
Interview with Professor John Veit-Wilson

Part 1: on his involvement in the research

So if you could just tell me, before I ask you specific questions, how you got involved in the poverty in the UK project, and how the process was, and just the whole story from start to finish, and then I'll ask you some questions after, specific questions.

I can tell you how I got engaged, so it's my personal account, rather than being an account of the research programme. The research team of Peter Townsend and Brian Abel-Smith were looking for a research officer, one of four, or rather one of three appointments of research officers, to work on the four pilot projects, which they had planned, to act as intensive qualitative studies of groups at risk of poverty, but not necessarily in it, to begin to be able to crystallise those issues in people's lives which might be used as indicators of deprivation. The four groups were the long-term unemployed, large families, single mothers in effect and long-term sick and disabled men.

I applied for one of the positions and was interviewed by Brian Abel-Smith in 1964, the summer of 1964, and was appointed from I believe 1st September 1964. And initially I worked with Hilary Land in Skepper House, which was an offshoot of the London School of Economics, on the large family study, which was carried out in London. The study was of a sample of family allowances and was records of families with five or more dependent children receiving family allowance, and it covered in principle the whole income range. And I worked on that with Hilary Land until my partner and I had bought a house in Colchester, into which we moved in the summer of 1965.

So I was living in London at the time and working in Skepper House, which is part of Brian Smith's LSE bit, and then I worked at the University of Essex and had my responsibility for the study of long-term sick and disabled men of working age. And that is what I did for the next two years. I had a three-year research contract, and when it expired, the funds didn't allow the continuation of three research officers and so I was, as they say, let go, and I took a teaching post elsewhere. But that was my involvement with it.

So in the two years that I was at Essex and Colchester I carried out a survey by first of all contacting all the GP practices in and around the town to see if they would pass on, with consent obviously, the names and addresses of any married men of working age, so up to 65, who had suffered from a sickness or other disability which had lasted for more than three months, that was treated as, and I think there was, I can't remember the precise details, but I think it was an official definition used in survey work and in reporting work by DHSS, DSS, whatever it was at that time. Ministry of Pensions still, I can't remember at any rate. I think they were all either national insurance claimants or national assistance claimants.

So they weren't defined by their benefit receipt but by having been off work for three months or more, which had to be certified by the GP, and that was why, there were one or two errors in GP records. And I found in the end 65 men who met the criteria, and interviewed them all. With a lengthy questionnaire, the actual documentation, all my completed questionnaires are deposited in the data archive at Essex, together with the notes I made at the time, everything I packed up at the end and followed me around in my various garages after that, until it seemed an appropriate time to send them off.

Yes, what should I say about the actual research? The interviews were in the area around, up to 10 or 15 miles from Colchester, because the men in question were all patients of Colchester practices, and so they would not have been likely to be further afield. The one thing I remember about the interviews particularly is that the median length of interview was 3¾ hours, some of them I went back again and even again, because it was very much a matter of letting the men and their wives speak about their condition, their lives, their current living experiences of living with a long-term illness or disability, the consequences for their lives, and of course the related economic and social aspects.

And the idea was to generate from those responses, as from the other three studies, as I said, a set of indicators of styles of life, of experiences, of patterns of behaviour, and of obviously expenditures which would generate usable deprivation indicators for the national study, for which this was a pilot preparation. My contract, as I said, terminated in '67, three years after I started, and the team was then preparing for the national survey, which was conducted I think '68, '69, but unfortunately not published until 1979. So I didn't take part in anything but the early stages of the National Survey of Poverty.

Part 2: on ethos and ethics

How did you kind of establish an ethos for the team that were doing the pilot study, you know, how to establish standards or the meanings of the research questions and all that kind of thing, how did that come about?

I think it was an extremely hit or miss matter. I don't recall us having meetings, and by that I stress I don't recall it, I don't say it didn't happen, and it may be that you find Hilary Land or Adrian Sinfield – Dennis Marsden is sadly not with us anymore – will be able to give you a better answer about that. In fact, Adrian was in the States during the first year I worked on it, '64, '65, and he'd done his study in North Shields before this stage of the national survey had started. So he was not actually working on a survey at the time when Hilary, Dennis and I were working on ours. As I said, I started work with Hilary and did the, what we were piloting was our handwritten questionnaire forms for use, and then we adapted them after the first dozen or 15 interviews to make them work better, and then as I said I went off to Colchester and Hilary continued with the large family study, just by the way.

