Reflections on becoming a researcher

PETER TOWNSEND INTERVIEWED BY PAUL THOMPSON

Peter Townsend was born in 1928 in Middlesborough and brought up by his grandmother and his mother, who was a single parent before Peter was aged two.

My grandmother was the key figure in my early life. She was left by her husband, with three small children, in a very tiny terraced house in Middlesbrough, and she took in washing, and she rented out the two rooms upstairs, in order to get by and make a living. She was also a midwife for the local area, or, at least, worked with the midwife, and there were extraordinary tales sometimes told by my mother of going into a woman’s house and finding a dead baby in a box on the shelf of the kitchen. So my grandmother came from a generation where there were large families and many early deaths . . .

My mother was a great singer, and at the age of 16, sang the lead role in Madam Butterfly. She became, in her teens, a very well-known singer in the town and the surroundings. She was in amateur dramatics, in the Amateur Dramatics Society of that time . . . She was very much like the famous Gracie Fields, and in effect, I used to think she sang better than Gracie Fields, but, of course, I was prejudiced at the time! . . .

She met my father in Stockton Amateur Dramatics Society performances and they sang together, and after she married, they had me a year or so later, and he got a job at Glasgow. Went to Glasgow—and to cut a long story short, my mother left him, with me, and we came back to join my grandmother in Middlesbrough. So in a sense, I was the child of a lone parent family, although supported very much, and more so, by a grandmother . . .

My mother went on [singing]. When she was in London, we had a very chequered existence, because some weeks she was very much in work, and other weeks she wasn’t . . . She moved from soprano work in opera, to comedienne work, which is both singing and comedy work. And then . . . during the War years, like Vera Lynn and others, she toured the country to Army barracks, she went across to France and sang to troops there, and the high point of her career came at the end of the War, when she sang in two Royal Command Performances, and I have one of the photos to this day. But after that, like a lot of other women, she was not wanted when she reached her 40s, on the stage, and she no longer got phoned up for new commissions. And she just, for a time, collapsed with that . . .
My grandmother was more the stable influence on me, because she provided the meals and looked after the home during the War when my mother went out to work. She rang true as a bell all her life. I don’t know of anybody who didn’t think the same about her, not only her simplicity but her utter dependability and warmth. She worked for others all her life. She worked round the clock. She worked into her 60s, looking after me, for instance, and she died at 97. In the last ten years of her life, she lived with me, with my wife and myself, in the 1960s. And she was still crocheting, she became rather frail, of course, and lived only at home, but she liked her Guinness every morning at 11 o’clock! And that was her idea of heaven.

Nowadays, I reflect a lot on the question of being an only child and what that means. It led, in part, to my enormous interest in family relations and extended family life, and the structure of families. Because I’m sure that single children have very different needs, and certainly very different experiences, and certainly a great deal of immaturity by virtue of not having siblings.

Peter Townsend’s later childhood was in London, and he went on from there to study at Cambridge.

I had, at the time, and I have now, a kind of love/hate relationship to Cambridge.

I suppose I learned a bit about sociological determinism there, and the way in which the construction of societies seeped into people’s thoughts, and bones, even, and accounted for some of these extraordinary variations across the world. And that was a very exciting discovery, because it made me come back to thinking about how wartime Britain had created a different society, for example, or how postwar Britain was evolving. And that set a train of thought which has stayed with me forever.

There was the idea that [the] social anthropologist was relatively comprehensive in outlook, that no particular thing could be explained, unless you explained everything, or tried to, at least, put it in an overall setting. This was because the functionalist theme of social anthropology was that each piece of behaviour, each system of relationships, was functional in relation to the rest of society. Perhaps the idea that societies are ‘systemic’ is something we find so overwhelming that we often depart from it, and we retreat into small-scale studies.

