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AUDIO INTERVIEW TRANSCRIPT

Hugh-Jones, Philip: transcript of an audio interview (05-Jul-2000)

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Hugh-Jones, Philip: transcript of an audio interview (05-Jul-2000)*

Biography: Dr Philip Hugh-Jones BA Cantab MB BChir MD MRCP FRCP (1917-2010) was Consulting Physician and Director of the Chest Unit at King's College Hospital, London, and Consulting Physician and part-time Director of the MRC Clinical Pulmonary Physiology Research Unit at Hammersmith Hospital, London, from 1964 to 1967, when it was taken over by the University of London. He had been on the scientific staff of the MRC Pneumoconiosis Unit from 1945 to 1952.

AN: Andy Ness

PHJ: Philip Hugh-Jones

AN: I wonder if you can tell me where and when you were born and a little about your early family life.

PHJ: Well I was born on the 27th of August 1917 in London, during the First World War. I was brought up largely by my mother because my parents were separated, so I didn't see a lot of my father, but I saw him a fair amount. And after that I simply went to Highgate School in North London, first to the prep school and then to the main school. It was quite a big school, I think it was about 700 boys. To my great surprise, I landed up as Head Boy, I can't think why because I had no particular merits, but there it was. And at that stage it was very odd, because they used to require people to do Higher Certificate as it then was, every year, because if you started young they got a lot of kudos for the school and I think I did it about three times, much to my intense regret, not that I failed it, but I never as a result went abroad and learnt languages, which has been a great regret to me. So my knowledge of languages is not as good as they should otherwise have been. I can speak tolerable French, a bit of Spanish, but not much else. And then I went up to Cambridge and got an exhibition and went to King's College, Cambridge, which was converted to a scholarship after I got a first in the Part I of the Tripos. I had intended to do Part II zoology, but because the Second World War was imminent and my tutor told me that if I wanted to do metabolic entomology, or parasitology, which was what I was interested in, I really ought to qualify medically, otherwise I would be under the thumb of the doctors. Instead of doing Part II zoology, I did some anatomy, and then when the War started, whether I liked it or not, I was made to qualify in medicine, and so I drifted into medicine, having firmly said that I would never do it. And so that was my background. And then because I did reasonably well academically at Cambridge, the MRC asked me whether I would do army operational research in the Medical Research Council armoured fighting vehicles unit, and so I spent my war originally as a civilian. Although I had my own tank, and my own driver, I was actually a civilian living in the officers' mess, and that curious arrangement was because, without rank I could then argue with the generals about the design of tanks, whereas had I been a mere lieutenant they wouldn't have taken any notice of me. I was then going into uniform for the Japanese war, in fact actually got everything going, and then the atomic bomb fixed that, so I never went. And so I spent my war, although in the army, as a civilian.

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* Interview conducted by Mr Andy Ness, for the History of Twentieth Century Medicine Research Group, UCL, 05 July 2000. Transcribed by Mrs Jaqui Carter, and edited by Professor Tilli Tansey and Dr Hugh Thomas.

After the War I decided, having got that far in medicine, I thought I ought to do the Membership, the MRCP, and to go back and do some house jobs. I had only done one house job. And I went to Sir Edward Mellanby, who was the secretary of the Medical Research Council and said I would like now to stop doing this sort of research and do some other junior hospital docs jobs and do the Membership examination. Mellanby's reaction was extraordinary. It was as if I had said something obscene. He jumped in the air and banged the desk and said, 'Wasting your time as a good researcher doing these stupid exams, no.' And he opened a drawer and flung medals across the desk and said, 'You will get things like that if you get on and do some research, you just go down to Cardiff, we have got a big problem on there.' I think I just sort of said, 'Yes, sir.' This was how I first met Charles Fletcher and went to the Pneumoconiosis Research Unit [PRU].

AN: Tell me a bit more about what you did in your time in the War and the work you were involved in.

