

**Paul Thompson
Life Story Interview**

with
Karen Worcman

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Interview held on: 12 June 1996

Interviewer: Karen Worcman, in Stockholm

Tape 1 - Side A

Could you tell me a little bit, about how you started working with research? What took you there?

Well, I started doing some kind of research when I was quite young, when I was at school [at Bishop's Stortford College]. And I think that there were two people there who were important influences on me. One was the biology teacher. I was never really particularly interested in science, because, on the whole, it was very badly-taught. But this teacher used to use the children - well, we were all boys, actually, it was a boys' boarding school - he used to use them in the research he was doing. For instance, he'd take a square yard of a meadow, and he'd discover all the different insects in it, and he discovered new insects that nobody had ever identified before. And I found that rather exciting, the idea that you could go to a local field and discover something new, that nobody had ever found. And he also go boys with walkie-talkie radio sets, out on the hills, plotting the movement of birds and their migration, for instance, in the autumn. So I think I learnt from that, the idea that you could do something on the spot where you were, that if you were ingenious, you could start working in your own area, and the research, therefore, could be wherever you were. And I think that may be an idea that stayed with me.

This is when you were, like, 15?

Yes. Roughly, yes. And also there was a French master - this is, I suppose, a bit less research - he taught me French, but he wasn't a terribly good teacher, I think, of languages, really. But he was so inspiring in other ways. And he got me into art. He did that partly because he had become a personal friend of Henry Moore, the sculptor, who lived about ten miles from the school, and he used to come in with new drawings that Henry Moore had done. And this was very exciting, because you got the idea, again, that art could be real, it could be part of the life around you, and

not something which was just behind glass in museums. And he also had an Architecture Club, and we used to go out on our bicycles into the countryside, looking at the old churches and houses and so on, and drawing. He started me drawing. So, again, I think it was the idea that there were things you could do on the spot, you know, which was quite important.

And then I went from there to university, to Oxford. I was admitted to read history. But the University was still really dominated by Classics when I was there. Now, the Classics have faded away, and it's probably most strong in medicine and English. But, at that time, English was good, history was good, but Classics was the best. I went to [Corpus Christi] College, where there were a lot of Classicists, very very clever people, and they had to do philosophy as well, so they were always arguing about ideas.

And I had a tutor, called Trevor Aston, I've recently written an article about him, actually, because - I didn't know this at the time, but he was a manic depressive, and he later killed himself. But he was deliberately driven to suicide by the President of the College. I didn't know that. He died about ten years ago, 1985, I think it was. Suicide, yes. The President had got really sick of him, because when he was manic, I mean, he was so dominating that he was interfering with everything, and buying books, very expensive books for the library, and altogether behaving in a very irritating way. And this President couldn't stand it! So, and the President himself, I think, was a most peculiar man, and he deliberately tried to drive Aston to kill himself, but nobody realised that he'd done that, until this President actually published an autobiography about a year ago, in which he boasted, he had a whole chapter called, "The Aston Affair", and he boasted of driving this man to death. And I was so outraged by this that I wrote a letter, it was published in *The Times*, and then I was encouraged by Raphael Samuel to do an article for the *History Workshop Journal*. So I phoned about twenty people who'd known him, and I reconstructed his life, a kind of oral history on the telephone.

He was my tutor, and he was a very unusual man. He ran *Past and Present*, which was a Marxist/progressive history journal, very very influential journal. He was a mediaevalist. But he took me incredibly seriously. I mean, we had to write essays, twice a week we had to write essays, and he would argue and argue with me about my essays, and then he would tell me to come back in the morning, and he would look up more evidence and argue again. And it's true to say that nobody, in my whole life, has ever taken me so seriously! (LAUGHS) And I didn't realise it was partly because he was a bit crazy! I didn't know about it! (LAUGHS)

So he made you work a lot!

So he made me work a lot! And he got me doing research on the College buildings, and that was really exciting, I can remember rushing around the building with him, in this slightly intoxicated kind of state of excitement, and him showing me mediaeval documents about the building of it, you know, and so on, and I wrote things for him, which were published [in the college magazine, the *Pelican Record*]. Probably my first published research article.

Because when I was at school, I hadn't mentioned, but the last year I was at school ... because I had a history teacher who was no good, he had a geography degree, and he never read any history, and, and he taught me entirely by reading up the notes of a former pupil who'd gone to university, he just borrowed these notes, and read them up. So when I passed my entrance to Oxford, he didn't know what to do with me, and so he said, "Oh, well, you're interested in local history or whatever, why don't you go down to the church and look at the old documents?" And so I did. I wrote a history of the town, actually. I spent, oh, about three or four months doing that, and that was my first direct experience of doing research from documents.

You were how old?

Well, then I would be 17. Still at school, yes. I wrote this, but it was never published... But these [articles] at Oxford were my first published researches. And I think I could have been, well, I know I could have been an architectural historian, because I kept up that interest in architectural history, and even when I was at University, I had started doing research on a 19th century architect who interested me, *William Butterfield* (1971), and I eventually wrote a whole biography of him. A great fat book, it's a very long book, and it's got some, about 400 illustrations which I, nearly all of them I photographed myself. And it's about this 19th century architect, who was very controversial, that's why I was interested by him. And who was attacked, at the time, for his style of architecture.

This would be when I was about 18 or so, I guess, I got interested in 19th century architecture, partly through listening to radio talks, and there was Nicholas Pevsner who, later, I got to know very well, who was the great figure in architectural history in England, and was editor of

The Buildings of England, and was author of, for instance, the book *Pioneers of Modern Design, from William Morris to Gropius*. But he did these radio broadcasts in 1951, I think it was, at the time of the Festival of Britain Exhibition, which was also the anniversary of the 1851 Great Exhibition in London, in Hyde Park. So he was talking about 19th century design, and really, how bad it was. And I was interested by that, and I remember I started talking about it at school, and my teacher wasn't very interested by it, actually.

And then I went on pursuing that, and I discovered that there was an article by Sir John Summerson, who at that time was the curator of the Sloane Museum in London, a very upper-class character. And he wrote an article saying that William Butterfield was the architypal representative of middle-class bad taste in [Victorian] England, and that it was because the rising bourgeoisie of the 19th century had come to a point where they could patronise architecture, that this terrible collapse in taste had occurred, and the worst example of all was William Butterfield, and he was a kind of visual sadist, who was deliberately trying to inflict pain on people who looked at his buildings. And so I got fascinated by this argument, and I spent years, probably 15 years, working on that topic, visiting all his buildings... It was a sideline, really.

To write about his biography?

I wrote articles earlier, but I took 15 years to finish it. I got the personal papers from his family, although they came through descendants of his sister, and the grand-daughter of the sister started burning quite a lot of the letters when I got there, because she didn't like the, the poem, the language that he was using in these letters. She disapproved of it. So I was never able to get the full personal story. He remained a bit of a mystery, really. But anyway, I travelled all around England. You would usually find in churches, where he built the church, there would be a lot of documents, maybe 20 letters or so, and so I travelled around visiting the clergymen and reading the letters, a tremendous research task. Actually, I went to Australia too, because there were two cathedrals he designed there.

So you started doing this during University?

Yes. But it was always a sideline, a hobby. But the point is, I could have professionally become an architectural historian. I do love architecture, and I have a sense of it, and I'd been interested ever since I started looking at buildings, space and texture, and so on. So it sprang out of a real feeling, which I still have.

But I think the point was, when I started doing it, it was something ... [First] you went to look at buildings, and then you became interested - and then you did research on the architect, or the building. So you started by the sensual experience, and then you moved to the research. I also wrote a book on *The Work of William Morris* (1967; 3rd edn 1991). So I've written two books, really, in that art historical field. And, for quite a time, I taught at East Anglia, one day a week, for the University there. But what I found was that, by the time I got there, [architectural history] had been professionalised. So instead of starting with the buildings, you read books. And then, if you were lucky, you went out and looked at them. I tried to take these students out and look at buildings, take them to a 19th century building, and say, "Well, now, what is 19th century about this? How would you say that this is different from a mediaeval building?" And they'd never had that experience before. But, for me, that's how it started. And I think that was one reason why I started to withdraw, it became ... too specialised, too much a library activity, and I wanted the direct contact.

I think the other reason was that I gradually realised that architectural historians were ... well, there were two types of people, typically, one type who were only interested in things, and not in people, and who were often very inhibited people, and maybe I was changing a bit myself. And then there was another sort who were attracted to the architecture of the country houses, because through this, they met the owners of country houses, so they were lovers of the upper classes! And I didn't really into either, I don't think!

I did a third book, too, two other people, which was a *History of English Architecture* (1965; 2nd edn 1979), was published by Penguin, and that sold more copies than anything I've published. It sold 35,000 copies.

Then I think the next significant book was *The Work of William Morris* (1967; 3rd edn 1991), which I did immediately after my thesis, you see. And the William Morris, I felt much happier about, because it brought together the political with the artistic. And that's why I stayed with that, it has been revised twice, so it's now in its third edition. I still give talks on William Morris. This year I'm doing quite a lot, because it's a hundred years since he died. So my heart is

much more in that than in the other kind of 19th century architecture. The other I can still see, aesthetically, but I don't feel emotionally committed to. But William Morris I do feel committed to, yes.

And your thesis was about?

Well, when I got to the end of my [first] degree at Oxford [in 1958], I think that my tutors were terribly surprised that I did very well. I think Trevor Aston, he knew that I was bright and might do well. [Michael Brock] the 19th century tutor who, in a way, was more important, didn't realise that, partly because I used to laugh at him too much! There was a lot of laughing went on in ... we had to read our essays out aloud to our tutors, that was the system, and Trevor Aston deliberately paired me with a rather pompous fellow student, and he used to try and get me to start laughing. In fact, he would deliberately let out a huge laugh, and then stop ... that I would be set off by him, and start laughing! But the trouble was, the other man, called Michael Brock, was that, that actually we laughed at him, rather than at each other! And I think he always felt, you know, underneath, he rather disliked that. So he wasn't on my side. But anyway, so he didn't expect me, I think, to do well at all, and he was terribly shocked when I did.

And there was another complication which was that, in my last few months, I got involved in the Peace Movement, which had just started, the anti-nuclear Movement, and I wrote an article, which was published in a student magazine [*Isis*], which resulted in my being charged under the Official Secrets Act, because I'd partly used knowledge that I'd got during my National Service in the Navy. And it all got mixed up with my Finals, between the, the first exams and the oral exams, for instance, I had the preliminary hearings of the trial. And by the time I came to the oral examination [the viva], I just couldn't think about history at all, I was only thinking about these questions about aeroplanes and so on! {I already had a First but} they had to get through, asking me two or three questions, and I just hardly could answer them.

So then I got convicted and I went to prison [for one month]. And then when I came out, I still didn't know what to do, and my parents said to me, "Well, you've always been very argumentative, why don't you be a lawyer?" So I tried doing that, and I spent six months in a quite interesting lawyer's office, they acted for the Musicians' Union, so you met quite interesting characters. But I hated the legal world, I thought it was so conservative and ... dirty, in a way, too.

They lived in these dirty old offices, it was really stifling. And eventually, after about six months, well, I realised that in the evenings, I was spending my time trying to do research and write, and that was what I really wanted to do.

So I felt I just must go back and try and do that instead. And then I decided I wanted a 20th century subject, because I wanted so history that was relevant, although my old tutor wanted me to do something mediaeval, and then I met somebody, just casually, who said, "Why don't you study the Labour Movement in London?" And I thought, "Well, that will be good, because I'd have to be in London a lot of the time". And so that's what I did. It was meant to be a study of the Labour Party in London.

And at the end of that, to be a book?

Yes. But it ended up to be about the founding of the Labour Party, I never got beyond the founding, actually. People said the interesting thing would be to study the London Labour Party in the twenties and thirties. But actually, I did from 1880-1914. And it was about why the Labour Party started to supplant the Liberals in working-class areas. And I reached a social interpretation. It started as a political thing, I suppose, but it ended up, very much, as a social interpretation. I started by reading about the political history of the Socialist Movement in Britain. There was always something about the social background, but I moved towards trying to make it predominantly a social explanation, and seeing that, to have a working-class political movement, you needed a stable working-class, who were earning enough money to support trade unions and political parties. And the point is that the London working-class, traditionally, were rather too poor for that, and certainly the inner London ones. And they also moved around tremendously. About a third of them would move house each year, so you couldn't keep a sort of stable organisation in that situation. And so, actually, it was on the outskirts of London that the Labour Party developed, in West Ham, to the east of London, or Woolwich, where they had the arsenal which made the ammunition and so on. These kind of places.

So they were more stable?

Yes. Yes. So that's the interpretation I ended up with. And, of course, it was a huge topic, because there was a separate Labour Party in each of the local electoral areas, and I read incredible numbers of newspapers to try and work out the development. And there were different rival groups, too, among the Socialists, so you had to see why one was on the top in one area, and one in another, so it was very complicated. But anyway, it was quite interesting.

And that was when I tried, for the first time, to do any oral history. And I did a few interviews with people who weren't well-known, and those were quite interesting, because I discovered you could, you could learn from them, for instance, what they read, you know, whether, as Socialists, they'd ever read Karl Marx, for instance, that kind of question. That seemed quite interesting. They didn't, in fact, no, not usually.

They admitted to you, they said, "I never read ..."

Yes. Yes. Oh yes. But that was the sort of thing I found valuable to ask.

But I also did an interview with Herbert Morrison, who was the Home Secretary in the late forties, under the Labour Government, and he had also been the Secretary of the London Labour Party, you see, so he was a big figure in my research, really. And this interview was a disaster. He was really retired, he was still an MP, but he was effectively retired by the time I interviewed him. So we sat on the terrace of the Houses of Parliament, overlooking the river, and had tea, and I started by, I wanted to hear about his earlier life. And I knew from what he'd actually written himself, and other evidence, that, for at least a year, he had belonged to a Marxist Party [the SDF], there was a Marxist strand of British Socialism, you see. And I asked him about this, and he completely denied having ever belonged to any Marxist organisation, you see. So I just accepted that. I mean, I thought that was the thing to do. You know, I had no training in how to do an interview. And almost immediately, he lost interest in the interview. And then he got out, he had something like a handbag on his shoulder, rather unusual at that time, for a man, and he got out this huge pair of scissors, and he started clipping at his finger-nails, and throwing the nails over into the river! (LAUGHS) And so I knew he was finished, really. And that really put me off oral history, you know. I just felt that showed it was no use. I didn't realise that I'd failed in terms of how to do an interview with him. He didn't respect me after I'd accepted a whopping big lie like that.

