

AUDIO INTERVIEW TRANSCRIPT

Yarnell, John: transcript of an audio interview (18-Apr-2000)

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Yarnell, John: transcript of an audio interview (18-Apr-2000)*

Biography: Dr John Yarnell worked in Epidemiological Research Unit from 1975 to 1993.

AN: Andy Ness

JY: John Yarnell

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

JY: Well I am a man of Kent, I was born in Folkestone, but my first memories of childhood were actually from Singapore, where I went when I was about four, for a few years. It was probably a very privileged colonial existence, although one didn't realize it at the time in that we weren't completely cut off from people living there, but certainly the lifestyle was very different between the British people out there who worked for the Air Force or whatever other service it was. My father worked for the RAF in Changi and we had several moves of house and fairly small classes and a good standard of education, but there were the beginnings of trouble there in the 1950s and I can remember going to school in a lorry covered with chicken wire to stop the locals getting us with the stones and what have you, which didn't occur very often, but there were some riots out there before independence.

AN: Were you an only child?

JY: No, no. I had a brother three years younger than myself and, of course, it was a marvellous place for exploring and a lot of groups of children with a large area to wander around in or near Changi and plenty of beaches and what have you. It was quite a good time to be a child. Rather a contrast to come back to Folkestone and have to start working for the 11-plus. After the 11-plus my father was in the Met Office, the meteorological office, had a move to the headquarters at Bracknell in Berkshire, sorry in Dunstable actually at that time, so we had to move to Dunstable, which was in Bedfordshire and I went to Dunstable grammar school. I had an interest in science, particularly in chemistry. We had a Remove form in our school that did O levels a year early, and we did enough to go into the sixth-form and then [I] rather regretted that I had dropped biology and decided to do human biology as an option, plus biology, chemistry, and physics. I was still interested in a career in chemistry, but was more interested for a lot of reasons, I think, in biology as well, being rather an introspective child I didn't know much about it. I think it was purely personal curiosity I think as much as anything else that made me interested in biology and topics like orthogenesis and evolution and what have you that you might recognize from [Pierre] Teilhard de Chardin.

AN: And when did you decide or what made you decide to do medicine?

JY: Again this is something that I can't answer very clearly. I am not sure what it was, it wasn't really a feeling of vocation as such, it was I think more a bit of intellectual curiosity and a bit of a feeling that I didn't want necessarily to go right into chemistry any longer. I remember being very naïve at an interview at

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King's College in London. I didn't intend to say this, but I will, but they asked me why I wanted to do medicine and I said that I didn't think I was clever enough to do chemistry, to do original research, so I thought I had better do something applied like medicine. It didn't actually go down terribly well surprisingly. Anyway I was quite fortunate to get into Manchester. I was about an hour late for the interview because I had come to Manchester and didn't know it at all and I was waiting for the bus on the wrong side of the road and it went in the wrong direction. But anyway the dean was a very sympathetic chap called Walter Schlapp, who was I think an expatriate from Germany. He was a physiologist and he was a bit interested in music and for the Duke of Edinburgh's Award I had done 20th century British composers as part of my special subject. So I had no definite idea what made him offer me a place, but I remember we talked about that and a few other things. I think he was a cellist or something. I enjoyed medical school, it was much more exciting and grown up than ordinary school. My schools had been, actually in those days, they were single-sex, grammar schools, so it was quite interesting meeting members of the opposite sex for the first time, well very much as contemporaries and socially. Yes in medical school I think the physiology and anatomy passed by and I was very interested in virology and would have perhaps considered doing that. Also psychiatry interested me, but when I started my clinical work in psychiatry, the first chap I met was a schizophrenic who was talking about little green men and I think that convinced me that it was so hard to unravel the problems of psychiatry, that it would be rather a difficult route to follow.

AN: And did you do a lot of clinical jobs after you had qualified, before you decided to do virology?

JY: I did hospital jobs in Salford, which was a companion city to Manchester. That was an interesting insight into a different part of life, casualty departments are usually very gory places that do open up a completely different aspect of life that you didn't know before. Then Lancaster and then I went to London for personal reasons. There was a girl I was quite fond of who was trained at the Royal Free and she had been house officer with us in Lancaster. I did about nine months or a year of obstetrics and gynaecology and the obstetrics I think made me interested in the possibilities of prevention, because the whole object of obstetric and antenatal practice was to prevent problems with the birth and that did actually interest me quite a lot. At this time I did wonder where my career was going. I didn't like gynaecology, I didn't like the thought of a) having to terminate pregnancies, or b) confining oneself to necessarily working with ladies. It sounds a bit odd, but I thought gynaecologists were rather strange beasts who had to spend their time being nice to the ladies.

AN: You moved into?