PHJ: Well I was doing a lot of gun fume trials, and that was really rather important, because they had more losses from carbon monoxide poisoning on the Dieppe raid, than they did from the Germans, or so I am led to believe. But that was partly because people started up the tank engines and the ventilation wasn't very good, and then firing off rounds, they got a lot of build up of carbon monoxide, plus ammonia and other substances, and we dealt with the effects of these on the blood and how it affected the accuracy of firing and what the limits were in the blood. I did a lot of work on the position in tanks, because tanks as you probably know had quickly to get onto target and they tended to fire hull down, and not only did they have to manoeuvre the tank quickly, but they also had to get the gun on, because the German Tigers were much better than the British tanks, once the shot went in. If the British could get in a shot first, they tended to work well. We tried to stop the fumes; and they also had a problem with the sights – this edge effect, an obscuration at the cross of the reticle when they were looking at target, and that was very simply solved by making an open cross with a little hole in the middle as it were, and that got rid of that problem. And we also designed the power tracks so that you could move the guns very quickly onto target and the combination of the two was I think helpful in the British getting in shots first before the Germans. But there were all sorts of things like the noise in tanks, and whether they could lift heavy shells. There was a whole question that affected the design of tanks as to whether, because things were getting so large and so big, they would have to split the ammunition and have the shell going in first and then the propellant subsequently and all that was very important, and so we had to determine just how much people would do in a given area. So it was all essentially applied physiology.

AN: And your qualification for doing that was poor.

PHJ: Well I had got a first whatever that means, and because of that I used to demonstrate in the physiology lab at Cambridge, with some very eminent people. Alan Hodgkin, who became the President of the Royal Society, was my colleague, and David Hill, the son of Professor A V Hill, and I suppose I was just lucky and was with very competent people.

AN: And then you went on to the Pneumoconiosis Research Unit.

PHJ: That's right. Charles Fletcher employed John Gilson and me. John Gilson was doing similar sort of work to the work I was doing, but with the RAF, and similarly, I think, as a civilian in the RAF, but doing physiological work, the design of clothing and all sorts of odds and ends. John Gilson was a very brilliant engineer, he was extremely competent with his hands, and he and I were faced with the problem of assessing the breathlessness of the South Wales coalminers. The only lung function test then available was essentially the vital capacity, which had been going since the nineteenth century. I have forgotten when [John] Hutchison published his paper (1846) and it was in general use in medicine. And we set about by trying to do other things. We first of all looked at the possibility of measuring the total volume of gas in the lungs, which at that time the Americans were doing by getting patients to breathe oxygen in and out of their lungs and measuring the rate at which the nitrogen originally in the air in the lungs was washed out. From that

you could calculate what of the volume in the air must have come by the serial dilution of the nitrogen. It was a very cumbersome and tedious method and we decided to use a closed circuit method, where patients re-breathed round the close-circuit spirometer with the carbon dioxide removing cylinder, and oxygen, with a small amount of helium, and we measured the concentration of helium using a catherometer, which measured the cooling ability of the helium, and John Gilson made it himself, which I thought was very impressive. He welded up a little platinum wire and put it into a metal block and we put it into the circuit and we published that separately as a way of measuring the total lung capacity. I don't know whether we were the first people to do that or not, but it was a very early way of doing it, and that has since become a standard method. Then having done that, we then decided that we would like to know how much miners could pant from the point of view of their treatment and so on, and we did the rather cumbersome business, which was the only way we knew of getting them to pant in and out of one of these big Douglas bags, but using a valve just to catch only the expired gas. We measured how much air they got out when they were panting as hard as they could over 15 seconds. We used to say 'go on pant, pant', and everybody was panting and panting. That was tiring for ourselves and for the miners and we then suddenly realized that one breath was very like another and if we measured one breath and multiplied it by a rate factor, we ought to be able to get the right answer. But it never worked well and it was left to a very bright Frenchman, a chap called [Marc] Tiffeneau, who measured the proportion of a person's vital capacity – that they are able to expire in the first second of forced expiration, and it's a fixed proportion, about 75 per cent of the vital capacity – called it the Tiffeneau-Pinelli index. That test has now become universal for the whole of medicine, everywhere in all the countries of the world, which is most extraordinary, though Tiffeneau was the person who had the bright idea and not us.

AN: It's interesting faced with pneumoconiosis, which is a very particular problem, that you were in a sense devising very generally applicable devices.