And you went to make this interview, why?

I thought it would be interesting, and I think my tutor, my supervisor - Henry Pelling - encouraged me. At that time, there were quite a few of the people around, older people, who knew about the early days of the Socialist Movement in the early part of this century, so there was quite a lot of opportunity, and you could sometimes combine talking to them with getting a document. But of course, then we always thought the document was much more important.

Yes. How was oral history at that time? Did it exist?

No, not in England. We're talking, let's see, about the early sixties, and, no, there was nothing called "Oral History" at that time, no.

So you would see making interviews as a compliment method, something like that?

Yes. It would be linked to finding the documents. In political research, though, it was always an accepted idea, that you talked to people to find out, you know, the gossip and so on. But then you tried to prove it through the documents.

So then you were working on research on the Labour Party. You were also working with architectural history?

Yes. Well, I finished the thesis in 1964, and that was turned into a book, it came out three years later [*Socialists, Liberals and Labour: the Struggle for London, 1885-1914* (1967)]. And then in the month after I finished the thesis, I began the book on William Morris, which used some of my knowledge about the Socialist Movement. William Morris bridged both interests, you see, as an artist who was a Socialist. Well, I was asked to write the book, in fact, by the publisher. Yes. I was finishing the thesis, because I did the William Morris book very fast. I probably took about six months over it. Five or six months - looking back on it, it's amazing ... I think at that stage in life, you don't have so many distractions, because I could never do a book at that speed now.

Well, it depends on what you are doing!

But I did some real research for it, I read all the manuscript letters, I toured round Britain looking at all the examples of his [furniture, stained glass and other decorative] work, and so on.

End of Tape 1 - Side A

Tape 1 - Side B

And the architectural history carried on, as a sideline, for some time, because, for instance, the book on William Butterfield didn't come out until 1971. Again, it involved going, travelling to see many places [not only in England, Wales and Scotland, but also in Australia].

That's wonderful. You should talk a little bit the connection between making research and travelling. Not all research is like that.

It is quite true, I enjoy that very much, it's very exciting to me. And when I travel, I'm always looking in a way which I've learnt, partly through the architectural history, and then there's all the social knowledge which one has developed over the years. But another important influence on me was W.G. Hoskings, who wrote about *The Making of the English Landscape*, and how you could read landscapes, historically, because you could see that, for instance, the shapes [of the fields], the positions of the hedges, were either mediaeval, or they were introduced in the eighteenth century, in the move from the open fields to the enclosure system. Or places like in the far West of England, where I was recently, you know we were in a house in January in a place called Zennor, overlooking the Atlantic, in the far West of England, in Cornwall, and the hedges there - they're not really hedges, they're stone banks - but they go back 2000 years. [Hoskins] wrote about that kind of thing, and taught me to look all the time at what you see as you pass by, as both beautiful, but also as a social and historical document. And I've found tremendous gain in that. So I learnt to read the countryside really.

But then I learnt, I got very interested, partly through the thesis on London, in reading towns, so that you learnt to see ... there's going to be, shall we say like here [in Stockholm], there's an old centre, and then there's a fashionable area near the centre, and then there's usually an inner ring which is rather poorer, and then you gradually go out and get to the better paid working-class, and then the middle-classes beyond them, and so on. And when I look back, I remember going to America, and reading a lot about their social history of the 19th and 20th century, and then visiting cities like Chicago, and looking at the architecture, but also looking at it socially. And I've got a terrific lot from that, for my life.

And then, well, [for me] oral history developed [travelling] in a different kind of way, because I started oral history without ... it was really local, very British, when I started my own

work. But then I met, in 1976, twenty years ago, I went to a conference in Bologna, which was the first international conference on oral sources. It wasn't really an oral history conference. There was a mixture of history and anthropology. But for me, the exciting thing was to meet, for the first time, people in Italy who were slightly younger than me, who were doing the same kind of thing. Up till then, going to Italy had been wonderful, but it had been, basically, as an art historical experience, as a tourist looking at art. I did the Renaissance, you see, as part of my BA degree, so I used to love going to Italy. But this was a completely new side to it, to find these people that I could relate to.

And so they were doing what?

Yes. They were doing straight oral history, exactly. In Turin, in particular, they were doing projects on the history of the working-class movements and working-class families. [Especially], well, Luisa Passerini, Daniele Jalla would be the two. Luisa was [at the conference] in Bologna. It was very intellectually stimulating, obviously, but also linked to what you're saying. It was, I suppose, as a dimension of this travelling and doing research, eventually, that I did manage to try a project with Luisa, which we can talk about later. But it meant that I started, then, going to Italy, and staying with people, instead of hotels, and seeing the city through the eyes of people who really knew the place. And then I linked up with Daniel Bertaux and Isabelle in the same kind of way. Started going to France.

I'd learnt French at school, with this French master, of course, but I'd never been able to speak it properly, I could just read it. I could read it very fluently, I'd got a big vocabulary, but I just couldn't really bring myself to speak it. And then when I was with them, I started speaking at last, because Isabelle wouldn't speak English. And I started gaining a bit of confidence. And then I decided to try and learn Italian properly, because I was, I had a little Italian, but not much. And I found, when I say "I found", Natasha actually found an Italian from the Veneto [Franca Povoleri], who was a nurse, at that time, but later has become well-known as an Italian teacher. And I had a marvellous experience, learning Italian with her, it was one-to-one basis. It's almost like therapy, actually, doing that! We became very close friends actually, and I think it's been one of the most exciting things of my later life, learning to speak those two languages, and getting confidence enough to do that. I think with most things, you pick up skills when you're young, and there are

not a lot of skills that you can get later. But that was one thing that came to me later. I tried too, as you know, with Portuguese!

Yes. But you're going to get there!

Yes, I hope so! Yeh, well, I find no problem with reading Portuguese or Spanish. And Russian as well, I tried with Russian. I have a bit of Russian as well, but really, Italian and French are the two languages I have.

But when you went to this conference, in Bologna, you were already doing some work with oral history?

Yes. Well, let me explain what happened. I went to Essex. Now, at that point, I personally knew very little about social science. In fact, I remember there was a rather strange occasion, when I was leaving university [in 1958], this was before I started my thesis, and there was a man called Chris Harris, who had actually been a big influence on me, because I met him during National Service, and I was in this dormitory, his bed was opposite mine, I think. And he was a Christian Socialist, and he converted me to both Christianity and Socialism at the same time. The Christianity didn't last all that long. I discovered, rather quickly, at university ... it was really Natasha that finished it off, in fact. When I first got close to a woman, the need for religion, active religion, seemed to disappear rather fast! But the Socialism stayed. And I didn't see a lot of him after that. But he was a very nice, a very sort of thinking kind of person. And then he appeared at a party in the last week or so of my time as an undergraduate at Oxford, and we all said, "Well, what are you going to do?" And he said, "I'm going to be a sociologist". And we said, "What's that?!" So this was the first time I heard the word!

I can't remember how I first thought about Essex, but [the early sixties] was the time when there was a whole group of new universities started in England. I was looking for a job, and writing *The Work of William Morris*, and I knew the job would start in October, at some university. Because at that time, it was easy to get [university teaching jobs]. It was a time when they were expanding very fast, and they were starting the new universities, and there was no doubt I could get

a lecturing post. Maybe I wouldn't get the first one I asked for, but there was no worry about getting it.

It was not something like nowadays.

No. Completely different. And, in fact, my supervisor had decided to move to Cambridge, and he encouraged me to apply for his job in Oxford. And I said "No". I didn't want to stay in Oxford, because I thought it was too antiquated, really, the whole atmosphere. And I wanted to go to one of these new universities, where they were trying inter-disciplinary things, and the whole atmosphere was much more informal. So I got this job at Essex in 1964.

As a teacher?

Yes, in Social History, but in the Sociology Department. And the point is that I had read a book by Peter Townsend, because one of his books was about London, it was *The Family Life of Old People in East London* [1957], and he was appointed Professor, at Essex, of Sociology. I had a friend called Royston Lambert, who I knew because he'd been in architectural history, but he'd moved over to sociology, and he was in touch with Peter Townsend, and it was through him that I was introduced to Peter Townsend, as a possible person for a job. So I went and had lunch and talked to Peter, and he liked me. And then they advertised the job, and I got it.

Peter was one of the big influences in my life. He's just retired now, he stayed at Essex until about ten years ago, then he went to Bristol University, and his research, his life's work has been on poverty. He started with old people, but then he moved from old people to poverty, and he did a whole series of big research programmes on that, and he's always been tremendously involved with the political scene, and trying to pressurise the government, or the opposition, to do things for people in poverty. And he founded various pressure groups too. And now that he's retired, and if you want to find him, the easiest way is to ring up the Houses of Parliament, because he's now married to a Member of Parliament, a younger woman, and he lurks around! The last two times I've been to the Houses of Parliament, each time I've bumped into him! So the point is, what I'm trying to say is, that he was actively involved in changing society, and that's what he thought research was about. And that really powerfully influenced me. He had a very powerful, moral

commitment, he's got a charismatic morality, which I found very very influential. And it was something I wanted at the time. I wanted to feel there was a social purpose in what I was doing. So that working with him was very very positive for me, in that way.

But also, on top of that, he had been interviewing old people, and he said to me fairly soon, he said, "Look, I don't know whether this is of any potential interest to you, but I found that they can tell you very interesting things about the past. And let me show you this box of extracts from my interviews". He said, "I can't do anything with them, with this, but maybe it might interest you". So he showed me this box with all these extracts from his interviews, which were things like memories of funerals and ... I don't know. I mean, they were of interest. But the main point was, the idea that he was pushing.

Then I got asked by Eric Hobsbawm, who was already, I suppose, the most important Left-wing social historian, whom I had met once, and didn't really like terribly, because ... how could I express it? He's not exactly... he's become more vivacious and pleasure-loving as he's got older. But he always seemed a person that had a down-pushing kind of side to him, and he didn't make you feel good. Do you know what I mean? When you saw him, in a way, and he was always keen to put you in your place, intellectually, as somebody down there, that's what I always felt about him. But I admired his work enormously, actually. I thought he was a very very clever man. And always original, what he writes.

He asked me to write a volume in his *Social History of Britain*; and that became *The Edwardians* [1975; 3rd edn 1992]. Now, he didn't want me to do oral history, but the people at Essex, like Peter and others, said, "Well, why don't you go and talk to people who remember the period?" And, at that time, the Social Science Research Council had just been set up, so we decided to apply to them for money, and I got I think it was about £30,000 which would be an awful lot now, and [in real terms] it was the biggest pure research grant I probably got, I think [before Qualidata. Today], multiply by ten, probably.

So, now, it is a lot of money.

For making the interviews, transcribing, sorting material out. And we did about 450 interviews.

You, who else?

Well, we had a team of people. But just to connect that to the book, because I was thinking about Hobsbawm. You see, eventually, having got all these problems of how to use the material, and actually, we collected far too much for that book itself, I only used a hundred for the book.

And how long did it take, 450 interviews?

We had three years. But I couldn't, I couldn't work out a way of absorbing that amount of material, because then computerisation was very primitive, it was only statistical at that point ... which we later tried. But you couldn't read that amount of material and take it in. So I decided to do a sub-sample of 100, and I wrote the book from that. And then I took out of that - I found a conflict between the cross-analysis, and the whole stories. Once you knew a whole interview, somehow you wanted to have that whole person there, that you always feel.

I always feel this problem. If you take a theme to compare what lots of people say about that, it's very different than to go in one person ...

Yes, that's right. Yes, that was the first time I experienced it. And the way I solved it in the book was that I had a series of chapters which used the interviews as a whole, with short extracts. It might be a chapter on childhood and youth, or marriage, for instance, or there was one on politics, and so on. But then I also had two groups of portraits of people and their families. In the main group I juxtaposed different social [levels]. So it started with the daughter of a landed aristocrat, a really upper-class wealthy family, and it ended with the child of a poor, unemployed labourer. Right in the middle, a big block in the middle, you see, about a quarter of the book roughly, and maybe more. And then two other [portraits] towards the end, which were both woman, and one was a political woman who'd consciously tried to change society, and the other was an example of unconscious pressure for change, a poor woman who struggled to get a decent life. Actually, in the end, she failed. It's a very sad story, but it shows the pressure that's there all the time for ordinary people [to better themselves (cf. 'Life histories and the analysis of social change', 1982)].

And that was an idea I got from the interviews, which I hadn't had before. Theories of social change were all about politics or megaforges like the economy, and there was nothing about this

way in which ordinary people, all the time, are making decisions, trying to do better for themselves. Whether they marry, whether they have children, whether they move to somewhere else, whether they get a different job, all those kind of things. And that's an enormous part of the pressure for change. So that was meant to be symbolised at the end of the book, you know, this was one of the new ideas that came out of it.

Then I gave the manuscript, you see, to Hobsbawm, and his first reaction was that I should cut out all the oral history stuff! So I said, "I'm sorry, I'm keeping it in".

He didn't want it, no. No.

Did you have an argument?

Yes. I said, "I won't cut it out, no". Anyway, I refused to, and then, well, he obviously decided it was better to have the book like it was than not at all!

And he published?

Yes.

Okay. One question I wanted to make to you. Who did you decide to interview?

Well, we decided to have a sample, and our aim was to have a representative sample, so we took the [national] Census of 1911, and [with the help of a study of occupation and pay throughout the century by Guy Routh, who had regrouped the Census occupations], we worked out [six major groups from] what the occupations were then, and male and female occupations, and then tried to get people who, in 1911, or if they weren't working [themselves in 1911], their main parent's occupation. We also divided [the sample] by men and women, we got the right [proportions] of men and women. And we split it regionally [into thirteen], and [also into three] by big city, ordinary town, and countryside. We created quotas, boxes, that you had to fill up.

Then we went out and got interviewers in each area. We usually advertised for people and we got them to do a trial interview before we chose them. We used about 20 or 30 people, actually, altogether, because a lot of them would start and then not do very many [interviews]. And they

were all using those heavy old UHER machines, do you remember those? They were reel-to-reel tape recorders, very heavy.

