AUDIO INTERVIEW TRANSCRIPT

Miall, William: transcript of an audio interview (13-Aug-2001)

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Biography: Dr William Miall (1917-2004) was a member of the scientific staff of the MRC's Pneumoconiosis Research Unit from 1951 to 1961, when he joined the MRC's Epidemiological Research Unit until appointed Director of the MRC Epidemiology Unit in Jamaica from 1962 to 1970. He was a consultant in epidemiology and member of the scientific staff in the Epidemiology and Medical Care Unit at Northwick Park Hospital, Harrow, from 1971 to 1983.

AN: Andy Ness

BM: [William] Bill Miall

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AN: Could I start by asking you where and when you were born and just a little about your early family life?

BM: I was born in 1917. I was born in Birmingham actually and the reason I wasn't born in London where my family lived was that there were Zeppelin raids going on at the time and so my mother had me in Birmingham. I was the second son in a family of three, two boys and a girl. My father was a scientific instrument maker, working in London and his father, interestingly enough, was a professor of biology at Leeds, I think the first Professor of Biology at Leeds. He started work for the Yorkshire College and joined Leeds University, he started without any paper qualifications at all, I think, got his professorship like that, but finished up with an FRS and there's now a building in Leeds, surprisingly, dedicated to him, named the Miall Building, which is interesting inasmuch as he died in 1922. I think none of the staff in the Biology Department at Leeds would now have known him personally. So that's the background.

My mother was a Quaker, and I was sent to Bootham School in York as a boarder and was much keener on sports than I was on academic work. That was a Quaker school, and I left I suppose at the age of about 18. I was at that stage planning to go into my father's firm, but not really wanting to, and that was just before the beginning of the Second World War and I joined the Friends' Ambulance Unit at that stage. So I joined the Quaker Ambulance Unit, and went with that out to Finland, for the Russo-Finnish war. I was working in a field ambulance unit for a few months in Finland and then we went over to Norway, [were] then evacuated from Norway, went back into Sweden, couldn't get out of Sweden because we were considered by the Russians to have cut off the noses of their prisoners, and things like that, when we were in Finland. And eventually [Sir Richard] Stafford Cripps, who was the ambassador in Moscow got us permission to fly, so we flew from Stockholm to Moscow and got down to the Middle East via the Black Sea area, and started work as a field ambulance unit again in Egypt for a while, and then went over to Greece.

We were working behind the lines as a field ambulance unit, providing transport for the wounded from northern Greece and sort of leapfrogging, that is the front line leapfrogging us and then coming back. On the drive back to the south of Greece, I got captured with about 10,000 other chaps in a port called Kalamata in southern Greece. From there I went to Salonika, via various nasty places en route, and in Salonika met Archie Cochrane for the first time, that was in 1941.

* Interview conducted by Dr Andy Ness, for the History of Twentieth Century Medicine Research Group, UCL, 13 August 2001. Transcribed by Mrs Jaqui Carter, and edited by Professor Tilli Tansey and Dr Hugh Thomas.
He was one of two doctors in charge of a PoW (prisoner of war) hospital in Salonika, and the hospital was staffed by medical orderlies and we were the medical orderlies. By then there were about, I can't remember exactly, perhaps 15 or so members of the Friends’ Ambulance Unit that were captured then, perhaps less than that. Archie in those days had an enormous big auburn beard and a Viking-kind of moustache and was really quite an impressive character. He was very friendly with our unit people and you know we got on very well with him. What he tells about in his book is that it was a pretty traumatic place to be. I was there I suppose for about three months, he was there I think for a bit longer. I was captured before him, he was captured in Crete. I should have said that. And after about three months there, in really awful conditions, the German transit camps for prisoners were terrible, I was shipped up with wounded convalescent chaps to the Black Forest area of Germany and was there for some time working in a hospital for prisoners of war, until there wasn't anything needed in the way of medical work. A friend of mine and I decided we weren't going to waste our time there indefinitely, and we tried to escape. This hasn't got much to do with the years I did with South Wales, but you can cut it all out if you want. Anyway there was an opportunity when I was being transported from one hospital to another of making an escape by jumping out of the train. And we did that, you know in the tunnel, which wasn't exactly the most sensible thing to do, because it was all black and smoke and things, but we survived that, and eventually more or less crossed into Switzerland. We were only 20 or 30 miles from Switzerland when we started, and we thought we had crossed the frontier. We were walking by night, and hiding up by day, it was winter, snowy, and eventually we saw the frontier posts and saw the guards patrolling the frontier and carefully crossed avoiding them, and then came to the outskirts of a village, where there were Nazi sort of notices up on notice boards, and having seen those notices we thought we would try and go right round the periphery of the village and started doing that, and then we got into deep snow and following the river up, and decided that we couldn’t go on that way, and came back and crossed a bridge over the river. On the bridge we ran into a guard with dogs and a rifle and things, and he challenged us, and we tried to persuade him we were Swiss tourists walking in the middle of the night. We were captured, we were retaken, we were punished with 14 days bread and water, we went back to an ordinary camp and again didn't get any satisfactory work for a long time, so we escaped again. But I won’t bother with that escape.