PHJ: Oh yes, we were. It was rather like when one worked in the army, you went to the textbooks, thinking you could find out how much a man could push on a pedal, and we found that there was no information, we had to find it out. For that very reason John Gilson and I had to find out ways of measuring people and we never thought of a good way of measuring gas transfer into the lungs, until Professor [FJW, Francis] Roughton, who was the grand old man of respiratory physiology, came to visit us. He said, 'oh you want to use carbon monoxide' and that was a very bright idea, because Marie Krogh had done it for measuring the diffusion capacity of the lungs, and we set about doing that. In fact, various people came to see us and thought what a wonderful idea and they published it before us, which was rather irritating, but we were in fact the first to do that, but we certainly weren't the first to publish it. But that too has become a standard method of measuring the gas transfer in the lungs, and so really stemming from that work, the measurement of the total lung volume and its subdivisions, the maximum breathing capacity, and the carbon monoxide transfer, all three methods, all three essential parameters of lung function, were developed at that time and have all been in use subsequently.

We then applied that and published this great volume, which the Medical Research Council did, on lung function in coalminers and with pneumoconiosis. Well here it is, a bit dog-eared, published in what we called the MRC green book. This was a big experiment, relating the different stages of coal worker pneumoconiosis to their physiological affects. And that itself produced other things, which we hadn't realized were of great interest. One was that we soon realized that if you measured something like the vital capacity you were also measuring natural breathing capacity, and these tests, they weren't as it were pure tests, but they inter-reacted. We couldn't think out how to sort this out, but Peter Oldham who was the statistician there produced a very bright model of a factor analysis. This was a sort of solid model of to what extent these tests measured the same thing, and to what extent they were unique, and at that time it was a hell of a sweat because things like computers weren't around, and I remember sitting for hours in front of one of these Friedman electric calculators, doing multiple correlation coefficients, with all the tests on all the subjects. It took me days on end, not just hours. And what's so incredible is at the end of the War I wrote to the Medical Research Council and said that I wanted to buy a Friedman calculator because of the research work I was going to do, and that was considered to be a major requirement. And I had to put in a special grant for it and finally this thing was approved, but now I could go to Woolworths and buy something that would do

better than that for about oh I don't know, £3, easily. It's staggering what has changed in electronics. So that was the sort of basis of it. Apart from that we all, well Charles Fletcher and I, and John Gilson, had charge of the ward for looking after the miners, and although I didn't have my Membership at that time, I was promoted to somebody of clinical competence, and despite Mellanby I did, in fact, take off some time from South Wales and went to Oxford and by incredible good fortune got the Membership first go. I just did a junior registrar job and those are the only two junior jobs I ever had, one was a house job in the War, and the other was this one before I did the Membership, and then I hardly dare say it now, I then became a consultant. But that was just luck I think.

AN: So tell me a bit more about – you did some work about social impact in the UK.

PHJ: Yes. Charles Fletcher and I naturally, because we used to go round all the mines and give talks to miners' welfare associations and so on and so forth, got very concerned with the problems of unemployment and misery in the South Wales miners and we decided to tell them just how important it was to do what the Pneumoconiosis Unit were then recommending, namely, taking out miners once they got a certain stage of simple pneumoconiosis category two, in order to prevent them getting progressive massive fibrosis, which was a killing condition. And we realized that that could cause an awful lot of unemployment, which might even be worse than the disease. But we didn't know how much it was, so we did, with the help of a man called Treasure, who we used to consider to be our little treasure, because he was rather a smart man, he came from, far from being a social worker, the head of some large advertising firm in London, I have forgotten which it was now, but who became a sort of multi-millionaire afterwards. Anyway he was very helpful and he was a highly intelligent man and with his help and with help from the statisticians in London and the various records of disease in the area, we did do this White publication for the MRC on the social consequences of pneumoconiosis, which were considerable.

AN: And you alluded to the two types of pneumoconiosis, which I think were first described by the Unit.

PHJ: Yes, well I, being a clinician at heart, although I was an acting physiologist at this time, was interested in the X-ray stages and we used to have sessions where we all read X-rays and then Archie Cochrane had the bright idea that we weren't very good at it. He would shuffle the films and re-read them, and he showed how bad we were, and then this idea of standard films came in and then we soon realized that you never saw progressive massive fibrosis unless there was a considerable amount of simple pneumoconiosis in the chest X-ray and so the two-disease hypothesis came about. Because it became apparent that simple pneumoconiosis, if you took somebody from the pits, remains static at that stage, and it didn't progress, whereas once they got progressive massive fibrosis it did, however far away from the dust they were, and so the idea was that the dust produced the simple disease and that some sort of infection, which we thought was probably tuberculosis, landed on top of that and caused the massive fibrosis.