But just going back to the sample. A problem we had [was that sometimes] it was very difficult to find the poorest people, and I think that, by having this quota system, we were forced much more to look for them, than we would have done otherwise. I think that was good. But we also knew, for each area, that, for instance, that this is a mining area, or a farming area, so we wanted to make sure that the main local occupations were included, you see.

And then, in each area, we would try to use a variety of methods of finding people, like the local radio, local newspaper. Sometimes there were social work contact organisations. There were day centres where old people meet. Sometimes the churches. Sometimes people were just found casually. It was a variety of ways.

And you didn't prefer the person who spoke a lot?

No, we didn't insist on that. The interviewers naturally wanted people who would speak, so that wasn't a problem.

Sometimes we had problems that interviewers were involved in one particular local network, and so it was very important to watch out for that. For instance, I remember, in South Wales, there was an interviewer who was connected to a local chapel, and so [at first] everybody she interviewed turned out to be religious. They were all very good, but they were all religious. Now, we knew from reading, that there was a big conflict in South Wales between the chapel people and the pub people. So we said to her, "Look", you know, "we've got to have people who don't go to chapel. Look for people who go into the pub". And once we said that, then she went and found the other people.

Now, there was one thing I wanted to say, just about the system. There was a problem, which we didn't think of, in this method, which was that because women only worked up till marriage, at that time, there was a distortion. They only worked when they were young. They were all born before 1906. Now, if you had a woman who was born in 1905, then, obviously, she wasn't working in 1911. But the problem is that her mother probably wasn't either, you see. So we weren't getting female representatives of working occupations. So we had to adjust this to allow for that.

My subsequent experience suggests that quotas for retrospective representativeness are rather good. I think they're better than random samples. Because the random samples, if you take the present population, in a random way, it's biased, in many ways. By gender, because of women living longer. There are also particular occupations where people die much faster, like mining, for instance. And also poverty. Better off people live longer. So you tend to get a sample which is more heavily-biased towards the middle-classes, if you use a random sample. With my more recent research, we've tended to use random samples, because that's what all the social scientists are asking for. But I think, actually, from experience, that the quota samples are better.

That's very interesting. So can we go to talk about the interview schedule?

Yes, we had developed that partly by our pilot interviews.

So when you are saying "we", who is "we"?

Oh, sorry. I should have explained who the people were. Well, the most important other person, undoubtedly, was Thea, my first wife, who had become interested, earlier than me, actually, in sociology. She had trained as a school teacher, and then she decided to ... well, we had our first child, Stephen, in '64, and our second, Sarah, in '67, so when this was all starting, in 1968-9... Well, the point is, we got some help in '69 from the Nuffield Foundation, and also Nuffield College. That was when we were doing the pilot work. Anyway, the point is that she had decided not to go back to school teaching, but to do this research with me. And she put fantastic energy into it. She was very interested in things like the family, which I, at that stage, hadn't got so much interest in. I think I developed it later, but partly through her influence. And also she was very interested in the whole interactive process of the interviewing. And my skill then was more as a researcher and writer ...

End of Tape 1 - Side B

Tape 2 - Side A

The other person who was doing the pilot work was Elizabeth Sloan, who was a somewhat unsuitable choice, because she had a problem with her speech. And also, she wasn't, I think, terribly good at empathising with people. I can't remember quite why she took that role. But anyway, she certainly didn't continue with it.

Then once we got the big grant, Thea became the key figure for the whole organising of it, you know, in practical terms. And another key person was the transcriber, who was Janet Parkin, and then there was ... I think Thea must have been half-time, and the other half was a research officer, Ruth Hawthorn, who was married to a close friend of mine in the Department, a sociologist, Geoffrey Hawthorn - he was more into demography. But she was good. And then, later, Trevor Lummis came, who carried on with me for some time, and did the East Anglian fishing project. So that was the basic team. But later on, actually, Brenda Corti helped too - Brenda did some interviewing, and she was the person who sorted out the Archive in the end.

But this basic team did the interviews?

Some interviews. We all did some. I probably did the least. I did quite a few in the Shetland Islands, and one or two in Essex. No, I did maybe 20 altogether, I'm not sure. Thea did definitely more. I was terribly shy of interviewing, actually. I was really frightened by it.

Really?

Yes. Very frightened by it. I was very very shy, very inhibited, at that point. And oral history really changed my personality in the long-run, I think, or helped to, anyway.

But then you made a script? Who made the Interview Guide?

That was made by me, and Thea, we developed that together, over a long period, because we started with a very short one, and then we gradually lengthened it. It was partly by experience, or

one of us would get interested in a particular area. For instance, she was very interested in food habits, so there were a lot of questions about food, that had come from her.

And did you start an interview by asking the name, and the personal life of the person? You had this clear idea in your head that you were doing a life story, or you had a theme-story?

Yes, it's much more of a life story, although one thing which I would do now, which I didn't do in the early, early projects, was to start with grandparents, because I think that's tremendously interesting. They're rather marginal in that first study. And the other thing, which is a complete catastrophe, looking back on it, is that we cut everybody off [from their narratives after] 1918, because we saw this as a historical project. And so, it's tremendously frustrating, because you don't know what happened to them afterwards, except in terms of occupation, that's the only thing which we carried through. To give you an example, Natasha did a re-analysis of the people who were in step-families, and it's very interesting. But, you've no idea what happened to them more recently. And to understand the long-term effects of growing up in a step-family, of course you need to see, did they marry themselves, what happened, and so on.

So you took the person from...

From birth up till 1918. So it's a truncated life-story. And then, within that, we follow it chronologically, but with blocks, thematic blocks. Like routine in the house, food, school, work, you know, courting and then marriage, and so on.

And then, after that, then you wrote the book?

Yes. We sorted out the interviews according to those sections we transcribed. And then we sorted them out according to those sections of the interview. There were 20 sections of the interview.

Like food, costumes, this kind of thing?

Yes, that's right. There isn't one on costumes. No, it's not very ethnographic. It is much more sociological, I would say. And if it had been ethnographic, we would have had much more on dress, I would think. And we were interested in food, to a large extent, because food being a symbolic way of seeing how a family related. And, in fact, the material is very interesting in that way. [We asked] in particular, what happened at the table. Did they all have meals together? Were the children allowed to talk? So you'd get an idea of the family structure.

What are the other main blocks?

It starts with a more demographic [section] about the household, and then there was something about the house itself. And the domestic routine, we were very interested in roles in the house: marital roles, essentially, but also children's roles in the house. And then there was leisure, inside and outside the house. There was religion, and weekend activities and religion. Education. And then, well, work was a very long bit, usually. We had a bit about social mobility, which was the family occupations. We recorded the parents, siblings and children's occupations, and that's interesting material, which has been re-analysed recently, by Mike Savage and David Vincent from Keele University. A lot of this has been re-used, actually. I think those are the main sections. But, you see, the thing is that it took so long. And the interviews weren't always conducted straight. We encouraged people to follow the informant, if they wanted to talk about something, follow that. And so [the interviews] had to be cut up and re-sorted. And there was no word-processing, at that time, to deal with that. It was all done with scissors, yes! Then we thought of trying to do a quantitative analysis, but maybe I should talk about that in a moment, because that was one of the subsequent projects, actually.

But, in practice, I mean, it was only analysed in this rather qualitative way. Although, in my own work, I did always try to count, to an extent, try to understand from the hundred that I'd selected, exactly what was more common and what was less common.

But then you took a hundred interviews. You were doing this for what?

For the book, was the original idea. But I think we fairly quickly realised that we had, really, fallen on an extraordinarily rich material, and we couldn't possibly exhaust it with one book. And

that was how the idea of creating the Archive came. And this was a completely independent decision. We knew of nobody who was doing anything in that way, creating an archive like that. In fact, we scarcely knew of anybody else doing oral history.

At that time, nobody was ...

No. Well, there were people around, and we found them after a little bit, including, there was a local person, George Ewart Evans, who saw himself more as an ethnographer in the Scandinavian tradition. But he did some rather good books on East Anglian farm workers. And we met him, and he helped us to learn how to interview.

It was him, for instance, that said, "You've got to listen to people. That is the key thing. You should never interrupt". You know, "You've got to hear them". And I remember him playing bits of tape, so that we could get the idea. And then the other thing that we learnt from him was the way that some of these older local people had got words which went right back to the 16th century, and that was very fascinating, of course, and very unexpected. Perhaps less important in the long-run. But I do think he was a key influence about interviewing techniques, because the sociological tradition is more rigid.

What I then did is half way between the structured interview with a set of questions, and the completely unstructured interview. The way I see it is that the guide is there as a list of topics, and you've got to know the guide, and know that you need to cover those topics. But you do an interview which follows what the speaker is saying.

But this was so clear at the moment?

No, they developed that idea, and, as I say, George Ewart Evans was a key influence in that. He didn't say, "You've got to do a half-way to a semi-structured ...", because he never had structured questions at all. He just went to talk. He used to talk to people, and get to know what they had to say, and then ask one or two questions to bring it out. Now, I don't believe in talking to people first at all. I think you should go straight in with your machine, and get recording.

Why?

Because I think some of the best stories come out at the beginning, and then if you ask people afterwards to repeat them, my experience is that they're much more stilted.

So you want to tell me about the book?

How did I get from the material to the book, you mean? Well, like I said, I realised that it was impossible to read through all these interviews, well, not only to read through them, but to keep them in your head. I think this is one of the big problems that people don't usually understand, with qualitative analysis: that you actually have to use your own memory, as a computer, because you're looking for something that isn't yet categorised. You're reading this stuff and categorising as you go along. And you can really only do that if you remember it all, because you have to keep going back. You find a new sub-division, and then you have to go back and look, sort it out again, and you can only really do that if you can remember. And I found that ... I don't know how many I could remember now, but anyway, I had a slightly better memory, I could manage up to a hundred or so [interviews]. But beyond that, I couldn't really do it, it started being like the Forth Bridge, you paint it right through, and by the time you've finished painting it to one end, well, you've got to start again! Well, it was a bit like that with the interviews, because you just can't remember the whole lot! So I reduced the number to around a hundred, and then went through this sorting out process.

I suppose, I can't remember ... yes, I'm sure I had a structure for the book, earlier, and then I tried to stick to that. But it was, the structure of the book, as it evolved, became a life-cycle. What I remember about writing the book is that I actually tried, first of all, with a longer version which had, for instance, a tremendous lot about different types of work in it, it was a much more elaborate version. And I wrote for about three months, and I produced a manuscript the size of a book, and I'd only got half way through it. And then I rang up the publisher and said, "Look, this book's going to have to have two volumes". And they said, "Sorry, we can't do two volumes!" So I started again. And that's probably when I evolved this present form.

Anyway, so it's really a life-cycle, you see, first, the first section. It's also got stuff about inequality, and a place and wealth, dimensions of inequality really, but it ends with the life-cycle. And then there's the group of portraits. And then the next bit is meant to look at the different sort

of engines, motors of social change, like politics, the economy and so on. The interviews are primarily used in the life-cycle chapters, though some of them are used in the others. But that's where it comes in mostly. And so I'd be reading and looking for material for those particular chapters, by that point.

But the way we sorted it out, the Archive is usable by lots of people with totally different questions.

So when did you get the idea of doing the Archive?

Well, I'd be surprised if the idea of the Archive is in the original application. I think it came to us fairly early on in doing the field-work. Yes ... but then we'd started, we realised that there was that potential. And it has, actually, been used by a number of interesting people. There was Standish Meacham, who, almost as soon as we'd finished, who ... American social historian, who wrote a book called *Worlds Apart*, which is three-quarters or more taken from our Archive: it's a social history book about Britain. And then another rather important one with John Gillis, who wrote on the history of marriage in Britain [*For Better, For Worse*], and there are, I think, three chapters from our Archive. And then there was one by Charles More, *Skill and the English Working-Class*, where we are again, the major source. There are a lot of people who have used it in a slightly lesser way, and there's a whole book on education based on it fairly recently, too. And this work on social mobility, too, which is recent. So it's a continuing source of value. That's in spite of the fact that until very recently, it was not being looked after, formally, by anyone. Brenda kept a watching eye over it, but it wasn't officially open at all. People would learn there might be a possibility and then write. But I think that it's going to be of tremendous value in the future, because it's absolutely unique. You couldn't possibly do it any longer, there are interviews going back to people born in the 1870s.

Mmm! Yes. Fantastic. And these are rich materials. What happened to the collection?

Well, we managed to get an inner room of the University, where it was put, and it was sorted out, particularly with the help of Brenda, who then in later years usually acted as if she was the Archivist, and kept it together. But it wasn't very satisfactory in that way.

I didn't mention that it was also used a bit for radio work, I was interested in that. With one of my later projects, we did a programme on one of the fishing communities, [with] the BBC/Open University, they came to this town, and produced a programme from the oral interviews we were doing, which was very exciting to do, I must say. And we did do a radio programme about child-rearing. Thea, in fact, did that, with *The Edwardians* material.

How was this? This is interesting. How did that happen?

I can't remember how the contact happened, but there was the kind of openness and interest in the BBC in this kind of work. You see, there were people working for the BBC who had done similar work. They didn't call it oral history, but interviewing ordinary people. So it fitted very well with what they were doing.

And the idea was to use the interviews already done?

Yes. And then subsequently, a number of other BBC people did use it ...

How did they use it? They edited it?

Yes, they copied and then edited. But then we had a problem with somebody from the BBC, who came and copied material without our permission, and after that I stopped them using it, because I just felt it was impossible with an unsupervised archive. And one of the advantages of the fact that it's now in the British Library National Sound Archive, is that it can be used in a proper way by radio people, and I'm sure, in the long-run, it will be used rather a lot.

But they use it for education programmes?

Oh no, no, no, no. No, well, I mean, not on specifically didactic programmes, but, yes, I mean, in a sense, anything historical is obviously a bit didactic, isn't it. On Radio 4, you can have a features programme, and, you know my friend Steve Humphries, who taught with me at Essex for a year, and then went to be a television producer, he's done quite a lot of radio work as well, like

programmes on youth, and whether or not they're different from how they are now, and so on. But that's not in an education programme, that's more in a general social interest programme.

But you don't take the personal life, you don't analyse it in the programme?