AN: Did you get away or not?

BM: No, we were out for about 14 days, and had a very nice holiday, again walking by night and hiding up by day, which was the safest way of doing it. Then they found us proper medical work and we were happy to stay there. That was in a hospital for PoWs in Germany and I didn't see Archie again really except once during the war, and then he was in a different hospital miles away.

Then when I came back to England, I got repatriated as medical personnel, with wounded, just before the end of the war, that was in October 1944, and I had been a conscientious objector and wanted the terms of my exemption from military service altered to allow me to read medicine, which I then wanted to do very much.

AN: How did it stop you reading medicine?

BM: Because I had conditional exemption from the armed services on condition I did such and such, and what my condition was that I remained in the Friends’ Ambulance Unit, doing that work.

AN: So you went to read medicine.

BM: So I went to read medicine. I hadn’t done anything academic for a long time and I hadn’t ever done chemistry I think, and I had to try and cram. I went to a cramming school actually in London, and managed to accrue my first MB [Bachelor of Medicine] in about six months, but I had the nasty business of having been accepted there. I got some old first MB papers and looked at them, and couldn’t make head or tail of them, and got very scared about what I was letting myself in for.

AN: So where did you go to read medicine?
BM: I read medicine at St Mary’s, which was one of the few medical schools which hadn’t been pretty seriously disrupted by bombing in the war, and it was near where I was living anyway, so I called in there and was interviewed by Lord Moran and he, rather surprisingly in view of my background, gave me a place. So I started reading medicine in 1945 really at St Mary’s, qualified in 1950, got the medical unit house job, which was with George Pickering, did other jobs, and then I went off. I was fed up with living in London, and we decided we wanted to go up to the Yorkshire Dales and got a job at Middleton Sanatorium just outside Ilkley. It wasn’t a good way of choosing a job at all. I was there for about a year, doing a sort of senior house officer’s job, and then got a letter from George Pickering’s lecturer in medicine, or one of his group, saying that George had had a letter from Charles Fletcher, wanting recommendations for people wanting a job at the Pneumoconiosis Research Unit and did they know of anyone who might want to apply. And I had read, as a medical student, about PRU, and thought that was just the kind of work I would like to do really. Do-gooding, but scientifically interesting. I knew by then that Archie was down there and I think I got in touch with him. He invited me down and I eventually got that job and was helping Archie with the early days of epidemiology in South Wales. When I joined the Unit Archie and his team were halfway through, not halfway through, but at the early stages of X-raying and screening miners in the two valleys schemes, which you know about presumably. And I joined when the Unit was in the control valley, the Aberdare valley, miners in four pits in that valley being X-rayed. That would have been in 1951, no 1952. I was only a year in Ilkley.

AN: So you joined the survey they were doing in Aberdare.

BM: Yes that was interesting and fun, it was very sort of non-medical work in a way. I was busy learning how to read pneumoconiosis in chest X-rays, interviewing miners, Welsh miners, and my job largely was, apart from the X-ray reading, going round the houses, interviewing those who had abnormal X-rays, or who ought to be recommended to go for compensation. I was doing that sort of thing for a year or two and then we were increasingly realizing that the facility of having a general population all censused and on what were then Hollerith cards. Do you remember them? Punch cards. That it was an opportunity to look at other diseases apart from miners’ chest diseases. Having been at St Mary’s with Sir George Pickering, I was like most of his staff, very interested in hypertension. I did one or two other things first, one was an investigation of the effect of isoniazid/PAS on coal workers, massive fibrosis. Another was an interesting one looking at Caplan’s syndrome. Do you know about that?