I still take that view, and it keeps coming back in every sort of way, about generalism and specialism. That you can’t be a good specialist unless you’re a generalist also, and you can’t be a good generalist unless you have some specialist interest, because the two feed upon the other. Because we can’t possibly have experience of every different country in the world. We can’t even keep up with the reading. We have to select, we have to specialize. Yet we have to have a sense of the big picture and how it is shaped, and how the different elements of it fall into place, because they are the little cogs in a huge machine.
And the anthropological method was also so important. The idea that you didn’t do things in an aloof way, you didn’t send out teams of juniors to collect your data, and sat at home in comfortable situations and looked through your microscope and wrote your reports. You actually lived, you engaged with that society. There was a sense of not being able to write anything decent about the societies you were trying to describe and understand, unless you at least spent part of the time there, and better, lived among them . . . [Another] excitement was feeling that Western society, our own society, deserved investigation and illumination, just as much as some of the poorer societies, which the social anthropologists had researched so industriously. That was certainly true.

**Learning from fieldwork**

*Peter Townsend began his research work at Political and Economic Planning (now the Policy Studies Institute) from 1952 to 1954 and then from 1954 to 1957 worked at the new Institute of Community Studies with Michael Young. Peter wrote his first classic, The Family Life of Old People (1957), while at the Institute, combining qualitative and quantitative approaches. For example, he took special care in sampling.*

In all the work that we did, we found that, by sampling, and then by interviewing, we were able to contribute something quite fresh to British knowledge about family life.

Now, sampling could be done in different ways. One way of doing it is simply taking a list of household addresses, and going to visit, say, every 20th house, or address. But I wanted to get hold of old people, and, of course, if you just went to houses or flats, two-thirds or three-quarters of them would be pointless, because there’d be no old person living there. It would cost a lot and waste a lot of time and energy. So the way we tried to do it was to work through the most available list of where old people lived. We worked through general practitioners’ lists, and I went to eight or nine, or ten general practitioners, requested, courteously, access to their medical cards, and just simply went through at an appropriate interval, every 30th card. That’s how I collected a list of 200 old people to go and see in Bethnal Green and surrounding streets.

But what is useful to convey is that in a representative sample of that kind, you do get extraordinary differences across the continuum from one person to another. It’s not just in age, it’s also in family living.

At one extreme was, I think I can say their name now, the Agombars, who had nine or ten surviving sons and daughters, all living in the surrounding streets, with their children. There were one or two grandchildren around, and a few great-grandchildren around too, and they lived in a house with an aviary in the back garden—like Club Row, they had birds in cages. And I made it my business to go to the house quite often, because it was so intriguing. I never caught the Agombars alone, there was always two, three, four, five, six other people among the family there, and usually
sitting around, when I was there, at least, and getting mugs of tea, with sterilized milk, I may say, which I wasn’t too partial about! And slabs of bread pudding, given from the oven. And we’d sit around.

The interview, I could never conduct it in a kind of formal, officious way. It was like a joking relationship, that everybody was making great mocking fun of almost every question I asked! So, even though many of the responses were rich in information, they were often also rich in humour and witticisms . . . But it was like meeting a tribe, because they had, literally, something like 80, 90, 100 related people in the surrounding streets, who had relationships among and with each other. And there were outings from the pub, where the whole extended family would go . . . It was rather like social anthropologists going visiting the Indians in Latin America, and taking a photograph of the entire tribe in the village! . . .

But at the other extreme from that couple . . . was a single woman, who was the only daughter of an only child, and she lived in a fourth floor room, it was hardly a flat, in a set of industrial dwellings, which were quite familiar in the East End of London, and still are, to a point, where a lot of the single rooms have been knocked together. She lived in this single room. I’ve never forgotten her, because when I first interviewed her, I didn’t really believe what I obtained from her, in terms of information. Because she was, as I say, an only child of an only child. Her mother had died 30 years previously, so she’d apparently lived on her own, she’d had nothing to do with her father, or he’d died when she was a baby. She was a spinster, she seemed to have no relative. And the only person she appeared to have anything to do with was a neighbour, three or four doors along, who, perhaps, once a fortnight, rather than even once a week, exchanged words with her on the landing, about whether one or the other would get the other a bit of shopping.

They did a little bit of exchange of that kind. But that was the only real contact. This woman went to the local library to look at the newspaper, it was a bit of a routine . . . She had a mandolin, she had a single iron-frame bed, and she had the mandolin under the bed, and which she occasionally took out, and once she took it out and demonstrated to me this was one way in which she filled her time, and entertained herself.