No. You use the ones which have already been done. And after all, now they can't find people to do these interviews about the period up to around 1910, it's effectively impossible. And we've got a huge block of material about that period, so it would be the only major source in Britain, for that.

And the other use, you told me you would try to make a computer analysis?

Yes, well, you see, when we did the work, as far as I know, it would have been impossible to have put the text on to a computer. Maybe somebody will tell me that the idea had already been developed somewhere, but anyway, it wasn't well enough known for us to use, and so we thought we would try quantification, and we used SPSS, which involved coding everything that we wanted to quantify.

What kind of things?

Well, they were a whole series of variables, they could be basic demographic ones, for instance, then political ones, ones about work, ones about attitudes to children, religion, leisure, and so on, all sorts of questions we'd try quantifying. And it was very very time-consuming, that's the trouble. Well, that programme was, anyway. Again, it's the limitations of the programmes the variables then. And then that was based in the UK Data Archive, which is also at Essex. But people can request that information from anywhere in the world. And it's interesting that we've had fewer requests for the statistical material, far fewer than we have had people using the qualitative material, which, I think, argues very strongly for the need to have the original sources.

You see, there are a number of other instances where interviews have taken place, and then the original sources are thrown away, and all you get is the statistical outcome, and you can re-work the statistics from what's deposited in the Data Archive, but you can't look at the original. We came across that with the work we did on stepfamilies, because we used as the sample basis for

that, a group of people who were all born in one week in 1958, who've been followed [by the National Child Development Study] right through their lives. Only a tiny minority became stepchildren, and we were then given the names of the stepchildren, and were able to contact them.

One of the curiosities that emerged was that there were conflicts between what they told us, and what we had been told about them, in particular, when they entered stepfamilies. For instance, there were people who told us they'd been stepchildren from birth, who were only put in [by NCDS] as entering step-families when they were, say, 16.

Well, from our interview, you might find the reason, which is, appeared to be that the, the mother had had an illegitimate child, and then subsequently had been able to marry the father, but never told the child about all this. In one case, a married mother had a child by another father, not her husband, whom she later married, and then she pretended that this man [the natural father] was the stepfather. And then there was another, I think, two or three, in which the women had an illegitimate child, and the father disappeared, and then she married somebody else, so she then pretended that the stepfather was the father, you see. And it only would emerge, for instance, when the child had to go and get a passport. So we discovered, from the interviews, how it's explained.

But what would have been very interesting, would have been to see if the [original NCDS] interviewers had any suspicion there was something strange going on. Because, in some cases, there were children by both marriages in the household, and quite often, interviewers put, write things on the interview schedule. But [NCDS have] thrown everything away. And so I think, even with quantitative surveys, there's a strong argument if it's a really important survey like that one, there's this argument for keeping the original information.

And about technical things, did you think about the tape recorder?

Well, we did. We got advice from the BBC. We used the UHER machine, very heavy ... semi-portable, really. And it's clumsy also, in that every time you turned the tape, you had to re-thread it. And also, it has a really very awkward microphone, a big microphone, so in many ways, it's quite difficult to use. But it does produce beautiful quality recordings, and it's good for radio work, because you can cut up the tapes, it's an open reel, you see. So it does have some advantages.

And, for a long time, it was thought to be the only kind of tape which had a long-term archival quality. It's only in the last ten years that cassettes have been regarded as okay for archives.

But I think, on the other hand, we had a real problem with the quality of the recording, because it is extraordinarily difficult to train people to use the machines properly. And, you know, we gave all the interviewers training sessions, but they again and again ...

What kind of interviewers? Did you want history students?

We wanted people with a sufficient sensibility to... the kind of information that we should know. So they weren't all historians, no. I would say rather a few were probably historians, but they were good at relating to other people, and interested in their lives.

They could be housewives?

Yeh, a housewife with some aspirations, interested in reading about sociology, or history. But, yes, I mean, many of them hadn't got work, and they weren't employed. I mean, I think ...

They were paid for this?

Yes. And, of course, if you go back to that period, the sixties, there were still an enormous number of married women who weren't in paid work, so there was a tremendous reservoir of talented people who weren't in paid work. And we really drew on that.

So you worked with a lot of these kind of people?

They were mostly women. There were some men, some interesting, strange men, as well. There was one man who came from a very poor area of Liverpool himself. The trouble with him was that, when he met people from that area, he got on fantastically well with them, and they laughed away together, but he was almost too shy to ask the obvious questions, because they both knew the answers. Yes, he was too close, really. But there was a doctor's wife, I remember, in Nottingham, who was one of the earlier ones, for instance, who she was very interested in research and had done

interviewing on other research projects [with John and Elizabeth Newson]. People like that were the best, really. We trained them both in using the machinery, but also especially in how to interview.

We'd learnt the technique, primarily, from sociological methods books. There were no history methods books at all, obviously. And we got some influence from anthropologists, but that was more a question of understanding that the interview was an interactive process, I think that idea was pushed at us fairly early on, by anthropologists. And then there was George Ewart Evans, who I mentioned before.

End of Tape 2 - Side A

Tape 2 - Side B

One thing I wanted to mention was that, after the training, one of the things that Thea was really good at, was the way that she listened to each interview as it came in, and commented on it. Incredibly long and detailed comments. And a lot of them we've still got, because I think that was a degree of supervision of the qualitative type of research project, which is quite unusual. Maybe it's the ideal. It takes a lot of time to do.

Did she listen to the interview, and then make comments?

Yes. And then phone them up or write to them, at tremendous length, commenting.

After that, what happened? Let's understand the context. You were teaching?

Yes. I came, you see, to teach social history originally, but immediately I started teaching also with Peter Townsend on social policy and social change. And no graduate work at that point. I didn't really start that actively until just as the project was ending, really. Start the social history MA, where we began to teach oral history. And that was, we were the first university in England to teach the methods and so on.

The Voice of the Past [1978] was a sort of distillation of all that we'd learnt, really, on that project, and also, I would say, of the kind of common wisdom of the people that I'd met, who were doing oral history in Britain. And by 1977, when I wrote it, so I just had made first contact with Italy, but Luisa Passerini, for instance, hadn't developed her ideas about the subjective. So the first edition of *The Voice of the Past* is very much a positivistic work. There is a mention about rumours, the importance of rumours, and people re-formulating their memories, but it's a terribly minor part of it, and the main argument is the positive value of these memories, and whether or not they're reliable, and how you decide whether or not they're reliable. That was the research tradition I was coming from, essentially a social scientific one.

So when you decide to write The Voice of the Past?

When did I decide to? Well, I would think probably 1976, it was. I was asked to by Keith Thomas, the famous early modern historian, who wrote *Religion and the Decline of Magic*. A wonderful historian, I think. But he was editor of the [OPUS] series at Oxford University Press, and he commissioned the volume, which was a very far-sighted thing to do, at the time, because oral history wasn't popular with historians at all. But the curious thing was that when I'd written it, he didn't really like it terribly, and the reason why he didn't like it was, he thought it was too practical. He said it should be just philosophical. He didn't like all the bits which - it had a whole chapter on the interview, and how you do an interview ...

But I maintained, both in my teaching and also in writing, but particularly in teaching, right through, that you only really learn about oral history through trying it. You just can't learn about it in an abstract way, because it is partly an interaction. And it's just amazing how you can tell people again and again, how to do an interview, but it's only when they hear themselves on an interview, they play it back and you listen with them, and then they're amazed at the interview, and remark by themselves. They've no idea that they're incapable of keeping quiet, for instance. Or that every question they ask is a triple question, that they can't ask a simple question. You can tell people that's how they should do it, but it's only when they hear themselves in action that they can do it. And, of course, the book doesn't offer that, but it's better than just offering philosophy, I think. I feel that you must have philosophy in a dialectical relationship with practice.

And the book, why did you make this positivist perspective in the beginning?

Well, I think, you see, I came out of history where there's no subjective tradition. I then moved into sociology where, again, at that time, there was no post-modern interest in subjectivity. It was a tremendously strong tradition of doing social research to establish facts, and I was trying to relate to that. I don't think, for the time, it was particularly positivistic, there just wasn't very much of an alternative vision. And the interesting thing is that through the interviews, I had already realised that there was something else.

In particular, it happened through an interview I describe in the book, when I went to Shetland, and [in one of my very first oral history interviews] I asked an old man [Willie Robertson] about whether or not there was much contact with the landed gentry in the Shetland Islands. In reply, he told me a story about a funeral [of a laird], and it was a very extraordinary

story, it was about how there were people invited from all over Britain to this funeral, and they had a tremendous amount to drink, and then they set out, as was the custom in the Islands, with all the men sharing and carrying the coffin. And normally, they tend to group a poor man and a rich man together, as pairs, and then they go for a bit, and then they change round, so that everybody has a share in bearing the coffin, and it's meant to symbolise equality before God. And then eventually he's buried. And in the Shetlands, it's usually rather dramatic, because most of the cemeteries are in a remote spot, or above the cliffs, with the sea below. Anyway, in this case, they set off, and they'd had too much to drink, so one by one they, they dropped by, fallen down by the wayside, until eventually there was just one man left, you see, and then he fell down, with the landowner's remains in the coffin. And then an old crofter, which is a local small farmer, comes by, and he sees the remains in the coffin, and he gets a rope, and puts the coffin on his back, and he buries the laird. But his kind weren't invited to the funeral. That's the last line, that "his kind weren't invited to the funeral". So it's very dramatic. He was saying there was no contact [even at a funeral; they were just totally different social groups.

I was very struck by this story, and I made sure that [Willie and his daughter] told me who it was, and they believed that it was [Gifford of Busta], within living memory, as far as they were concerned. And then I discovered from people in Edinburgh, at the School of Scottish Studies, that this was a folk story which was widespread in different versions, throughout Scotland, but that [Willie] had his own version. His way of telling it was strongly influenced by the fact that, on the one hand, he was an Elder of the Kirk, they're lay people who are important in the Church, and so it had an element of the Good Samaritan, the man who picks up the coffin, in the way of the Good Samaritan. A parable from the New Testament.

But also, [Willie was a class-conscious] Marxist. He'd been converted by visiting speakers, Socialists who came up to the Shetlands on the herring boats. So he was rather unusual in being both an Elder of the Kirk, and a Marxist. And you see that in his story. And I point that out in *The Voice of the Past*, that you could tell something about his consciousness from the story. But it's isolated there in the book.

Then, by the time of the second edition, ten years later, all the people in oral history had been very strongly influenced both by Luisa Passerini on her ideas about subjectivity and silence, and memory, and I think even more by Sandra Portelli and his ideas about the way in which people

changed memory, and how that is part of the value of memory. So there's a whole chapter on "Memory and the Self", which is completely new, in the second edition.

But I haven't moved as far as the post-modern position, where people argue that everything is subjective, and there is, therefore, no point in thinking that you are doing any kind of research which has a real relation to something that happened in the past. Because I think that's a totally self-destructive position. I would argue that it's always very difficult to know what's happened in the past, and in a way, it's a delusion to imagine that you can get to what really happened, but if you work in a genuine social scientific way, you can get nearer to understanding what it was like in the past. And I think we should [aim for] that, and at the same time, understand the difficulties of doing it; and also the way in which the distortions of memory, in themselves, do help us to understand the past, as in the case of that, that old [Shetland] man who had a unique kind of consciousness, that you couldn't have today.

So you think that the oral history is a way of understanding the past, in a positivistic way?

I think that making a sharp dichotomy is the problem. To my mind, everything is somewhere on a line between being objective and being subjective, and you never get to the pure objective.

So you never get to the pure subjective!

Well, I was wondering ... No, you don't, because people's subjectivity is made up of their real experiences, isn't it? You don't invent dreams from nothing.

The Voice of the Past was a conclusion of the work you'd done before?

Yes. The other work which I would have done by then is the fishing project. I'd done the main research for that, because, you see, what happened after the research for *The Edwardians* was finished, we asked to be able to go on and do a whole series of other subjects, to become a Centre, funded by the Research Council. I must say, I still think that would have been a very good plan if they'd put in enough money to fund, say, two people to work on a long-term basis, so the Archive

would then grow, and gaps would be filled and so on. But anyway, they decided they didn't want that. And so they said, on the other hand, they would be sympathetic to further applications.

So we then put in for one which was a study of the upper classes, because, with our representative sample, there had been very few interviews with the upper classes. Now, Thea, actually took that on. And she did a set of interviews with land-owners and business people. And they have been virtually unused, in fact. I never really felt that I wanted to use them, partly from the emotional problem, because by the time she was half way through, we'd split up, and so it became very difficult.

But then the other one was on the fishing communities. The Research Council funded a project on East Anglian fishing, which was carried out by Trevor Lummis, and he wrote his own book from that, *Occupation and Society*, and that's a very thorough piece of work. And he also wrote a book about historical method, *Listening to History*, which is much more quantitative than mine, because he'd done that quantitative work [with SPSS] I mentioned earlier. He carried that out. And he's always been more interested in the quantitative side.

Then I did the research on Scotland, and I did most of the interviews there, [for *Living the Fishing*, 1983] well, I did about half of the original ones in Shetland, and then I went on and did more there, and I really was so excited by Shetland.

How many interviews did you do?

Well, for that project, there's [around 150, and I did about] a hundred altogether. Two or three hundred hours, maybe.

Each interview would be like three hours or two?

Originally, they were longer than they ended up, because, later, I was doing shorter, more strategic [interviews]. Once I'd got enough main life stories, and they were repeating, I didn't want to keep getting the same accounts of, say, work on a particular kind of boat, when I had enough of that. So then I would use shorter interviews to try and confirm what I was doing, or to find out new things. I think if you're doing your own research project, you can afford to be much more flexible in that way. [But] it's less good for other people to use the material. And I would say, too, that because

the Scottish work wasn't fully funded, only a minority of those tapes were ever transcribed. But we sent copies of most of the Scottish tapes to the Scottish Archives, so they are available for other people. And I think it's been of some quite considerable use there.

But tell me, when you did this Scottish project, you had an idea?

Yes. What happened was that we found ... You see, we went to Shetland first, for *The Edwardians*, partly because we thought it was the most different bit of Britain, and we thought, well, "We'll do Essex ourselves, and some of London, because they're near". And then we thought we'd do the most different bit - in Shetland, furthest away. And I knew nothing about Shetland when I arrived there. In fact, it was a terrible shock when I arrived the first time at the airport, and I realised that they were speaking what sounded like a completely different language. Actually, fortunately, it's a dialect, and they all used English to outsiders. But, I felt a total fool. I hadn't discovered that they spoke a different language. They have about a hundred Norse words, from their Norwegian past, and they include a lot of the common words, and also the intimate words. So that's why, particularly, hearing people at the airport, you hear a lot of this [dialect].