But obviously I remember between Hilary and me there was a great deal of discussion when we were generating the first questionnaires and the adaptation, but I don't recall the content of that discussion at all. Issues of ethics which nowadays rank very highly I don't recall being mentioned at all. I think there was the usual kind of courteous consideration for the dignity of the people whom we were interviewing, and it was rather taken for granted that we knew what that would be and it did not have to be a checklist or anything of that kind. There certainly wasn't a checklist to tell us how to do it. For me it was at times quite an eye opener, not that I was unfamiliar with poverty, and I'm not going to go into all that here, for various reasons, I've been involved in socially active work since my school days, but the extent and nature of it in different contexts was quite eye opening. I was a naïve mid 20-year-old, and Hilary too, and Dennis, we'd all graduated only a few years earlier, all had done something in the meantime. Hilary will tell you her own account.

Did you feel comfortable interviewing people who were in that situation?

Well, I've personally never had a problem in interpersonal exchanges, so to speak. I didn't always feel comfortable with the degree of what then appeared to be, either to be prying or to be exposing the intimacies of people's lives. Because with the large families and with the what we called chronic sick respondents, we were dealing with couples, and we were asking about every aspect of their lives that had been affected by having a large family or having a sick, a chronically sick husband, and some of them went into quite considerable detail about how it had affected their marital lives and so on. Which, as I say, I was married, it wasn't that I was unfamiliar with what the issues were, but having them discussed with me by complete strangers, I mean that's my problem. They did or did not open up according to how they felt about it, and it was perfectly clear that some of them, it was an enormous relief to have somebody to talk to about it who was not otherwise engaged.

It's an old experience, we all know about it, but I didn't know about it to that extent then and I hadn't experienced it in that kind of kind of way. We did make a point, and that was, I remember, a deliberate decision that we would try to interview both husband and wife separately in both of those two samples. Dennis obviously there were no partners involved in the interview process, and some of the single mothers he interviewed will have had partners but not formally, so to speak; it was the old days of the cohabitation problem and so on. So that was merely part of the dynamics of interviewing, that wasn't the survey. The survey itself we did decide we wanted to open the possibility of a gender distinctive responses to some of these issues and so that is what we did.

But otherwise on the ethics front I don't recall that we made a particular point to either use particular forms of language or keep off areas or things of this kind, apart from saying all the data, saying all the data will be kept strictly confidential and won't be published or won't be used in any identifiable form. I don't recall that we got signatures or anything like that. They had, I think had to sign a consent form certainly for the large family study because that went through the, whichever ministry it was administering family allowances at the time. And that was an opt-in one, they had to return, they were sent a letter about the survey with a postcard which they had to return if they were willing to be interviewed. And I think I used that method with the GPs, that the GPs had to get the consent of the patient before I was allowed to have the details of the addresses.

But I do recall even after that going to one house of somebody clearly quite well off who when I turned up and explained why I was there said I don't want to do that! So there were non-response, there were refusals to respond even at that stage, but that's the only one I can remember.

And it seemed in the actual survey itself that some of the researchers did something or kind of intervened in the lives of the people being researched in terms of sending them some money or sorting out their housing situation or something. Did that happen at all in the pilot, or was there any discussions?

Well I didn't know that. It was certainly put to me, yes, I do recall being asked by, in the large family study, one family who were clearly quite desperate from a financial point of view asking me if I'd lend them £10, which was a lot of money in those days. I mean their whole income may only have been, I don't know, £10-15-20 a week or something.

THE VIDEO IS MISSING FOR THIS SECTION OF THE INTERVIEW

Okay, where had we got to? We were on the subject of having, whether I was ever asked, and I was actually very moved by a number of the people whom I interviewed. That was not a question.

That wasn't an issue. I didn't become engaged to the point of so to speak getting sucked into it, but I've always been affected by the other in those kinds of relationship and some of these situations were pretty desperate. In the case of the family you asked me for, for a loan, first of all I wasn't in a position to give them one on the spot. I'm not even sure I carried that much money around in my wallet in those days. And I asked a person with great experience of this kind of work about it afterwards, and she said no, you can't ever get engaged in those situations. That is you just have to keep your distance, so to speak. You're not the solution to their problems, even though you're emotionally engaged in the fact that they have problems. And so I didn't. I said I would think about it. I don't think anybody else ever asked me for money. And this wasn't a matter of paying them for the interviews, I can't remember if they got paid, but I can't remember ever having been involved myself in dealing with payment issues - because if they did get paid then it wasn't done by me. Possibly by the others, but I don't think we did. And some of them were, and particularly with the

chronic sick respondents very, very difficult situations of severely disabled people, lots of very depressed people about their condition.

And was there any kind of arrangements for debriefing you, meeting with you after to see how you're feeling about the research process or anything like that?