AN: Rheumatoid arthritis.

BM: In coal miners with characteristic chest X-ray findings, an interesting condition. Then I started on a blood pressure survey, which George Pickering was keen should be done in a general population and there was a big battle between Lord Platt, he was then Sir Robert Platt, and Sir George Pickering. The jousting knights they used to be called, but they had daggers drawn about the nature of essential hypertension. And the data I collected very much backed George Pickering’s view, rather than Robert Platt’s, but those blood pressure surveys went on for a number of years. And at that stage Archie was encouraging looking at other diseases, for example, there were surveys on anaemia, on bronchitis, on coronary heart disease, ophthalmology, quite a wide variety, thyroid disease and arthritis, sometimes using his contacts with UCH people, where he had been trained. [Jonas] ‘Yonky’ Keligren was the arthritis man, [Wilfred] Trotter was the thyroid man. And from those days onwards I was more involved with other diseases than pneumoconiosis.

AN: I was interested in a couple of things, one you said about fairly early on there was this recognition of the value of the general population work. It was clear to you in the early 1950s that this was a unique ability to do these studies in the South Wales valleys. Was that widely accepted?

BM: By the medical profession or by the public?

AN: I guess by both.
BM: In those days it wasn't obvious that hospital populations were very unusual and odd populations, but Pickering's work for example on hypertension was not, couldn't really be interpreted properly, because it was a hospital population that he started with, people from hypertension clinics, and compared them with others attending the hospitals, and they had been selected in terms of having blood pressures below a certain level and the whole thing was a mess. And that applied really to coronary disease and everything else. They needed studying in general population samples and we I think were pretty well the first group to be using the general population for epidemiological purposes, for chronic disease epidemiology. There had been acute disease and infective disease epidemiology before that.

AN: And just to say a little bit more about the blood pressure work and the debate between Platt and Pickering, because part of Platt's criticism, as I understand it, of some of your work was the very fact that it was done on the general population. He seems to see that as a critique of what you did. Can you just tell us a little bit more about how he contributed to that debate?

BM: Well he believed that hypertension was inherited according to a single pair of genes, and Pickering believed that hypertension was one end of a continuous distribution, and that those above a level were at increased risk, but that the level of blood pressure was influenced by a whole lot of things, including polygenic inheritance, and other factors as well. And that's really what our general population study showed.

AN: And how did you select the things to study? The blood pressure, etc, how did you choose the things you were going to survey people for?

BM: Well Archie would have done most of the deciding, and he was one who was always talking to his friends, and I would guess got a lot of sort of feedback from his friends, about interesting medical problems.

I was very interested in Caplan's syndrome, that was a fascinating one to study.

AN: Tell me a little bit more about your work on that.

BM: Well, Caplan had described a characteristic type of chest X-ray, which he thought was associated with rheumatoid arthritis in coal miners, and it was really a form of massive fibrosis, but rounded lesions scattered over the lung fields. And we got to select from X-rays of all the miners in the two valleys scheme, and Archie read the whole lot of those, several thousand X-rays, those that he would think would have rheumatoid arthritis, without knowing anything about their joints, and he did that and picked out I think about 20 cases that he said would have rheumatoid arthritis, and I went round visiting that group, a group with ordinary massive fibrosis, a group with simple pneumoconiosis, and a group without pneumoconiosis, I think in the Little Rhondda rather than those valleys. And these 20 as well. And I didn't know which were in which group and just looked at them from the point of view of arthritis, and it came out highly significantly that Archie was right. It was an interesting condition, in as much as you could recognize that a person was liable to get arthritis, even if they hadn't actually begun, but from their chest X-ray, so there were cases that had the characteristic chest picture, several years before they developed any arthritis. It somehow should have led on to something useful. I remember being rather criticized for saying it showed that those with the diathesis for rheumatoid arthritis had a different and characteristic sort of reaction to what was going on, solo reaction, which seemed fair enough, but didn't take you any further really.