But anyway, we became very interested by their extraordinary culture, particularly because it combined a very simple kind of way of life, in material terms, these very poor farmer/fishermen, with a rich verbal culture, and oral tradition, lots of stories and legends and so on. But also a tremendous vivacity they had. Very lively, very thinking people. They're almost the only people I've ever met who, when you're doing the interview, continually turn round and ask the interviewer, "What do you think about that?" You ask them about say, religion, and then they suddenly turn round and say, "And what's your view about that?" "Do you go to church?" and so on - which is quite unsettling, sometimes. But it's typical of them, they like an argument about something. So that was very attractive, and for me, it was very much parallel with - William Morris thought he'd found that kind of society in Iceland, actually. So, to me, it was exciting to find a place like that.

The other thing was the way they brought up children, which was, and still is, or certainly was when I went then, very remarkable, in that they keep children as part of the whole social life, and they don't use any physical punishment, they just use reasoning. Now, if you contrast that with Britain, and particularly Scotland, it's very exceptional. And it influenced me, personally, quite a lot, too. Because when I thought about things then I realised that, particularly with

Stephen, that I'd smacked him, for instance, and believed, at that time, that maybe that was the right way. And then I realised that my own parents had never smacked me, and I thought about it again. So with Esther, I never used any physical punishment at all with her. What they did was, by keeping the children as part of the adult world - they always, for instance, stayed up with adults when adults were talking, conversation. And even today, for instance, they go to concerts: there are little children running around. And in England, people would be shocked, and tell them to get out of the room. But in Shetland, it's just accepted as quite natural, and the children run from one adult to another, and other adults will hold them, and caress them, so the children made no noise! And I was really impressed by this completely different attitude.

And then I thought, "Well, why is this happening?" You could read one or two progressive handbooks that are around 1910, for middle-class people, about it, to not beat their children. But they wouldn't have been reading those sort of books in the Shetland Islands. So then we developed a kind of Marxist idea of why this was happening. Because we noticed, also, the role divisions were much less sharp, and the men were sharing the cooking and the cleaning of the houses, and the women were working out on the land. So we decided, it's to do with the economy, because they are doing this fishing, where they only go out for a day or so, and they come back all the time, and so the men are continually going out, leaving the farm under the care of the women for a couple of days, coming back. While they're on the boat, they have to cook for themselves and clean the boat, and so they learn these skills, and then the women are learning the farming. So they're, they're both multi-skilled, instead of there being a gender divisioning of skills. So that was a nice theory. So then we thought we'd try it out and look at other ...

And the children, did you have an explanation?

Well, I think it was the democracy of the family that affected the parent/child relationship.

This was the idea, when this started. Then what happened to the idea?

Well, what happened was that we found the purely Marxist explanation didn't work, because we also then went and [interviewed on] the Western Isles, where they had a similar kind of fishing, but the family was totally different. The men were completely domineering, and the women were kept

quiet and when you went to interview there, the women got shut into the kitchen! And the children were slapped, and so on. It was completely different.

So then we realised that it was also linked to the role of religion, and how, in Shetland... The important point about Shetland, I think, was that there was a multiplicity of religious views, that people had. It had been Christianised very late, and it was never, I think, never very deep, [though they] incorporated guilt, to the extent that children bear a strong conscience. And what you found was that every person seemed to have their own religious position, which was often an amalgam of different religions. Or it might be a Socialist one, or a semi-Socialist one, and so on. And they would say, "Well, the reason why we're like that is that, when we go to sea, we have a long time out with nature, and we contemplate the meaning of life". Well, of course, that was rubbish, because you found plenty of other fishermen who were out at sea, who would spend their time drinking, or held very rigid religious attitudes.

And I think it really, probably, is partly a survival of Scandinavian culture, in fact. But it's very difficult, [because for family relationships] you can't go back beyond the late 19th century, so you can't really be sure of whether the pre-Christian epoch which had similar attitudes. But my guess would be that it does, because you do find this gender equality and attitude to children on the West Coast of Norway, where a lot of the [Shetland] people had come from. [Shetland was] Norwegian up until the 15th century. So in the end my interpretation was that it was communal culture, including religion, and family culture, which were the missing variables in the explanation.

And these life stories, who did you choose to interview?

Well, it's a very different kind of interview sample. We had the Census, and we looked at that, to find out what the main significant occupations were. But you don't want a representative sample, really. Obviously you want a lot of fishermen, because these are fishing communities. But then, what's going to be essential is to ...

For instance, when we went into one of the first places, Buckie, now, this interested me. I chose it because it was a town which converted from sail fishing to steam fishing, in the 1900s, around 1910-1920, without going capitalist, effectively. Their boats stayed owned by the local people, the families, instead of by companies, and I thought I'd like to try to understand how that happened. Now, we arrived in this place, and we got recommended all these marvellous old men

[to interview]. But after a while, we noticed that they were all skippers of boats. So we said, "Well, what about the ones who weren't skippers?" And they said, "Oh, well, anyone with any intelligence, became skipper of a ship. There's no point in your talking to the other people, they wouldn't have anything to say to you".

And it was a long time before we eventually found some people who'd ... There was a man who'd been a lifeboat man, and had served earlier just on the deck of a boat. And through him, we got on to another network, and it turned out there was a different sort of network, a slightly different social group. And they had definitely a different story to tell. Also, their ideology was different. They were much less religious. And they were more traditional. And they had more weather lore, weather superstition. Their language was a bit different, too, and so on. So it was very very important to get them. And we wouldn't have done, without this concept of sampling.

Of course, it was obvious we needed women too. And then we realised, after a while, that there were key financial intermediaries, like the sales people, in particular, who bought the fish, and arranged the loans for boats. So then we tried to interview those. And so, in a sense, we proceeded [to sample] in a strategic way, rather than in a representative way. And I think that's the only way to do that kind of study.

I also did some more anthropological observation, like sitting in the fishermen's cafe on the harbour, and sort of chatting to people and listening to them, while they watched the boats come in. That's where the older men used to go. And then I went out, I got to know a fisherman [Andrew Noble], who was a deckhand on a very modern boat, who was interested in history too. A very intelligent man. I still am a friend of his, and we go and stay with him and now and then, and he comes down to us. But he took me out on their boat, and that was a very interesting experience, and very powerful, I must say, I found it ... Three or four days out at sea.

And how long did this book take you?

A long time, off and on. You see, again, it was a fragmented kind of way of life, really. I got interested in 1970, and the book came out in '83. So it was a long time.

But at this time, you already had the idea that the material should be a source for other research groups?

Well, we had the original set of material [for *The Edwardians*], and that had been archived properly, and we also archived the upper-class families. And then the East Anglian fishermen, we sent to a maritime museum in Yarmouth. But I didn't know what to do with the fishing material, so we kept the tapes at Essex, but we did send copies to these various archives in Scotland, over the years. And we never had the money to transcribe properly for that. Not full transcriptions, no. No. Some of it is transcribed, but quite a lot is not.

So how did you [write it up]? You were listening?

Yes. Yes, I had to listen.

So even for The Edwardians, and Living the Fishing, when you wrote the book, you believed you have to read or listen to the whole interview?

With *The Edwardians*, I read and then I checked all the bits I wanted to quote. And I think that's the ideal practice, to check everything. But I must admit, in some of my more recent work, I haven't actually checked. I've relied on the transcriber. And I don't really think that one ought to do that.

But you read the whole interview? You don't, for instance, just read through the part that the person is just talking about a theme, say] religion, funerals, about death?

No, I'm sorry, I can't claim that I've always read the whole interview. I think it's the best practice, and certainly with the fishing, I read all the interviews. With *The Edwardians*, I [eventually] read them all, but [for the book itself initially I focussed on one hundred].

End of Tape 2 - Side B

Tape 3 - Side A

The answer is that the only exception was the Coventry set of interviews, where I wrote largely from the cut-up versions, rather than the, the life story version. (We'll come to that.) But, for instance, in our most recent work, the one on stepfamilies, it's been terribly important to read the whole interviews right through. And I certainly think, as long as you have a manageable set, the number is not too great, it's the best way of doing it.

So you would read the fishing, and then you made the book?

Yes, *Living With Fishing*.

The result of this research was your book. There something else?

We did a radio programme, that was while we were doing the fieldwork that I mentioned earlier. But no, I've always thought it would be a great subject for a radio or television programme, but I've never done it, unfortunately.

And what was the main idea in the book?

Well, it started from this question about child-rearing, and then it gradually re-focussed, in terms of my bit, around why was it that some fishing communities had the adaptability to change, and to deal with new technologies, new markets, changes in the species of fish you could catch, and so on, and others didn't? I tried to explain that in terms of socialisation and the community culture. So the extreme was between Shetland, where children were brought up to talk and have their own opinion, and think for themselves, and take responsibility for themselves from an early age, and the fishermen were remarkably creative, technically, for instance, they developed new adaptations of the equipment that was available, and they were prepared to shift their fishing patterns and so on. Like the people in the North-East of Scotland. But, in contrast to the Western Isles, where the children were brought up to follow their elders, and they just seemed to be incapable of adapting themselves, economically. So that became a theme that I became very interested in.

Now, I should mention the book includes work by Trevor Lummis, which is about religion and superstition, which is a separate chapter. And then quite a big section by Tony Wailey, about a small inshore fishing community in North-West England. So it's not only by me.

The book is all about fishing?

Fishing communities, yes. No, there's a lot about women, as well, in it.

But the thing is, this whole question of socialisation and economic behaviour was again the inspiration behind the new project that I went on to, which was called, "Car Workers in Britain and Italy". For that one, we got funding from the Leverhulme Trust. And I did that with Luisa Passerini. We did it in the mid-eighties, and it was a contrast between Coventry and Turin. [Luisa] lives in Turin. And it really sprang from two different sources. One was the fishing project, and the idea of whether socialisation is fundamental in looking at economic adaptability and creativity.

But the other was going to the FIAT factory with Luisa, and seeing the extraordinary changes that were taking place, with robots being introduced. You could see the contrast between the assembly line where they were working in the old way, and the robotised one, and there would be, say, 20 people on the robot line, and 200 on the other line. And just thinking, this is going to be an extraordinary change, and if you want to study car workers, you'd better do it now, because there won't be any, before long. And so we got money, in England, from the Leverhulme Trust, and in Italy from the Provincia, the provincial government.

And that, unfortunately, as a piece of comparison, proved a complete fiasco. Luisa is somebody I admire very much as an intellectual, but she's not exactly a practical researcher. I don't think it's her kind of tradition. She's trained originally as a philosopher, and I think such ideas are what really interest her. And also, gender issues had come to interest her most of all, I would say. So, although she'd done [notable] early oral history work on working-class culture, I think by the time we started our project, it wasn't really what she was interested in. She had a team of [younger women researchers] there. Well, we agreed a common Interview Guide, and then it turned out they didn't follow the Interview Guide. Also, they didn't use any care, as far as I could see, in following the quota sample that we'd agreed. So that, for instance, there was an incredible over-representation of minor political groups, some of whom happened to be [connected] to one of the interviewers. You know, this sort of problem. She just didn't take enough care over watching

things in that way. So when I later came to use the interviews, it proved impossible to work on my main theme, because there was nothing on childhood in them, which was for one of my main interests, to link up socialisation and the family, with life in the factory. All you had was the life in the factory.

You only had life in the factory?

Well, most of the interviews tended to be like that, or Trade Unionism in the factory. But then, the other problem was that, after about two years, there was a local election, and a new political group got control of the Provincia, and they immediately cut off the research funds! And they even impounded the material, so that nobody could look at it, and for over two years, they weren't allowed to look at the material. Well, by that time, the research team had disappeared, so that none of it was written-up. Luisa, herself, had lost interest, and instead, she wrote an autobiography.

An autobiography?

Yes. It's it's a group autobiography, *Autobiografia de Gruppo*, and it's about her childhood and then the sixties, '68 in Turin. And then her experiences with her psychoanalyst. And there's a lot of quotation from her diary. And the only reference to our research project, which was happening while she was going to see this psychoanalyst and writing her diary, is of coming one time to Oxford, and she says, "A friend cooked me a fish supper". That was me! "And then, after that, I had a strange dream!" (LAUGHS) And I thought, when I read this book, if I had known she was in that state, I probably would have not gone on with the research with her! So, that was a disaster...

What happened to the English side?

Well, the English material went to the City Archive in Coventry, and my research assistant [Linda Grant] was very keen to do a book, using it, and she spent ages trying to write it, but didn't succeed. So the only thing that came out from it was, I wrote three pieces, which were intended, really, for a joint book with Luisa, but she wasn't interested. And of those, one's already been published, and

one's coming out. ["Playing at skilled men", 1988; "Imagination and passivity in leisure", 1998].

One is about skill, and the whole way in which the skilled workers in Coventry tried to maintain a belief that they were skilled, when they were actually doing assembly work. It's got a tremendous amount about the work culture in the factory, as well as a bit about it outside. And I think it is a very good way of using oral history, because Trade Union material never has anything about this. You don't really understand, I think, from the Trade Union records, what people thought they were doing. They'd had this extraordinary Trade Union power in the factory, and they insisted on collective payment, for instance, of the different groups on the assembly line, and it was all in order to maintain the illusion of being multi-skilled, and that anybody could move to any point on the assembly line, and so acquire all the skills [needed to build a car]. So that was part of their idea.

And then I wrote something on leisure, and this is interesting, partly because of the way the leisure activities didn't relate, at all, to car work. They were completely different [worlds]. On the other hand, I did find there were two people who went in for very elaborate technically creative leisure activities with their children, one of whom developed a sort of automatically controlled rockets which they used to let off in their garden. And another one [encouraged] scientific experiments. But both of those children, interestingly, have gone on to become scientists. So that was quite neat!

But the idea which was emerging from the research, was of the way in which, in England, both the work culture and the family culture tended to stultify any kind of inventiveness to do with work. Although you found people who were capable of inventing things, they were suppressed almost all the time. While in Italy, there was a much higher valuation of this [work creativity] in the culture.