AN: You mentioned Professor Cochrane. I wonder if you just wanted to say a little bit about working alongside him, because I think he joined you in 1948.

PHJ: Yes. Well I got to know Archie Cochrane very well, and Archie Cochrane was the most entertaining person, absolutely charming, a confirmed bachelor, meticulous in everything. I mean he never liked a dandelion on the lawn, and he had this rather smart house in Rhoose, because he was a very wealthy man, and it stank of weedkiller if you went near it, but the lawn was meticulous and spotless. And he had this marvellous large Jaguar car and he was great fun was Archie. He used to come with us for some of these overseas physiological meetings, he spoke good French, and was very helpful there, and I remember once we drove down to Madrid in Archie's car, with Archie, and the car was an absolute shambles, because although he was meticulous about a lot of things, he couldn't care less about his personal appearance and about money or anything, there were old shirts and pound notes all over the back of the car, and when we got there we went to the conference and then we were going to drive back with Martin Wright who had just joined the unit. Martin Wright, being a sailor, was absolutely the opposite, and instead of having a car that was a shambles completely, it had to be shipshape. Everything was thrown out and it was all cleaned and we drove

back. And the other thing about Martin Wright, he said we are not wasting our time stopping, you can pee at a certain time, and you needn't do so for another three hours or something, and we then discovered that he was so keen on windmills, that if any of us wanted to go to the loo, we would say 'Martin, Martin, there's a wonderful windmill there', and he would immediately halt on the road and everybody rushed out. So they were great characters actually. Martin Wright and Archie were very splendid people.

AN: And what do you think Archie contributed to the PRU work?

PHJ: Not to our work particularly, to his own work, and as I said earlier I couldn't remember his assistant, I think he was called Ted Roach, is that right?

AN: I think it was. He had Fred Moore as well, didn't he?

PHJ: Yes. Archie contributed by setting an example. I mean he, as I say, was meticulous in what he did and I wasn't concerned with the epidemiology at all, apart from the use of the X-rays and so on, I was entirely concerned with the clinical effects and the physiological effects, so I can't really help you over that.

AN: Did you ever, I mean Archie made a big thing of doing representative surveys, I was just wondering how much you studied samples he collected in his surveys?

PHJ: He never studied it physiologically. It was of interest. Well, Peter Oldham was another big factor, both with Archie and with us, as a statistician, he was very good indeed, and a lot of the design of the experiments we would discuss with Peter Oldham before we started. So they were all designed so that the results were likely to be of use at the end. And what I think we did have was a remarkably good team. Charles Fletcher, I don't know what Charles himself did, what he did do was to keep everybody keen, [he] was a wonderful PR man with the miners. I mean he couldn't have been better, and he was very concerned, as his subsequent work with relationships between doctors and patients and informing patients, was a very human sort of person, and very bright and very competent. I don't think Charles was a scientist as such, I don't think I ever think of him as that, but he was a very surprising person because, as you know, he had been up at Eton and he was a Cambridge rowing blue at Trinity and he was to be the great consultant, and the last person one would think would have gone down to South Wales and packed up all that and taken on this job, but he did with superb skill and created a very, very good team. He was a very nice person, he had got very good people working around him. Everybody had a great loyalty to Charles.

AN: What was it like going to work there? From the descriptions of the people who worked there, this would be seen as the back of beyond to go to this place.

PHJ: Well it was in a way, yes, but it became very successful I think, so one didn't particularly mind. I got fed up with it, because I couldn't stand the rain in South Wales, and although I liked the countryside, I mean I used to collect fossils under the cliffs near Barry Island, where you can see the evolution of various things like lichen and a whole lot of other things, which interested my zoological and past geological interest, but I really did get very sort of fed up with the rain and the dreariness of it all. And then when I saw an advertisement in the *BMJ* [*British Medical Journal*], they wanted a senior lecturer in medicine in the University of the West Indies in Jamaica, I thought what a wonderful thing that would be. I didn't really know where Jamaica was I don't think at that time, but anyway I applied for it and to my absolute amazement got the job, because as I say I had only done two house jobs and nothing else, and went off to Jamaica. And I remember Professor Witts at Oxford looking at me and asking, 'Is it wise to leave the Medical Research Council and go off to some place like Jamaica?' And I said, 'Well, it probably isn't, but it will be great fun.' And so that's really [how] I got back into clinical medicine.

