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# Interview with Professor John Veit-Wilson

## 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.

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**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.

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**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

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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

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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.**