And so [as I wrote in an article in the *Guardian*] then when you come to the recession, the people in Coventry were in despair, and they didn't know what to do, while in Italy, [the unions] were already telling FIAT that they must invest in new technology, and retrain the workers to use new technology, and they were also setting up co-operatives to develop all sorts of extraordinary kind of new inventions that individual workmen had developed. It was a completely different atmosphere. You know in FIAT, you know the famous baby FIAT? [Before 1945 Fiat had built just quality cars.] Well, the model for that [baby FIAT] was created by the workers and the Trade Union, and there was a Trade Union campaign, in favour of building this popular car, as a mass car

for the people. And eventually, the management accepted it, and produced the most successful model that they ever had. But it was due to the pressure of the workers. There was nothing like that in England.

I think this is a very big cultural difference. I think it's due to generations of factory work in England, and the way in which the factory worker, and the management, have tried to separate people's work lives from their general lives. If you were a designer, if you were in the design office, you designed, okay. But if you were an ordinary worker, you weren't expected to be interested in technological innovations. And the Trade Unions weren't either. They were interested in what people were paid, and work conditions, but that was all. While in Italy, they tended to see the thing more as a whole.

This is very interesting. But then, this didn't become a book?

No, it never became a book. It had to be abandoned. After Luisa withdrew, I spent some time, probably for a couple of years, I went back to Italy a lot of times, and I was reading the Italian interviews, and for me to read the interviews in Italian, takes a long time, and some of them were half in dialect, which made it even worse! So I could only read about three a day, because they were long interviews.

How many interviews?

Well, they had 120.

And you here?

Yes, 120. I never got through all the Italian interviews, because, in the end, I realised that the material, there wasn't enough of the material [that I needed] there. I could have used them for something, but it wouldn't have solved the problem. Then I thought maybe I'll manage to find another source of money. And I found a very good potential interviewer, in Turin, to do some interviews on the family life.

Because one of the things that became interesting was that, we have the illusion in England that Italians have always been keen on babies. Because, after all, you see how they are today, if you travel today, there's this wonderful attitude to babies in Italy. And then you go to museums and there are all these pictures of Madonnas with lovely, happy fat babies. So you think it must have gone back. And, in a sense, it is true, that you find in Florentine culture, in the Renaissance period, there is a concern for babies, and how you bring them up, and the relationship between the mother and her child, which is very modern. But, on the other hand, if you look at Italian peasant culture, it's quite different, it's absolutely the reverse. It was very hard, incredibly severe punishment, worse than in England, and they don't like children to talk, you know, children are expected to keep silent, while in the popular society in Italy now, families always eat together, the children are always talking very loudly, and if you listen to Italian schoolchildren going around, they are nearly always talking incredibly loudly, a society which, more than any other in Europe, encourages children to talk, I would say. And in the schools, they, they deliberately encourage children to talk, they teach them to talk in front of the class, so they learn oral presentation before written presentation. So it's very interesting in that way. But it appears to be new, it's not something that goes back centuries. And I don't know how it's developed. And I thought, maybe we could do a study in Turin, and we could compare people from rural backgrounds with people from the city, who were either from middle-class backgrounds, or from skilled working-class backgrounds, because there was an urban artisan tradition as well. I don't know what it was, nobody knows, it's never been studied. So I thought that would have made a really interesting subject, but, unfortunately, no [funding] body agreed! (LAUGHS)

And then, up to that, or doing that, you were working on? Life Stories and Ageing? Families and Social Mobility? They started together?

No, that's right, I was confusing it with something else. Now, yes, what happened there was, that the, the Life Stories [and Ageing] project was one I was asked to do. The Research Council had an initiative about research about older people, and I was invited to put in an application to do something. And so I did that. And that was intended to be about the experience of getting old. But, at the same time, I had, through another initiative, managed to get funding for this comparative study of families and social mobility, with France. There was a joint initiative between the English

and French Research Councils. Now, the Anglo-French idea was one which I'd developed some time much earlier, with Daniel and Isabelle Bertaux. I should think five years before, probably, we first produced this idea.

And what was the main idea of the study?

Well, he [Daniel] had worked in social mobility from [the 1960s], but used, originally, quantitative material, as social mobility studies largely do. But he became very dissatisfied by that, and decided that he would do a detailed study. So he then did one of bakers, and he did publish this work on French bakers, and the way that the people worked as couples, and what that implied for [the social trajectories of] the children. How, for instance, they choose one of the children to run the bakery, to succeed them, and there's only room for one, because they have to be a married couple [to run a bakery]. So he was looking at a process of succession in social mobility. And he got very interested by that and wanted to do a wider study, and we had the idea of doing a comparison of this in England and France. And so then we thought we had funding, [and] I got the other one, the Life Stories [and Ageing grant], so I decided to put the two together, and do the fieldwork together.

Because that enabled us to do two or three generations in each family. And so, for the old people, you could [hear from them] about their children and grandchildren, and [also hear] what the children and grandchildren thought about them. So it developed in that way. This is when I first started doing trans-generational interviews, within the same family, and I felt it was a very exciting new approach.

And [at the same time] I got involved with Gill Gorrell-Barnes, the family therapist, through Natasha's influence, because I'd been married to Natasha for some time, and I got interested in the ideas about family therapy and the way in which the relationships within families are a kind of [interlocked] social and emotional dynamic, so that what happens with one person relates to what happens with the others, it is all inter-connected, so you understand the family as a whole. And you also find repeating systems of people who do the same thing in each generation. Or reject. There are also rejections across generations, so people do the opposite to their parents. And then the next generation does the opposite again, so they end up doing the same as the grandparents! So

that was the idea. And we got some additional help from [Gill], we had some workshops with her where we looked at clinical interviews with families with transgenerational problems.

I also went to a family therapy workshop with Jeremy Holmes, about the family therapy and the older generation, which I found very helpful. And it was showing how, [for instance] an ageing man comes for help because he's suffering terrible pain and psychological anxiety, and then you discover what it's really to do with is the family set-up, in that he wants, he needs his son to take over, but he's too dominating to let [the son] take over. This sort of thing, you see.

By this time, did you change your way of doing the interviewing?

Not in a really important way, I would say, no. No, I don't think so. I think, really, my approach to interviewing has been fairly constant since *The Edwardians* and *Living the Fishing*. But with this most recent project, we introduced the idea of starting with the grandparents. We also tried, experimentally, starting with family trees, but I came to the conclusion it was not a good idea, because it got people to go into very detailed [factual] information, instead of the more feeling, more vivid kind of information that one wants. But we also introduced a lot of other questions to do with ageing, some of which I've used in more recent projects. No, I don't think [the interviewing style] changed. It was more in the way of interpreting it, and therefore, some of the questions. I think there's more of a separation of fact and feeling, not that that's ever really tight. But you're trying to [ask], "Well, can you tell me what's happened?" And then say, "What did you feel about that?"

This is new?

That was a relatively new approach, yes.

15 June 1996, second part of the interview.

Paul, let's talk a little bit about your project, "Lifestyles and Ageing", that you did in 1985.

Yes, well, I think we talked quite a lot about the research plan on, so it's more useful, perhaps, to say something about the way in which we analysed that. Because for the actual fieldwork, the two projects were combined, but they were analysed separately.

In the one on Life Stories and Ageing, what we were looking for, originally, was an ethnography of the everyday life of an older person, both what it felt like to become old, but also, in practice, what people did in terms of domestic relationships, leisure activities, and obviously health problems. But not primarily focused on the problems so much as what was the normal life of older people. Because there had been a tendency in the research, to focus more on the problems of old age, which, I think, is very unfortunate. It reinforces the stereotype of older people being decrepit and useless, while, in fact, most older people are not sick. Of course the older you get, the more likelihood there is you are ill. But even when you get into your mid-eighties, there are more people who are not ill, than people who are ill. So it is a very unfortunate stereotype.

Now, when we wrote it up, Michele Abendstern and Cathy Itzin and I did a book called *I Don't Feel Old*, and that is composed partly of the material directly from that project, but also a re-analysis of some of the material we had from older evidence, including *The Edwardians*. We hadn't deliberately collected material [on ageing] in *The Edwardians* [interviews], but we did have some evidence there, and that's used. But with this project, where we were fortunate enough to be able to do that, we had three generations in the same family. We also had memories of [each person's] grandparents, so we were able to use those memories of grandparents of the oldest generation, to go right back and give a picture of older people in the early twentieth century. So we used a series of different interview evidence, both within the project, and also out of it, to construct both a picture of old age now, and also a social history of old age. So the first part of the book is a social history of old age, and then we look at old age today.

I think the thing that struck us most powerfully, was the way in which old age was a phase of life where people had, in a way, to start creating their own destiny again, because it was as if in the middle years of life, you were fixed, you had a job, you were a parent or whatever, but you've got a trajectory which is fairly clearly established, and you also don't have a great deal of free time.

But once you retire, you're a bit more like young unmarried people are, you have this curious freedom, but with it, a problem of identity and meaning in life. So to search for meaning is even more crucial, I think, at that stage in life, than earlier. And we found that, obviously, was related to whether or not people had found something purposeful to do. If they hadn't, they were usually rather depressed, and, in fact, there's quite a lot of evidence suggesting that, with older people, being physically unwell and being depressed, are more powerfully linked than they are with people in the middle years of life. So it's actually rather crucial, to find meaning, even to survive physically, at that stage. And the book's fundamentally about how people found meaning. So you can find meaning either ... Particularly men, carry on working, [and there are] some women who carry on trying to make housework the centre of their lives. And then there are people who live for love. They're usually old people who have relatively recently remarried, and it's very striking to find these two people in their seventies, who are rather like ...

End of Tape 3 - Side A

Tape 3 - Side B

No, no, no, this was ... I think we explained that earlier, this was a new set of interviews where we put together the field work for Life Stories and Ageing, with Families and Social Mobility. And what we did was, we took a sample from another study [the Stagflation study by Howard Newby, Graham Marshall and David Rose], of people in their middle years, in their thirties, forties, early fifties – [we interviewed] over a hundred of them, 120 [families altogether because there was a deliberate over-representation of Scotland], but I used a hundred for the analysis. [After interviewing the middle generation informant selected from the sample], we then tried to interview an older person, the grandparent generation, and a child. Actually, in practice, often we weren't able to do that, because either the older ones had died, or the children were too young. So that proved not possible to do all the time, but we did it for a lot of them. And, of course, it was very interesting when we were able to. And it helped to develop the theory for understanding the families, which then one could use. Even when you had a single [middle generation interview only], you could begin to see what the [transgenerational] relationships were as well.

But anyway, going back to Life Stories and Ageing, you see, this idea about finding meaning: I hadn't mentioned leisure, that was the other area of it. And that was particularly impressive, because it was the one where people tended to innovate most, at this stage in life, they would take up a new form of leisure. There was a woman who took up flower arrangements, for instance, and it became absolutely essential to her way of life, and even her whole garden depended on growing leaves for her flower arrangements. What was interesting was, though, that nearly always people were drawing on things which were in their earlier life, or their family, because, for instance, in her case, it went back to her feeling that she was artistic, it was in her kind of inheritance, and her father had been an artist.

So, it fits in with my interest in creativity in everyday life. In order to survive, in other words, as an older person, you have to have a certain degree of adaptability, creativity, because you've lost your work, and as you get older, of course, you lose friends, because they're dying. You often lose your home. And you have to have an ability to re-create something new – to make new friends, make new activities and so on. If you don't do that, you get depressed and very likely you will die relatively earlier. Well, if you find meaning, you can be incredibly happy and fulfilled. And some of these people we interviewed were absolutely wonderful.

I would say that the television programmes that I did with the BBC on the people over 90, *The Nineties*, those people were very impressive in precisely that way, because they're people from an age group which is normally totally written-off. I mean, we talk about the "old old", one of the things people tend to say now is that being "young old", is okay, up to 85, but once you're over 85, you've totally had it. And what we discovered was that these were extraordinarily vital people, who appeared to be 20 years younger than they were. And why was it? Well, one of the fundamental reasons was that they had really meaningful activities.

It was very varied. There was a woman, for instance, who was very political, her father had been Secretary of the Fabian Society, a Socialist society. We tried to film them all doing something now, as well as talking about their past. So we filmed her chairing a Labour Party meeting. And then again, there were others, there were at least two very moving cases of people who had found a husband or wife, when they were in their late seventies: one case was a woman, the other case was a man. And it was wonderful, very moving to see the love that these people had found at that point in their lives. And then again, there were people with hobbies, and so on.

And this is a long-term interest of mine, creativity, and it's something I want to do more about, actually. It connects up with my interest earlier in the arts and architecture. And the way that it now has come together, is of interest, particularly, in this creativity of early everyday life, but also an interest in popular art.

That's one of the things I found very exciting about going to Latin America, first of all, Mexico, [then Brazil]. I think it's just extraordinary, the variety of things that people make, very poor people, and not stereotyped things, but continually inventing. Natasha and I, about two years ago, went to this village [Ochimicho], where they make pottery with devils as the theme. You can get devils there doing almost everything. There are many women potters, very very poor, they live in one room, and half of the house is the kiln. But they're all doing different things, they've all got their own styles. I think it's very impressive, the potential of ordinary people. And also, in Pernambuco, North-East Brazil, I went with Ana Dourado to a place called Alto do Moura, where we met an extraordinary man [Manuel Galdino], who was a potter and poet, again, very poor, very simple man, his kiln in his back yard, and just about two rooms in his house, and he's both a potter and a spontaneous poet [repentista]. Every piece of pottery he makes, almost, is different. I'm very fascinated by how it is that some cultures allow people to develop in this way, and, I think, realise the potential which is in human beings, and so many other cultures just repress it.

Certainly, that was a major theme of the Coventry study, the repression, really, of that creative potential in the car workers. I don't know whether talked about that. And it's a continuous theme, that keeps coming up. It's also in William Morris, you see. William Morris believed that there was a creative potential in everybody, and that everybody, of the simplest level, should learn to make something, like making pottery, or something like that.

I haven't ... well, I haven't explained about analysing the other half of the project, the families and social mobility.