AN: And you did some work on isoniazid, did you say.

BM: Yes, that was just to see whether treating chaps with early massive fibrosis with isoniazid cut down the progression rate of the X-ray changes.

AN: And this would be based on the basis that it was related to tuberculosis.

BM: Yes.
AN: So you did a series of surveys, both in the Welsh valleys and elsewhere.

BM: Yes, and then from the blood pressure point of view we wanted a population which was very different. I had done surveys in the valleys and in the Vale of Glamorgan, and we wanted a population that was sending out very different blood pressure levels, and see whether the practice that influenced blood pressure in Welsh mining families applied in other populations. And I can’t remember exactly why we chose to make it, but one reason was they spoke English, or a kind of English, another was that they knew their ages. There was birth registers and death registration from way back in the middle of the last century. Not the last century, but the century before that, so you could with a bit of trouble find out their ages, and so I applied really the same techniques that we had been using in South Wales for a rural population and an urban population in Jamaica, and the pattern of inheritance looked to be very similar. Blood pressure seemed to be influenced in the same sort of way to the same sort of extent, in Jamaicans, as we had found in the Welsh.

AN: So you went to Jamaica in 1960?

BM: I went to Jamaica in 1959 for the first time, just to see what the situation was like, and what would be feasible, and did the first round of the blood pressure surveys then, and then came back in 1961, I think. I was there just a year and a half or something like that, and we decided we would see whether we could apply and get funding for a branch of the Unit out there, and we approached the Wellcome Trust, and the Wellcome produced money for that, and we starting building a unit at the University of the West Indies, where there was already an MRC unit, John Waterlow’s Tropical Metabolism Unit, and they built us a very nice building on the university campus, and a block of four flats and a director’s house and I was there off and on, but coming back to carry on with the Welsh work on blood pressure, until 1971 I think, early 1971, when I came back to England.

AN: So in 1959 the work you did in this year would have been funded by the MRC.

BM: Yes.

AN: And then they continued to pay your salaries and for your team, but the Wellcome Trust built the building.

BM: Yes. Well I think Peter Williams was the head, did you know him? He was the director of the Wellcome Trust at the time and he had been in the MRC and I think they sort of shared out the funding in some amicable way.

AN: In those ten years or so at the West Indies, you did a series of sort of cross-sectional surveys, did you do other work as well?

BM: Yes, we did heart disease work. A number of the teams that had helped us in this country sent out teams to do studies there. For example, Jonas Kellgren who was Professor of [rheumatology] or something at Manchester, sent a team out to do a study of our joint disease, and there was an ophthalmology study done by the Welsh team there, and anaemia studies. Really a lot of the work that had been done in Wales was repeated in Jamaica, to look at racial differences really.

AN: And did you get the same cooperation and response rates that were achieved in the valleys?

BM: Nearly yes. We had to do a lot of sort of foot-slogging epidemiology, walking on the steep cracks in rural Jamaica, very hilly. A very pleasant life in many ways, very interesting.

AN: And what was the contribution of this, repeating in a separate and defined, racially and environmentally different population. How do you think it contributed?
BM: I don’t think it did contribute in any meaningful way. We found on the blood pressure side that the pattern of inheritance was extremely similar, that the factors influencing things like weight and urinary tract infections, that sort of thing, which operated in the same sort of way, and almost more or less to the same extent in Jamaica. So from that point of view in a way it was disappointing.

AN: And then in 1971?

BM: In 1971 I came back. By that time, the Unit had left PRU and had split off and was down in Cardiff.

AN: The formal separation was in 1960, but it certainly by then was based at Richmond Road Cardiff.

BM: Yes.

AN: I think Archie retired in 1974, so it would be towards the end of that time.

BM: I think by the time I came back, Peter Elwood was in charge of the Unit, or operationally in charge anyway. I think Archie still used to use a room there, and I wasn’t terribly keen to go back there and got the opportunity of running the MRC mild hypertension trial.

AN: Can I stop you for a second? So in 1971 did they close the unit in Jamaica?