I don't remember any. We were expected to do what I think we all understood was normal good practice, which was to write up our notes as quickly as we could and put them in a form in which they would then be usable by ourselves. I don't recall that there was ever an issue about doing so in a form in which other people could use them. It was very much assumed we were responsible for our own studies and we would have to write them up. So there was no issue about using, as I said earlier, we made up our interview forms. If I used the word questionnaire, I shouldn't have done, because they were actually interview schedules. There were a whole lot of them naturally of the usual [unclear 04:13] variables and other things of this kind, household composition and all that. But then there were pages left for just sitting there and scribbling what was being said - this was prerecording days. And you just had to get it, the vox pop down as well as you could, and some of us managed it better than others. But you did what you could, and then reconstructed it as well as you could afterwards.

So it was a very informal system of interviewing. Nothing was done in a form that could then be coded or anything like this. And I think there was a sense that with the sample sizes that we were working on, I think Hilary had a similar total sample of 60 or 70, and Dennis was of the same order even if it was a different number.

And we didn't feel that this was something that was really amenable to, in those days machine processing, the punched cards, I think it was pretty well pre-computerised. Well, there were mainframes, but the punched cards, but not something that I think we were thinking we would be working on. We were working on large sheets of squared paper, filling things in in pencil.

Part 3: Reflections on the study

Overall did you have any kind of feelings about the impact of the project overall? Did you know that it would have such a big impact?

The national survey?

Yeah.

I think we had hopes that it would, because no quantitative survey of the real meaning of poverty had ever been done nationally - and probably hardly even locally. There were a great many ethnographic studies of communities in Britain, perhaps elsewhere, but they were always focusing on, or almost always focusing on the lower income sections of the population. I say almost always because I can't remember if the Institute of Community Studies survey of better off families as well as worse off families had been done at that time. Dennis had been working in the ICS, I believe. So we were doing something that hadn't been done like this before, and there was a considerable amount of enthusiasm about being involved in that project, I put it in personally because it wasn't only my enthusiasm. We were very committed to what we were doing. We didn't obviously know what effect it could have on changing the discourses of poverty internationally in the end, because, in two ways.

One is that it was looking at what the population itself considered to be poverty, so it was the, you could say democratic approach rather than the expert approach, and the second was that it was not based on constructing artificial budgets and then seeing what households, which households had incomes to achieve those budgets or not, but a totally different way. So it was both the kind of response the population whose standards were being used, but a different way of getting at what were the significant issues to measure, if you're going measure the reality of deprivation and then the associated power of resources, chiefly income, which was the poverty element of it - if it is poverty.

Okay, great. Are there any lessons from the research, what do you think could have been done differently?

I don't think I've ever sat down and had sort of second thoughts about it. Given that we were literally in uncharted territory - I mean there were not guide books on how to do this kind of research. There may have been guide books on how to do qualitative research, but I don't remember them if there were. And I don't think that Peter Townsend or Brian Abel-Smith were pushing us in the direction of formalised approaches of that kind. I think we did pretty well in the circumstances. I think later, no, I think actually the national survey did well because it then used national survey organisations that were familiar with and used the appropriate techniques for large scale sampling and population interviews, on a structured basis. And I haven't really anything to say about that, because the methodology there has obviously developed over the half century since that took place. But for us doing our pilot studies, we were going into uncharted territory.

We had to find out what the terrain was like, what the issues were, and that was partly why we took such a lot of time and effort over doing the interviews. I think sometimes one can spend, one spends what seems to be an appropriate amount of time in preparing schedules, questionnaires and all the rest of it, and then shoots in and out and gets them completed and comes back again. And is not touched by and hasn't felt in the same way what it is that the respondents are trying to convey. It wasn't a participant observation because we didn't live with the families we interviewed, but the fact that I spent, you know, almost days with them, and repeat visits to complete the questionnaires, says something about how we were really trying to find out what it was about. And if one were in such a new situation today, I'm not sure that I would suggest people to do differently. The situation today, with retrospect, about research methods, interview methods, qualitative research methods, has developed so enormously since that time that it's very difficult to use hindsight constructively to criticise what we were doing.

As I've said, if there were guides to better qualitative data collection methods at the time, we were not familiar with them. But they weren't brought to our attention, and we did do quite a lot of work around what had been written so far, what methods had been used and all that kind of thing. So we were familiar with the ethnographic studies of the time. They were community studies but carried out in various different towns and cities in Britain.

Okay, that's great. Is there anything else you'd like to say about the study that I haven't asked you? Do you want to say anything about the kind of relationships between the people involved or?

I'm not sure that the dynamics of the relationships, well they're not relevant to the issue of poverty as such, but I think there were issues which, if one is looking from, at the whole research process, then they need to be raised. Now there's a book, the title of which, an edited book, the title of which I can't recall, but either it, it certainly had a chapter by Colin Bell, and I believe he edited it. I can't remember, but it was about the real research experience. And that came out afterwards, because I met Colin first during that period when he was still a graduate student working on his own research - part of the Banbury Study, I think. And what that had to do with, and putting it in the context of the book so that it isn't just my personal reflections on what happened in London and Essex. What that had to do with, or what that reported on, or the contributors reported on principally was the role of interpersonal relationships in affecting the way in which the research was conducted, how it was conceptualised and carried through.