Well, that was, that proved a more difficult task. You see, this was a joint project with the French, I was supported by the CNRS, in Paris. But then it turned out, after we got our money, that the French side had not been given the same amount. They had no money for interviewing or transcribing, and that the people who had applied for the research, Daniel and Isabelle Bertaux, were expected to do all the interviewing themselves, and I guess, all the transcribing. Which was, in a way, totally absurd, because they had lots of other responsibilities. So this was a bit of a shock. And the result was, actually, that while we did interviews with 120 families, they did only about 30, and so there was no possibility of doing the proper comparison that we envisaged. So that was the first problem.

We took a very long time, really, over it, because another difficulty, a more personal one, was that Daniel and Isabelle were splitting up, after the research had started, so it was very difficult to get everybody together and discuss everything, let alone write together. So it was a somewhat ill-fated project, in both senses.

And then again, yet another difficulty was the sheer mass of material. You see, with Life Stories and Ageing, we actually had only 55 older people to analyse, and that was very manageable. But with the other [project], that meant that we had about [250] interviews, with all the generations, and I think we just had not worked out how to handle that number of interviews. The conflict was that you needed that number to impress the more statistical sociologists, who were interested in social mobility. But once you've got it, you've got an overwhelming mass of [qualitative] information, and it was very difficult to see how you could ever [absorb] it. You can use, obviously, modern techniques for sorting it, once you've got to that stage, but you do have to read it, and, really, remember what's in it. No machine can substitute for that, I think. So what emerged, in the end, was that, first of all, well, I did a volume of the *International Yearbook [of Oral History and Life Stories]*, with Daniel, called, *Between Generations* (1993),

which was mainly other people's work, but did include a first attempt, by me, to analyse the material, partly in terms of family movements, in relation to family models, and partly in terms of myth. And I was really interested by both, because the idea of the models comes from the family systems theory, and the family therapy influence.

For instance, you can look at the way that each family, you could do a big family tree, of all the occupations, and then discover that there's not just father/son transmission of jobs, but mothers are also influential, or aunts, and then you start looking more deeply. "Well, why do they choose to accept or reject family influence?" And you can find that links in with some of the emotional things that are going on.

So I was trying to do this complex kind of analysis, but basically showing that transmission between generations is much richer and more complicated than just father's occupation and son's occupation. Women as well as men; and it's emotional as well as material and occupational. And trying to look at the things which were transmitted, which include attitudes, different sort of ways of behaving, emotional relationships. You'd get patterns, like patterns of violence, patterns of divorce, patterns of role division, like you get cases of families where all the women marry men who cook, for instance. All these strange repetitions that go on, and I got very fascinated by that. And I'm still interested in that.

And then the other thing was about [family] myths. Again, I think it was the family therapists who alerted me to that, particularly John Byng-Hall, who wrote about the tradition in his family, which goes back right to the 18th century, [from] Admiral Byng, who lost Minorca to the French, and was shot for cowardice. And the result was that generation after generation of men in his family, behaved in a most extraordinary way, in order not to be seen as cowards. Like his grandfather, who was Governor of Nigeria, and when there was a rebellion, he stood on top of a hill, dressed only in a white smock, or gown, and the rebels were so amazed by this, they thought it was a ghost, I imagine, and they just went back home! But these are family myths. And we get these family myths also in the step-family interviews, very interestingly. And so I wrote a paper about mythical transmission.

And then went on from that to do a book which is not yet out, which I have, again, edited with Daniel Berteau, *Pathways to Social Class* (1997), where I've written a bit more along those sort of lines. But I also did a paper with Isabelle Bertaux about houses, and what these mean to families in different generations, and how attitudes to houses can be transmitted, and how houses can capture

people. The transmission of a house can actually capture the person, like the transmission of a family job can catch people, it's about that. And we've also got papers by a variety of other people who are, in one way or another, related to what we've been doing. So that Brian Elliot, for instance, in Canada, used our ideas and did a social mobility study, and so he's done a paper on Scottish migrants to Canada. And then David Vincent has used the original *Edwardians* material to do a paper on the occupations that people wished they'd had, but didn't have, the imaginary in social mobility. Now, that book's with the press, and will be coming out early next year.

It's partly by us, as authors: in the Introduction by Daniel and I - a more theoretical one - and then there's a paper by him and Isabelle, on a single French family, and then there's the one by me, which is particularly about gender and social mobility. And then there's [me and Isabelle] on the houses. And then Daniel has done one on transmission in Russia, what happened to the upper-classes after the Revolution. Well, those are all by us. But then the other people are people we are closely associated with, who have been either directly working with us or who have been partly inspired by us, or are using our material. So it's not been totally authored by us, but I think it's fair to say it's like a "school" of research, you see.

So this research ended in this book, in a paper, and the material that is now in Qualidata?

Well, no, it's not in Qualidata, it's been transferred to the National Sound Archive [but we also have a set of transcripts at Essex]. Yes. But that theme is one that I'm still very interested by, the transmission theme.

You are interested in a lot of things!

Yes. For instance, we're going to come to the National Life Story Collection. But one of the themes we developed there was the Second Generation Holocaust survivors, which Natasha, in particular, has worked on. And it's, again, the same kind of interest, what are people transmitting in a very difficult situation for transmission? And the step-families, again...

Yes, well, if you could talk a little bit about the stepfamilies, and if you want, about the work you have on oral history and the history of medicine?

Well, the oral history and the history of medicine is, really, hardly worth saying much about, because that's a teaching activity, basically. We've every year done a one-week residential course for people who want to do oral history interviewing, who are historians in medicine. It's a very well-funded area of research in this country, because of the Wellcome Trust: the Wellcome Foundation have, for a long time, had the profits of Burroughs Wellcome, so they have immense resources. Part of this was funnelled into the history of medicine. So it's a very flourishing area, with a lot of very good people working, and, it's been very exciting to do these courses. We usually get a mixture of people from students, post-graduates in their twenties, through to quite distinguished retired medical people, or scientists.

But they learn to do their own interviews?

Yes, they do interviews as part of the week, and we also interview each other. So some of the people in the course, all the significant interviews are offered to the National Sound Archive, and that means some of the people on the courses are actually interviewed. Some of them are doctors, but they're basically people studying the history of medicine, so they may be historians. Some of them are retired doctors.

The result has been quite a lot of new studies have happened, very good ones (Joanna Bornat, Robert Perks, Paul Thompson and Jan Walmsley (eds.), *Oral History, Health and Welfare*, 2000). There was one on the origins of geriatrics as a speciality, another on GPs, before and after the Health Service. Another on pharmacists. And all these [students] are trained, they do a high standard of interviewing, and the interviews are deposited in the National Sound Archive. So it's not my own direct research, but it's a way of stimulating material.

So, and about the stepfamily, which is, it sounds to me, connected to the social mobility one [Gill Gorell Barnes, Paul Thompson, Gwyn Daniel and Natasha Burchardt, Growing Up in Stepfamilies, 1998].

Well, the origin of it, it is connected in a way, but the origin, of course, is having experienced stepfamilies to an extent, myself, having re-married, and both Natasha and I having children by our

first marriages, and so getting some personal sense of some of the emotional agonies that these complex relationships can cause. And feeling through that, that there really wasn't enough evidence of what was okay for children, and what wasn't. How best to handle things. And wanting to make some contribution.

And then I got very impressed ... I went to various sort of research gatherings, originally, through Natasha, and met Gill Gorrell-Barnes, who is a leading family therapist, and I was very struck by her approach, and the originality of her family systems approach, as she presented it. And then I was keen that we should work together. It definitely started as an idea, her influence, through the stepfamilies. But actually, we then worked together on the Families and Social Mobility, because we had sessions when she showed videos of their work at the Institute of Family Therapy, videos of interviews with patients. And then she also listened to some of our material, and we had a collective discussion on that. And it was extremely worthwhile and interesting, there were differences in methods, but we were able to find ways of thinking in common.

Then we had a lot of problem raising enough money to do this step-family research, and we had a much more ambitious plan than eventually arose. Then, finally, we got just £10,000 from the Wates Foundation, which wasn't really enough to do the job properly, but it was enough to do the interviews, with expenses paid, and [besides ourselves as interviewers – Gill, Gwyn, Natasha and myself] some people were paid for interviews [including especially Michele Abendstern], and then [we could] get the material transcribed.

The key problem was to find a sample, because it's been the experience of many people who have tried to do studies of stepfamilies, that people are reluctant to come forward. If you publicise that you want to do a study of stepfamilies, people won't come forward. Or, if you do get people, then they're not typical at all. And this means that you can't generalise, obviously, from the work. So we were very keen to do something based on a proper sample. For the only other book, for instance, in Britain, that's an in-depth study based on interviews of stepfamily life, *Making a Go Of It*, by Jacqueline Burgoyne and David Clarke, they just really had to give up [their attempt at a reliable sample]. They started by going to Marriage Registry, to get couples who were remarrying, but they just couldn't persuade enough of them to be interviewed, and they had to end up by working through their own personal network.

So we were fortunate. We found a solution, which was [the National Child Development Study], that's a longitudinal study of all the people born in 1958, in one week, in Britain. So they

have documentation of the people who grew up in stepfamilies, and who are now in their thirties, [and they generously agreed to give us] a list of them. We decided we wanted people who'd entered stepfamilies between 7-16, because we thought if they'd gone into stepfamilies earlier, the problem would be, they wouldn't remember the transition. And so we were given a list of these people, and then we did a regional sub-sample, because there were too many, and we decided to cut down the travelling. But we balanced that out, so it was representative. We interviewed 50 men and women, we balanced out men and women. We tried to have somewhat more stepmother families than we would have done, statistically, because they're much fewer, but we wanted enough to be able to compare them with stepfather families. We also had families from death, and families from divorce.

But then we found there was an immediate problem with the sample, which was, I think, very interesting, that one-fifth of our people had not actually become stepchildren after the age of seven, because the earlier interviewers on the longitudinal study had been misinformed about what was happening. Several of them had been stepchildren right from birth. The children had been misled, and so had been the interviewers. They pretended that the stepfather was the natural father. And there was a bizarre instance where it was the other way round: the child had been born out of wedlock, extra-maritally, when the woman was married to somebody else, it was a child of an affair when the mother was married. And then the man died, the original husband died, and she married the child's father. But she went on pretending that the original husband was the father. So this child was brought up as though she was with a stepfather, although he was really her real father.

And these children only discovered the truth, usually, when they were in their late teens, like they were trying to get a passport, or something like that. And then, of course, there was a total shock.

End of Tape 3 - Side B

Tape 4 - Side A

15 June 1996 (continued)

So you were speaking about the project on step-families.

Well, the sample was the first problem to solve, and I feel very happy about that solution. The next problem really was the question, if we were going to use the family systems perspective as part of what we were doing, because if we wanted a fusion between that perspective and the more social historical approach, was the single life story going to provide material also suitable for a family systems approach? Because, after all, the family systems approach was developed through clinical work, when families were seen as a group, and we were thinking about interviewing people singly, on their own. And we wanted to do a little pilot study on that, but we couldn't get anyone give us support to do that. So, eventually, we decided to do a rather impoverished pilot study, as an experiment. So we took some families who were willing to be guinea pigs, to be experimented on - who were going the Institute of Family Therapy - and we recorded their sessions, and then carried out in-depth individual interviews with some of them. Only two or three, in practice, because usually it wasn't possible to do more. And then compared them. And I found that very interesting. There were clear advantages in the group session, in highlighting the immediate problems faced by the family, and there was no doubt that - you might almost say that the life story interview actually softens as it comes to the present, it becomes blander, really; while, if you put them as a family together, in a therapeutic situation, there is a lot of colliding, and it's almost explosive. There's nothing like that in the interviews. But, on the other hand, to understand the trajectory of people, how their experiences developed, how life made sense to them, it's only really possible to do that well through the [individual] interviews. Because in the group sessions, nobody was able to talk for very long, and so everything they said is fragmented, it doesn't follow through, there are bits missing with one person, in comparison with another, and so on. So when we looked at this material, we all concluded that there was no doubt that we could, very effectively, use life story material from a systems perspective, although it would be quite a novel way of doing it.

I don't know whether I explained, but, apart from Gill Gorrell-Barnes, and me so we'd already got two disciplines there - we had another family therapist, Gwyn Daniel, and then Natasha,

who is a child and adolescent psychiatrist, and who has also a clinical perspective, but a somewhat different one, because she's medically-trained, and I think also, although she's been very interested in family therapy, she's also practised in other therapies with families, and more straight medical ones. So that we had a series of approaches.

Now, it proved to be a very long, drawn out process, doing this research... We were trying to work on it early on in the Families and Social Mobility project, which takes me back to the mid-eighties.

So it's more like ten years?

Yeh, it could be longer, even. But then we eventually got this research [grant] from the Wates Foundation in it's down here, I think ... yes, 1989, you see. But even that's quite a long time ago!

And the problem was, firstly, the interviewing was somewhat slow, because over a half the interviews were done by the team of four, and we had two or three other people doing interviewing. But that took quite a long time. And then we had to read all the material again, and start draft chapters, and we did it, different people taking responsibility for different bits. But those who were therapists, simply didn't have the time in their lives to take a week off and write, and so it was extraordinarily difficult for them to produce drafts of chapters. We're now at the stage in which we have drafts of every chapter, and we're trying to tie it together into a final form, as a book, and it's going to be called, *Growing Up in Stepfamilies*, it hopefully will be out in just over a year. But it's been quite a long struggle in that practical sense. I think, for me, to realise how different the lives of clinicians trying to do research was, was quite salutary. I realised, however busy my life feels, at least I can say, every now and then, you know, "I'm going to spend a week doing research, and writing and thinking". And you can really move fast when you do that. But how you can do it if you only have a day, I don't know, because it seems to me, you hardly get started before you're finished! So that was the first problem.

And then, there have been other problems in developing the analysis, in that there is a [potential pitfall in taking] a rather post-modernist view of the evidence, a subjective view, influenced by Giddens, for instance. His idea of reflexivity, how people are continually reflecting on their own experience and the world around, they're re-creating their life path and self-image and ideology, that people aren't set on tracks with particular ideas, but they have to be continually re-

creating it, and this, of course, is very helpful for [stepfamily members] who did have to re-create, quite clearly. But this is also linked up with the post-modernist view of the world, where none of the evidence is what it looks. Everything is subjective as well as objective, or maybe totally subjective. And that's a real problem. There's a certain point with post-modernism, and you move over to saying, "Everything is subjective, and it's all really a myth that we're creating. And one myth is as good another". So, "How am I to say that my, my interview with somebody about a step-family is any more valid than anyone else's?", or "My interpretation is more valid than anyone else's." And that's an extreme position.