BM: No, they handed it on to a chap called something [Dr Andrew Rikk] Davi, what was he? I can’t remember his first name now, and he was a schistosomiasis man, and there wasn’t any schistosomiasis in Jamaica, although there was in some of the other islands I think. Anyway he didn’t stay very long, and then Graham Serjeant, who was out working at the University of the West Indies on sickle cell disease, wanted accommodation, and the MRC funded him and the unit changed from being epidemiology to being sickle cell disease, a sickle cell disease unit, and it continued like that until a year or two ago when Graham Serjeant retired.

AN: So was it your choice to come back?

BM: Yes, oh they reached a stage where they needed somebody at home to help them with university applications and things and they were no longer cared for in the way that a school does, a boarding school, so we came back and did the mild hypertension trial, which was a most interesting job.

AN: So you came back primarily because of the family and because you had said only ten years.

BM: Yes, well they reached a stage where they needed somebody at home to help them with university applications and things and they were no longer cared for in the way that a school does, a boarding school, so we came back and did the mild hypertension trial, which was a most interesting job.

AN: So tell me a little bit about that trial. That was based in?

BM: That was all in England and Wales. It was run from Northwick Park [London]. I joined Tom Meade’s unit and the trial was run by a working party, Stan Peart. Do you know of him or not?

AN: I know of the Peart Rose Clinic at St Mary’s.

BM: I didn’t know that, after Geoffrey Rose. He was chairman of the committee and I think it was Colin Dollery, Professor of Medicine at the Hammersmith, he had proposed to the MRC that we didn’t know whether treating mild hypertension did more harm than good, and that we should know. So a group got together and I was in that group before and started it back in England, and we recommended that we should do a
pilot study and design a trial that will give us the answers and the pilot study started in about 1972 I think or possibly 1973 and I was running it with a team from Northwick Park and we decided, we were trying different kinds of clinics, for example screening organizations, what else was there? General practice, industrial practices, anyway by far the most successful appeared to be general practice and we recruited altogether 20 clinics, some from each type of setting, and the general practice came out much the most profitable to follow up. And the calculations based on the experience of the pilot trial was that we would need to screen or to have about 18,000 patients in the trial, which meant recruiting from a lot of different clinics, general practices, and we finished up with slightly less, 17,000 something, and the recruiting of the practices was interesting for me, having never been in general practice. We were really selecting on the basis of interviewing them and their reputation, and it really worked very well indeed.

AN: This would have been one of the first large, one of the most controlled trials ever carried out, would that be right?

BM: Yes, I think it is right, it was certainly the largest, and when people referred to the MRC trial, they knew which trial it was, it was the mild hypertension trial.

AN: What interests me is that there were trials from 1948 onwards, in mice and man, and the increased use of randomization, but there's no way in doing a trial of this size that you are looking for modest benefits, whereas previous clinical trials I suppose would be looking for a stunning benefit.

BM: Involving a lot of people is a big benefit. The results of the trial – we compared propranolol and a thiazide diuretic with placebos – and the result of the trial was really that was as much benefit in terms of stroke, but no measurable benefit in terms of coronary disease.

AN: And how long did that trial run for?

BM: It ran until about 1983 or so. And I was in charge of it until then. I retired in 1983.

AN: And were you doing other things as well as running the trial?

BM: No, no, it was a full-time job, and a very busy job, a very interesting job. Stan Peart made the comment that he thought the demonstration that general practice could produce useful research in that sort of way was more important than finding out whether the treatment is any good to people with mild hypertension, and I used to criticize that comment, but I am not sure that he wasn't right. And that organization, it's called the General Practice Research group.

AN: Or Research Framework maybe.

BM: Research Framework, yes. That's still in existence, it has been largely expanded, but it still functions, Tom Meade is running it.

AN: So the trial in itself, you established as part of the trial GPs who were interested in research, and were subsequently involved in other research.

BM: Yes.

AN: Was it right that Julian Tudor Hart was part of that trial?

BM: No.

AN: I had a feeling that his practice was one of the practices involved, but maybe I am wrong.
BM: He wasn’t involved in the mild hypertension trial, his practice was much too small, we wouldn’t take on a practice that size, because we were putting in a lot of help in terms, we were providing part-time nurses and age-sex registers on computers and that sort of thing. But Julian I think got involved when they were doing other things like hormone replacement therapy and possibly warfarin.