AN: When did you leave the MRC unit to go to Jamaica?

PHJ: I will have to tell you my knowledge of dates is getting so bad I can't remember. I can tell you in one minute. I was in the Pneumoconiosis Research Unit from 1945 to 1952, taking off six months at Oxford in 1950 to

do the Membership examination, then I went to Jamaica from 1952 to 1955, and then to the Postgraduate School at Hammersmith from 1955 to 1960. And what happened there was while I was down in Cardiff we were doing all this problem with gas analysis and we were visited by Purcell, the great Edward Purcell from the States, who got the Nobel Prize and so on, and we talked to him about various ways of gas analysis, because John Gilson and I were then using a parametric optical meter, which wasn't very satisfactory, and we all came to the conclusion that the ideal thing would be to use a mass spectrometer, if it were only designed for the purpose. Mass spectrometry, apart from separation of isotopes, had been used in the oil companies to detect different fractions of oil, but even then the resolutions were very, very tight. And what was needed was a much lower resolution, which would jump from one mass long wave to the next, and to try and get all the gases through a respiratory mass spectrometer. And I spent some time, and I wrote to the MRC about all this, saying I thought it was a good idea and outlining what the requirements were and to my great surprise they employed a very bright physicist from Australia, a chap called Kemp Fowler, who at Hammersmith with their wonderful workshops, not only designed but made the first respiratory mass spectrometer, and when I was in Jamaica this was made. I remember Professor Sir John McMichael rang me up on the telephone in Jamaica and said that this machine was now working, but they had nobody to come and use it physiologically and would I come back and do so. And I thought about this long and hard, because I liked Jamaica and I then said well I will if you give me a consultant job and he merely said 'not bloody likely'. But after a long toing and froing they finally said they would, and I came back to England on that account and having got back I felt suicidal, because I missed the sun of Jamaica and I found being a consultant in the Royal Postgraduate Medical School on the strength of two house jobs, really quite a strain. I mean I was doing postgraduate teaching and I felt a lot of the students knew a good deal more than I did, and all the things one did, or many of the things, tended to be published in the *BMJ*, so I found Hammersmith initially quite a strain, but then I thoroughly enjoyed it.

AN: Going back to your time in Jamaica, tell me about the three years you spent there.

PHJ: Well I went out there because Eric Cruickshank, who was the professor – I was senior lecturer – was ill temporarily, and so I was largely responsible for opening the new University College Hospital. When I first went there the building wasn't quite finished, so I spent a very happy two or three months really getting to know Jamaica and all the parts of the island, which was fascinating, and then I rushed around as if I were in London and then I soon realized that there were gorgeous girls on the beaches and the rum was only six shillings a bottle, so I settled for the quieter life. But once the hospital got going, I was in fact working very hard and I then intended to do a lot of work on respiratory work and in fact came back from Jamaica to London to finish that MRC book with John Gilson for a short time. Well what happened then, because I was often responsible for the hospital when Eric Cruickshank was away and I was the other consultant, and I had all the teaching to do, and the clinical medicine was obviously different from London and I had to sort all that out, I really was working hard, but thoroughly enjoyed it. I found that teaching was great fun and then decided not to do anything on respiratory work, because it clearly wasn't of much interest in Jamaica and I, in fact, started the first diabetic clinic there and did work on diabetes and published a paper on the particular type of diabetes which occurs in Jamaica and that's still sort of standard practice in diabetes. So that was all very entertaining, and it was cut short by my coming back to Hammersmith.

AN: And you were at Hammersmith for five years.

PHJ: That's right yes, and then again I always get rather restless, I suppose I felt that I was doing so much physics and mathematics, that I am not particularly good at and don't really enjoy, and just really wanted to do more ordinary clinical medicine and undergraduate teaching. And the MRC very kindly said they would move my unit – I was a director of their unit at Hammersmith – to King's and they built a building for me and I moved to King's, which was where I landed up.

AN: In your five years you were basically working on physiology.