Now, I am not at all happy with that, because there is a positivistic side to me, still, which believes that we may not be able to get to reality or the truth, but that we should be striving to get towards this, and that we can get a bit nearer to it, we can get a bit closer to understanding, for instance, why children suffer more in some families than others. I think it's important to try to understand this. But that moves you towards a much more quantitative way of looking at the material. So instead of looking at it in terms of subjectivity and text, and images and so on, you start looking, counting, how many of them had stepmothers, and how many had stepfathers? And were the ones with stepfathers, in a worse or a better position than those with stepmothers? And what difference did it make if they changed houses five times, as opposed to never? All these calculations. And we did the calculations. But, we were never happy, on the other hand, with only 50 interviews, because we knew that you can't really generalise from only 50 interviews, so there was a quite a difficult tension on that. And I think it's still there in the book. It's one of the things that, ultimately, we've got to try to resolve before it goes in to the publisher. In a way, what we want is a mixture of the different approaches, but we don't want to suggest that anything quantitative is saying, "This is how it must be with all stepfamilies". We just want to say, "Well, for our 50, that's how it is", and our fifty are an unusually reliable group, because they are from this very strong sample base.

I just should mention, too, another interpretative problem. It's about attachment theory. There are very different views between, on the one hand, Natasha, who looks at this as a psychiatrist. Bowlby came from that kind of background, and the [research] people in the Bowlby a psychoanalyst. But the point is, there are differences in the way that the theory is used. I think it created tension, having these different perspectives in the work.

Again, I think it was very interesting for the non-historians to realise that you have this group of people - we were looking at outcome, by the time they were 30, how they'd survived, who survives best, how do they survive? - but, of course, it turns out that because they were children in the sixties, it's also a historical study, and children, now, are not growing up in those kind of contexts. And, of course, the irony is, you never can study the outcome of people in their thirties, the outcome of childhood, in a way which gives you any guarantee that that's going to be any good for the future! But I still think it's very interesting.

But did you get any conclusions?

To get to your question, there are some things which a lot of other people have found. Like it's much more difficult in the stepmother families.

But I think our study shows more vividly than any other I know, how important the role of grandparents, and particularly grandmothers, is. And that was something quite unexpected, because the tendency is to assume that the extended family [today] is less and less important. But our study showed that once the nuclear family breaks, the extended family is really there in a very powerful way. And for me, particularly interesting also, that the grandmothers were, in many ways, much better parents than the original parents, because they were much gentler, perhaps it's because they had the confidence of being older people, but they brought up the children with much less use of physical punishment. There was a terrible amount of harshness, or even violence, in the original parental homes, the nuclear families, before the break. So one of the few things which was really positive for the children in losing their parents as a couple, was that they very often found themselves with these much gentler grandparents. So that, I found that a very important finding [*The role of grandparents when parents part or die*, 1999].

Then we found that schools were incredibly unhelpful, and often positively unsympathetic. They didn't recognise the problems, there were cases of them even taking away [from the child] the things to do with the original parent who'd gone. And another interesting question was the effect of moving, or not moving house and environment, and the role of houses. Sometimes, remarriage could be a gain, because the children would be in a really poor housing situation, and then the remarriage would be a real gain. On the other hand, of course, staying in the same neighbourhood was a great advantage, because playing in the group was one way out of a difficult home situation.

And school, for some children, was also a refuge from a difficult home situation. So we were able to explore those kinds of possibilities, you know, through the in-depth information.

All those step-families interviewed were an example of unhappiness?

No, no, I wouldn't say that. Our aim was to look at a normal cross-section of step-families. And so, if you take the psychological outcome, there's only a very small number that are in real psychological trouble.

We had a health questionnaire, to try to measure that. But, interestingly, again, methodologically, there were 24 questions, they were things like, "Do you suffer from nightmares?" and so on. And normally, these correlated, the ones with more than seven yeses, usually seemed to be, they were a small group who usually seemed, from the interviews, to be in trouble, and we'd already picked them up as in trouble. But then there was a tiny group of men, I think almost all men, who we thought were in trouble, and in some cases, in fact, in the interview situation, the interviewer had been really quite frightened of this man seething with rage and aggression, and been very glad to get out of the interview at the end! And then this man would fill in the form that he had no problems at all. So, actually, we did classify them as having psychological problems, we added a few from those. And I think that's an interesting instance of [the interaction of] qualitative and quantitative forms of research, and how they're better when they go together.

You see, the sample is an example of that, trying to use the two approaches, and it gives us the chance of getting information on the whole sample, for testing some of our ideas. That's part of the last stage, which we're just doing at the moment. You know, we have this idea that it was crucial how many times you moved house, and then we can look at all the 400 plus stepfamilies, and find out if there's any correlation with outcome. NCDS can produce quantitative [findings]. But what they can't do is connect up and find the meaning of things.

One very interesting thing is that previous studies have tended to suggest that stepchildren are normally downwardly mobile, in terms of occupation, but I think this is probably because they're usually clinic studies, and we found that there's a lot of upward mobility with these people. They've done very well, occupationally.

16 June 2002, third part of the interview.

We're going to talk about the research project, "On the Edge of Later Life - life stories and ageing in late middle age". It started in 1992.

Well, this was a project that I was invited to undertake, because the [Economic and Social] Research Council had a research initiative on ageing, and they wanted a qualitative in-depth research project. It picked up some of the ideas that came out of the Life Stories and Ageing project, the idea that you had to draw on the resources you had developed in early life or middle life, in order to have a successful later life. And so we thought we'd look at the end of the middle life period to see whether people were looking forward to later life, retirement, and so on, or not. And we did 50 interviews, in-depth ones, very much on the same model [with men and women aged 55-60].

How did you get the sample?

Well, this was another instance where we wanted to link up with a quantitative study. So we went to the British Household Panel Study, which is also at Essex, and originally, I had thought that we might be able to link up with their main sample. But eventually, we were given part of their pilot sample, and, in fact, it turned out to be slightly disappointing, because there was an over-representation of middle-class people in this pilot, and so we had rather a struggle, actually, to get a satisfactory number of working-class informants. So anyway, apart from that difficulty, we designed an interview schedule very much on the same lines as the other projects, particularly close to the Life Stories and Ageing one, but obviously with the focus more on preparing for retirement and later life. We published very little, because Rebecca Abrams, who was the research officer, had really just finished the interviewing by the time the project finished, and so we did a report on it, but I had hoped to do an article with quotations from the interviews, and I hope we still will, but she has been very preoccupied with other work, and also she had a baby. So that hasn't happened. There's been no outcome, at the moment. So this research has been among the less successful ones, I suppose, along with the car workers.

What emerged was a bit disappointing, which was that incredibly few people really were looking forward at all. There were, basically, typically two groups. There were the majority who were still at work, and weren't thinking of retirement, were just trying to carry on, and were totally absorbed in their current situation, and I think it was almost, actually, it was a taboo thought, what they would do later. And the others were the ones who'd been forced into [retirement] by unemployment or ill-health. And they tended to be rather miserable people, rather depressed...

I was interested by some of the literature on preparing for retirement and so on, and talked to Chris Phillipson, at Keele, about this, and I could see that this was something that could be usefully got into, really, planning for a creative later life. And there didn't seem to be nearly enough help for people who were thinking that way.

Because, basically, because of the taboo, the stigma against old age, which was one of the things that came out from Life Stories and Ageing, where it was a major feeling. *I Don't Feel Old*, well, one reason [for the title] is that you can't feel a really okay person if you feel old. Because old people are [seen as] ill, stupid, they've lost their ability to do this, that, lost their memory, lost their joie de vivre and all those things, and the image of a person functioning normally, because of a stigma, doesn't include an older person. And that's, again, the reason why people can't think ahead, towards being old. They just can't bear the thought of being like that. I think that people are victims of ... I think it's an appalling social stigma, really, old age, in a society which focuses so much on the ideals of youth and beauty. I could see that one could have got into that, but I think we hadn't got the energy, really, to take that further.

Did you see anything about the relation between the person and his or her family?

Well, there was a bit about that. We got the material, but it hasn't been very thoroughly analysed, I must say, that side of it. Certainly, it's quite interesting how, when men retire, there's a problem for the women, if the women have not been working, of household space, there's a problem of having the men around, renegotiating space and roles. And there is the material there to write something on that. But we haven't done it yet.

So the outcome is a paper?

Just the report. And then there was the idea, because really, the basic notion about the research was to predict that people who were making more preparation and a more positive approach to retirement, were going to do better when they retired, and so the way, of course, of testing that, would be to do a further survey later. So we kept the information, so that we could keep in touch with the people. But again, I'm not at all sure that it's likely to be done. And it would be interesting, but I somehow feel it'll probably fall by the wayside. I suppose one could just think of those interviews as a resource, which, hopefully, will be used. They're another of the set that have gone through Qualidata, to the National Sound Archive, the tapes and the transcripts.

Tell me a little bit about this family and economic adaptability in Russia [On Living Through Soviet Russia, 2003]?

Well, that was taking up the same idea which I'd been playing with with the fishing and the Italian car workers. I'm not sure whether I quite explained that the notion there was that with the fishing, the childhood of people was crucial in how they acted as adults in the economy. And what we wanted to see was whether that was also true with the industrial workers, and in that, there did seem to be emerging, a contrast between Italy and Coventry, which was, I think I talked about the difference in reaction to economic recession, but there was a parallel difference in the way in which children were brought up. It was a very depressingly harsh scene in Coventry, the way children were treated, a lot of rather harsh physical punishment, and children not being encouraged to talk and so on. It was a very very sharp contrast with the Italians, which didn't actually emerge from the interviews, but was well-known, everywhere, that children are loved in public, and encouraged to talk, and going out, the whole family going out just for a meal, and sitting and talking tremendously loudly, the usual Italian thing. It's a huge contrast with working-class habits in Coventry where, if they go out together, the children are expected to keep silent, and you get all this shouting at children for making a noise. So it did look as if it fitted with the idea that we had for the fishermen.

And then I thought it would be interesting to look at what was going on in Russia, with the changes happening. And I linked up with Ray Pahl in a pilot project, and we thought that, here they were, moving into a market society, and it was quite likely that families would differ a lot in their ability to adapt, and that for instance, families which were all bureaucrats, straight Party

bureaucrats somewhere in the industrial or state system, that they would find it very hard to change to the new kind of competitive market economy, while that possibly, families where there was a tradition of peasants, or entrepreneurs or whatever, would find it easier. So that's what we were looking at. And, in fact, certainly the interviews tended to show that.

We only did 40 families. We got teams, one in Leningrad and one in Moscow, and we had a lot of practical difficulties, I must say, because ... it's the sort of society where, I think, because of 70 years under the system, nobody trusts anybody at all, and that was true even among our group of researchers, among friends. People would be incredibly deceptive, they'd say they'd done an interview, and somebody else had got the tapes. And the other person would say that the original person had got the tapes, when, in fact, there was no tape at all. They would be expecting to be paid for these interviews they hadn't even done. So we had to work out a Stakhanovite system, and pay them by results! It was all rather ridiculous. Anyway, we got the interviews done, and I think on the whole, they're quite good and interesting. And then [Ray and I] wrote just article from it [*'Meanings, myths and mystifications: the social construction of life stories in Russia'*, 1994].

You made a transcription?

Yes. Well, we translated about ten of them. I can read Russian very very slowly, with a dictionary, but we translated ten of the interviews, and then we got some of the others summarised.

End of Tape 4 - Side A

Tape 4 - Side B

So, you were telling me about a family, this military family.

Yes. Well, it's a marvellous case of the passing down of the very disciplined attitude to life, and it's such a beautiful example of it. And then there were other ones which were of totally different kind of traditions. But 40 wasn't nearly enough to work out a theory like that. And Daniel Bertaux was, meantime, collecting material with a group of Russians, so we tried to do some analysis in parallel with that [data]. But I can't say that that's worked out too clearly. In a way, the thing remains somewhat obscure. It is quite clear from all this research that there are family transmissions, but whether there are such stark contrasts, I wouldn't like to say.

And, of course, they're very interesting in terms memory, because you get deliberate forgettings, in families that, say, come from a background which is disapproved, like a Kulak or Jewish, or they were a general on the wrong side in the Civil War, so it's missed out. And they have photographs with bits cut out: the medals cut out, if they were on the wrong side. We found sometimes, that it was rather interesting how the youngest generation knew things that the middle generation didn't, because the older people were now willing to talk to the younger ones. But [Ray and I] wrote this article which was really partly about memory and transmission in these Russian families, in a society which had been, really, where this secrecy had been such a heavy kind of burden for all that time. [I know of] no other society that's ever maintained such a need for personal secrecy, for so long. Because [of the fear of repression and imprisonment], anything you revealed to anyone else, could be used against you. Nobody trusted anyone. So it was a very interesting place to do work.

Ray and I saw this as a pilot project, and we applied to go further, but we were somewhat tricked by the people who were running this initiative, it was an [ESRC] East-West initiative, and it was dominated by people who were old-style researchers, had been studying Sovietology, Soviet culture for decades, and who were not at all keen on having two new people walking into their field. So as far as they were concerned, I think, a pilot project was enough! So, rather disappointingly, we didn't go any further. And there was nearly a rather big argument about that, but fortunately, it didn't get much further.

And the interviews are at the National Sound Archive?

No, those ones are in the School of East European Studies [at the University of London].

I think you should maybe make a final analysis about your research work, like if there was some point that you tried to explore, to do various perspectives.

Yes, I can read the whole transcript [of this interview] and think about that, and if there's things important that are missing, then we can talk about them. But, in terms of organisations, earlier on there's the Oral History Society, which is very important, and founding the journal, *Oral History*. And there's the International Conferences which started in 1979, and I think it would be useful to record a little bit about that. And leading to the *International Yearbook [of Oral History and Life Stories]*, and then [the series], *Memory and Narrative*, you see. And then there's the National Life Story Collection, which started in the late 1980s. And then there's Qualidata, which starts in the 1990s. That's quite a big agenda that's left, in a way!

So we should be relatively satisfied by having got that far!

Yes!

End of Tape 4 - Side B

END OF INTERVIEW