AN: So the practices got quite a lot out of this cooperation in the trial.

BM: Yes, they got a nurse. The idea was that they shouldn’t be out of pocket, because of their contribution, and they got the same sort of remuneration from the MRC that they would get if they were doing a hospital clinic, you could do a sort of three-hour stint in the hospital I think at a fixed rate. And that is about the end of my career, medical career.

AN: And from 1983 did you stop completely, or did you continue?

BM: I wrote a book about the trial, which you may have seen. And that was a very nice way of getting into retirement really, having something sensible to do. And that was published in about 1985 I think, and then we came here in 1987. I must show you that picture up there that Archie gave us, he commissioned us when he was visiting us out in Jamaica at one time, and it was painted by a person who did that, for example, and some of these other ones here, who was John Waterlow’s wife, Angela Waterlow, who was a very good painter, and Archie wanted one of our family, and he insisted that it should be painted according to the methods of the Cubists, where you get a full face and a silhouette at the same time for each person, and you can see they are really jolly good, the shadows being the silhouettes really. And the other thing he insisted on was that Laurence, our paediatrician son, who’s the baby in the middle, that his umbilicus had to be the very centre of the picture, and you know it is actually, with the family all around it, which is very characteristic of Archie’s sense of humour.

AN: Just tell us a bit more about what Archie was like and what singled him out as being so successful. I am quite interested in what was so special about him.

BM: He was a very broadly interested chap medically I think. He had a lot of friends in London that he used to spend time with, and I think some of the ideas he would discuss with them.

AN: So he had a breadth of interest and he certainly kept in touch and discussed what was current in lots of different fields, which I guess many people wouldn’t have done.

BM: But I mean his main ideas, I suppose, were firstly the use of proper populations for chronic disease epidemiology, and then the use of randomized control trials. And those ideas and the ramifications of them were the reason for his success I think, that he was the first really to sort of plug in an obsessional way, things like randomized control trials, and people used to get almost fed up with hearing him mention the words, but he was a very nice chap and very bright.

AN: And looking back on the work that you did, what did you think was the best piece of work you did, or the most important piece of work you did?

BM: The Birmingham mild hypertension trial. That was the most fun, and the most successful I think.

AN: You don’t think your work on blood pressure?

BM: Well I think that was important, yes. I don’t know whether either will go down in posterity, but still.

AN: It’s just interesting to look back and think what am I proudest of?

BM: The mild hypertension trial was really great fun, nice people involved, and a very interesting problem, and it involved meeting a lot of people.
AN: I am just interested in whether you think there were defining characteristics of epidemiology or of epidemiologists, where it has clearly changed considerably. Do you think there were things which singled people out to make them end up as epidemiologists, or was it just a hotchpotch?

BM: I don’t know, I think the best epidemiologists are clinicians, and Geoffrey Rose I think would be my hero in terms of a very thoughtful and very competent clinician, who questioned everything and expected answers from epidemiology.

AN: I was just interested in what were the things that singled them out. I don’t know whether there are.

BM: I think Archie was pretty obsessional, but in a good sense. He was neurotic as well, remarkably so really. Did you hear the story about his surgery for epithelioma on his hand? I think he developed on his right hand, some epitheliomas, and he had been screening for tuberculosis cases in Germany and he attributed these epitheliomas to X-ray exposure. He got himself to a sort of senior registrar in dermatology or something who did a biopsy of one of these, they thought it was malignant. Archie then developed an infection in the one that had been biopsied, he then developed a gland in his axilla, he assumed that they were malignant, got himself to London, and surgery for his glands, had a dissection of his axilla and removal of one of his pectoralis muscles, and then found out that there was infective changes only. But in that sort of way he was very neurotic. But he was a charming chap and very nice to our family and very nice to our kids.

AN: Certainly his charm was part of his success in that he seemed to as you say have this circle of people and he seemed to have been on very good terms with the people at the Medical Research Council and something of his charm was part of being able to persuade to set up units in Jamaica and things.

BM: He was a good mixer Archie, and was at ease with all kinds of different people and all different nationalities, and he was well liked by lots of people that he visited, or who visited him. He got on well with West Indians, and with Africans, an interesting chap.

