intellectual or methodological justification for not attaching, as a regular practice, sub-samples of qualitative interviews to such surveys. The potential added value 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—partly from its very sense of impotence—has less and less interest in engagement with the ‘real’ world.

In short, 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. 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 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.

Research would then be increasingly envisaged, not so much like a straight run down a predetermined track, or a pilot exploration pilot
leading to a straight run, but rather in the form of a zigzag, when first experience in the field leads to reformulations of aspects of the problem, and so on through successive waves of fieldwork, reformulation and analysis. This zigzag would incorporate a similar alternation between in-depth and survey fieldwork, with the qualitative fieldwork carried out especially in the earliest stages of formulating issues and the last stage in analysis, of re-exploration to refine hypotheses.

There is also a need for a much more open debate of difficulties in sampling and how these may influence results. My own strong belief is that openness, and willingness to share across methodological frontiers, would bring an immensely richer sociology for the future. To conclude on the note on which I began, as researchers we need to use both of our eyes. In Jamaica they have a proverb: ‘Before you marry, keep your two eye open. After you marry, shut one!’ My view is that it is precisely the married approach to research that we most need to abandon.

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**Notes**

2. At a seminar in Nuffield College to which I was invited in November 1993 Goldthorpe challenged me to give a single counter-example to this assertion. There are of course very many examples: two would be research investigations on the social mobility of transnational families whose members live in different continents, or the social mobility of the Jewish cohort which entered the holocaust concentration camps.
3. This interviewing programme has been funded by the University of Essex, and copies of the texts will be available through Qualidata and the tapes through the National Sound Archive. Those interviewed to date include Michael Young, Raymond Firth, Peter Townsend, George Brown and Tirril Harris, Frank Bechhover, David Lockwood, Dennis Marsden, Stanley Cohen, Janet Finch, Ray Pahl, Colin Bell, Muriel Blaxter, Glen Elder and Raymond T. Smith.
4. ‘There’s nothing more satisfying than doing a survey, and then “cleaning” the data—that’s removing all the inconsistencies, and then manipulating it, in all these ritualistic ways, so you get a nice clean output’ (Smith, interview, 2001: 63)
5. At the National Archive of Social Policy and Social Change in the University Library, University of Essex.
6. At the National Archive of Social Policy and Social Change.
7. Archived at the Institute of Human Development in the University of California, Berkeley.
References


Finch, J. (2001) Interviewed by Paul Thompson, Qualidata, University of Essex.


Smith, R.T. (2001) Interviewed by Paul Thompson, Qualidata, University of Essex.


