

AUDIO INTERVIEW TRANSCRIPT

St Leger, Selwyn: transcript of an audio interview (27-Jul-2000)

Interviewer: Andy Ness

Transcriber: Jaqui Carter

Editors: Tilli Tansey, Hugh Thomas

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St Leger, Selwyn: transcript of an audio interview (27-Jul-2000)*

Biography: Dr Antony Selwyn St Leger FFCM (b. 1948) has been a Consultant and Senior Lecturer in Public Health Medicine at the University of Manchester since 1980. He was at the MRC Epidemiology Unit (South Wales) from 1974 to 1980.

AN: Andy Ness

SSL: Selwyn St Leger

AN: If we could start by you just telling me a little bit about where and when you were born and your early life.

SSL: Certainly. I was born in 1948 in Watford in Hertfordshire, and I was brought up mainly in Harpenden and St Albans and I went to St Albans Boys Grammar School and I then went to the Welsh College of Medicine in 1966, the autumn of 1966, graduated from the College of Medicine in 1971, did my house jobs at Singleton Hospital, Swansea, and was awarded a Medical Research Council junior research fellowship to go to the University of Reading, the department of biometry, to do their Masters in Biometry, the first year being an ad hominem course to get my maths up to the right level, because the students they recruited onto that were primarily mathematics graduates, and the second year being the Masters course as such, and during that time I was then appointed to the Epidemiology Research Unit in South Wales and joined that in the, I think it was the September or October of 1974.

AN: Just tell me a little bit about your parents and your brothers and sisters if you have any.

SSL: I have no brothers or sisters. My father was an engineer and manager, it used to be called the Eastern Gas Board I think it was, it has all amalgamated now into British Gas, and he was based in Watford originally and then in other places around about there. My mother she is Welsh, her maiden name was Selwyn Jones. Her father in fact was a Welsh clergyman and she didn't work apart from a few occasions when she did a bit of part-time work.

AN: And when did you decide to do medicine? What spurred you to do it?

SSL: I had always been interested in biology. In fact deeply fascinated by biology at school, and in the first sense I didn't have a clear direction where I wanted to go, I was also interested in the latter stages of school in mathematics, I didn't have any technical talent for it, according to examinations and grades up to that point, and I suddenly threw myself into it and loved it. If I had known that there had been career opportunities where I could combine the two interests, I probably wouldn't have gone into medicine. With regard to medicine, I suppose it was the headmaster's idea, he thought I would be a good chap for medicine, so OK sir I will sign up for medicine. And that's what I did. And I got offered places in Cardiff and in Aberdeen. The snobby London colleges wouldn't even give me an interview.

AN: Really. And then tell me how you came to do the biometry course. How did that happen?

* Interview conducted by Dr Andy Ness, for the History of Twentieth Century Medicine Research Group, UCL, 27 July 2000. Transcribed by Mrs Jaqui Carter, and edited by Professor Tilli Tansey and Dr Hugh Thomas.

SSL: Well my interest, as I said, had been around for a long time, and I maintained that interest at medical school. In fact I found medical undergraduate education so boring, or intellectually unstimulating, that I spent a great deal of my spare time reading and studying mathematics, logic, and particularly the philosophy of science was one of my great interests. That kept me going mentally and I realized that I did want to combine that interest somehow. I got to know Hubert Campbell, who had recently been appointed as the Professor of Medical Statistics, I think he was senior lecturer when I first arrived there. I got to know him and he helped me to think about what to do. In fact I [had] an eclectic period, which involved going into his department, because he had got some interesting data and I got to know more about the options there. And it was he that suggested that I perhaps look at Reading and one or two other places and I perhaps try the MRC as a source of funding. So I am very grateful to him for giving me that advice. And then, of course, we stayed in close touch afterwards, as I obviously went back to Cardiff, and I joined the MRC unit.

AN: So how did that happen? After you did your year's biometry and then?

SSL: The post was advertised in the MRC unit, probably in the *BMJ* (*British Medical Journal*), but I don't recollect. It was actually for a statistician. I said that I was interested in the post, but was there any way they could make it a clinical post, because I was anxious to try and maintain the advantage that a clinical post gives you in terms of finance and other career opportunities. And I think I went up to talk informally with Peter Elwood and the others and they were very keen and I think it was Peter who fixed it with the MRC, they just changed the grading. I was also at that time offered another job, which I hadn't actually applied for. I was in touch with, I can't remember the guy's name now, the chap who ran the biostatistics unit in London, in Gower Street, it has gone completely out of my mind, if it comes back to me at a later stage, I will let you know. I was offered a post there, they could have slotted me in apparently, but I didn't take that post. Well the reason I didn't take that post I couldn't stand the idea of living in London.

AN: So you joined the MRC unit when?

SSL: In about September or October of 1974.

AN: So that would be around the time that Professor Cochrane was retiring.

SSL: He had just retired before I arrived.

AN: What were the first things that you did when you got to the Unit, what studies or work?

SSL: That's difficult. For the first short period of time, I actually spent reading past papers of the Unit, which they gave me with great pride, so I went through an awful lot of pneumoconiosis that Archie had done. I can't remember the very first thing that I did, but I have got a list of publications you can keep that if you like, that I was involved in. These were some of the earlier studies. That one was not work I did at the Unit, the fourth one on the list, that one arose out of my MSc thesis from Reading, but this will give you an idea. My memory isn't good enough to remember precisely what I was doing.

AN: So your first work was with Michael Burr, you worked quite closely with him.

SSL: I worked closely with Michael on a number of projects, also with Peter Elwood, John Yarnell at a later stage, when he joined. There's a number of publications we did jointly.

AN: And how was work divided up in terms of you coming as a junior between the various bosses?

SSL: No, there weren't various bosses, that wasn't really the way it worked. There was Peter Elwood who was the director yes, and he would hold the ring so to speak, but we were encouraged really to develop our interests. Clearly as I was going in primarily as a statistician, I would link in with projects that were going on. But what we used to do, and I remember this with some affection, we used to on I think it was a

Friday morning, we would have a staff meeting, where after just dealing with the nitty gritty housekeeping matters, most of the time was spent discussing research proposals, ideas, batting them off the wall, perhaps chucking them out as rubbish. And we would have quite vociferous discussions, I mean various camps would form and argue their points. And after that would very often arise a research proposal or idea, so Mike Burr might come and say 'I am interested in asthma and mites' as he was. With all of us we batted off those ideas, and then obviously the person who was leading on that day would go away with whoever was linking in, and they would sort of work out proposals. They would still come back and be batted around.

AN: I got the impression that Michael and others worked quite separately.

SSL: They had their own interests. Certainly they had their independent interests, but at the time that I was there everybody knew what was going on, because the ideas were always brought into the melting pot. It wasn't a forum that gave them permission to do it, so to speak, and I suppose Peter saying that we could use our resources in that way.

AN: And what studies did you initiate yourself early on?

SSL: The main one I initiated was the one on staphylococcal carriage. I was especially interested in the dynamics of staphylococcal carriage and I was looking at staph carriage within a surgical context, in some sort of ward, in one of the hospitals, and looking at what they brought in with them, what they went out with, and what they had a couple of weeks later at home, and relating that to what had happened to them, whether they had been put on antibiotics. They were sort of descriptive studies, so eventually you ended up with pictures of the transitions of people from no staph to staph or staph to no staph and all sorts of sensitivities and fast types.

AN: And how did you come up with that idea?

SSL: I was just interested in the dynamics of infectious diseases I suppose. That's the only one I have ever actually done a study on, but I was interested in that and I think talking to the guy who ran the PHLS [Public Health Laboratory Service], Haydn Howells, we used to meet quite a lot to talk about various things and we thought oh that's a good idea, let's get it running. So the resources were myself, Maureen Mahler, is it Maureen? Professor [Robert] Mahler's wife came as a research assistant in our unit, and she was helping with a lot of the fieldwork and we shared that between us, and Howells' technicians did the laboratory side for us.

AN: And a working day, what would it involve for you when you first started? What were the sort of things you were doing? Was it mainly doing statistics or fieldwork?

SSL: Mainly statistics, I have said that throughout my time there eight-tenths were statistics, and no more than two tenths was fieldwork. A lot of my time was also spent trying to sort things out on a computer, we were just getting into decent computation facilities after the first year or two that I was there and it was making our lives a lot more easy, we could do more sophisticated analyses.

AN: Because Peter Sweetnam describes doing things by hand and also the Hollerith machines and so on. Did you just miss out on that?

SSL: Well I didn't do analyses using a Hollerith machine. We had it there, of course, it was still being used, because it was still used for a lot of Archie's old data, which was on punch cards and Fred (Moore) was a real expert with the machine and it was quicker to get Fred to do the tabulations than to put the whole damn lot into the computer, and Archie preferred it that way. I only used the Hollerith machine for sorting cards and things like that, because we were still using punch cards for programs and data, but at least we could feed them into the computer, which was housed at the university's facility in Park Place.

And then I remember, I don't know, two or three years after I got there, we finally had a landline link, one of those teletype printer inputs, to the computer, which we had down in our basement.

AN: And how did computing change, because it seemed when I was talking to Peter Sweetnam to be quite a big occupation of his life, of how to run things, and where you could find computing . . .

SSL: Well, if I could just put Peter's perspective. He was involved with the cervical cytology data and that was a massive data set and they used to do their analysis down in London or somewhere like the MRC, or MRC-related facility. He and his assistant used to go down to London quite frequently, and I think that was sort of tailing off when I joined. They were still doing it occasionally, so that might have been some of Peter's anxiety about computing. With regard to our unit work, our computing facility was a local one at the university.

AN: And how did it change over the time you were there?

SSL: Sophistication. For example, when I first started everything we did was punch cards for the data and also punch cards for the instruction code. Then they produced the teletypes within the computer centre. That made it a great deal more easy, and then we had the convenience of a teletype in our building. But, of course, I left before – I don't know what the next stage was – they had more modern PCs and connections like that, but that was after my time.

AN: But in the time that you were there you thought that the Unit was up with the rest of the people in terms of its computing.

SSL: Oh yes, it was as good as you got in those days.

AN: That's interesting to know. And Professor Cochrane was still around I think when you were there.

SSL: Oh one of my first memories of going to the Unit, when I started, was to say hello to Peter, and he had the big office on the first floor, and there it was, a big desk this end, big desk that end, and a table, a long table, between the two, because they used to use it as our meeting room as well. At one end, that end was Peter, this end was Archie, and it stayed like that, I am not accurate on time scales, but for many months, perhaps a year or more. What changed it was after the MRC had done one of their routine small visitations, not one of their big reviews, but they used to do small visitations, and it may have been Barbara Rashbass, who we were talking about earlier, but somebody had a word in Peter's ear and possibly Archie's ear, saying that this isn't a satisfactory way to do things, having the director and the ex-director. I think it was probably into Archie's ear, you know cramping Peter's style, so Archie was then relegated to the attic space, three floors up, and that was great. I was also in the attic, I had a small room in the attic, which had a sky light, and then just beyond my room there were two other rooms, there was quite a big room in which they had several people, [nutritionist] Caroline Walker worked in that room and people like that, and then next to it, another big room where Archie was and I think one or two other people would work in with Archie, and that was extremely interesting and lively, because we would all sort of congregate around Archie every so often to hear, he would either just be talking about research proposals, or he would be talking about his life.

AN: Just staying on the topic of Archie, just thinking what else you particularly remember about him or what his contribution was in those years.

SSL: Well, as I say it certainly was a contribution of the grand old man, so to speak, because there was a great deal of wisdom there. There was also a lot of humour from Archie. I am just trying to remember some of his characteristics now. One of his characteristics was that he was on the one hand incredibly humble and on the other hand he could occasionally be incredibly arrogant, and it was a fascinating sort of mixture. I remember occasions like well he would quite often name-drop. There were interesting names that he would drop, he was awfully proud of the invitations he had to certain things, which he had a right to be

proud of I am sure. For example, he was flown first class out to America to give a speech about something, he was glowing about that for days. I remember once he was on the phone to somebody at the Department of Health, I don't know who it was, and he got some woman on the switchboard who seemed to be giving him the run around, and he said 'do you know who I am? I am Archie Cochrane, I want you to put me through to . . .' whoever it was. But on the other hand, he was very, very humble about his own achievements and abilities very often. Terribly supportive of young people in the Unit, as they came through it. The way he was mentally, in the way he thought and behaved, more like a younger person than an older person, so everybody sort of related to him, and he was incredibly mentally active.

AN: One thing that strikes me is that in the early days of the Unit, it was very much, when it was still housed within, the Pneumoconiosis Unit was very much people from Cambridge and a very different set and then the unit culture, my impression is, it changed. Was there a sort of clash between essentially Peter's way of doing things, which I guess was very different?

SSL: I am not really conscious of a clash. In a sense Archie's work, the main things that he was trying to do, were tailing down, and he was writing up, with Fred's help he was writing them up, and he was doing the things that grand old men do, like he became the first President of the Faculty, and he was doing the lectures and things like that. I think we wouldn't have been aware of the two cultures in that sense. I was aware of it in a historical sense. Archie used to talk a great deal about the early days of the Unit, of his house, and the gang that they had there at various times, and Peter Oldham, who was, of course, not in our unit, but down the road, was somebody that I got to know very well, he was a very useful statistical mentor, a very nice person to talk to, we used to go down for RSS [Royal Statistical Society] meetings on the train, and the amount of gossip that Peter had was absolutely incredible. Are you interested in some of his gossip?

AN: Yes.

SSL: It's wholly relevant to the Unit, but I remember going down there once and he was talking about goings on at Cambridge, which was his original home. Roy Acheson was the professor at the time and they had a statistician guy called A W F Edwards, who wrote that book on likelihood [*Likelihood*, 1972] I think. And the professor apparently decided that Edwards ought to do some teaching. Edwards said to him, 'no I am not going to do any teaching', and he said 'if you start to try to make my life difficult basically, I can get rid of you, because I am on . . .' whatever the body was, the supreme body of the university, which he wasn't. As told by Peter Oldham it was a hilarious story.

AN: And you were just describing the visits from the MRC and how there would always be this panic. Could you just say a bit about that again?

SSL: Yes. Well the MRC used to do occasional visits and a figure would emerge, and the figure who emerged was Dr Rashbass. She was a senior scientific administrator of some sort, but that type of position I don't recollect, but she was a doctor and a barrister, and she had a formidable intellect. She was actually a very charming woman as well. But she managed to put the fear of God into Peter. Also I remember he used to look askance at the idea that he had to entertain her when she came. Now she came down on a number of occasions.

AN: The other thing that interests me is that when the Unit first started it did very much study around the needs of the Welsh people. It was very much driven by the need to look at pneumoconiosis with the help of miners, with a very strong relationship with the people in the Rhondda and thereafter it perhaps drifted and by the seventies they were doing less studies in the Rhondda, more nearer to Cardiff. I was just interested in whether there was any discussion or perception that there needed to be a Welshness about what was being done, that it needed to be particularly linked to the Welsh field problems.

SSL: Not when I was there. I think the Welshness was that the Welsh population was our study population. The only things that were related specifically to Welsh problems would have been Archie's work, which was still continuing. And I suppose some of the things perhaps in the nutritional area would be said to have a bearing on Welsh problems, because the nutrition was pretty poor in the valleys. I got the impression that in a sense from having a single focus, which was before my time, when I was there it was more a case of perhaps looking for areas to develop and do, and in certain respects it was driven by things going on elsewhere. For example, one theme which played quite a big part in my life, one theme which was going on there, was this whole question about water hardness and ischaemic heart disease and mortality. We got involved in several studies on that. Certainly I don't think it was any work in our unit that originated that, I think that came from somebody else, but we decided to pick up on that and did several studies looking at quite a number of trace elements and other factors. And there was a very interesting debate we got into with our colleagues in Ron Lowe's department [of Epidemiology, Welsh National School of Medicine, Cardiff], and the colleagues at that time would include Colin Roberts and Robert West. And they were also interested in this, but Robert West thought it could be explained more in terms of socio-economic factors than in terms of trace elements. And he constructed something that he called a socio-economic index, I can't remember what was in it, but basically it was trying to use available data to give some feel for that. And we had had lengthy debates, I am going back to the debates we had in the meeting room, where we had their party and our party. I remember at least two of these meetings, we spent three solid hours debating, backwards and forwards, two very much mixed camps. Now our camp took the view that the socio-economic index didn't explain it. This is where we started thinking about the specificity of effect, it led to one of the papers on there, which I did with Peter Sweetnam, somewhere. We were arguing that you can fit your regression equations in terms of the magnitude of the regression coefficients, if you standardize them in an appropriate way, and that if any particular factor was to be causally related to ischaemic heart disease mortality, you would perhaps use the criteria of Bradford Hill, and expect specificity to be one of the things that might pop up. So we then claimed that we showed that the socio-economic index was not specific, because it worked from a whole number of things. Anyway this debate went on for some time and it was great fun. Now certainly my stance in all this while we were doing it was, I didn't believe a word of it anyway, but I enjoyed it, I couldn't believe that calcium levels in the water would have any effect whatsoever, it was such a minor contributor to calcium in the diet and all the rest of it. I think my colleagues also didn't really believe that, but we were just the peer selection procedure that's all.

AN: You talked about the relationship with the local university department. Can you tell me a little bit more about how the Unit related and worked with colleagues in Cardiff. I was just interested in having an MRC unit and how much it was part of a wider academic community and locally.

SSL: Right, it wasn't closely linked with the Welsh College of Medicine really, except that Archie's share I suppose was. Peter didn't have any links other than an honorary teaching link at that time, and I was always very surprised that they didn't give him an honorary chair or something, but there was something going on in the background, which I never quite got to the bottom of. I had an honorary – what was it called? – honorary teacher I think it was in medical statistics, so I did a tutorial and things. I think Peter Sweetnam may have done some as well and various other colleagues, I think Mike Burr did things, and I did some other things with the medical students, sort of the social medicine side of it, later on. But that was the limit to it, and the only other links really would have been these study areas where we had a mutual interest, where we had this sort of interesting conflict of ideas, which was very enjoyable.

AN: But you didn't have specific regular scientific meetings or seminars that you would go to or that they would go to?

SSL: We would go to some of their seminars, yes. We would go to some of theirs and what we did, our debates were more sort of in-house.

AN: That's interesting. Also looking back on your time there, what would you say were your most notable pieces of work, or the things that have stood the test of time?

SSL: Well they also happen to be the most amusing pieces of work. Going back to Archie's court up in the roof, we used to discuss lots and lots of things. There was a good old group of us, and that was the time when Brenner had been producing his things, looking at mortality and unemployment and other people, I can't remember their names now, trying to explain mortality patterns in other ways. We were just speculating and just saying well look if provision of health services and all the rest of it is so effective, shouldn't we find some correlation between efficiency levels and mortality perhaps in different countries. And that led us to think, well let's do a study, an ecological study, of course, we didn't use the [term] 'ecological study' in those days. I know it had been coined some years before, but we didn't know [it], we used 'correlation study'. And eventually it was Archie, Fred, and myself, who then took this forward, and we decided what sort of information we would want, but not all of it was easily available. We got some from various digests and the rest of it, but some of it, the economic information, was very hard to get, and this is where Archie came into his own. He had got contacts, and he managed to get quite a lot of stuff, international information, which would be readily available now, but in those days it wasn't. So we got all that together and Fred then took all this information and put it onto big pieces of paper on which we had 18 countries, and we had all the variables along the top, and we had ischaemic heart disease in different age groups, etc. etc. These big sheets of paper, from which we then – I am not sure if I transcribed it myself, or whether I got somebody to punch it in – but we got it onto the computer, a nice little database. And I had a lot of fun playing with this and that led to the paper, which is just here, which is called 'health service input and mortality output [in developed countries]'. Now we regarded that as great fun, we didn't take it terribly seriously. What it did show was no particular link with health indices, a very strong link as you might expect, with indices of wealth of the nation. It's just a natural product, and what it also showed was a curious little anomaly. Now if one standardized for everything else and then plotted the standardized residuals against provision of doctors per head and did that for infant mortality, we found that infant mortality increased with doctors per head, after all like everything else, which we thought was the funniest thing in the paper, and we duly sent the paper off to Hubert [Campbell] who accepted it and said that he would publish it. When it was too late, he had dealt with the proofs himself, he was being a bit naughty I think, because when it was too late, I noticed that they had taken this lovely graph out of the paper. I was furious with Hubert, but it was too late, so we managed to persuade, I think it was *The Lancet* or was it the *BMJ* I can't remember where we put it, ah yes it was *The Lancet*. We sent a letter into *The Lancet* with this graph in it, the anomaly that wouldn't go away. And then we forgot all about it. But I will say why this has been influential in a moment. We decided that the next thing we wanted to do was, sorry I misled you earlier when I was talking about skin collagen, skin counts, we were looking at total mortality. And the next thing was we were interested in the skin counts, being sceptics, does it really relate to saturated fat intake and all the other things that people look at. We were using the same 18 countries, we used ischaemic heart disease in various age groups, but we also managed to get hold of alcohol consumption and this is where Archie was particularly good with his contacts again, he managed to get it broken down by beer, wine, and spirits, in terms of ethanol equivalents. And we had a fantastic time with this, we put it into our regression and looked at it every which way, and every which way I looked at it, I found a strong association with total alcohol and an equally strong association with wine consumption and very little with beer or spirits. And confounding all possible combinations, all logical things like that, we could not make this go away. So we were delighted by this news, we didn't believe it in the sense that we thought it was a causal-effect relationship, although we wished it were true, because Archie in particular was a great wine drinker in those days and I have been ever since. And so we wrote this up. In fact I remember writing, well both those papers I actually did the drafting. I think Archie wanted me to get the experience of writing a publication, but also I had done the analysis, so it was in my mind just how it was structured. But I remember what he did in the second paper, I remember sitting one afternoon just writing it up, that was more or less the final form, most papers I do believe he did a little bit more writing. He added one thing to it towards the end, it was something about it would almost be a sacrilege if anybody found out what the component was that was causing the beneficial effects, because basically the wine's all very well packaged as it is. Those aren't the words that he used. And then he added 'we are sorry we cannot advise our friends whether or not it is best to have red, white, or rose'. Then that went off to *The Lancet* and that caused quite a bit of interest. In fact I got invited, shortly after, to do an editorial based on that, and on the basis of that I had a whole string of editorials during the time of was it [Dr Ian]

Munro's editorship, which was very enjoyable and, of course, editorials were all anonymous in those days. So we didn't take it any further. We had a bit of speculation round the table. And remember Peter was teetotal. To be fair to Peter he never shoved his views on others in terms of what it means in terms of a research question, because we were seriously discussing with Peter and others, should we actually do some sort of clinical trial or something of alcohol, and I think Michael Burr was suggesting that we did something with monks and nuns or something. We came to the conclusion that really it was just far too tenuous. This was just an example of a coefficient analysis, which crops up sometimes. Of course that's how history proves you did it wrong! Now that paper, when I last checked on it, I think we got about 250-odd citations, it has gone throughout the medical literature, it's in the sort of general stuff, it's in the biochemical stuff, it's always the paper that is cited as the foundation of this what we thought was a red herring. So that's been very famous. And the other paper that we did, although it hasn't been cited as much, the one on the 18 countries mortality, that was reproduced in the World Health Organization Pan-American, some book of classical cases in epidemic papers and epidemiology and also reproduced in John Ashton's book *Epidemiological Imagination* [1994] and also reproduced more recently as a classic historical paper in the *Journal of Epidemiology and Community Health*. It's funny that the things that one did not think were going to be things that actually change anything in the real world, are the things that are picked up.

AN: Taking first your anomaly paper, the one showing the reverse relationship between mortality and wine drinking, how do you think that has changed things or shaped people's thinking?

SSL: I don't know to be honest. That issue, just that issue of the anomaly, is still running. There have been a number of publications from people looking at it and trying to sort it out. In fact just a few weeks ago, I refereed a paper which purported to have found an answer, at least found a partial answer. So at least that hare is still running. I think it arose out of a context of its time. As I said our interest in it arose because of people like Brenner and various others, and it's really when doctors strike the mortality went down, like the strike in New York. There were all these things going on. There was a sort of general scepticism. It's not just Brenner I am thinking of actually. Who's the other guy who was very famous at that time, used to go round lecturing, basically saying that health services may not do as much as we think they do.

AN: McKeown?

SSL: No. Well McKeown certainly influenced me greatly and I think that was in the background. But there was another guy at the time, oh it will come back [Ivan Illich]. I think those were ideas of the time, I think we contributed to developing those ideas at the time and actually throwing out some more anomalies, which makes you think a bit more, but I think that the reason that that paper, the first paper, is interesting, and why it has been quoted again, is because it's an example of a kind of an ecological study that actually can be quite interesting and useful. The second study has been influential because I think it's led on to some biologically actually plausible hypotheses again.

AN: Do you think that the association specifically with red wine has been borne out by the observational data and the individual-based data that's now around?

SSL: I am almost convinced, yes, and I think if you look at the biological plausibility in terms of the work that has been done on the components of red wine and its various antioxidants and all the other various things, and how the in vitro work seems to also bear out that they have beneficial effects, and then you tie that in with the other epidemiological studies, which seem to be confirming at least a U-shaped curve, if not a steady increase. I think certainly with wine it is almost certainly true in my judgement and I think I would back it on red wine because of its constituents.

AN: There's a recent review I think in which others suggested that it was any form of alcohol.

SSL: Well that was always believed, and I think that's still true, but I think it's the specific effect over and above that of alcohol. I think this is the trouble with epidemiology isn't it, we get so many studies of different sorts, that have partial or complete conflicts, that it will always be open to debate. I like to believe it's true.

AN: And you don't think it's the ability to sustain behaviours in moderation? That consumer moderation is the answer?

SSL: Could be.

AN: I was just thinking of the work of hangovers being associated with risk and the mortality expense in the Eastern bloc.

SSL: I think the association of the French paradox is interesting, and it seems to stand up with the other elements of the French diet, independently of them.

AN: It's quite interesting that the two things that you describe that had the most impact and perhaps are your most memorable, were ecological analyses, rather than the task in hand, which was the fieldwork. Why do you think that was, was it just chance?

SSL: I think there was something about them that caught people's imaginations. I mean the first one was asking a naughty question, let's find out that health services don't have the impact. The second one was asking this intriguing question, you know everyone going round as miserable as sin saying you shouldn't drink, and here we are coming out with this paper saying, 'oh it might do you a bit of good if you drink some red wine'.

AN: And of your fieldwork studies that you were involved in, are there any that stand out as well, that you think well I was really proud of that bit of fieldwork or that study?

SSL: Well there were a lot of studies where the degree of fieldwork, I can't remember, some of them didn't have any, or I didn't have a very considerable amount of.

AN: Well something where you felt you made a contribution.

SSL: All the studies here I made a contribution to, during the planning, the analysis, and the writing up, and certainly I would, well in many of these studies, I would write very considerably, I would usually write the results section, when we had agreed on what they meant. But there are a number of studies here which I found fascinating actually. There was the water-blood-lead work, and I can't remember how we got into that, but that was an issue that was around at the time about what is the effect of lead in water or lead in general in the environment, and this was something that Peter was very keen on as well. One of our studies, well as I say we did several, but one of them we managed to look at different ways of sampling water. Was it the first run-off? Was it after you had run it for a certain time? All these sorts of things. And we were relating that to blood lead. That gave us an indication about which were the best ways to do it. But the study that was most intriguing was the natural experiment we did. There's a lead-producing part of North Wales and I can't remember the name of the place now [Llanberis], but there were two council-estates there, both of them had lead-piping, and the council had decided that they were going to change the piping to copper piping. Now they could not do both estates simultaneously, so they were doing one estate first, and the second estate second, and Peter recognized that as an opportunity for a natural experiment. So what we did, we got the team in there and we got water samples on both estates, before and after, and we got repeated blood samples I think it was from women and children living on the estates. We excluded males because they would be out working and might get much of their water from elsewhere. Now what we found was that if you followed up the blood leads after the piping had been changed, we saw almost a sort of decremental decline, which was a beautiful finding. And we managed to get *Nature* to publish that as a letter. But we had a lot of dissension in the Unit about that, there was a lot of disagreement and I think it was basically Peter versus everybody else on that one. I can't remember the details of it, but it was something about whether it was, I think he wanted to analyse it in one particular way and the rest of us said 'no you can't do that, it's wrong'. And we wouldn't back down and then I think Peter at one stage told Hugh Thomas, because Hugh Thomas was the guy who wrote it up as the first

author. Hugh Thomas got very, very upset, because he was under a lot of tension about this, and I think that Peter phoned back from somewhere, he was out of the country or something, saying withdraw it from *Nature* or whatever, but in the end it all settled down and it was sort of accepted.

AN: You talk about the relationships in the Unit, what was the relationship with the administrative and fieldwork staff? How were they involved and were they actually involved?

SSL: They tended not to be involved in the staff meetings, the brainstorming sessions, although sometimes they would be, when we were dealing with a particular project, they would be involved, if they were making big contributions to it. The actual working relationships were very good and I think there was mutual respect, and there are people like Fred, who are just universally respected by everybody I would imagine. An amazing guy, but we had others there who had seen it all really. I think relationships were good.

AN: And how long did you stay at the Unit in all?

SSL: Six years.

AN: And what prompted you to leave?

SSL: I thought I was getting into a dead end in a sense. There were a number of things. One was I was beginning to get interested in public health and it seemed difficult to develop that within the Unit. The second thing is I was very interested in teaching. I told you earlier I did a little bit of teaching at the Welsh College of Medicine and I wanted to do more teaching. The third thing was I was actually getting a little bit disillusioned with epidemiology. I will tell you why. I felt that a lot of what we were doing was quite intellectually challenging at the level at which I would play with it. I felt that the design and analytical side was fine, intellectually challenging and stimulating, but I was increasingly questioning the 'so-what' factor, which was something that influenced me later in my life. Why were we messing around looking at water hardness and ischaemic heart disease, what possible difference does that make? I began to feel the same about a number of the other study areas that I or others were involved in, so in a sense I felt that perhaps I needed a change, so I came to Manchester.

AN: And what particularly attracted you about Manchester? Was it the first job that came up?

SSL: Well there weren't many going at that time as it happens. I think what attracted me was I came down here and I met people I liked and they sort of encouraged me. In a sense I made a mistake by coming to Manchester, I think I should have gone somewhere else, perhaps I should have waited a little while and gone somewhere else, because I hadn't realized, and this is a comment which has to go on the record in a few years time, I hadn't realized that Ian Leck who ran the department was such a hopeless pratt, and he ran it into the ground, despite all our efforts. But I haven't regretted it in the sense of the contacts that I have made, and I haven't regretted it in the sense that it has allowed me to take forward another agenda, which does relate back to the Unit if you want me to.

One of the things that I first became aware of when I went to the Unit was Archie's book, which he thrust into my hands. It had only just come out I think, or fairly recently, and a lot of our discussions with Archie and others, Bill Shaw who's now the dean of dentistry in Manchester, he wasn't in the Unit, but we used to meet him and talk to him, he was involved with epidemiological studies. A whole group of us used to talk about issues like evidence and effectiveness and, of course, Archie was postulating the randomized control trials and it was all so exciting, and it was all terribly rational, and it was clear to me in those days that all we had to do was to get evidence about procedures, it has to be got through the randomized control trials and then we will change practice for the better, and that was clear to Archie and it was all obvious. So, when I came to Manchester, I thought what we need in the health service is evaluation, evaluation units, people spending their time doing evaluation and that information will change things. I gradually came to realize, because I became involved in real public health, in the sense that in the eighties the health authority was bogged down in running its hospitals.

AN: You came to Manchester in 1980, would that be right?

SSL: '80 or '81. Now I began to realize that things were a bit more complicated. Firstly people managing the health services didn't give a damn about evidence and all that stuff out there, because they couldn't use it, not because they weren't very capable intellectually of using it, but because of the circumstances in which they were working made it irrelevant. So I became increasingly interested in, I suppose, how do we somehow make it relevant? And that led me to my first book, which was *Evaluating Health Service Effectiveness*, which was quite traditional in a sense in that it does an analysis of what evaluation is. I think it does introduce quite a few original ideas, but it is traditional in a sense that it doesn't say much about how you take the product into the market, except for the final chapter, which raises a lot of interesting issues. And that final chapter I suppose spurred me on to thinking for the next few years how do we make this link, which led then to the book that has just come out, which is *Change Promoting Research for Health Services* [1999], where I am essentially arguing that the research process, the development process, has actually got to be 'owned' by those who manage resources in the health service. They need to set the agenda very directly, they need to actively commission work, they need to ensure that the work actually meets the needs of those who are making the decisions. And you don't rely on, I suppose what I would call the sort of Oxford school of thought is that you have evidence, the evidence base and there it is, and somehow people will get that knowledge that will change their attitude and it will change their behaviours, which is nonsense. So that is how my thinking has developed and it has been very much prompted by Archie and I think it has actually gone around in a circle, because I think I have identified the thing which Archie didn't get, and the reason he didn't get it was because of the historical context. I believe the same as he did at that time and that is that you cannot have this intellectual pursuit in isolation of the system that uses the resources. So that's the main message and theme that I am developing at the moment and it did go very much back to Archie.

AN: And your role here is as a public health person or a statistician?

SSL: I am a public health person. I work jointly with the health service, within the health service my main interest has been for the last several years in research and development. We have had a number of initiatives like research and development liaison groups, we have managed to persuade a number of health authorities, we have managed to persuade the regional directorate to give us notional monies so that we can actually actively commission work rather than passively have people giving us ideas, and that has been very successful. And I have been involved in various service effectiveness issues. In the university I am heavily involved in the administration of postgraduate courses and their management, and I am also involved in a number of projects which are not part of any sort of continuing thread of my intellectual interests, because I have explained what that interest is, but they are more that I have got the skill areas that I contribute to certain studies, methodologically.

AN: And describing what you are doing now and where you work sounds a very different environment from the Epidemiology Unit.

SSL: It was a wonderful environment, for example, resources. If you wanted something within reason, you could get it very easily and there was no hassle about it, I think there was quite a lot of loose money slushing around, and if you wanted to set up a study, Peter didn't have to go outside, he had got enough loose money to set up quite small studies and get them done. Technical support, we had I think it was Shirley [Addicott] who acted as our librarian. I don't know whether she had any library training or not, I don't think she did, but she learnt on the job. You know if you wanted a dozen papers that you'd picked up somewhere, you just gave the list to Shirley and she went and got them. Now there's nothing like that here.

AN: And you don't think the Unit was in any way constrained because one of the things, although there was some petty cash to do small studies, the Unit's role was almost defined by the number of staff, a number that was held and fixed. I mean I am just interested in that you said that money

never seemed to be an issue, but looking from the outside now one would think that maybe it was.

SSL: Well I am talking about money on a small scale. If you wanted to do a pilot, or something like that, and most of the studies were ready waiting, that's the only way you can run a research unit. There were other, the bigger studies, they had to be part of the unit plan in a sense, which as far as I understand it Peter still had very considerable discretion but the MRC would keep a watching brief. Then we had these five-yearly reviews, when the good and the great came in to look at us.

AN: Sure. And do you think this is a sensible way to, having worked in an institution which is purely dedicated to research, and then one that has responsibilities to the service side and also to education, which do you think is best, are they different models, which is the best way?

SSL: This is where I have views that many would find unacceptable. This is again looking back to this theme of mine about change promotion and the like. I have now in my mind and in my writing drawn a distinct distinction between what I call science and change-promoting research. Now science has a number of characteristics, one of which is that it is circular in the sense that it is scientist-to-scientist, the scientist decides what the issue is, largely what it is going to be, his peers decide whether the funding is going to be given, the peers decide on the quality of the scientist and all that. Now change-promoting research, which I should think all of R&D should come into, is actually determinedly referential, it is the NHS's priorities as specified by managers. They decide largely what the research issues are although they are informed by the knowledge of researchers as well as what the potentials are. The criterion of good work is not whether it has got published and Brownie points, the criterion of a good work is whether it has brought about change or improvements. I won't go into all the details of that, that is in my book, but what I am saying is that if you have got fundamental scientific research, nobody has found a better way, including myself, of organizing it than the sort of MRC-type model, where you have got to give people quite a lot of freedom and leeway to get on with things, you may as well give them resources, but essentially it's the scientists themselves that are largely deciding what are the issues to be researched. When we are coming into health services-type research, to which I would also put epidemiology, I think well they are not treating it as science in that sense, it's no less rigorous an activity, and it still has many of the trappings of science, and the methods are the same, but I think when you look at it in that context one is saying why? what for? Now it may be that when you are at the level where aetiological epidemiology is closer to traditional science, but even that needs a bit of oversight, because there is an awful lot of money goes into very large epidemiological studies and one wants to ask how they are going to inform future decisions or future research that leads to something practical. So coming back to what you were saying then, the idea of having a unit of people and you give them plenty of resources and you leave them to get on with it, is something that I no longer believe in. Now in 1975, '76, '77, or whenever it was, I did believe in that, I thought it was the best way of doing things. I recollect in that time another figure who's influenced me greatly retrospectively. Lord Rothschild. He was the government chief scientist, he came up with what was quite a radical idea. It was that there should be a customer-contractor relationship between government departments and research councils. Are you familiar with this?

AN: No.

SSL: Stop me if it becomes irrelevant. This was announced about halfway through my tenure in the Unit. Now that had potentially tremendous impact on research units, because it would mean that essentially, well my view at the time, a civil servant who knows nothing about science would be telling us what to do. How dare they? So I was absolutely anti-Rothschild at the time, as was Archie and everybody else, in fact the scientific establishment was anti-Rothschild. This is something that I picked up from Archie's book, and I think he told us at the time, but I couldn't remember it when I read it before, but it sort of came out. One of the things that happened was that apparently Richard Doll was supposed to be the first president of the Faculty of Public Health, well Faculty of Community [Medicine], but he withdrew so that he could concentrate his efforts on coordinating the opposition to the Rothschild proposals on behalf not just of medical scientists, but I think of scientists generally. Now the Rothschild proposals, I was looking them

up recently, they just disappeared. There wasn't any government announcement ever that they were not going to be implemented, they just disappeared. The reason I raise Rothschild is that again there was a complete change in my attitude. Then I thought this was terrible, how dare they? Now I realize that my idea of change promoting research and its difference in size, actually it doesn't so much stem from Rothschild, but it is consistent with Rothschild and I realize the reason why Rothschild failed, because government departments said what's the point of trying, and also hadn't realized how they had got to not only be interested, but they had got to set the agenda in a positive way.

AN: What is quite interesting is in a sense the Pneumoconiosis Unit which was the forerunner of the Epidemiology Unit, or it continued and gave rise to it, that was where the MRC tackled a specific question. But what came out of it was probably more pure science in many ways than the actual applied, because the issue of pneumoconiosis in coal became quite clear once you went down the mines, that there was so much dust in the air, that even if coal dust was inert, that much had to do some damage.

SSL: Yes, it was the case of the bleeding obvious, wasn't it?

AN: But actually respiratory physiology was an unworked field and these bright people set about using this as an opportunity to develop it, for measuring respiratory physiology. So from an applied question, they ended up with very much a pure agenda, although people like [Martin] Wright developed his [peak] flow meter and other things that are widely applied and they essentially are the spin-off of Archie's unit and of giving Archie his free hand to study these captive populations. And I just wonder how you see that, because it seems that looking back over the relationship, it gets a little bit blurred then.

SSL: Oh, relationships will get blurred, but I suppose I am stating it from the point of view that at the moment the emphasis is very much on the model of science, which squashes everything else out of the way. In turning it upside down, you don't necessarily destroy the best that's remaining there, but I would still I think object to just setting up an epidemiology unit and letting it just do what it wanted, without any external reference.

AN: And how would you run a unit like that? Because the MRC has just reviewed its epidemiology units, two current directors are coming up for retirement, how does the MRC sort epidemiology in the 21st century?

SSL: Well, the problem is that I don't think the MRC will be able to sort that out for itself, because it is in the culture of science, most of its study, all of its senior people on the scientific side would call themselves scientists, and most of them are. I can only see, well again I will be provocative. If you consider something like the Research Assessment Exercise [RAE], which is a driving force in universities in terms of their agenda and what they value. If you want to change it, as I do, to do work that is actually for the most part useful in the health service. I am not talking about the pure sciences again, I am talking about the disciplines which do have a practical output, well you might say 'well why don't we go for a single stream of funding?' At the moment the funding you can get is relatively small, but some funding from NHS R&D, and that is the one thing which actually has an agenda, and you can get the prestigious larger beer from MRC, for example, or Social Science Research Council. My feeling would be that we actually take from the, firstly from the HEFC (Higher Education Funding Council) that proportion of their monies which is meant for medicine, take from the MRC that proportion of its money which is meant for applied or directed research, and put that in with the NHS R&D and manage all of them through the NHS R&D and essentially instead of having RAE in a sense directing the funding. What you would do is that you would fund slightly differently. Most of the stuff is in the medical schools, there's lots of pure science, would fund through the R&D stream in a sense of rolling projects, which would come with the infrastructure built in, so that it's not built on top of the existing infrastructure, so that would include the RAE component. Now the reason I say that, and I don't think it will happen in any short period of time, the reason I say that is that I don't think the MRC is capable of thinking along those lines, of things that

are of interest to the NHS linking with management interests. Well, for example, the concordat we have got at the moment, we have got basically so that the MRC can do its thing and the health service will sort of step out of the way and let them get on with. Now that's not the way.

AN: Sure. What do you think of developments like the NHS's research collaboration with the MRC?

SSL: Well again, yes it's the MRC. I don't think there's ill will there, but I just don't think the MRC has come in with the right mindset.

AN: That's interesting. Going back to the Unit, one thing that strikes me in its history was that the first part was if you like liberal, maybe even very left-wing, and had a certain sort of character to it, very much cultured by Archie, and then when Peter Elwood came, you have got a non-drinking person who had a very strong religious belief system. That may just be coincidence that for one person it was politics and for the next person it was religion and I was just wondering if you feel that epidemiologists need to have some sort of world view, be it religious or political, to guide them or to make them epidemiologists. Or whether that is an irrelevance.

SSL: I have never really thought of that. Well do you mean more than other people would have to have it?

AN: I suppose so and whether in some senses if you look up the early talk of epidemiology and very much the social medicine movement of the forties, it was very much linked to a commitment.

SSL: Well a social-reform type of commitment, yes. Yes.

AN: A very much sort of left-wing movement?

SSL: But that's not evident in the sort of work we see in the latter days of the Unit. I mean I don't think say, for example, the arguments about water hardness has any significant social dimension, other than I suppose, we weren't using the terms of course in those days, but other than trying to reduce social inequalities, but I don't think that was at the top of our imaginations at all, it was just an interesting question.

AN: That is interesting. So your view would be that the work when you were there under Peter Elwood's direction was very much apolitical in a sense?

SSL: Oh, yes I think so, yes. I can't speak for Peter, but that's the impression I got. He certainly didn't put any political edge on things.

AN: I mean he certainly addressed policy questions. Like the lead question and so on and so forth, but in terms of actually a broader political agenda?

SSL: No, not that I discerned.

AN: Is there anything else that we should talk about the Unit. I think I have covered most things.

SSL: Well there's one anecdote, which I can repeat to you, unless you have got it from somebody else. I wasn't there at the time. This was told to me by Archie. As you know there were commissions during the war, and then there were reservists afterwards, and then the Korean War came and they decided that they would call up reserves, or at least put them on standby. And there were in the PRU at the time two letters arrived, one addressed to Captain Archie Cochrane, and the other addressed to Major Fred Moore. And Archie used to tell me that with relish. And Fred, of course, was a wonderful raconteur about his experiences. He was with the Desert Rats, explaining to me why he never washed his mug, I supposed it was a tin mug, never washed it. Other people they washed theirs and they all went down with the Ds and Vs (diarrhea and vomiting) he never washed his, because they had limited water supplies, therefore there was contamination. So there were a lot of things like that.

AN: I am struggling to just place this unit in terms of what else was going on in epidemiology. It spanned 40 years from 1948, when essentially Archie started doing this work, to 1999, which is a time of huge change in epidemiology. How much did the Unit contribute to that change or shape that change even?

SSL: Well I don't really know how one would answer that you see. Perhaps we are too close to the events. I think it was clearly part of the shaping process, because that unit and those in it knew and were known by other units and there were groups and an interchange of ideas. I mean Archie, for example, would often be talking about what Jerry Morris was up to, or whatever it was, and some of those ideas would link into how we researched them, but I couldn't trace the paths through.

AN: Because it didn't run an educational programme, though people like Hubert Campbell were teachers, and others.

SSL: Well, firstly, I don't think it would have been in the brief for an MRC unit to do that. I certainly don't think Peter would have been in the least bit interested either.

AN: But I suppose at one level it had a training process in that there was no career structure in epidemiology and people came through it, and so they would be exposed to the techniques and the way of thinking about things.

SSL: Yes and we had a number of people come through, particularly in my latter years there. I mean one of those notable characters I mentioned briefly was Caroline Walker. Perhaps other people have told you what you need to know about her.

AN: Well I haven't got it on tape. Janie Hughes and I have talked about her, but she was a nutritionist I think, a founder of the Caroline Walker Trust. Or was it founded after her?

SSL: No, it was founded after her. A very sad, early demise. She was a breath of fresh air in the Unit, very bright, very nice, very pleasant, and a very forcible young woman. She and Peter used to be at daggers drawn, but she would stand her ground. She lived in a particularly different world, I mean she was an anarchist, she used to tell me about her trips to, well she lived in Holland, in Amsterdam, a lovely place. She lived in Amsterdam for years and she used to tell me all about taking pot and all the other things you know. So here was this character who was completely different from the rest of us. I mean the rest of us, although most of weren't completely staid, we were very staid in comparison to her, and she was delightful. And she got herself involved in this nutritional epidemiology, but she was very, very sceptical about it, and she was very sceptical about how Peter was trying to do it, she felt that he was being very naive in the way that he was trying to apply things that worked in other aspects of epidemiology to nutrition. But as you appreciate those were early days of nutritional epidemiology. A lot has happened since. But it was very interesting having her. We had Andre Dupont over for a period. He did one major study and perhaps some minor things. He sadly, after he went back, committed suicide later. I don't know what it was about.

AN: And was there a sense that, I mean there was a real sense of a very young group being recruited in the forties to this PRU, a sort of vitality, a social commitment, and then, is that vitality still there, is it more workmanlike, is it less exciting somehow in the later years or is that a misconception?

SSL: Well I can't answer in one sense, because I wasn't there in the earlier years, but I certainly for most of my time there, not all of it, most of my time there, found a sense of excitement. But that was a sense of excitement that arose I think from what Archie was pushing. Some of it arose from things that we were doing as a unit, but somehow Archie managed to add the excitement to it, even though he wasn't in any managerial role there or director, he was something that stirred the pot.

AN: That's interesting. I think I have covered most of the things that I wanted to. Thank you very much.

[END OF TRANSCRIPT]

Further related resources:

1. Ness A R, Reynolds L A, Tansey E M (eds) (2002) *Population-Based Research in South Wales: The MRC Pneumoconiosis Research Unit and the MRC Epidemiology Unit*. Wellcome Witnesses to Twentieth Century Medicine, vol. 13. London: The Wellcome Trust Centre for the History of Medicine at UCL.
2. Ness A R (intvr); Tansey E M, Thomas H (eds) (2017) *Bainton, David: transcript of an audio interview (11-Jul-2000)*. History of Modern Biomedicine Interviews (Digital Collection), item e2017044. London: Queen Mary University of London.
3. Ness A R (intvr); Tansey E M, Thomas H (eds) (2017) *Elwood, Peter: transcript of an audio interview (14-Apr-2000; 28-Feb-2001)*. History of Modern Biomedicine Interviews (Digital Collection), item e2017045. London: Queen Mary University of London.
4. Ness A R (intvr); Tansey E M, Thomas H (eds) (2017) *Hugh-Jones, Philip: transcript of an audio interview (05-Jul-2000)*. History of Modern Biomedicine Interviews (Digital Collection), item e2017046. London: Queen Mary University of London.
5. Ness A R (intvr); Tansey E M, Thomas H (eds) (2017) *Hughes, Janie: transcript of an audio interview (28-Mar-2000)*. History of Modern Biomedicine Interviews (Digital Collection), item e2017047. London: Queen Mary University of London.
6. Ness A R (intvr); Tansey E M, Thomas H (eds) (2017) *Jones, Marion: transcript of an audio interview (10-May-2000)*. History of Modern Biomedicine Interviews (Digital Collection), item e2017048. London: Queen Mary University of London.
7. Ness A R (intvr); Tansey E M, Thomas H (eds) (2017) *Kilpatrick, Stewart: transcript of an audio interview (23-May-2000)*. History of Modern Biomedicine Interviews (Digital Collection), item e2017049. London: Queen Mary University of London.
8. Ness A R (intvr); Tansey E M, Thomas H (eds) (2017) *Miall, William: transcript of an audio interview (13-Aug-2001)*. History of Modern Biomedicine Interviews (Digital Collection), item e2017050. London: Queen Mary University of London.
9. Ness A R (intvr); Tansey E M, Thomas H (eds) (2017) *Sweetnam, Peter: transcript of an audio interview (31-May-2000)*. History of Modern Biomedicine Interviews (Digital Collection), item e2017052. London: Queen Mary University of London.
10. Ness A R (intvr); Tansey E M, Thomas H (eds) (2017) *Tudor Hart, Julian & Thomas, Mary: transcript of an audio interview (14-Jun-2000)*. History of Modern Biomedicine Interviews (Digital Collection), item e2017053. London: Queen Mary University of London.
11. Ness A R (intvr); Tansey E M, Thomas H (eds) (2017) *Waters, Estlin: transcript of an audio interview (14-Jul-2000)*. History of Modern Biomedicine Interviews (Digital Collection), item e2017054. London: Queen Mary University of London.
12. Ness A R (intvr); Tansey E M, Thomas H (eds) (2017) *Yarnell, John: transcript of an audio interview (18-Apr-2000)*. History of Modern Biomedicine Interviews (Digital Collection), item e2017055. London: Queen Mary University of London.