But it was an extraordinarily restricted life, not very much more than the experience of imprisonment. I deliberately went back several times to try to get to know her, and she enjoyed those visits, I know. But, particularly, I’ll never forget going to see her on one Christmas Eve, I think I took her a box of biscuits, and the only sign of Christmas was a piece of Woolworth’s Christmas wrapping paper, stuck up over the mantel-piece . . .

Participative observation allows you to get the kind of sense of priorities as people regard them. And participative observation also allows you to observe, more objectively, behaviour, which also gives you another sense of priorities . . .

For me, being able to talk to, my first piece of major work, talk to the general practitioners who gave me access to the cards of the old people’s addresses, that was quite significant, because they saw old age, of course, rather differently, from the people . . . let’s say, those who were older themselves. And for me to actually visit, because I did go with the Agombars to the funeral of a brother, was an extraordinarily revealing experience . . .
This was my first major experience of research. And, looking back, I think I did right. People will find when they look at the actual account of the interview that it wasn’t merely ticking off the questionnaire . . .

Another important thing was . . . getting ten old people to keep a diary, and going through the business of trying to persuade and coax them, because that’s what it came to, into doing that for seven days, was enormously revealing. Of course, there were some people whose literacy was weak, and who don’t appreciate that some of the activities of life that they do every day are so interesting to others, and what they think is only wanted is something very restricted indeed. And that is, itself, very revealing, as we all know.

**Were you keeping an ethnographic fieldwork diary yourself, or not?**

I wish I had been. I didn’t have time. I had the desire to keep a diary. I had a big exercise book—there are periods of those 1950s and 1960s when there are certain days which have got interesting accounts of what had happened to me that day, and this is fascinating to me to look back on now. But, quite frankly, we’re talking of 1 or 2 or 3 or 4% of my time and activities, which is a shame . . .

**Balance in writing up**

One of the things that struck me . . . is that when people gave quotations in books, from interviews, much turns on how you weigh the importance of that, in terms of who said it, and in what circumstances, and what kind of person they were. And so I [decided] to have fewer quotations—instead of having two or three lines every so often, to have a longer passage, but to precede that with three or four lines, which made it absolutely crystal clear to the reader, what kind of person was saying this, from the sample that you’d interviewed . . .

I often had criticisms, subsequently, of this method, because people said, ‘Oh well, he just picked out those things he wanted to illustrate, and, therefore, he got very biased’. But then, frankly, that’s true of almost every method that you adopt, because you still have to select. It’s true of statistical stuff. We all know that even in collecting quantitative answers about behaviour, rather than even opinion, the way the question is framed, the way it’s pre-classified, the way it’s coded, what you do about ‘don’t knows’, what you’ve done about people who haven’t answered the questions in the first place, these are all highly significant in terms of how you interpret the statistical results . . .

Because Michael [Young] and I had different views about writing up a text, and what it should convey, and he did me the service of editing my first drafts of the old people, enormously, but had the courtesy and the strength of mind to allow me to have the last word. And I’m afraid I didn’t accept a lot of the, as I saw it, journalese that he was attempting to impose . . .
I was enthusiastic about working-class culture, and certainly I was aware of the strengths which survive to this day, and there are those enormous strengths of self-capacity. But equally, I think there were certain oppressive qualities about extended family existence, for example, certain oppressive factors about male domination, in terms of gender, which we were then less sensitive to . . .

Michael wanted to come in very heavily at the editorial stage of the books, and there were senses in which I think I was more academically or objectively inclined, whereas he was more organizationally and politically inclined than I was . . . I think he wanted the story to be unvarnished, and rather powerful and rounded, and well-expressed, and without its hesitations, qualifications, and exceptions. And I was much more inclined to put exceptions in.

In 1957 Peter Townsend moved from the Institute of Community Studies to the LSE, where he worked with Richard Titmuss, and carried out the research for his second major book, The Last Refuge (1962). He went on to become founding Professor of Sociology at Essex in 1963.