PHJ: Yes, again I landed at King's just as in South Wales, although I was a director of the unit there, I had got very good staff, I mean I had nine people who became professors of medicine in different parts of the

world, from within the unit, which is incredible, but we had a cyclotron which was near. One was able to sort of browse through the periodic paper and wonder what particular cyclotron isotopes might be of interest. John West who was an extremely bright researcher, and who is now the professor of medicine, or physiological medicine, in the States, in San Diego, and I worked with a mass spectrometer, we did a lot of work on its use down a bronchoscope to measure the gas and blood flow in different lobes and segments of the lung, and then at the same time we used oxygen 15 with the cyclotron, which was 2 1/2 minutes prepared by the deuteron bombardment of nitrogen, which being a cyclotron diffused isotope is neutron-rich in the nucleus and shoots out positrons, which annihilate a electron and produce a gamma ray. So with a gamma counter, or we used Geiger counters at the time, you could measure the radiation through the chest wall, and so by inhaling a breath of air with a few molecules of oxygen 15 in it, you could determine where the gas went to different parts of the lung, and then as you held your breath it was swept out of the counting field by the blood flow, so you could measure the blood flow, which was very exciting. The only thing was not many people had a spare cyclotron and so it wasn't a very wide application. But we then used CO15 too and that was interesting and other things, and then, of course, it was superseded in general use by the advent of Xenon as a way of measuring the gas in the lungs, and then people introduced these other tricks for measuring the blood flow, injecting labelled albumin and that sort of thing into the blood stream.

AN: And you moved to King's with the spectrometer.

PHJ: Yes, except that because King's was very dilatory about it all, and weren't particularly interested in research, a large number of the team left. Leonard Strang, who was a very bright person working on paediatric work, got a chair of paediatrics at UCH [University College Hospital], he left and oh a lot of people. I was down to practically no-one by the time the unit was built, but I managed then to get some more help from different people, and then we did go on and yes we did a lot of work on emphysema, operating on emphysema, and did a lot of work on early use of fibre-optic bronchoscopy and also, finally just before I retired, I was doing a lot of work on laser treatment of lung cancer, which I published.

AN: So when did you retire?

PHJ: Oh I was about 65 and I am now 83, so you can work it out from there. I have been working very hard since then, and I still am actually, because when I was at Hammersmith, we did a lot of work on the effects of asbestos on the lungs, and I became in demand as an expert witness at all these asbestos trials and that and occupational asthma keeps me fairly busy.

AN: And the MRC PRU became interested in that and asthma disease.

PHJ: Indeed John Gilson did a lot on asbestos after I left, but we did it in Hammersmith, on the physiological aspects of asbestos in the lungs and so on and I published some papers on it. I don't know, I read up a lot about it since. And at the moment I am now doing less, I still see one or two medico-legal patients a week, that's all. I am doing a lot of painting, for this exhibition.

AN: Yes, it's a surprise your painting. Tell me just a bit more about the time that you spent at King's.

PHJ: Yes I was just a consultant physician and director of this unit. Well, I was in charge of the asthma clinic at King's, which was interesting. I enjoyed undergraduate teaching. I used to do, it's extraordinary, because I read zoology originally, I always, in those days we had the students for much longer than they do now, and if they were on the firm for six months, I would do the ward round at the London Zoo with them, which was very popular, because I knew the vet, a man called Martin Hime, I wrote a paper, two papers, with him, one on the gas flow in the giraffe's trachea. We took the mass spectrometer to the zoo and measured up the giraffes and that was very interesting. And the students got quite a lot out of it, I mean the bright ones, the stupid ones just thought it was a day at the zoo the others did. It was very interesting for instance at the zoo hospital we were able to look at the eye fundi, all the different animals, which were anaesthetized, and there are not many people who have a chance of looking at an elephant's eye fundus or a tiger's eye fundus, it was very, very educational to me. And also Peter Armstrong, who is now the Professor of Radiology at Bart's,

who I think is a particularly bright radiologist, was very good on the fundamental principles of radiology and he and I used to go to the zoo in the early morning once a week to read a lot of their X-rays for them, which was great fun.

AN: Really how interesting, we never had anything like that. You seem to have had an on-going relationship with the MRC, which is interesting.

PHJ: It was just that they provided cash that was the thing, I don't think they do it now.

AN: I just wondered how it came about in a sense and I am just interested in that MRC somehow took you under its wing almost from the way you describe it.

PHJ: I suppose they did. Well when they sent me down to South Wales and I suppose I don't know, I published about 150 scientific papers whilst we there.

AN: I am just interested in the way they almost picked you out from day one.

PHJ: Well it was always the MRC's proper policy in general to support people, it always had been in the past, and in particular I didn't think I got much, I was just a clinician, but Mellanby was entirely interested in supporting scientists, and even Harry Himsworth, who was clinically orientated. I mean they put all their money into things like Crick and Watson and all these people, who really made advances, but I think Mellanby was right, his argument was always that the advances of medicine come through the scientists, not through the doctors.

AN: There were epidemiologists there.

PHJ: Well I think that's true, but most of medicine, the epidemiology or physiology, is applied work isn't it? I mean it is important, I am not underestimating its importance, but I think the really big changes and advances in medicine have come from basic scientists.

AN: By the time you were, just after you joined PRU, there was a streptomycin trial, the first trial in human subjects, which I guess had a huge impact on the way that medicine is practised or the evidence on which we shape practice.

PHJ: Oh I think that's true, I wouldn't underestimate the application of these things certainly, but the actual production, the production of streptomycin made a big difference. You still had to know how to use it of course. Oh I think that is dramatic. I remember as a medical student going into the paediatrics ward and it was a terrifying experience because children used to come in with headaches and we would do a lumbar puncture and find a typical case of tuberculous meningitis and you could have almost said to their parents, one didn't, but they will be dead in three weeks and you just watched them dying, getting one nerve palsy after another.

AN: Looking back now at the Pneumoconiosis Research Unit, what would you say were its major contributions?

PHJ: I did think that out when I wrote this. Well I mean I think its major contribution was dealing with the problem it set out to do, mainly this elimination of coal pneumoconiosis and I think the combinations of realizing the two-disease hypothesis and the fact that coal dust was harmful and when and why it was harmful coupled with methods of dust suppression, compared to the 20-pit Scheme, largely removed coal pneumoconiosis. I think it has been dramatic and successful. Epidemiology, I thought that was a major result. Lung pictures, standard radiology is important, I don't know what else. I don't know enough about the sort of pathology and engineering side of it all. I mean Heinz Wolff the other day remarked that he was just a lab technician and John Gilson was responsible for realizing how bright he was and where the MRC could give him further training, and then without a degree they gave him a research unit, and he became the

director and the great professor Wolff of television. But I think that was largely a phenomenon of the last war. I certainly remember that during the last war if you were good and had some good ideas, it didn't matter whether you were an FRS or a lab boy, if it was worth having, you were in. I mean it was very refreshing that, and I think that happened in the United States. I went as visiting professor to Stanford some time ago, well many years ago now, but I was terribly impressed there by how competent the students were. Of course, it is acknowledged as the best university in the United States and they ought to be pretty good, but there was no nonsense about it, I mean just because I was a visiting professor they didn't kowtow to me in any way. They questioned what you said, in a very polite way, but they were quite definite about it all, in marked contrast to the Continent where the great man is the great man, however good he is.

AN: And you think that at the time of the War there was a sort of a melting pot in terms of science and ideas.

PHJ: I think two things happened in the War. One is that, and the other is it was the first time that there was a great integration in science and I am sure that in medicine it became an integrated force, that the advances couldn't have been made if physicists, biochemists, hadn't been introduced into medical sciences. I think that thing is now going on to the molecular phase and so on.

AN: After that period of sort of inclusiveness in the War, do you think that continued after the War, or do you think in a sense Britain returned to its old ways in terms of science?

PHJ: Well I felt very optimistic about this country. Now I have become very pessimistic about medical research. I think we are so exam-orientated and so concerned that nobody must ever make a mistake and so on. Accidents do happen and I look with horror at the sort of bad press the doctors are now getting, that Dr Shipman murdered 50 patients, it is nothing to do with medicine, it's to do with the police and the social people and I feel very strong about that sort of thing. I myself had some irregularity with my heart and I went over to King's just as an ordinary patient and sat in the queue there and everyone was moaning about how they had been kept waiting, and I said to my neighbour why do you think that is, what do you think is causing it, and he said, 'it's them damn doctors, there they all are drinking coffee, absolutely irrelevant', because they had read that some gynaecologist had behaved badly or some doctor with some patients. And now I look with extreme concern about the future, certainly of clinical research, I don't know about scientific medical research, because it is very badly paid. I mean when I was young the acme of success was to be a professor of medicine, I wouldn't advise anybody to go into academic medicine these days, which is very, very sad.

AN: And they are having difficulty in recruiting. But you think that the advances in the molecular lab, the MRC are talking about post-genomic research, this is the brave new world opening up.

PHJ: Well I don't know, I don't know enough about it. I know perfectly well that if I go to the Association of Physicians, which is an unashamedly elitist set up, it's far more difficult to get into that than it is to become a Fellow of the Royal College of Physicians, there's no question about that, but the fact is that although the paper's actually first class I never understand more than one in ten. To me it is a different world but then I haven't tried to catch up with it and I don't think I could, so I am not in a position to say how well it is dealt with. I think it is a very exciting world.

AN: And looking back over essentially fifty years, what are the things about the way physiology is done, that transformed clinical practice for over fifty years, what difference have physiologists made?

PHJ: Certainly when I first went to Hammersmith everybody knew all about electrocardiograms, the liver function tests and all the rest of it, but they had no conception of the lung as a working entity. They knew about the X-rays, and that was in fact very important because although the surgery of tuberculosis had largely died out by then, nevertheless you could always cure lung cancer by taking out lots of lung, but the question is whether the patient is going to be alive when you have done it. The assessment of patients physiologically, I think became very important, and it certainly became very important in drug trials and in

particular in asthma. I mean the following of asthma, or physiological following from the point of view of the airflow and breathlessness, even down to simple tests like the peak-expiratory flow which Bonnie and Wright developed. So I think respiratory physiology has made a huge difference, but I think that has now largely come to an end, I don't think there is much sort of future in it. I mean it is working well and that's that. I mean there is a huge following in it, because people go on doing these things. I can't see anything likely more to come out of it, I think it is worn out as a subject.

AN: In a sense a subject has its time.

PHJ: A time when it is developed. I think respiratory physiology is now largely founded, and now what people will go onto is the lung as a metabolic organ, or the immune effects and so on. There's a huge amount to do in that. And the genetic facts. What is so fascinating is something like cystic fibrosis, although I believe the chromosome chains have been known for a number of years, nobody has yet stopped cystic fibrosis. So I think there is going to be a huge amount now that the genome has been worked out to apply. I don't think it will all come suddenly.

AN: I suppose the other challenge facing respiratory physicians in the sense that they are old diseases, tuberculosis, pneumoconiosis have declined, but the increase in the incidence prevalence of asthma.

PHJ: Well asthma has increased, but I am not so sure that tuberculosis has declined. I think through misuse of drugs tuberculosis is making quite a satisfactory comeback from this point of view. And then malaria of course, which the World Health Organization hoped to eradicate rather like they did smallpox, has been winning hands down. Whether it will go on winning is a different matter, because now there is a suggestion of breeding mosquitoes which don't carry malaria, and they are sort of interbred with ones that do, quite apart from all the possible effects on the *Plasmodium*. So I think malaria, and even for a vaccine for malaria, there are all sorts of ways that it may be tackled, but it hasn't been yet.

AN: I would just like to ask whether you think there are things about to be done on asthma?

PHJ: Oh a terrific lot, yes, understanding asthma is OK. I think there are certain potential advances, like the discovery of leukotrienes, and all their importance and so on. I think the pharmacology, I think there are a lot of fields of pharmacology in asthma certainly and in psychopharmacology, which is terribly exciting.

AN: Well it has been very interesting, thank you.

[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.
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5. 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.
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8. 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.
9. Ness A R (intvr); Tansey E M, Thomas H (eds) (2017) *Sweetnam, Peter*: *transcript of an audio interview (31-May-2000).* History of Modern Biomedicine Interviews (Digital Collection), item e2017052. London: Queen Mary University of London.
10. Ness A R (intvr); Tansey E M, Thomas H (eds) (2017) *Tudor Hart, Julian & Thomas, Mary: transcript of an audio interview (14-Jun-2000).* History of Modern Biomedicine Interviews (Digital Collection), item e2017053. London: Queen Mary University of London.
11. Ness A R (intvr); Tansey E M, Thomas H (eds) (2017) *Waters, Estlin: transcript of an audio interview (14-Jul-2000).* History of Modern Biomedicine Interviews (Digital Collection), item e2017054. London: Queen Mary University of London.
12. Ness A R (intvr); Tansey E M, Thomas H (eds) (2017) *Yarnell, John*: *transcript of an audio interview (18-Apr-2000).* History of Modern Biomedicine Interviews (Digital Collection), item e2017055. London: Queen Mary University of London.