And I have to say that while both Peter and Brian were on a personal level very decent and affable people, and Brian did not have hands-on responsibility for this part of the research, the pilot studies into it, and I was in any case in Essex with Peter, Peter was not particularly good at managing people, and that's quite a widespread experience. It's no detraction from his reputation to say that. He also had a reputation for not being good at managing people, being bitter at it. And I suffered from that. I had my own problems at the time with the marriage was on the rocks and so I was distracted at times. Probably needed a bit of management, but he wasn't the man to do it. But that's a personal comment which is really a reflection on what really goes on in research teams, which is why I started with the reference to the book that other people have experiences like that as well.

When I came to writing up, writing papers on the, my project, I have to say that I was fairly put off by the acerbic tone which Brian Abel-Smith took about some of my drafts. I mean there are ways of commenting on your junior's drafts which are more or less constructive; I found his really unconstructive. So much so that being the person I am, I responded pretty assertively myself. But in the long run none of that made a difference. But my own problems meant that by the time I

finished, I finished the survey and had written and conveyed the relevant bits of it which were needed for contributing to the national study, but I never actually wrote a publishable monograph on it. As Hilary did with the occasional papers and social administration series on large families in London, and as Dennis did with the fatherless families or whatever it was called - single mothers' book. Adrian produced a chapter for the concepts in the poverty book in 1970, so there are three published accounts, but mine were unpublished.

So you never published them then?

I never published anything on the large families study, no.

Okay.

Is there anything else I want to say about it? I think it could have done with better research management, quite frankly. And that is the retrospect that one would have, that nowadays we know that that kind of thing needs to be thought out better and needs to be planned better.

Do you mean in the sense of who's doing what, when and...?

I think a bit of that. I think a bit more open, you know, I'm responding to one of your earlier questions as well as to looking over the questionnaires that we devised. I think probably we did submit them to Peter and Brian, because I can't believe that we went off and did that entirely on our own. When I say our own, it was Hilary and me working together at that point. Dennis was doing his own thing. And I think that would have been a good idea. But don't forget that what people bring to their review of such an interview schedule is their accumulated knowledge not only of how it's to be done but what they're expecting to, what they believe to be the salient issues which need to be, you know, the respondents need to be asked about, or which are peripheral. And at that stage we put the whole damn thing in, because we simply didn't know what was going to be important or what wasn't going to be important.

I think that is a point that one needs to keep in mind. We really wanted to find out everything. We wanted to interview people about things that people hadn't been interviewed about or had only come out by the sort of participant observation of the community studies kind, the ethnographic studies - which was

quite a different approach, over a much longer term, much better relationships, we were going in and coming out again. So it was highly experimental in some ways. But I don't think it was ineffective. If you think about the difference between, now coming back to your world of the Townsend deprivation indicators and the Mack and Lansley deprivation indicators, which really were the first reactive development to the Townsend ones, there's not an awful lot of difference.

The difference lies mainly in actually the theoretical approach, different approach of those two studies, where Townsend was concerned with the totality of people's lives, the styles of life, the experiences and so on. And when people would articulate things in one way, he retained the sociologist's power and capacity for interpreting them in ways which would allow him to devise deprivation indicators he felt would represent things which the respondents had not necessarily framed that way. And he got then criticised for having used his own deprivation indicators rather than what emerged out of the, although I don't think it's true that they didn't emerge, the ideas for them emerged, but they were his interpretations of them. Or his team's interpretations, as I've said I wasn't a member of the team at that point. Whereas Mack and Lansley went much more directly straight to the things you can buy and the things you can't buy, or things you can do with money or can't do with money, because money was what the issue was about. And we were more concerned with the whole picture of the big poverty, the deprivation overall rather than the purely income poverty. And that's a theoretical difference.

So differences in deprivation indicators were to be interpreted in terms of what they're for, and I think that David Piauchaud's well-publicised criticism of I don't want a cooked breakfast so why is that in here is an example the misunderstandings that then took place, of what Peter had written about. It was representative of a style of life, which if you wish to choose it, you should have the resources to do. It wasn't saying this is a yes/no deprivation indicator. I've never liked a cooked breakfast, so I share David's view of the subject. But it was a misunderstanding; it wasn't a criticism of what Peter had done.

Okay, that's great thank you. Any last thoughts?

No, and if I do have I'll probably communicate them to you or David or somebody.

Okay, that's brilliant, I'll switch the-

I hope you've got that.

I've definitely got that.