We all [Michael, Peter Willmott and Peter Townsend] had got excited about investigating the extended family, and that was what I thought was the major purpose of the Institute of Community Studies. When it turned out, three years afterwards, that it wasn’t the major purpose, I was less attracted to it. I felt I could learn more, at that stage, from association with Richard Titmuss. And so I applied for money, to go to LSE, under the auspices of the Nuffield Foundation, to write The Last Refuge, which was my book on old people’s homes . . . That’s where I devoted my major research energies for the next three or four years . . .

The Last Refuge: interviewing and participant observation

Peter, we have these wonderful records that you took. Could you just convey what it was like, contrasting types of visit, how you were received and so on?

What we did was to visit, I believe it was altogether around 180 institutions and Homes, and these ranged from enormous old institutions like Luxborough Lodge near Baker Street, which housed 1000 people . . . right through to tiny private residential homes for only four or six registered residents. The bulk of our interviewing was done with Bob Pinker, who later became professor at the London School of Economics. The differences were remarkable . . .

We worked our way in certain deliberate steps. The first step was to go to the Chief Welfare Officer in his office, and to cross-examine him, interview him—usually it was him, very rarely at that time was it a her—
about all the residential institutions under his management, and to hear his side of the developments that were taking place, and what progress was being made since the passage of the National Assistance Act in 1948, in terms of trying to get rid of the old Poor Law institutions and replace them with more homely, smaller, residential homes . . . The average, I think, was about two hours. I tried to take notes, and then write it up, it got typed up afterwards, about all aspects of how many homes they had, what types they were, how many people lived in them . . . Then I went on, from that, by arrangement, to visit the home itself.

One fascinating thing which I learned, which only came up really by chance, because our purpose was, after seeing the Chief Welfare Officer, was to go and get an inventory of a kind. It was a very rough inventory, as you will appreciate, of going into every room in the home. This, when I got to the old Poor Law institutions was a tremendous asset, having this principle, in terms of objectives of research. Because, of course, the rooms people want to show you when you’re on a kind of royal visit are not the rooms where some of the most incapacitated residents [live], and where there’s water coming in the roof. You won’t be shown those bits of the institution. And, of course, I frequently discovered when I went back to the Chief Welfare Officer or the Matron or Warden of the home, and said, ‘Oh, I’m awfully sorry, but I’m supposed to check on all the beds in this particular place, I find that I’m 20 short. Where are these? Is it that your records are wrong?’ And then I would find some miserable attic, or some miserable temporary erected room on the margins of the home, which people were really rather embarrassed about and ashamed of, because this was one annexe which was never heated enough, was too wet, or they tended to put some of the most difficult of their residents, who shouted a lot, or were incontinent or whatever . . .

But at the extremes, . . . the situation was very different in an old Poor Law institution for, let us say, 500 people, or 1000 people, compared with a private residential home, in a seaside town like Eastbourne . . . where there might be only half a dozen or ten residents at most.

If you take the big institution first, that tended to correspond with my initial experience in going to Southern Grove in Stepney.

**Could you give me a word picture of that first experience going there?**

. . . It was a very daunting experience. It was really quite a shocking experience. I suppose someone like myself, aware, young and hopeful, and coming into a situation which had been created in the aftermath of the War, of building a bright new social Jerusalem and the Welfare State, and wanting to get rid of all the old workhouse attitudes and institutions, suddenly to discover it was very much present in the area which I was visiting, and there were people in a very abject and disconsolate situation.

One went into this rather gaunt, vast place, busy . . . There were the noises of trolleys being pushed backwards and forwards, there were stone passages, stone slabs on the floors. There was little carpeting anywhere, there were mainly bare boards in the dormitories and the dining rooms.
People were congregated in large numbers. You went into a day room, as they were called, and there might be 20, 30, 40, 50 people on Windsor chairs—wooden chairs usually with arms—arranged around the walls, sometimes with an old, I think it was then still black and white television, at a distance where, of course, many people without good sight couldn’t possibly see it beyond about 10 or 20 feet.