AN: I was just wondering what else drives people to do epidemiology. Some of the people I have interviewed have been bird watchers, and I wondered whether it was part of the collecting and watching and observation, whether that was an important thing.

BM: I think my interest in epidemiology from a sort of selfish point of view, was I recognized you could get important findings from epidemiology and yet of an order of magnitude quite different from what you would be likely to achieve by bedside research. I think I rather liked the fact that it wasn’t sort of hands-on medicine all the time, although having done my house jobs, and having done a year at PRU, I felt gosh I would like to get back into clinical medicine, but that didn’t last too long.

AN: And it seems to me that there were big changes over the time that you were in epidemiology.

BM: Yes, I think there were. When I went in without any formal qualifications in epidemiology, I was I think quite a good house physician, and did a senior house officer chest diseases job, and that’s about all, clinically.

AN: It seems that at the end of the Second World War the first randomized trial in humans was conducted. The first prospective studies are set up not long after, the British doctors cohorts, and at the same time technology leaps forward in terms of computers and statistics and so on. So the scale of the undertaking and the sort of studies that are being done today are in many ways just so different from what was previously being done.

BM: Yes.
AN: I was just interested in how you saw it evolve. Actually being there.

BM: I think the big difference is epidemiologists now are much more au fait with statistics than I ever was, although I was very happy to have a lot of contact with statisticians and recognized their value and worked closely with them, but never doing the actual sums myself. You know Peter Oldham, who was involved with the hypertension work, was a very good statistician.

AN: Quite involved maths, it must have taken a long time to do the equations and so on, by hand.

BM: Well he had calculating machines, not the sort you get nowadays. But a chap like Geoffrey Rose knew more, and Archie himself didn't really know an awful lot about statistics I think, but you know people like that recognize the importance of getting statistical opinions from an early stage and involving them closely, but not actually doing the work.

AN: Well it’s certainly true that most of us would be expected to do our simple analyses and simple meaning a lot more than probably what was technically feasible back in the fifties.

BM: Yes, it’s a matter of pressing the right button, isn’t it.

AN: And knowing what it means, that's still tricky.

BM: Well knowing what it means, I think you can do without being able to do it yourself.

AN: I agree. But I think that the ease with which one can do it, means that instead of having someone who is dedicated to sitting there doing all the sums, you can do some, and it does mean that you can explore your data a bit more, which I think is good. Having said that, probably people like us are much less involved in the day-to-day doing of the research, than your generation. I mean I think you were doing the surveys in a much more hands-on, taking measurements and things. So what we lose in the sense of knowing the data in that way, we gain from actually looking at it, and looking at the distributions and handling the data on the computer. Did you feel that, having worked in South Wales, where the scale of the studies was . . .

BM: Small, too small.

AN: And then having worked on this huge trial, which was the biggest, were you struck by that as a contrast between the scale of the two?

BM: Yes, I think one criticism you can make of Archie’s work is I think he drew conclusions on inadequate numbers, but often on the basis of a hunch, which was right.

AN: And were you aware at the time, thinking we should be doing bigger studies than this, we should be doing more?

BM: Well of course the blood pressure work was on a much bigger scale, than you know the studies of coronary disease, where he would take a random sample of 100 chaps and hope to draw conclusions from them, which wasn’t realistic.

AN: Another criticism I have heard of the work of the Epidemiology Unit was the sense of not only the size but connected to that was the taking of a series of cross-sectional slices, that perhaps it would have been better to create a large prospective undertaking in the valley. If you could do it all over again, what one might have done is to have surveyed the Rhondda and surrounding areas to a greater population, to 10,000, 15,000 people, and to measure these various things, either on the whole lot or on sub-sets, and to follow them through time. Whereas what tended to happen was that people were, though the Rhondda I know was followed up, that if it had been bigger and set
up and people had been explained that it was prospective, one could have perhaps have had this richer data set.

BM: There were something like 30,000 in the Rhondda valley, the little Rhondda that were used for sampling. Everyone was X-rayed at once, or all over the age of five were X-rayed.