JY: Well what I was doing at that time, I read a book by René Dubos which was called *Mirage of Health* and that was a lot about limitations of curative medicine and I got interested in the possibility of doing epidemiology or public health, something like that. I went along to see a chap called Michael Warren at the London School of Hygiene [LSHTM] and he thought I hadn't done enough clinical work, so he didn't offer me a place at the School of Hygiene, but he said that if I really wanted to do epidemiology or public health, Bristol was a good place and I could do a Diploma in Public Health [DPH] there and so that's what I did. I had never been interested as an undergraduate, except in lectures we had been given by Fraser Brockington who was a consultant to the World Health Organization. He taught us for a little bit in Manchester, but he was just on the point of retiring and we only had one or two lectures with him and the others weren't at all inspiring. But I came to Bristol to do a DPH. R C Wolfenden was the Professor of Public Health and Medicine there and he was a very strong, charismatic figure, [a] Yorkshireman as well I think actually, and he had very firm opinions on how public health should be organized and the great historical tradition going back to Snow, Chadwick and the others, town planning and development of cities etc. There was quite a lot of epidemiological work that had been done in Bristol by Budd and others, but the epidemiology really interested me and that our lecturers had chosen the book [*Epidemiology: Principles and Methods*, 1970] by Brian McMahon and [Thomas] Pugh, an American book and that was really quite fascinating to me and I made probably quite a nuisance of myself, always interrupting, asking questions in the epidemiology lectures. It was quite a small group, there were quite a few people from overseas, including a couple of people from Sri Lanka and Nigeria I remember and Pakistan and a few

British people on the DPH as well. But after that year I didn't specially have anything to go to, but there was a vacancy in something called then the Health Education Council's medical research unit, which was on the third floor of Canynge Hall, which now houses the [School of Social and Community] Medicine in Bristol, which you probably know quite well. And it was run by somebody called Dr John Dale, who had come from Birmingham actually, and he was interested in computers, information type medicine, and also statistics. He was one of the Birmingham group of bright young doctors who were interested in information science etc. He had formed this unit with another chap from Birmingham, called [Chandra P] de Fonseka who was interested in home accidents and also traffic accidents and they set up this unit partly for that, but it had been linked with the Health Education Council at an earlier stage and they had set up a department or a unit to do surveys of home accidents and other evaluations of other health promotion campaigns or health education campaigns as they were called in those days. I think I was really recruited to try to help the report writing and writing up of the projects, because I was employed as a junior epidemiologist and Chandra de Fonseka was the senior epidemiologist, but he was self-taught and I think it is like the groups that ask for a statistician after they have completed the work, a little bit like that. I was there for about two and a half years and I did help in a number of projects and I did a small project of my own, which I was able to write up afterwards, and it was an interesting time because it was social medicine and psychological medicine. Most of the people that belonged to the group were psychologists or social scientists. A good friend of mine was the systems analyst with a lot of common sense, and they don't always have a lot of common sense, but he had done a lot of the work, which was pioneering at the time, on home accidents and they did a documentary on gas water heater accidents. There was a television documentary on that, but that was really nothing to do with my work, but there was some very good work that came out of the unit.

AN: What was the piece of work that you did while you were there?

JY: I did some work on evaluating why people didn't accept measles vaccination and there was just a small before-and-after survey. There was a promotion on trying to increase the uptake of measles vaccination, but it was just a basic health education model on attitudes of behaviour and knowledge as to why people didn't accept or didn't want their children vaccinated, what the reasons were, they would or would not accept the offer of vaccination. There was another project going on venereal disease in conjunction with St Thomas' Hospital and I wrote that up with one of the research officers that worked there as well. Nevertheless they decided, there was a change of director at the Health Education Council, and they decided to close the unit and John Dale and John Roberts left and I closed up the unit, before everybody else had left. I was quite fortunate in that there was a vacancy at the MRC Epidemiology Unit and I went over and had an interview with Peter Elwood and he seemed to be interested in what I had done and I was accepted as a junior epidemiologist to join the staff there. He had actually recently become director of the unit on Archie [Cochrane]'s retirement from the MRC, he had become director in about 1974 and I joined the unit a week after I got married in February of 1975. I met my wife in Bristol actually and she had been a friend of one of the Sri Lankan friends that I had met on the Diploma in Public Health course.

AN: When you first went to the MRC unit, what did you start working on?

JY: Peter had got a small project. He was trying to develop links with the local authorities and the health authorities at that stage and they had been asked to investigate a strange housing estate in the Rhondda. I think it was called the Penrhys housing estate, but it was set up on top of the hill, very high, often in cloud, and a lot of residents has complained that there seemed to be chronic respiratory problems living up there and it was rather isolated and we were trying to design a study that would adequately look at whether there was truly a higher incidence of disease in this housing estate compared to people who lived down in other villages or in other comparable housing estates. This was a very small study which we did, based on children and their families. The sampling units were actually houses and we looked at a comparable housing estate, which was not quite so high, not quite so new actually, and the older housing, children who lived in some of the older housing in the locality. We did objective measurements as well, but we also got a history of roughly the number of episodes of respiratory illness that people had had in the last twelve months. At this time we were the epidemiology unit at Richmond Road, which used to be

the old tuberculosis laboratory in Cardiff and their sister unit, the Pneumoconiosis Unit, was at Llandough. We didn't move until later. And I asked for some help, some equipment, from the Pneumoconiosis Unit and they lent us a spirometer and so we were able to do lung function as an objective measure plus using less robust measures of questionnaires of symptoms and incidence of respiratory illnesses. Somewhat to our surprise we did find that the incidence of respiratory illnesses was somewhat greater in the new estate, but the lung function was pretty comparable among all three and we felt it was probably something to do with the recent influx of people coming to a new estate and the mixing of new cohorts of families all together. I did another couple of studies based on this work: some hospital admissions and also for asthma and respiratory disease based on the data available for the health districts in South Wales. Later I went on to do another study among 2,500 children in Avon and South Wales to compare the lung function of the South Wales children who had a much damper climate – the rainfall was much higher in Wales compared to Avon – and to see what objective measures there were, differences in lung function between the health districts there.

AN: A couple of questions. When you arrived, having come from Bristol, I just wondered how you felt it was different or what your impressions were of the Unit.

JY: Oh right, well I was really quite overawed with it. I think the relative order that there was there compared to the complete chaos in the previous job. But to be fair I think the Unit had a very long tradition of high-quality work, high-quality fieldwork, starting from Archie in the late 1940s when he was actually at the Pneumoconiosis Unit and he was Professor of Chest Disease at the University of Wales, then called the Welsh National (School of Medicine). It had this long tradition of high-quality fieldwork. It was a completely different organization to the Bristol-based organization and really one knew that, well I felt that it was very challenging, that you had to get involved with probably more medical things than sociological and psychological aspects of epidemiology that were more clinically relevant things, or public health relevant than was the case in Bristol. So it was a challenge.

AN: Michael Burr had done some work on asthma. I wondered how yours fitted in with that.

JY: Yes, he was very supportive and helpful in that respect. I was really not trying to compete in any way with his work on asthma. We were both interested in the legacy of chronic respiratory infection. Michael was able to direct me to the right questionnaires etc. to use, but after the respiratory studies, which I did use for my thesis as well and for the MFCM (Membership of the Faculty of Community Medicine), I did a study in the elderly that I had been interested in doing. It was on histocompatibility antigens and we were trying to see whether there was a change in the profile of histocompatibility antigens with age and I really wasn't able to complete the study that I wanted, but we did two groups of elderly, a young elderly, compared to an older elderly group, to see if there was any difference in the spectrum of HLA antigens. We also tried to build into that some work on auto-antibodies and also evidence of previous infection, but there was a lot of pressure to do work in those days and I was also asked by the MRC with Peter Elwood and one of the medical officers of the MRC to do a bit of work on incontinence in the elderly, so we built that into that study at the same time.

That was a study where I was given a fieldworker to work with, Tom Benjamin, who had been of one of Archie's most determined and successful fieldworkers in the past and who became quite a good friend of mine. He had a lot of stories about the Unit in the past and many words of advice on our strategy and other things. So it was very challenging and a very busy period. I suppose the next step really was partly chance in that I had the opportunity to get some funding. The government policy at that time (as ever) was to get people back to work; there was a job employment scheme getting women back to work, particularly those who had had training and then had families and then wanted to get back into some speciality. So I had the opportunity to get this money from the Job Creation Scheme and we did a study in women, a thousand women, to look at incontinence as a disorder, and also I wanted to develop this a bit further and we also looked at the determinants of HDL (high-density lipoprotein) cholesterol. And we teamed up with a chap called Colin Richards at the University Hospital of Wales and Tim Stephenson

who was at Cardiff Royal Infirmary, to get a questionnaire together on this and so this was for the MRC. It was one of the first surveys of urinary incontinence that had been carried out in a population sample.

AN: And this was carried out in the Rhondda was it?

JY: It was carried out in the Caerphilly district and surrounding villages. We went up further towards the Rhondda, but not right into the Rhondda. The Rhondda was a special coal-mining community and that was why Archie had used it so much because, of course, they were exposed to coal dust and he was interested in pneumoconiosis and then tuberculosis.

AN: Because at around the time that you joined the Unit, began working for them, there was a move from doing fieldwork within the Rhondda, a retrenchment down towards Caerphilly.

JY: Oh certainly yes. Peter Elwood had started off doing some work in the Rhondda, both he and Michael [Burr] there had built up a relationship with the Caerphilly general practitioners and also practitioners in Barry as well. David Bainton had been at the Unit, I should have mentioned that, and he had done quite a lot of work on the prevalence of gallstones and he used the Barry population.

AN: Were there any other reasons for using people from the Rhondda or was it largely in terms of convenience?

JY: Yes, probably the selection of the population as well. The population and movement from the Rhondda was very, very high. This was something I was quite interested in – the study of respiratory diseases. There had been massive emigration from the Rhondda with the winding down of the mining industry and not so many mines operating and more mechanization so fewer people needed.

AN: That's interesting. The other cohorts have suggested that perhaps there was the beginning of a breaking of a trust with the miners in a sense that they had been studied frequently. The studies originally that Professor Cochrane had carried out had a direct relevance to them, they ran a series of cross-sectional surveys and after 10 or 15 years of being surveyed, and some people had been surveyed several times, that perhaps people felt that they had had enough of this and this doesn't relate to our concerns. Was there any sense in that that you were aware of?

JY: Right. I knew a little bit about this from Tom Benjamin afterwards, but I think actually the unions did feel a bit let down by Archie's work and that he was a vigorous scientist and he tried to examine prospectively whether the level of pneumoconiosis made a difference to outcome. Unfortunately, looking at it purely in terms of mortality, and not necessarily in terms of quality of life, he found that almost always premature mortality was linked to smoking habits as well as to a particular degree of pneumoconiosis. He and others came to the conclusion that simple pneumoconiosis was not a significant predictor of mortality in itself and this upset the unions quite markedly and it could possibly have affected the compensation issues. I think they probably regarded him as having joined the establishment or something. That was perhaps one of the reasons, but I am sure that another reason was the fact that the Caerphilly population was much closer to Cardiff, the home of the Unit. There had been a clinic operating in the Rhondda, but it wasn't so much used, it was probably easier to operate much closer to base.

AN: After your work in Caerphilly, you then moved on to the setting up of the Caerphilly Prospective Study.

JY: Yes it really started out of this work in women in that we went back to these women to ask them for blood and we had this fairly elaborate theoretical structure looking for the determinants of HDL cholesterol, both dietary determinants and physical determinants, and we really thought that it would be a good idea to do this in men as well. I had some encouragement and some discouragement from other people. Peter Elwood was quite keen. David Bainton had actually just moved to Bristol and there had been the screening clinic. I am sure you know all about this at Speedwell and I knew the chap who had

actually run the original Speedwell clinic and I mentioned him to David, whose contract, well it wasn't a contract, he had been away doing the MSc I think at the London School of Hygiene, and I knew that he had previously been working with Archie but not for Peter Elwood, and he'd got this move to Bristol to start his work there and being at that time much more interested in epidemiology set up this Speedwell clinic, following and doing a much better job of the medical officer who had set it up in the first place, John Woffinden, but without having any very clear directions what it was going to do. David was a friend of George Miller, who started up the work on HDL with his brother [Norman Miller], and they were set up really to do another cohort study to look at whether HDL was truly an independent risk factor. That's what David set up his study to do and I started this small cross-sectional study in Caerphilly, encouraged by Peter Elwood to look at the determinants of HDL cholesterol and look at a number of life style and a number of physiological factors. I got interested in hormones and David wasn't especially interested in hormones, but the work of Gerald Phillips in the States looked as though it should be followed up, so we did some pilot studies in the Unit, measuring little groups of volunteers, our staff really and their friends, on five occasions to see what the biological variability of plasma testosterone, oestrogen and cortisol actually were and they seemed to be stable enough to get away with doing single measurements and they were crude calculations if you like. One time we thought we could possibly add this to David's work in Speedwell, but David felt he was taking enough blood, so he didn't want to take any more, and we wanted to make a special effort on the hormones. So I did this cross-sectional study in a wide age range, which we haven't looked at very much, but they were 30 right up to 70, and encouraged by Peter Elwood, we decided to continue to form a Caerphilly cohort as well, looking at all the extra things that they weren't looking at in Speedwell, and so we went on to do the study in 44 to 59 year olds and getting a sample size similar size to that in Speedwell and looking at things in common where we could and separately, obviously where we were doing different things. But the other thing that we were quite interested in doing was doing sub-fractions of HDL and we worked with Colin Bolton who collaborated with Norman [Miller] in setting up the method to do that in Southmead Hospital, in Bristol. So that's really how it was set up, it was started from a cross-sectional study and grew into a cohort study when it seemed to be that we would get a lot of extra information. Also at that time I think the MRC had realized and I realized it myself that we probably should be focusing on important chronic diseases and that anaemia and respiratory diseases were not killing nearly as many people as they used to, so we thought that it would be very sensible to try and focus on ischaemic heart disease as a major killer in Wales and the UK generally. We had also, Peter and David Bainton had developed links with a very pleasant and helpful consultant haematologist, Bob Easton, in Bristol, who did fibrinogen measurements and also we had an extremely valuable collaboration with John O'Brien in Portsmouth that Peter had started earlier. But they were brought in to do clotting tests in both studies.

The hypothesis was that oestrogens, high oestrogen levels, particularly endogenous oestrogens, might be predictive of heart disease. This was Gerald Phillips' hypothesis. We were quite keen on testing this properly in a cohort study. Gerald Phillips had done case control studies in rather a selective group of patients in his clinic. We found a man called Michael Taggart, who was interested in hormones and stress and heart disease. He was at the Maudsley at one time, but he put me in touch with a Danish consultant who had a small charity called EOCC, I think, I can't remember what it stands for (European Organisation for Clinical Research). But essentially this chap had been giving testosterone for many years to his patients with peripheral vascular disease. He claimed that it was very, very successful but he had never done any controlled trials, in fact he felt it was unethical to do so, as a lot of eminent clinicians do, and the long and short of it was that I was invited to his clinic and he supported our work, which was very modest funding in those days, just to do the assays. The MRC had actually been quite generous and funded two technician posts for the Caerphilly project and we had one of the technicians doing the hormones in a host institution called the Tenovus [Institute for] Cancer Research, which was looking at oestrogen receptors etc., so it was measuring steroid hormones almost routinely. It was the regional hormone assay laboratory in Cardiff and our other technician was working on the HDL and the HDL sub-fractions. So unfortunately when we followed up the subjects the oestrogen hypothesis wasn't strongly supported by the data, but I believe that this is going to be looked at again and a new ad hoc hypothesis has been formed in Bristol about it, that George Davey-Smith is quite keen on, but he never has time to discuss it whenever I see him.

AN: Can I ask you, Caerphilly almost marks out a departure in the Unit from its history of doing cross-sectional studies, though I know of course there was the Rhondda Fach follow-up studies and from the description you have given so far it sounds very much serendipitous that it turned a cross-sectional study into a cohort with a bit of prompting from the MRC. Is that right or was there a conscious thing each time this unit did a prospective study and prospective studies are the future for epidemiology or more important perhaps?

JY: I think it was a bit of both. I knew and Peter Elwood knew that probably we should concentrate on ischaemic heart disease. We had the example of Speedwell being set up by David Bainton as a prospective study. We knew that it was not possible to do everything within that, so I think it was a strategy formulated in the Unit itself to develop the Caerphilly study, which was genuine team work with the director Peter Elwood and Peter Sweetnam, designing the study fairly, insisting that we concentrate on these 45 to 59 year olds to try to maximize the cost-effectiveness of the exercise. And I suppose I had been prepared to do a lot of the donkey work as well, which was two evening clinics a week which we used to do out of normal working hours, because in the 45 to 59 years old there was still a fairly high level of employment in that age group, although a proportion of men had retired or been forced to retire early and we did a single morning clinic, which we invited everybody to come [to] and have their fasting blood sample taken in the middle of the week, so it worked on that basis and Peter Elwood and others helped on a very practical level with the morning clinics. Yes I think a lot of the staffing arrangements were geared towards the Caerphilly study and I think as it went on Peter Elwood probably became more enthusiastic on that and he was still doing some aspirin work and it became fairly obvious that we ought to carry on with the cycle of work and we did make actually an unsuccessful bid the second time round to get funding from the British Heart Foundation. There were some differences of opinion with the reviewers as to why that wasn't done. They were Gerry Shaper and Tom Meade, the people who came down to review what we wanted to do the second time round. One of the things that we had got funding for, or unofficial funding for, because he was very keen to do it, and actually I met Serge Renaud the first time on the same platform as Tom Meade talking about platelet studies. He had done a lot of cross-sectional work and was very convinced that his methods would be convertible to large-scale epidemiological studies.

So he invited me over to his unit in Lyon and basically he had this mobile laboratory, which he wasn't using at the time and he really jumped at the chance of bringing it to Caerphilly to use in our study and after some discussions with Peter, the two Peters (Elwood and Sweetnam) we decided to do a pilot in the grounds of the Caerphilly [District] Miners' Hospital and we got a group of volunteers plus the reproducibility group. We called back about 200 people from the first phase of the study to see whether these tests would be reproducible in terms of single measurements for an epidemiological study over a period of time. Serge brought one technician with him. She worked for about six or nine months in parallel with our two technicians to get the thing up and running and we did find a similar design based on volunteers and also on these extra people who had been called back, that these tests did seem robust enough or precise enough to characterize individuals with a single measurement. So that was the cornerstone of phase two. And we did have some interest from Nottingham actually from Stan Heptinstall in doing additional tests on the platelet active markers etc, but unfortunately we weren't able to get all the funding for all of that. We were able to do some more platelet function tests in phase three of the study, but during phase two we went to some trouble to reconstruct the cohort by inviting people who had moved into the area, or somehow we had missed the first time and this provided us with a new baseline of a second phase of the study and so it carried on with fresh tests being added at various stages. I think in phase two and perhaps more particularly at phase three we realized that this might go on for some time and we began being interested in looking at stroke as an outcome and we persuaded David Bainton and Ian Baker to put stroke into the Speedwell study at that stage, which was working on its own regular three-year cycle as you know.

AN: Was there any explicit discussion of the size of the cohort then? Perhaps in comparison to the Shaper heart study, the British Regional Heart Study, or to Whitehall or to other cohorts that were

being set up at the time. Caerphilly would appear quite small. I just wondered whether there were discussions around this time about the size.

JY: Yes, there were discussions about the size and I think we had to balance this on the very intensive fieldwork, which was very time-consuming. I think both Tom Meade and Gerry Shaper had suggested that it should be larger and based on a fresh sample. We thought we would lose an awful lot of momentum to start again somewhere else and we thought that was very negative criticism. So we weren't able to accommodate that. I think we (really Peter Sweetnam) did some calculations, and we felt that we had just about got enough size and power to be satisfactory, but one of the ideas was that at some stage things could possibly be tested out in the sister study in Speedwell or vice versa so for some things we were going to rely on the combined cohorts whose total is close to 5,000 men. But practically speaking we just hadn't got the resources or the funding to do a larger sample with what we had. We did feel, I always felt that we could possibly overload it, but I was interested in getting as much as we possibly could out of the study and the second time round I think we, in terms of what we were asking the men, we probably asked them quite a lot, two clinic attendances, the first visit a minimum of one and a half hour and the second visit including anything up to about 80 to 100 ml of blood, which I don't think anybody else has ever asked anybody for. We did take the advice of one of our consultant haematologists who was working with us, Dave Hutton, and he felt that for most people it was quite safe to have that amount taken.

AN: Were there other projects that you were working on as well as Caerphilly?

JY: I think Caerphilly was my main project. I did a little bit of work later on when I was interested in trying to do some European work on statistics, particularly Eastern Europe which seemed to be getting this epidemic of heart disease and we had a visit from Alun [Evans] who was setting up the MONICA [WHO Multinational MONItoring of Trends in CARDiovascular Disease] project in Belfast and I remember getting quite interested in what was being set up in that particular project. But the majority of my work was working on the Caerphilly project as such and trying to develop it and see it through. One thing we had worked on quite a bit, which we would have liked to have expanded a bit more, and prior to the closure of the Unit I did think that it would be worth trying to develop nutritional epidemiology a little bit more and do some large surveys using diet as a focus of the studies, a little bit like Michael Burr's health food shoppers types of approach, but based on population samples rather than selected samples. Unfortunately the MRC had to probably make the political decision about the Unit at that stage because it was quite late on in the day before Peter Elwood's official retirement in 1995.

AN: In your early life just one little thing, you mentioned that you had a younger brother, I just wondered whether he had gone into medicine or what had happened to him.

JY: No, he was interested in science but became interested in sociology and did a little bit of teaching and then decided to become a social worker, and now he is a lecturer in a college of further education just south of Yorkshire actually, in north Lincolnshire.

AN: When you moved into epidemiology was there a sense of what an epidemiologist was and a career structure that you were aware of?

JY: Probably not, but there was the long tradition of what the Unit had been and probably was and clearly it was changing quite a lot. There had been a series of epidemiologists who had worked at the Unit under Archie Cochrane. There was, I think it was W E Waters, who was at Southampton, and Ian Higgins who had gone to the United States and I think even Julian Tudor Hart had worked at the Unit.

AN: He was there for about a year and I am actually going to see him on Thursday.

JY: Are you really. I would really like to talk to him a little more. I don't know him very well, but my friend Tom Benjamin used to speak of all of these people who had worked at the Unit and it was personally very fascinating and we were actually very grateful to the MRC for providing the support during the years that

they did for the Caerphilly study, because I did two clinics a week plus the morning one in phase one of the study. The MRC relented a little bit for phase two, and decided to employ a junior epidemiologist. This is really a very inadequate description for Ian Baker who was actually older than I was, but he came to do his own projects and then helped in Caerphilly and he actually had an idea for a diabetic project, which unfortunately the numbers were too great to actually do that study. Otherwise he may have stayed there and not moved to Bristol to help David with the Speedwell project. And then we had a series of more junior epidemiologists who came to the Unit, like Jim Gilbert, Steve Rogers, and Jane Lascelles was the last one I think.

AN: The thing that staggers me about the Unit is the length of time that people stayed there, at all levels. Peter Elwood stayed there from the early sixties until his retirement, you stayed there for 18 years, but there are also people like Janie Hughes who stayed for a long, long time. I just wondered if you had any ideas as to what it was about the Unit, why people stayed so long.

JY: I think if you start cohort studies then I suppose you feel you have to try and complete them in some ways. That was one of the things. I was fortunate in being offered tenure with the Medical Research Council after about, I don't know what their statutory period is, it was probably early in the 1980s and there was a panel, which included Geoffrey Rose who is very supportive, and I think that is really why people stay. And partly because if you are accepted onto the scientific staff on a tenured basis with the Unit then there is a drive to continue and keep on with your projects. I don't know what Michael Burr said, how he responded to that question, but I admired his work with the setting up of DART and then following it with another basically modified hypothesis on another group. These were much bigger and bolder projects probably than had ever been attempted before within the Unit. Previously people had done smaller studies on smaller and of a more diverse range of topics.

AN: And personally did you ever think 'I have done had my time at MRC, I should move on or I should look elsewhere'?

JY: Once or twice, yes. My wife actually wasn't very keen on my staying and she actually tried to get me to go to Jordan at some stage, because we had said that we would try to have a period working abroad, but I actually felt so involved in setting up the work that I really didn't want to leave it at that stage.

AN: Another thing talking to people about the Unit is the working hours. People would work very long hours and go away on field surveys and work long hours until they were finished. I am just interested a little bit in what it was like when you first joined and whether it changed over the time. What was a working day like for you?

JY: I suppose with the Caerphilly project we really knew we had to work at the convenience of the men into the evening. So you know when they finished work they could come to the evening clinic without having to take time off work. It was rather similar in the mornings as well in a sense that the earliest person I remember taking blood from was a postman at about five to four in the morning, but the miners, those that were still working, there were a few working collieries at that time in the area, they usually had to be dealt with before about quarter to six, because six o'clock was when they went down the pit. Usually there was only a handful of those at every clinic, so there might be a couple of early ones and the main clinic, particularly on phase two [you] might start at six thirty or something like that, a more civilized hour for our colleagues to come along. But you wouldn't work the full day then, you would really be too tired. It's like milkmen, they usually have a sleep in the afternoon, because you can't keep going to that extent. But it was fairly routine, there was a routine to it, which, of course, there's not so much now, balancing more projects and what have you.

AN: I understand that when Professor Cochrane retired he was around the Unit for a fair time after he had retired.

JY: When I came to the Unit it was rather interesting actually that he was sharing the same room as Peter Elwood. I am surprised Peter didn't mention this, but it was rather amusing at the time. He was President of the Faculty of [Community Medicine, now the Faculty of Public Health and Medicine] as well. Now Peter Elwood had just been appointed as unit director, Peter had a telephone with an intercom system there and Archie used to sit at the other end of the table in his old room, sharing this room with Peter Elwood, and he used the wall phone there, but it was a really strange set up. The MRC secretariat paid a visit and I think they saw this arrangement and they had fifty fits and they tried to expel Archie from the Unit and we had a little letter that we petitioned everybody to sign to say that could Archie be allowed to stay because he is not interfering with Peter Elwood's work and he is contributing his own expertise to the Unit etc. So they did let him stay but on the proviso that he moved upstairs to the attic. So at Richmond Road Archie went up to the attic and still carried on his work on the Rhondda follow-ups with Fred Moore and others who had really mainly worked for Archie in the past. And Fred Moore I should say was an extremely valuable fieldworker. He was a trained nurse and he did all the ECGs meticulously for the Caerphilly survey at least for phase one and two. He retired finally after that, well into his seventies I think, and after this they were done by Marion Jones, another invaluable fieldworker.

AN: I didn't know that.

JY: At some time another person you might want to talk to is Andrew Beswick, who worked at the Unit from 1980 and performed all the platelet aggregation tests. He is working full-time now for Robert West in the Department of Epidemiology in the Welsh National School of Medicine.

AN: That's interesting. Another thing I would like to ask about is that the Unit itself was quite a long way away from MRC and I would like to talk about that in a second, but there's sometimes a danger with these units that they would become isolated either from their local communities or from their wider scientific community. I was interested in how much you felt involved, how many came to visit, whether the Unit felt that it was very much at the hub of things, or whether you did feel any of this.

JY: I think it was very good at Richmond Road, the relationship with the School of Medicine. Originally the Prof there was a chap called, I can't remember his first name, but he was Professor C R [Cyril] Lowe, and he was an old-fashioned epidemiologist, I think he had come from somewhere in the Midlands or possibly even from Manchester or Birmingham, been interested in respiratory epidemiology and we had regular scientific meetings with the School of Medicine and there were regular seminars etc. Now we did probably, we had a move forced on us by the academic and financial changes to the College of Medicine, they wanted to sell the building that we were in, and so essentially we had to move and find new accommodation. We tried to find it at the University Hospital of Wales, but essentially there wasn't any spare accommodation there, so we had to move back to the old Pneumoconiosis Unit, which had been closed previously by the MRC. It used to employ about 80 people, so it was quite a large unit. The long and short of it was that the quality of the scientific collaboration there probably wasn't as strong as it had been at the University Hospital of Wales which was six or eight miles away across Cardiff, so we were a bit more isolated than I would say.

AN: What about with the wider scientific community? Reading early accounts of people coming out when Philip D'Arcy Hart came to do the first field studies and report, they set up the Pneumoconiosis Unit. In the early days of the Pneumoconiosis Unit one gets a real sense that you were a long way from home. I just wondered about links between you and London, and to other academic centres around Britain, or elsewhere.

JY: Yes, possibly. There were international meetings. Michael continued with a lot of his work on asthma and has pursued that vigorously since the closure of the Unit, but I suppose it was rather specialized and we had to find collaborators I suppose from other departments and from Stan Heptinstall's group, and from London and Bristol, and Manchester in the case of lipoproteins, and Portsmouth, so I think there was pretty much an on-going collaboration with these other scientific units. I had joined the European Society

Group on Epidemiology and Prevention and also the British Society of Haemostasis and Thrombosis, which were both important for networking and keeping in touch with current or potential collaborators. Most recently we have had a continuing valuable collaboration with Gordon Lowe whom we first met in the early 1980s, and as you know we still hold a grant jointly with him for work on the Caerphilly study, largely from phase two.

AN: **The other thing is the relationship with MRC. Many of you describe visits, an MRC entourage would come down and there are also some records in the Public Record Office describing earlier visits to see Professor Cochrane. I was just interested in how you felt about MRC, what was good about the collaboration with MRC and perhaps what was bad about it, not towards the end necessarily, but during the time when they were clearly in support of the Unit.**

JY: I think they had scientific panels associated with the five-yearly reviews, which I don't think had been an absolute permanent feature of the system prior to the early seventies, but the review panel, for example, it saw what work we were doing and the work in nutritional epidemiology for example, and it actually proposed that we had a nutritionist join the staff, and the first one was Caroline Walker and did some work with us in the study among women and then started the work in men. She left and we had Ann Fehily, who remained also for an extremely long time, first of all working very strongly with the Caerphilly project and then after that working strongly with DART and DART 2 before that was cut off.

AN: **Another thing that is interesting is that many of the senior staff were not drawn from the local community and I am just interested in how much you think that the work that was embedded in the local community responded to the concerns of the community. One can see a very clear relationship at the beginning when Professor Cochrane was first doing his epidemiological work in the fifties before the Unit was formally constituted, when he was very much studying pneumoconiosis, things that concerned the miners. I wondered from your time there whether there was a consciousness of responding to challenges from the community, of dealing with problems that were paramount to them and also whether there was a sense of a Welsh agenda that you responded to, because so many people were not in the sense Welsh, and yet they were studying defined populations of Welsh people.**

JY: I suppose we knew that in Wales there was a higher level of ischaemic heart disease, for instance, or a higher mortality from heart disease than there was probably in some other parts of the UK. Many men had seen evidence of people dying of heart disease either within their own families or with friends of theirs. One thing that did actually make a difference I think, and I say this with hindsight, originally there had been a tendency to take our public volunteers somewhat for granted, I think the situation with regard to volunteers was changing. I have always felt that in the little bit of general practice that I have done that it is extremely important to have a good working relationship and to be very open in your communication with people and one of the things that became increasingly obvious to us as the phases went through, that men and the small studies in women that we have done, but possibly more particularly the men, would appreciate having some feedback after the results of the examinations. We originally sent this to GPs, but the second and third times and fourth times we started sending it to the individuals themselves, with some pointers as to whether they should actually take any action on it or not. I think this actually did help improve the collaboration with the men and I think Peter Elwood will tell you this as well that they almost know after the second and third time if you like, that they felt part of the experiment, to find out about heart disease and they appreciate the value of the research. But this is not a relationship you can build up over night, it's something that takes a good number of years to establish.

AN: **Sure. Janie Hughes said some other things about how in the Caerphilly study there was a real sense of responding to local needs, a sense of a community undertaking almost.**

JY: Slightly similarly, you asked about other work that we were involved with besides the Caerphilly study. We did some work with women during a pause in the main fieldwork and that was on a smallish size sample, but we had consultancy work with general practitioners in the area to try to respond to some of their

needs and they would come to us with particular projects to investigate and I can remember particular projects with my local GP looking at levels of morbidity in selected geographical areas in association with possible atmospheric industrial pollution, which is notoriously difficult epidemiologically to do.

AN: One further question. Professor Cochrane was, if anything, very left wing, or so that was his stated position, and his successor Dr Elwood was driven by a strong Christian belief, as were several other of his contemporaries. I wondered if you had either a strong religious conviction or a political conviction and whether this you feel has any part in the way that people do epidemiology or drives them to do epidemiology.

JY: Possibly people start out with that direction, but I would regard myself as a socialist, albeit rather a pink one these days, but there was perhaps among Archie Cochrane and his particular group of close collaborators, there were some strong socialists, and some rather conservative people actually. One's political convictions don't necessarily either confirm or refute one's scientific contributions, but they may certainly alter the perspective and possibly the attitudes towards the scientific ideas and the staff and volunteers.

AN: There was certainly a very strong religious belief that Peter Elwood and others had. I am just interested in their very different styles, very different backgrounds. Thank you so much for all your contributions.

[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) *St Leger, Selwyn: transcript of an audio interview (27-Jul-2000)*. History of Modern Biomedicine Interviews (Digital Collection), item e2017051. London: Queen Mary University of London.

10. 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.
11. 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.
12. 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.