They were often just sitting rocking themselves, rocking to and fro, very little conversation. Again, one needs to understand the situation to explain it. Carpet slippers the women wore, aprons, flowered aprons. The men usually had tweed jackets, let’s say, stained and poor quality woollen trousers, often caps on indoors. Many people with spectacles that were tied together with string. People with chipped mugs. I don’t think they were actually allowed to have them in the day rooms, they only could eat and drink when they went to the dining room, and had sort of general service. And you got people, and I remember this clearly, old men and women who were a bit more mobile than others, literally queued up outside the dining room to get in for—whether it was breakfast, whether it was dinner, as lunch was called, or whether it was tea, or post-tea, waiting sometimes for half an hour or more, to get in and get what they would regard as a better place, and make sure they—if they had any chance of getting an extra biscuit, they would get it.

That kind of dragooned mass warehousing situation was very shocking to a youngster like myself, who hadn’t witnessed it before.

And the arrogance, as I remember it, of the Warden in charge. He had a lofty flat which was, I must say, very spacious, very comfortably appointed, and where he could look out on the squares below, which were supposed to be almost the exercise yards of the past, of the workhouse, where people were allowed to wander out from a day room into the yard, and he could look down on them. And he was very arrogant about [how] he and they were caring for these people, and you couldn’t expect much of them, but the staff were doing a wonderful job, and they were very well organized.

There was a sense of the management, rather like the Boards of Guardians, when some visiting committee from the London County Council would come, they would have starched linen tablecloths in a dining room with a silver salver and silver forks and knives and all the rest of it, so that they had a very good slap-up lunch, in order to review how the institution had been managed in the previous three months . . .

Between the staff and the residents there was almost a class situation, a polarized situation, where the staff, even when they got relatively modest salaries, because they did, were able to exercise a control over very weak and infirm people, which they shouldn’t have been allowed to do in those circumstances . . .

You did immerse yourself in the most extraordinary way in this project. Didn’t you also take a job as a bathing attendant for old people?

I worked in a former Public Assistance institution, a former workhouse, called ‘Newholme’ in Manchester [in 1958/1959] for three or four days.
It wasn’t quite as awful as some of the other institutions that I’d been to around the country . . . I did the usual thing of going round every part of it, and then trying to interview some of the new residents, it must have been as many as ten I interviewed . . . [And then] I stayed on several more days and used the opportunity, like the good social anthropologist, of being around when I wasn’t expected to be, and being able to witness the place not being conducted round it at times chosen by the management . . . You have to allow for the ordinary events going on in these places, and not at times chosen for what I’ve described as royal visits . . . I think sometimes that some of the research we did in those days, I was allowed access to doing things which I wonder whether my successors would be allowed to do.

So . . . after doing all these various interviews around the home, I then became an attendant, looking after the bathing of the old men. And what forced itself upon my consciousness was a number of things. One was the extraordinarily kind of—abject passivity of some of these elderly men. It was almost as if the process of institutionalization had forced upon them how to behave, and how to, as it were, preserve their integrity only within—it’s almost like the ways in which we have fantasies, we have an inside life and we have an external life, and some of them communicated this by talking a bit about their past and about what their feelings were for their loved ones. And mostly it was feelings of being bereft and being abandoned. But it was also feelings of being neglected by staff, who didn’t want to know, and didn’t have any forms of communication . . .

But it was also physically the problem of seeing these very thin, many thin characters, how gentle one had to be to get them into the bath, how careful one had to be in using one’s elbow to make sure the water wasn’t too hot, how little they had in the way of personal belongings, how the underpants they had, the kind of combinations they were wearing, sometimes had six, seven, eight, ten laundry marks, or tabs attached, which almost underlined their loss of identity. They were just numbers in an institution where they didn’t even possess underpants of their own, which they took off . . . Yes, it’s not as extreme as [the violence of concentration or refugee camps], but it’s equally disconcerting and devastating to absorb what the process of institutionalization had done to people, without them having any means of fighting back. Yes, of course there were those characters, some of them had enormous resources in terms of personality, but they’d turned it inwards, in order to—oh, I don’t know, make sure that they got another slice of toast. It was conforming. They knew that conformity was the order of the day.

What I was discovering was that the tradition of being indulgent towards the deserving poor was not put into effect. The other side of the workhouse story, the punitive discipline, was really ruling the roost, because you can’t have staff behaving in a polarized or contradictory way with different groups of residents. And this is the ultimate story to learn, that an institution can’t be both disciplinarian and indulgent.
The Last Refuge: securing the quantitative findings

With the two studies that we’ve been talking about—on ageing and later on poverty—in both cases, you were concerned to always try very hard to interview people who were missing or who refused, initially, to be interviewed.

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

But I learned, to my cost, first of all, really, in the studies of the elderly, that you were missing, even among a small percentage of refusals or non-contacts, people who really mattered when it came to generalization. In the case of the elderly, what I say can be fairly obvious to a listener or to a reader, which is that there were clearly, among some of the people involved in the random samples that I applied, there were people who were confused, or so severely disabled, or sick on the particular days, that it was impossible to arrange an interview.

I then formed the idea that it was important, certainly in the case of the elderly, and especially the very old, to get proxy interviews. I’d noticed that during interviews, and this is very rarely reported, there are others present as well as the informant who is the particular subject of the interview. And I think research workers do not heed enough the variations in the answers they get when other people are present, compared with when just the single informant who is recorded, whose answers are recorded, is present . . .

That led me to believe that when we carry out interviews and sampling, it’s extremely valuable to obtain as much information about those who are not present when you call at an address, or those who refuse, or, more likely, people who refuse on their behalf. Because it’s usually, in those cases, a husband or a wife, or a mother or a father, or another relative who deters you from meeting the informant you want to address. So . . . in the end, I formulated a mini-questionnaire for the interviewer to complete to deal with refusals and non-contacts. This obviously covered straightforward issues like the type of house, and the ownership of the house [but also] how many people were there, what age they were and anything, roughly, that could be described about them, even to the extent of an occupation.

But that possibility seems to me capable of considerable development, and could easily be an extra item among the responsibilities of the interviewer to carry out . . . I got some opposition from those managing the interviewers in different parts of the country, because it was quite clear that some interviewers didn’t regard this as a vital part of their work . . . But we did do this in the Poverty Survey, and I only regret that we didn’t really make enough of it.
Peter also stresses the energetic statistical engagement needed to make the most of survey findings.

Phil Holden was a very patient computer programmer in the poverty survey in the early 1970s. I kept changing and developing lists of tables . . . Numbers matter, not only quality. He taught me quite a lot about the ways in which [I could achieve] something that I wanted to commission, in terms of novel tables, of trying to select particular categories, and to devise new ways of generalizing the information we were obtaining. He was extremely patient with me about all that. And, quite frankly, it’s worth saying that, sometimes, if you think of a survey, of carrying out a set of interviews, and you want to calculate what percent of people did this, and what percent did that, I think you have to think of doing it in three stages.

Don’t, for God’s sake, develop a list of requests as long as your arm, when people will produce four feet deep of printout. To wade through all that is almost an impossible job. Your best plan is to devise a short set of requests, to open up the subject, get the rough outline of the scope of a structure that you’re investigating . . . In the first instance, you need to get your information about what the shape of the sample, the people you’ve interviewed, what it looks like, where the majority of people put themselves. That’s an obvious point, but sometimes you will find that 70% of people put themselves in a particular category, and you’ve got to break that down into the second stage, and try and discover to what extent it’s composed of certain subgroups.

But . . . not only should you proceed like that in dynamic interchange with the programmer, if you don’t do the programming yourself, but you need to remember that you’ve got a third stage of being really creative with statistics. And by that I mean that in the process of carrying out a survey, you discover certain things about housing, or you discover certain things about political opinion, or things about disability, if you’re any good, which hadn’t occurred to you beforehand. Part of the point about doing any research is to discover things you weren’t quite aware of beforehand. And that will mean, sometimes, devising, putting groups together that you hadn’t anticipated, to explore the results of doing that. Looking at gender, for example, in a new way, if suddenly you become aware that gender differences are not, perhaps, as covered as you’d like . . . It’s almost like saying, the first stage is the initial outline, the second stage is the follow-up into subcategories. The third stage is the really creative stage where you are checking your conclusions. And that is a very important way of using statistics creatively, and sometimes changing what you thought about methodology in the first place, but also using your statistics to the hilt . . .