AN: You just wonder whether setting up a cohort, the Rhondda cohort, which sought to capture maybe not all of them, but maybe 10,000, 15,000, 20,000 of what was at that time a fairly static well-defined and cooperative population, explained the endeavour to them. I suppose Julian Tudor Hart would have argued that medical services should come with it, that part of the cooperating would be to try and improve where they could.

BM: Don't forget that the main objective of the two valleys scheme was to see whether cutting down tuberculosis in one valley influenced the attack rate of PMF compared with the control valley, and from that point of view, it was designed from that point of view, and the only reason that the whole population was X-rayed was to try and find cases of tuberculosis, to help answer that particular question, so there wasn’t at the start of it, any intention of it being used for wide-scale epidemiology I think. You know it was after that that we realized how useful it would be.

AN: Yes, I suppose for the first ten years though, it was the effort and under Archie’s direction. It was still under the auspices of PRU and they were still trying to sort out what caused pneumoconiosis, as a unit, the Epidemiology Unit, which was open for 40 years.

BM: Was it?

AN: Well it depends how you count time, if you count from 1950, around 1950 Archie had his own little group within it, which you joined, it was formally constituted in 1960, or around then, and it formally closed in 1999. Now it was essentially being run down over the last few years, so it's about 40 years of epidemiological research. I just wondered what you thought its main contributions were. What did it achieve, in 40 years, what did it do?

BM: Well I think a lot of its effort, and certainly a lot of Archie’s effort in it, was directed to pneumoconiosis and the follow-up studies, which went on until the end more or less were very much pneumoconiosis. Peter Elwood’s work is the other thing. I don’t know as much about it, because I am less interested in anaemia and what he was doing then. But I haven’t any real knowledge, or enough knowledge, of what went on after I left it to be able to comment usefully on that.

AN: I think I have asked you all the questions I wanted to ask. Is there anything I haven’t asked you, or things that we haven't covered or you want to go back to and say a little more about?

BM: I don’t think so. There are some interesting tales about Archie. His interest in pictures was one thing I didn’t mention. When I first went down to be interviewed by Archie he was living in Rhoose Farm House in the Vale of Glamorgan and at that stage he was there with Martin Wright and Peter Oldham. And Archie was a wealthy man, you know, he had inherited a lot of money, and he was buying pictures, but he bought from artists that weren’t well known, or hadn’t fully made their reputations at that stage, and I don’t know how much he was paying, but sort of £100 for a picture, I think not more than that. When he died he left half his money to Joe Stalker, and the other half to Green College Oxford, and pictures formed a problem, and some of the pictures had to be valued I think, and Archie had a Ben Nicholson, which I think was valued at something like £180,000 and he had a J D Fergusson, the Glasgow School, was over £200,000. And you know he was very interested in paintings, and had good taste. He had a Barbara Hepworth sculpture in his garden and when he was ill at the end, he was living with Joe and Maggie Stalker, in Dorset somewhere, and he left his Barbara Hepworth sculpture in his garden at Rhoose, it was there for three or four months as far as I remember, but luckily it survived that alright. He was on the Welsh Arts Council and he was a widely cultured chap really. But you know this business about him having porphyria, I never really saw any evidence
that he was in any way ill as a result of porphyria. Have you read his autobiography? The one he wrote in the BMJ. Now that’s a severe case of porphyria. He has quite a lot of relations in New Zealand apparently and he did a sort of postal survey of them, which I imagine caused a lot of neuroses in New Zealanders. He was a funny chap, he was an odd character, as well as being a brilliant man. [Professor] Herb Briscoe – did you come across his name or not, he was a respiratory physiologist who worked for PRU and then went off to the States – made a comment to me on one occasion that he thought Archie should have had the Nobel Prize for medicine, that that was the one thing that he deserved and didn’t get, and I thought that was a bit over the top at the time, but subsequently he has been sort of posthumously recognized, hasn’t he, in an enormous way.

AN: Well I think Iain Chalmers setting up the Cochrane Collaboration thing and the groups, his name has come through that in there. It’s quite amazing how many people will talk about it, almost the canonization of Archie Cochrane, St Archie, and so on.

BM: Well certainly he has been recognized more since his death, than before it, I think.

AN: I think that’s probably fair. I think people want to know who this person was, whose name is attached to these things, and from that they realize the things that he did.

[END OF TRANSCRIPT]

Further related resources:
