

AUDIO INTERVIEW TRANSCRIPT

## Bakhle, Mick: transcript of an audio interview (10-Aug-2016)

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## Bakhle, Mick: transcript of an audio interview (10-Aug-2016)\*

**Biography:** Dr Yeshwant Shriharsh (Mick) Bakhle DPhil DSc (b. 1936) read chemistry, took chemical pharmacology as a supplementary subject, and went on to do his DPhil in the Department of Pharmacology at Oxford; in 1993 he received a DSc. After two post-doctoral years as a Fulbright Fellow at Yale, he joined the Department of Pharmacology at the Royal College of Surgeons in London in 1965, working with John Vane and was appointed Reader in Biochemical Pharmacology in 1980. After nearly 30 years at the Royal College of Surgeons, he moved to the National Heart and Lung Institute at Imperial College, where he is a Senior Research Fellow. For five years (2001-2006), he was a Senior Editor of the *British Journal of Pharmacology*, and became Press Editor in 2006.

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**TT:** Tilli Tansey

MB: Mick Bakhle

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**TT:** I'd like to ask you how you became a pharmacologist. How did you become interested in science as a boy? Was your family involved in science or medicine?

MB: My father and my grandfather were both army surgeons, so that was, in a way I suppose, how it started. But I think it was the second form at school, a crazy time, I think, to make decisions, but that was the way it was in those days. You had to make a decision as to whether to go on the science side or to do Latin and Greek. And there was indeed a question, because my Dad did medicine at Guy's, my uncle did mechanical engineering somewhere in London, and my youngest uncle went to Corpus to read Greats (Latin and Greek and philosophy) and went into the Indian Civil Service, the ICS. So they said, 'Are you going to be like your uncle and do Greats, so it means you'll be doing Classics? Or are you going to... *etc., etc.*?' I for some reason decided to go into science, and then they said, 'Are you going to do medicine?' And in a sort of a fit of reaction against what was expected of me - because I was expected to be the third generation doctor - I said, 'No, I don't want to do doctoring.' Now, it's all a bit odd that, having decided that, the only option really then available was to do chemistry, I have actually spent most of my time almost getting back into medicine [laughs]. Even in my undergraduate years, the last year I took a special subject, in chemical pharmacology really, because by that time I thought, 'I can't see the point of chemistry.' I couldn't see the point of pure chemistry, which of course is not a very educated attitude, but at that point I couldn't see it. And so I thought, 'This chemical pharmacology ties chemistry into an outcome which you can see.'

**TT:** So it sounds almost as if you became a scientist by accident?

MB: Yes, I certainly wasn't driven. I don't think I've ever been driven, which has been a failing for me as a scientist. I think one does need to be driven in some way or another. I think you get driven by ideas, a project gets hold of you and you sort of eat, sleep and drink the project. But I was not a dedicated scientist, I didn't have an urge to find out how things were. Well, I can't remember ever feeling like that, is all I can say.

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\* Interview conducted by Professor Tilli Tansey, for the History of Modern Biomedicine Research Group, 10 August 2016, in the School of History, Queen Mary University of London. Transcribed by Mrs Debra Gee, and edited by Professor Tilli Tansey and Dr Apostolos Zarros.

**TT: Where were you at school?**

MB: Dulwich. Now that's where my Dad had been and his two brothers. It was very useful and odd. All through my career I've been lucky in the sense that, I'm pretty sure I've benefitted enormously from the Old Boy network or having what I call "uncles" to see me through, because when I came to this country, I could read and write, but I couldn't do much else. And I had to sit the entrance exam for Dulwich.

**TT: Aged 13?**

MB: Aged 12. I didn't understand what the questions were about. The English questions about language and reading, that was OK, but there were these mathematical questions. For instance, they had series and asked, 'What's the next number in the series?' I had absolutely no idea what the next number in the series was. I could add and subtract and divide, but that was about all I could do. And, but despite that [laughs], they gave me a place. I suspect they did it, as an Old Boy thing, you see; 'This was the son of an Old Boy and the three brothers didn't do too badly, so...'

**TT: You said you came to this country, could you just say a little bit about your family background to put this in context?**

MB: It started with my grandpa, because he came to France to look after the Indian soldiers in the First World War. And, for reasons best known to him, he decided that that would be a good opportunity - he must have had a senior officer, an English senior officer who convinced him of this - to put his wife and his three sons into South London, and put his three sons through Dulwich. So my grandma, who had never been anywhere essentially, suddenly found herself in South London with three sons, while her husband was in France. And so they went to school and she managed. Then I think it was about 1917 or something like that, halfway through the War, they decided they weren't going to have any Indian soldiers on the Western Front anymore. I think they were losing a lot just through cold. And they essentially transferred the Indian soldiers to the Middle East, and my grandpa went back to India. So the three boys went through Dulwich, and then one went to Guy's, so on and so forth, and then they all went back home.

**TT: Home being...?**

MB: Home being, in India, wherever you are. So for instance my dad, also being in the army, it was wherever you were stationed. And it just happened that most of the stations that we had in my time, were in Northern India. He would have gone back, this was 1927/28 something like that. My brother, the eldest of my siblings was born in 1929, and so I think they got married probably in 1928.

**TT: And they moved around a lot?**

MB: Oh yes. But then you were posted here and posted there, and certainly before 1939 there was this semi-tradition that every season there would be an expedition, an Expeditionary Force into the Northwest Frontier to biff one of the tribes. You worked your way down and then you worked your way up again, and every year somebody else was biffed. And that's what he used to do. He, and the family, would spend the non-active service in a cantonment in the United Provinces or Punjab. And then, when the biffing time came around, the Expeditionary Force, he would go off to Razmak or wherever the base camp was for that year, but we - my mother, my brother, then my sister, and then me - we stayed in the cantonment. He, and us, were posted to various places, but as I said mostly in Punjab and the UP (United Provinces). In fact, my grandpa had a house in Lahore to which he retired. And so after, on Partition, in 1947, all that my grandpa had just went, and that caused quite a change in everything, because he and his wife had to live somewhere. And they had to go and live with my uncle; they couldn't live with the eldest son, my dad, because he was never anywhere for long, and he just had married quarters. He didn't have a house which was big enough for two families. So my grandpa went and lived with my uncle, who had a much more stable job in Poona. And he lived there with them after Partition, till he died.

In 1948, just after Partition, there were other moves. My mother was English. She had met my dad somewhere in the East End, during his time at Guy's, because she was working as a sort of charity worker and she used to talk about Toynbee Hall and things like that. She once said that she met Mrs Attlee, who was also one of the charity workers at the Toynbee Hall, and she said, 'Met Clem, what a terribly boring chap!' [laughs]. I think that must be true, because everybody said that about Clement Attlee - that he was terribly boring. Anyway, so my mother and dad met here in London, my dad went home and then some time later - I'm not sure exactly how and why - my mum went out and they got married. So in 1948 - obviously they had discussed this - she said, 'Right, this place is going to be in considerable upheaval, but I want the kids to go to England and have a proper education,' because we'd been to army schools here and there. We could read and write, but we couldn't do anything else. My brother had been to a good boarding school in India. In fact, now it's a sort of Eton called "The Doon School". And it actually has a Mafia sort of hold on the upper echelons of Indian society. Anyway, he'd been to The Doon School and he'd finished at the Doon School, and in 1948 he was 18/19. And he wanted to do architecture. The person who really missed out on this was my sister, who was in the middle, and she never really got a proper education even when she came here, because it was really quite difficult to find a place for her, in 1948.

I was lucky. I went in at the bottom of Dulwich, and I got myself all ironed out there. And my brother went to architectural school, the AA [Architectural Association] School of Architecture, but my sister had a difficult time trying to get educated beyond reading and writing.

**TT: You arrived aged 12 from an Indian childhood, after or in the midst of the turmoil of Partition which disrupted millions, and you arrived in Dulwich? That must have been quite a culture shock, and emotional shock, and every kind of shock? Or did you just adapt, were you aware of it?**

MB: I don't know what it is, I never felt like that. I knew there was, for instance, turmoil out there. During Partition, my father was stationed in a place called Ferozepur, which is right on the border. And for two months, or at least a month beforehand, the road outside the house was full of bullock carts and other sorts of vehicles, of people moving either into Pakistan or out of Pakistan. All the time people were going in all states of disarray. They weren't being attacked right then, because it was a cantonment and there was order, but obviously they weren't very happy as they were trying to go somewhere. Where? Who knows. And then I came here and the strongest reaction - emotional reaction - that I can remember when I came here was that we landed off a troop ship in Southampton, and it was May, and it was raining, and I could not understand how it could rain in May.

**TT: That wasn't monsoon season! [Laughs].**

MB: I just couldn't understand that. And we all got terrible colds and it was, that was the biggest shock. The other thing that happened of course, although I've never thought about it, was that in India we had a servant, he was called a "bearer", who had been essentially always there. He started with my father as my father's dressing boy, you had to have a dressing boy to get you into uniform for the Mess Dinners, and he just stayed with us and he really brought us up. And when we came here, we had nothing, no nothing. My mother for instance hadn't done any cooking, she hadn't essentially kept house for all the time we were in India, 20 years, and it must have been quite difficult for her, as her only close relation, her sister, had died the year before. And so there really was nobody here that was family. There were some cousins, but they were a little bit distant in all sorts of ways. Of course she hadn't seen them for a long, quite a long time. So, I think, she had a hard time.

**TT: It sounds like you have two quite remarkable women in your background. Your grandmother being put in South London in the First World War, and then your mother coming back after 20 years in India.**

MB: The one thing I can tell you about my mother was the fact that she was an English woman married to an Indian in an army environment, and it was very structured. The hospitals - you had an Indian Military

Hospital (IMH) and you had a British Military Hospital (BMH), so although you would have staff either Indian or British in either hospital, the CO (Commanding Officer) of the hospitals, of the BMH had to be a Brit, had to be a white man. And many of the COs of the IMHs were British as well, but obviously an Indian could be, as my dad was in his time. But I just don't know how my mother handled that. She was always an oddball, I think, because she didn't, as far as I know, have any paid job before she was married. I don't know how she stayed alive as an adult, I don't know what she did, because her father, my Grandpa Chapman, must have been dead by the time she was an adult, and so she was accustomed to doing things which were different. Certainly, the whole idea of getting married to an Indian even in the late 1920s was a fairly different thing. And the other thing which is very important, but very snobbish and not politically correct, is the fact that very often when Indian men married English women, they were what my mother called "barmaids" [laughs]. People of no breeding, as she would say. Now my mother, whatever she was, she wasn't like that, and she was very conscious of the fact that there were ways to behave and things to do and what to say, and she was correct. Even though she was differently correct, she certainly believed a lot in us being brought up properly.

And so she turns up in this highly structured society of army life in India with a certain degree of colour separation, and expects and behaves as she would in England. And I have a suspicion there were many Army officers who she would have mixed with, British Army officers who didn't quite know what to make of her. And I think she must have had really quite a hard time. The other thing was our family never pretended to be English, because we are strictly speaking Anglo-Indians. The ethnic body of Anglo-Indians in India, most of them consisted of Indian mothers and English fathers. And most of them adopted as a lifestyle, English ways straightaway. They used to talk about England as home, and they would adopt English habits almost as a default style. Now our family never did that. I think to a great extent because my mother and my (paternal) grandpa really hit it off, much, I think, to the annoyance of all the siblings and their wives. She was the favourite daughter-in-law as far as he was concerned, and he wasn't very subtle, I think, about showing his preference. And it was very unfair, because the other two wives had actually been selected by my grandma, who although she was 4ft 10, and my grandfather was just about 6ft, she was a tough old biddy and extremely authoritarian and determined. So she, as was the tradition, determined the wives of her two younger sons and somehow, I don't know how, my dad said, 'No, I'm going to marry this woman from England.' And so that in itself was quite a thing to do. But we always said that we are Indians with an English mother, but our lifestyle was such, we didn't refer to England as home, we didn't think of England as home, we thought of India as home. And in that way, we had a non-standard home ethos, lifestyle, all these sorts of things.

**TT: So it must have been quite a shock when you then came to England?**

MB: Oh yes, it was different, but as I say, I was very unthinking. I don't think that ever came out. People have said, 'Oh, did you feel you were being discriminated against?', because I was clearly a different colour, there's no doubt about that. When I was younger I was much darker skinned, and still I'm told people can hear an Indian accent in my speaking. It's worse when I've been drinking, I'm sure [laughs]. So, for myself, I was in this new school, and it was all new, and I don't think I had the wit or, I had no appreciation of the difficulties I might have been in, or perhaps I was in.

**TT: But you didn't recognise them?**

MB: I didn't recognise them. And that's something I've never really had: a good appreciation of sort of social things like culture shock, things like that. I don't recognise that as happening in my life. It must have happened.

**TT: Perhaps the lack of recognition of it has enabled you to move ahead in lots of ways, where lots of people might collapse?**

MB: No, I don't think so.

**TT: You had resilience?**

MB: Yes, but I think it's a lack of introspection in a way. I don't think I have that inward analysis, which again, it's the way I am.

**TT: So home was London, or was your father moving around?**

MB: Well, what happened was in 1948, as you might imagine, it was almost impossible to find accommodation. And we had a very curious life, because we landed in May and I went to school in September. And we were in all sorts of peculiar places. Do you want to hear about them? I think my mother at some stage must have become sort of mixed up with the Quakers or had a Quaker friend or something like that. Anyway, I think it's probably through the Quakers, but we were in the Polygon Hotel - it's in Southampton - for some time. It must have been horrendously expensive, and we then moved out and we went to a place, an ex-stately home called Netley House, halfway between Shere and Gomshall, near Guildford in Surrey. This stately house was then a holiday hostel place for what was called the "Holiday Fellowship". And the Holiday Fellowship in those days was like the Ramblers Fellowship. It had a sort of mutual socialist - with a small "s" - country background, and it was in fact an organisation which had taken over these big houses to provide holidays for the "working classes". And we went there because there was absolutely nowhere else to go. And we were there for several weeks, where everyone else would come and go, for a week at a time. And then we came to London, and we had some apartments in downtown London, just by the Haymarket, but we moved out of there quite soon to a place which was meant to be more convenient for my schooling, in Crystal Palace, Upper Norwood. There was a very large hotel - the Queen's Hotel - there, where we stayed for some time until somehow we managed to find a flat in an nearby Edwardian house, where the owners lived on the ground floor and we lived on the upper floor, and that's where I spent all my school days.

**TT: You went to Dulwich as a day boy?**

MB: Yes, as a day boy. And it was two bus rides to school. I would take a bus into Crystal Palace, and then a bus down the hill to Dulwich. Three ha'penny half in both cases [Laughs]. And so that was very convenient. You see, that must have been quite an achievement for my mother. I have no idea how on earth she managed it, bearing in mind the scarcity of any accommodation, but she did. And then after six months, my dad went home, because he could only get six months long-leave.

**TT: So he was still a serving member of the Army?**

MB: He was still a serving member. And he then went back and, when was it, it was in 1955. When I went up to Oxford, my mother returned to India and she stayed there with my dad and...

**TT: So whilst you were at school, your father was in India?**

MB: Yes, most of the time.

**TT: At school you go into sciences rather than the arts. How did you decide what A levels to do? Or was there a choice?**

MB: Exactly! Once you were in the science side, the only choice that you had in our school - which was very dirigiste - was to do biology, because you could become a doctor, or to do chemistry, physics and maths, because you're going to go into doing chemistry or physics or maths. Actually there was, by my recollection, a Maths Sixth Form, and I opted not to do biology. And some of my good friends, colleagues, school fellows, did. So we spent two years in the Sixth Form, separated: they would do the biology and we would do physics and maths when they were doing biology. We both did chemistry, both had to do chemistry. And the chief master in the science side was a man called George Way, who was very, very, very organised. Actually, now, I think he wouldn't be allowed to teach that way, but it achieved what he wanted or what he thought, as it were, 'the boys' required and needed, and that was to pass the examination and get into Oxford

and Cambridge - or London if you had to [laughs]. It was very organised. There was an end, and the end was passing the examination and gaining entry into a good university, and that was the whole purpose of this education. And so you were drilled to pass the exam and it worked, it worked.

**TT: And given the options at University, chemistry seemed clearly the best option for you?**

MB: I didn't even think about changing anything. I thought, why don't I just stay with it? And it was only later on that I thought that I was getting a bit stuck in the mud. And chemical pharmacology, just seemed so much more interesting. Because the chemistry course was, in those days and even now, four years. It's essentially three years' book learning, and then a year completely in the lab. So Part 1, you took the special subject in the last year of Part 1, and I remember going for the end of term discussion with the tutors and the President of Corpus. And there was this and that, and the President, I remember him saying, 'You're doing a special subject?' And I said, 'Yes, it's actually quite fun.' And he said, 'You realise it won't help you in your class,' because in fact the rules said what happened in your special subject could not and should not affect your class in finals, either for or against, whatever it was. And he was a very old-fashioned academic, the President of Corpus; he couldn't understand anybody who wasn't trying to maximise his class in finals, and I didn't think of it in that way at all. I think he was being realistic and pointing out to me, and I have a strong suspicion that he probably looked at my behaviour, my academic behaviour, and thought that 'You're not going to do well, my son.' I wasn't going to be one of the better people in my year or even anywhere, even Corpus. So, I think, he was gently suggesting to me that perhaps I should concentrate on getting a better class and not bother with a special subject. Anyway, of course, I was young enough first of all not to understand what he was saying, and secondly not to take any notice of it, because I was enjoying the course.

**TT: How had you got onto doing chemical pharmacology? What had been the influences there?**

MB: I'm trying to think why I started doing that, because it was a relatively odd thing to do. It wasn't the most popular special subject. There were three or four. One of them was quantum mechanics. I knew how to spell, say the words, and that was it. I think the most popular special subject for chemists, was biochemistry.

**TT: Was Krebs in office then?**

MB: Yes, Krebs was there. And there was biochemistry for chemists. And then there was this chemical pharmacology, and I think several of my fellow students did biochemistry as a special subject. And I don't know, it's probably again a sort of wanting to do something not the same as everybody else, and being a bit bolshie, and I started on this other choice. And it just seemed to me to make so much more sense of the chemistry, because just doing pure chemistry for chemistry's sake, it didn't fire me in any sense. It wasn't interesting me anymore.

**TT: Who taught the pharmacology course?**

MB: The chemical pharmacology course was run by a man who himself was an oddball, H R Ing, Harry Raymond Ing. Very odd. Well that's another long story [laughs].

And what might have been relevant was that somebody had preceded me from Corpus in this course and was doing rather well, Edward Gill. And so there was a precedent, but once I started it was so much more interesting, than the Chemistry course. I thought, 'I'm going to do this.' Well, anyway, at the end of the story, I managed to get a Distinction, and I am sure it helped me. When you take finals in your third year, you don't get classed. You only get classed at the end of your fourth year, and I am pretty sure that, although it's not meant to make a difference, I think that without that Distinction I would have got a Third in finals. I really wrote such a bad finals paper. It made me embarrassed, because I realised I just hadn't learned enough, grasped enough of chemistry to make anything of it. But the chemical pharmacology, I wrote that paper on chemical pharmacology and I enjoyed every minute of it, it was amazing. I really enjoyed writing that paper. And that's what happened. And then I did my Part 2, with Raymond Ing.

**TT: This was a research dissertation, a project?**

MB: Yes.

**TT: What did you work on?**

MB: I worked on acetylenic bis-quaternary compounds, because Ing was very interested in quaternary compounds, he had started making bis-quaternary compounds. Eventually, I don't know whether he made these particular ones, they were investigated in a classical paper of Paton and Zaimis. Ing had a love-hate relationship between him and Josh Burn, J H Burn, Professor of Pharmacology at Oxford. Josh Burn had hired him from UCL [University College London], where Ing had been to be the chemist in-house and, I think, to make compounds so Burn could analyse them pharmacologically. But they didn't really see eye to eye about many things, that was very much like Burn. Anyway, Ing used to say that he'd made lots of compounds, but he could never get them tested. Because, of course, it was easier to make a series of compounds, relatively easy, but to have them all tested took much more time, and it would take somebody from another section to do it. And so it was quite a difficult relationship. But, anyway, he was interested in quaternary compounds, and at that stage they were probably the most instructive chemical compounds for studying structure-activity relationships. They were very simple and they had a wonderful range of activities. And the reason he wanted the acetylene in there was because the acetylene is essentially a straight bond. So you have four carbon atoms which cannot flex because of the triple bond, whereas the methonium compounds just being ordinary CH<sub>2</sub> methylene groups, they can wave around. And in fact Edward Gill had done a DPhil trying to predict or work out the most likely configurations of, say, hexamethonium.

At that time what was a great mystery was, why were bis-quaternary compounds blocking agents, when the mono-quaternary compound was a stimulant? And this was a really important question to try to answer, and we had no idea why. It occupied many persons' minds. So that's what I did: I did the acetylenic quaternary compounds for my Part 2. And they were jolly potent, but they never really got properly tested, but they were blocking agents like every other bis-quaternary. This project was slightly politically correct, because the new Professor of Chemistry had just arrived in 1955, when I came up, and had come from Manchester, and his speciality was acetylenic compounds.

**TT: Who was this?**

MB: Jones, E R H Jones. Anyway, I think that Ing thought, 'Well, here's a new sort of bis-quaternary compound and we can get help from the Professor.'

**TT: The right place at the right time?**

MB: Yes, that sort of thing. So that's what I did. And then I stayed on to do a DPhil, and Ing was very good in that he said, 'I'm not going to do a DPhil so you're going to have to do it with Edward Gill.' Edward had been through essentially what I'd been through. Edward was a chemist, he must have been about five years, at least five years, ahead of me. He was a chemist and he'd done chemical pharmacology and he'd done his Part 2 with Raymond, and I can't remember what on, and then he'd done his DPhil, trying to make the calculations about hexamethonium. And he'd got a paper in *Proc R Soc [Proceedings of the Royal Society]* and he got a First as well. And he was such a person to follow [laughs].

**TT: He was on the staff in Pharmacology?**

MB: He had gone to do his statutory post-doc in Riker's lab in New York, W F Riker, at Cornell Medical School. He'd spent a year there, and then he came back and he got a research fellowship and, eventually, he got a fellowship at Worcester and went through the ranks at Worcester, ending as Senior Tutor. I was his first DPhil-student and he was only about five years older than me. We got on very well together, we really did.

**TT: What was the subject of your DPhil?**



MB: My DPhil. I was making long chain fatty acid lactones with the lactone at one end and a water-soluble group at the far end. The idea was that was a model of digitalis. It was very clear that nobody had much idea how digitalis worked, nor were there any substitutes. Since curare had been cracked, i.e., we knew how it worked, and we had made decamethonium based on the structure of curare, although the effects of these compounds were different, we thought we'd cracked it. But nobody had made any sort of successful attempt at a synthetic digitalis. And so Edward's idea was that we knew chemically that the lactone was essential, because if you opened the lactone ring, activity disappeared in digitalis. And the fact that this was a steroid type of structure, but it's not like the normal corticosteroids or sex hormone steroids, any of those compounds. It's a different steroid structure. And the other thing was the compound called digitonin, which is a very good saponin. You can froth it up. So Edward's idea was, the important thing that you got from digitalis was the lactone, which could be called the pharmacophore and the rest of the molecule was just a way of getting it into an interface, like a detergent. It was getting into an interface between lipid and water. And so he said what you do is you put something which is similarly lipophilic, and the simplest lipophilic thing is just a long chain fatty acid. So I used undecylenic acid because you can buy that, a C<sub>11</sub> acid, and put a water-soluble group at one end and a lactone at the other. And the easiest water soluble group to make chemically is a quaternary group, so that's what I made. They turned out to be quite difficult to make, but I did essentially three years of chemistry.

**TT: So it was more synthetic chemistry?**

MB: Synthetic chemistry, exactly. But at the end of it there was a little bit of biological testing. By that time Ian Glynn at Cambridge was working on the sodium transport in erythrocytes and that was the system that we used. Because it wasn't going to be possible to test my compounds on hearts, it was too complicated and would take up too much time to learn a whole new expertise which I didn't have time to do. And so I was filling red cells with sodium – just by keeping them cold - and then seeing it pumped out when they are warmed up. The action of digitalis was to block this sodium pump.

**TT: Was that very state of the art at the time?**

MB: I don't think that measure was state of the art, and the chemistry was new, but it was not that complicated. It was a pretty undistinguished DPhil Thesis, I tell you. It really wasn't at all mind-grabbing, because the compounds weren't particularly effective. And, of course, being detergent-like there was a terrible tendency for them to lyse the red cells anyway. So you were very limited in the concentrations you could use. And nowadays, I suspect what you'd do is you'd get the sodium transporter system as an *in vitro* system and test the compounds on that. So the chemistry was interesting, but the biology wasn't really very interesting at all.

**TT: Did you have any sort of pharmacological or pharmaceutical objective? Burroughs, Wellcome were producing digoxin by this time.**

MB: Well, I think the initial driver was the fact that there was no substitute for a herbal medicine which you still had to collect and purify. Here we were in the middle of the 20th century, 1960, and we were still extracting it, like morphine. And as it happens, later on, I found out that Wellcome had fields of foxgloves just to produce digitalis. And there was nothing else which did anything like it. People recognised that digitalis was actually quite a toxic compound, but for those people it worked in, it absolutely changed their life. And if you read the stories that Withering collected, the effects it had were dramatic. So the driver was to try and find out something more about digitalis which you could use to manipulate its activities and still, nobody has produced a synthetic digitalis.

**TT: Even 20 years after you, people like Jeff Aronson and David Grahame-Smith were still looking, weren't they?**

MB: Yes, even now nobody has. I don't know anybody who is actually looking to substitute for it. They are trying

to approach the pathology through a different mechanism. Because the mechanism of digitalis, blocking of the ATPase [adenosinetriphosphatase], it has too many side effects. Even now you have to be careful with digitalis, because the therapeutic window is really very narrow and you can get as sick with an overdose as you can with an underdose. And so, you have to be careful how you use it, but when it works, my goodness me, it works.

**TT:** When you were finishing your DPhil, was this any part of your thinking about when you might carry on, to explore this more?

**MB:** Yes. I think that I certainly told everybody, and I probably thought as well, that I would go into the drug industry, as it was called then, at the end of my time. But then again, very odd things happen. It was at the Department in Oxford, which itself was a very curious social environment [laughs], but I think highly effective. The person who, I think, made the greatest impact on me was Blaschko. I was extremely impressed by him, and he was such a nice man [laughs]. I believe he was horrendous to work for, but as somebody who would talk to you in the coffee room and things like that, he was really good, really good. Encouraging and with enormous knowledge. He had this bizarre memory and he would say, 'Yes, I think that was...' and he would essentially pick the volume of *J Physiol* [*The Journal of Physiology*] off the shelf and say, 'Yes, I think it's page 302,' and you'd turn to the page and it was just so; he had this ability. And it was very impressive and his comments were always valuable. Anyway, I decided, or it was decided, somehow, I applied to go for a post-doc to Yale. There was a shuttle which went between the Yale department and the Oxford department, which had been established some years ago and people would come and go all the time. So it was 'are you going?' 'I don't know. Ok, yes, I'll go to Yale.' It was standard, if you didn't have any dire need or urge to go somewhere else. And it turned out, we again were very lucky, because I should have gone to work with the chemist in that department, there was a proper chemist who made compounds, just like Ing's subsection, and that was the person I was headed for. But the year that I got to go there, which was 1962, Henry Mautner - that was his name - his lab was full and I was ready to go, so I had to find somewhere else to go. Someone else from the Department at Yale, who had been a year or so before in Blaschko's lab, a man called Prusoff, Bill Prusoff, he said, 'Oh well, I'll take him.' And Bill Prusoff, he was a straight biochemist, and the Department in Yale at that time was essentially a cancer chemotherapy department. Several people, all beavering away on different aspects of cancer, putting compounds into cancer and seeing what happened. Obviously they were using cells most of the time, but there were some *in vivo* experiments as well. And right across the street from the School of Medicine, there was a big clinical cancer centre, where eventually my wife worked as a secretary [laughs].

So I went there to Yale and I had a very good time with Bill Prusoff, learning biochemistry essentially. But I think again, I was fairly disappointing as a post-doc. For one thing, Bill said, 'Well, how many papers have you published from your PhD?' And I said, 'None.' He said, 'What? How did you get your PhD?' I said, 'Well, you don't have to publish the papers, or any papers.' And he was really quite surprised you could do that, even in 1960, early 1960s. And I still haven't published anything ever from my Doctoral Thesis. I don't think it would survive now. So we had two years at Yale, which were really enjoyable, despite a variety of disasters. But it turned out to be a good experience, and the Americans said, 'How long have you been here? Two years? Oh, you'll be back.' Because that was the time when the brain drain was very alive, people were going to the US, all the time. But neither my wife nor I had any doubts; we liked the people, but we just couldn't see enough attraction to say, 'We're going to move here.' And we could see too many distractions, unattractiveness. And so it was never a possible choice. So we came back to the UK, and I spent the next year trying to find a job. We came back to Oxford, my wife's an Oxford girl, as it happens.

**TT:** Did you get married before you went to Yale?

**MB:** Yes, just before in fact. The June before we left - we must have left in August or something like that - I remember, she had to spend her entire life savings on the passage, on the boat passage, because we could only get a Cabin Class cabin. This happened to be the USS United States, which was in its time quite advanced. I was alright, I had a Fulbright Fellowship so I could do that, but she had to spend, I think it was £80, it really cleaned her out. Anyway we came back steerage [laughs] on the Queen Mary. Never mind. So

we came back for a year and I looked around for a job. I had a year's fellowship at Oxford in the Department. I didn't really do very much scientific work, just chuntering around trying to find somewhere. I went to Smith, Kline & French, had a look at them, and I went to Sheffield, a chap called Pickles was in Physiology there. They didn't have a Pharmacology Department in Sheffield. And I must say I thought seriously about that, and then there was this chance of getting a post-doc, another post-doc or research fellowship in London, in the same Department, the Research Department, as John Vane and Gus Born. Now the relevance of all that is two things: one, is that John Vane and Gus Born had both been through the Oxford Department. Gus actually was the examiner in chemical pharmacology who had given me the Distinction, although I don't think he knew about that. I found out later. And John Vane was a chemist by training, he'd come down to Oxford and he'd undergone a change into a pharmacologist, with J H Burn.

**TT: Your career followed his quite closely.**

MB: It seemed to. And he'd been to Yale, and he spent two years there.

**TT: And he almost took a job in Sheffield, did you know that?**

MB: I didn't know that. Soon after I went to see Pickles and talk to him about a job, Pickles hived off from Sheffield and went to Cardiff, where he spent the rest of his time looking at prostaglandins, but not very successfully, as it happens. And then, when we were at Yale at the end of our stay, John Vane came for a short visit to the Yale Department and Gus turned up on a sort of fleeting visit, and we all met - I quite distinctly remember - at the local swimming lake, because my supervisor, Bill Prusoff, lived in a little country housing estate, and they had as a part of it, not very far away, a lake where people would go for weekend, summer weekends, splash around, have a barbeque, and so on. And we all met there as it happens. Bill said, 'Oh, we are going down there, you will have to come along, and all these other British people will be there, you'll be happy.' And that's when I met John Vane and I'd seen Gus in the Oxford Department back and forth, because he wasn't still in the Department in Oxford when I was there, but he would sort of drift in and out. So I met them again there. And Gus had been appointed, well he wasn't appointed to the Chair, but to the Department, it wasn't really a proper Chair until afterwards, at the Royal College of Surgeons, to follow Bill Paton, because Paton had gone to Oxford after Burn retired. So there were these threads, and I think somehow I must have applied and they said, 'Oh yes, why not? Come on.' And so I took the path of least resistance, which was to come to London with a wife and young child and work in the Department in the Royal College of Surgeons.

**TT: But before we get there, can I just ask you about Raymond Ing. I don't know very much about him.**

MB: I think he's very much underrated, because I really think he invented structure-activity relationships in this country, pharmacological structure-activity relationships. Because even when he was at UC [University College; UCL], he was making compounds for pharmacological analysis. It would have been for Schild's predecessor, E B Verney. I don't know where Raymond Ing went to university; I think he probably an Oxford undergraduate, but he was working in Manchester, along with a man called Manske, as a chemist, in Robert Robinson's Department. Ing was a chemist at that time, absolutely a chemist. And he said Manske was a European, but in fact Manske had come from Canada where his parents had emigrated to from somewhere in Europe. And Ing said that - as one did in those days - on a Saturday morning he was in the lab, because everybody worked in the labs on Saturday mornings, and he was doing something in the lab, and the Prof came in and said, 'Where's Manske?' because there were two students, and Ing said, 'Prof, I don't know, he happens not to be here.' And so Robinson said, 'Well, I've got this letter from a chap at UC. He wants a chemist. Are you interested?' and so Raymond Ing said, 'Yes.' What else do you say? Yes! So he went down to a job at UCL and he was there for several years (late-1920s to mid-1930s). But clearly he made a bit of a mark with Josh Burn, who was a Professor of Pharmacology in London then at the Pharmaceutical Society Labs in Bloomsbury Square. He was there, and I think Edith Bülbring (EB) came to work with Burn when he was still at the Pharmaceutical Society. But, anyway, they soon moved to Oxford and she was one of the originals with Josh Burn. And for some reason Burn said to Ing, whom he must have met through the Phys Soc or something like that, 'Why don't you come to Oxford?' And so he did.

But I think Burn's idea was that Ing would make compounds for him and he'd be happy just to leave it like that, just to be a chemist. Make the stuff, here's your white powder, number 53, that sort of thing. But I think that Raymond Ing had got this structure-activity relationship idea, the five atom rule was one of his ideas for acetylcholine, and things like that. He was thinking in those terms. And then he produced R B Barlow as one of his DPhil-students, and I do think Steve, R P Stephenson, went through his hands, Stephenson from Edinburgh. But R B Barlow, Dick Barlow, went up to join Stephenson at Edinburgh, and they were really quite a power in their time. The combination of chemistry and a bit of mathematical analysis and getting both structure and rate reactions and analysis of receptor interactions. I think it started with the training that certainly that Dick Barlow had from Raymond Ing.

**TT: That's really interesting, isn't it, because that brings in people like Cushny as well, both Edinburgh and UCL.**

MB: It's possible that Raymond was talking, but nobody was listening, and he wasn't a prolific publisher - nobody in those days was a prolific publisher. But one of his most, perhaps his most, important paper was published in *Il Farmaco*, some rather peculiar Italian journal, which might even in fact have been a pharmacy journal, I don't know. But there is a long tradition of important scientific discoveries being published in bizarre places. I think Robert Robinson, one of his most important chemical papers about chemical reactions, was published in *The Dyers and Colourists Gazette*, because nobody else would take it; *The Journal of the Chemical Society* wouldn't take it on. So I think Raymond found himself trying to make his voice heard and being much overshadowed by the biology big guns in the Department. J H Burn was a fairly forthright character and quite a big character; EB, who had a very good reputation; Blaschko, much more shy and retiring, but still doing good work and people listened to him; whereas Raymond Ing, I think, was largely overlooked. And he felt overlooked, I think; as I said, he couldn't get as many of his compounds tested as he would have liked to have, and that made him feel a little bit unhappy.

**TT: Unappreciated almost?**

MB: Yes, exactly, unappreciated. But he was there in the Department, and I think he'd got a fellowship. New College? I think nowadays either Burn would have got rid of him, as it were, or he would have moved on. But those sorts of solutions weren't viable in those days, you just got on with it. And also Ing had his little group. He had a couple of labs and he could nearly always get Part 2 students for one year, to do just a little piece of work, make a few compounds. From time to time he had DPhil-students as well, and so it was viable, but I don't think it was fully rewarding, and I think he just became accustomed to not being appreciated.

**TT: I was just intrigued by a remark of yours about the social life of the Pharmacology Department.**

MB: I've never been in a place like that before, or again. But we all had lunch together every day. It was fairly basic lunch, but we all sat in the library and everybody had a plate full of meat and two veg, that sort of thing. It really was, in terms of food, disastrous, but in terms of the environment it was fantastic, because you just sat down next to whoever else was on the table. At the top, usually, you left the top space for Burn, for the Professor, and the next two spaces you left as well, because they were very often occupied by EB and some colleague of hers. But everybody else would just then fill up the spaces. However, Blaschko would just come and sit down anywhere and everyone else would just come and sit down. And we had - it was part of the set up - a very good mechanical workshop, and latterly an electrical workshop in the basement. And this mechanical workshop, the man in charge of it was a proper engineer, who for some reason had taken early retirement, O B Saxby, always called OB, everybody called him OB. It was perfectly respectful to do that. And in fact everybody, even the chemistry DPhil-students, had to go down to the workshop and learn how to use basic tools to make things. The more adept ones, or depending upon your needs, you actually had to make your apparatus, especially for biological apparatus, clamps and electrode plus tissue holder devices, you had to make that yourself. And it was a part of the DPhil process, you couldn't have it made, first of all you didn't have the money, and secondly Burn said, 'If you want this, you have to learn how to make it, because then you know how it works and you will know what to do when it doesn't quite work.'

He was very authoritarian in many ways, and it was very sensible in those days.

So you have this great mix of people all sitting down there at lunch and sure you talked about this and that but you also talked about science and bounced ideas and so on. And added to that you had coffee and tea time in the library, where again it was expected that most people would come down for a cup of tea at that time, that they would organise their work so it would happen. Not everybody would come always for every coffee or every tea, because sometimes you were setting up and you couldn't do it, and you'd have a tea taken upstairs because you were in the middle of a preparation or something. But you absolutely had to come down to lunch. If necessary, if there was an animal on the experimental table and you wanted to keep it going, you'd take it in turns. You'd come down and have lunch and then you'd go back up and your chum would come down and have lunch in turn. And this, these events, were, I think, they were very important. It was a small Department; I suppose we were about 30-40 people? 40, maybe. It was not a big Department. It was feasible, and the lunch persisted. I supposed Josh started it and it persisted when Bill Paton was there, and I don't know how long it lasted, and I don't know if it still goes on. But it was, it really was, an important part.

**TT: I've never heard of a Department having lunch together. The tea and coffee, this is a very common theme. Where did the lunch come from?**

MB: Oh, there was a kitchen, there was a kitchen.

**TT: So you had a kitchen and you employed somebody to cook you a lunch every day?**

MB: Oh yes, in the basement. Yes, we had a cook, yes. And [laughs] we had several cooks in fact in my time, because I was there for, let me think, four years at first and then another year. Because I did my Part 2 at the Department and then I did three years as DPhil. There was indeed, in the basement, at one end of the basement there was the kitchen and at the other end was the workshop. Actually, it was quite close to the animal house.

**TT: No Health and Safety then.**

MB: Oh, can you imagine? Absolutely no compliance - it would have been completely banned. Everybody would have been sent to prison straight away [laughs]. But we never had any problems like that and so, there was a kitchen and we had a cook who just came in and did the lunches. As I said, they were very basic sorts of lunches. Almost sausage and mash and mushy peas, but not every day obviously, but that sort of stuff. It was very, very basic. As I said, the food was in no sense good; in fact, the food might even have been a distraction and non-attraction. First of all, it was the way it was done so you did it, as part of the Department. Eventually, I think you realised that actually it was a benefit and you did talk to people, and there were people who said, 'Why are you worried about the different heights of those horses? One of them's black and one of them's white, don't you see that?' And you, of course, had been measuring the heights, forgotten to look at the colours. And that's always valuable, I think.

**TT: When you say everyone, was that...**

MB: Students, faculty, secretary...

**TT: ... secretaries, technicians?**

MB: Technicians? Only the Chief Technician: Harold Ling, who had been with Burn since Pharmaceutical Society days, I think. He was the other constant figure.

**TT: That's fascinating. I'd never heard about this lunch.**

MB: It was like, it was sort of family, but he was quite a strict *pater familias* was Josh Burn, and I didn't work with

him directly, but he, I think, expected quite a lot of his students. He drove them quite hard. I think John Vane once said to me that when he came down from Birmingham to work with Josh and learn pharmacology; all that he did was measure glucose for about two or three months. He did nothing else but just measure glucose. Samples would come in and he'd have to give the answers back at the end of the day, that sort of thing. I'm not sure that that was anything except formative. I don't think he learned very much.

**TT: Yes, good for the soul! And the other thing you just threw out was when you went to Yale you went on a Fulbright? Now Fulbrights are very prestigious and hard to get. And not many scientists get them, do they?**

MB: I don't know, it was not considered, I never had the appreciation that, 'Gosh, this is unusual,' or anything like that. I don't know why? I'm very unaware in these things. I never saw it as a big event. It was a means of getting over there.

**TT: So somebody just said to you, 'Apply for this!'**

MB: Yes. Bill Prusoff, 'Yes, I will have a space for him and I can probably get him a post-doctoral fellowship over here, but he has to get himself over here somehow.' They said, 'Get a Fulbright, they'll take you across.' I said 'OK' and sent it in. So I think there's something about being lucky and having 'uncles', because clearly somebody made decisions, as it were, about me. But I never felt, never had to work very hard for them, which is bad.

**TT: The other thing is when you were in Yale you finally got a publication, and your first publication is a paper in *Nature*. That's also pretty prestigious.**

MB: Yes [laughs]. I had a look through, I don't know why, it was somebody long ago that said, 'You got quite a few publications in *Nature*, didn't you?' I said, 'Really?' And he said, 'Yes.' He said, 'In the first dozen publications, I think seven were in *Nature* or *Science*.' And I said, 'Oh, really?' But again it seemed not to be such a big deal. Now, of course it's quite a different thing. So that's what always happened to me. When you look at people's work, or what they are doing, now, I think you rate it according to present day standards. What you can't always do is to rate it at the time. You see, at the time I went to Yale, I'm sure Bill Prusoff thought, 'My God what have I got here? Somebody who hasn't published a single paper, and I've taken him on as a post-doc.' And one of the duties even then for a post-doc in US academic life, was to generate papers like a machine, both for himself and for his supervisor. He would have to teach me all the stuff that he wanted me to do. Years ago, later, because we stayed friends, he said, 'You were a dreadful, dreadful post-doc.' I said, 'I know, I'm sorry about that.' He said, 'But you were fun.' I said, 'I'm glad about that!' [laughter]. But that's what, we did have fun, and we got some work done and I have very pleasant memories of that whole event, those two years. But it sounds and looks on paper to be a brilliant career to begin with, and as you say, 'Fulbright Fellowship, Yale,' this and that and the other, but it's never felt like that.

**TT: Papers in *Annals of the New York Academy of Sciences*, *Nature*, *Science*. One thing I did notice, one paper is with Creasey? From Burroughs Wellcome?**

MB: No, no, that Creasey who in fact was a Brit. He was an Englishman, but he was on the staff at Yale. Now that was the iodocytosine paper, I think. That was very curious: that was a compound which absolutely disappeared into the middle of nowhere. You see, iododeoxyuridine, IUdR was the real winner, but it got overtaken because its efficacy was really in smallpox, and we got rid of smallpox some other way. It was very good for smallpox, it really was, it actually cured it. And it had remarkably low toxicity for an anti-metabolite. Because all those anti-metabolites, they come with cytotoxicities, which is unavoidable, but this had much lower toxicity than all the others

**TT: That sounds like a whole other chapter. But the whole thing about anti-metabolites was very much what Burroughs Wellcome were doing in the States, wasn't it?**

MB: Yes, but this whole Department, the Department at Yale at that time, was involved in cancer chemotherapy. The chief was Arnold Welch.

**TT: That was how John Vane got a connection, didn't he, with Professor Welch?**

MB: Yes, because Arnold Welch came to do a sabbatical in the Department in Oxford with Blaschko. And almost certainly, that's where he met John Vane, JRV. And I don't know about the timings, but it's about right. And that is probably how JRV went for - I think it was a couple of years - as an Assistant Professor at Yale. And then of course he came back to Oxford, and he certainly spent some time at the Nuffield with Geoffrey Dawes, and people like that. And then eventually he went down to the Royal College of Surgeons. To the Department of Pharmacology with Bill Paton, in the old place, the Examination Hall, Queen Square, before they had moved over to Lincoln's Inn Fields. I think they didn't move over into the Royal College of Surgeons' building until Gus came as Professor after Paton had moved, and Gus was the first Vandervell Professor. The relationship between the College and the Vandervell Trust, as it became later on, was always a little bit fraught. But they (the Royal College of Surgeons) got a lot of money in, which was put to good use.

**TT: At the Royal College of Surgeons, you were on an MRC [Medical Research Council] Fellowship - what were you doing?**

MB: I was trying to make slightly different analogues of my digitalis analogues. Instead of putting the quaternary group at the end I was trying to put glucose, make a glycoside. And so I did that for two or three years and it really didn't work.

**TT: Were you on your own doing this?**

MB: Yes, I was on my own. There were two of us up one end of the corridor [laughs]. It was interesting. Physically, the Department was along a corridor on the 6th floor, and one end of the corridor was Gus Born and a chemistry lab, two rooms which was chemistry, which was run by a chemist who was beginning to leave. Slightly odd, but it was like that. John Thompson had already gone to Newcastle when I came in 1965. At the other end of the corridor, beyond the coffee room, which in fact, worked as sort of dividing line. The library, the coffee room, and the Senior Technician's office, and then all the other labs on the far end were all John Vane's pharmacology, more like heavy duty biology, down that end. So I was up this end...

**TT: The Gus Born end?**

MB: I was at the Born end, which is where the chemistry laboratories were. And there was me and another post-doc who had just returned from the States, a chap called Alan Prince, A K Prince. And what on earth was he doing? I'm trying to remember what he was doing. This is naughty, I can't remember at all what on earth he was doing. But Alan Prince went on to get a job, a proper job, at Kings College in the Strand, in the Pharmacology Department there, and he stayed there until he retired. And so I bumbled around doing chemistry in a not very effective way for two or three years and, I think it must have been some time in the last years that I got a message from a post-doc I had known at Yale, who we'd got really quite friendly with, called Alan Reynard, who was now in Buffalo, in a faculty position in Buffalo, at SUNY [State University of New York]. And he said, 'I think I need a sabbatical. Why don't I come over to London?' I think he was between wives as well, and so we thought, 'Alright,' and got him in. He was a good scientist, he was a biochemist, but he was working, I think, on something to do with renin in Buffalo. And John Vane at this time started doing his blood bathed organ technique, he'd finished most of his CNS [central nervous system] 5-HT [5-hydroxytryptamine; serotonin] project. And John said, 'Well, what's Alan going to do?' I said, 'I suppose something to do with renin? I don't know.' And he said, 'Well, alright. Why don't you have a look at this angiotensin converting enzyme.' I said, 'OK, why not.' He said, 'You'll have to sort out what he's going to do. Because he's going to come here and he'll have to come in and use a system which is working.' So I spent some time trying to get a system where you could measure angiotensin conversion in lung homogenates. John had this idea of my expertise, he said, 'You are the biochemist around here!' [Laughs].

And I thought, 'My goodness me, if you'd said that when Bill Prusoff was around he'd have said, 'D'oh!'' Anyway, so I was the source of biochemistry for John, and I bumbled around trying to get a system working.

**TT: In homogenates?**

MB: Yes, exactly so, homogenates. So there was something that Alan Reynard could safely lock into and spend some time doing. Actually, we did some nice work and we also had some perfused lungs working by that time. So this must have been at the end of my MRC time, you see, because I think that was three years with the MRC fellowship. That would have been 1965 to 1968, something like that?

**TT: You published on the metabolism of the angiotensins, which is *Nature* 1969.**

MB: 1969, yes. Well that would have been, and there's also another one that I published with him, me and him together, some further characteristics of converting enzyme. So that would have been - yes - you see, because my first publication about converting enzyme was 1968, so, by definition, the work for that would have been done probably early in 1968.

**TT: And that's your single author paper in *Nature*. One of your many papers in *Nature*, Mick.**

MB: That's right [laughs]! I remember I said to John 'Well, this is presumably going to be Bakhle and Vane?' And he said, 'No, only your name's going to be on that paper.' He was clued in and he said, 'Your name is going to be on that paper, nobody else's.' Now that is an amazing thing, I think, for a Professor to say. He recognised the significance of that. I really didn't have any feelings one way or another, because to me it was natural that he should be on it. He'd actually told me more or less, 'You should do this.' And I said, 'Arr, why should I do that?' He had this explanation which was complete nonsense! He said that 'It was because it was a bradykinin potentiating factor' - which I was testing - 'and both bradykinin and angiotensin end with a Pro (proline).' 'So,' he said, 'they're bound to be related!' [Laughter]. I said, 'John, well, alright I'll do it.' Where do I start to explain this to him: that the end (of a peptide) has nothing to do with the action that happens. But anyway, we now know that the proline, is a stop function, for converting enzyme. It is the other side of the proline that is really important. I was reluctant to do the experiment, but he said, 'Go ahead!' But the thing about this converting enzyme project, it actually started to produce results. And as you know there's nothing so seductive as results. Getting an experiment which actually gives you a clear answer one way or another, no matter what the answer is. It sucks you in, and I had three, four, five years of doing experiments which all worked and got really involved. My wife was very upset with me [laughs]. She said, 'This is a hotel I'm running!' because I would get up early, go to work, and come home quite late. And the kids, both of them were small, hardly saw me. She was very good, she put up with me [laughs].

It's only now that I think of it as a sort of heady time. Long after, there was some sort of a Festschrift at the Royal College of Surgeons for either John or Gustav, I can't remember which. By that time I had sort of rationalised it, I saw the significance of those years. And I think we were almost like brigands. You'd go out and you'd beat a path in the bushes of "not knowledge", and drag out a result and take it home and analyse, and then you'd come back and you'd drag out another. But there was this same thing going with Gus Born's people. With that platelet system that he had going, the aggregometer, he was getting brand-new results with every experiment, because nobody had been able to do that before. It was, everything you did was new. And his group did. You couldn't help being sucked into the fact that you were really doing new things all the time, and nobody else...

**TT: It must have been so exciting.**

MB: I think it was. It's only now that I realise that I used to spend so long in the lab, and of course I'd never stop thinking about the project. You're always thinking about it even if you're not there. And as it happens until 1970, we lived - my wife, kids and I, lived behind the old Prudential building on High Holborn. So I used to walk to work.



**TT: You could hardly be closer.**

MB: I couldn't really have been closer, which was when my wife said, 'It won't take you long to get home. How about leaving now?' I went...

**TT: 'Just one more experiment. One more reading.'**

MB: Yes, yes (laughs)! It was so - it was very rewarding. It really was rewarding, because the experiments were working, and they were all new. So it was a great time.

**TT: It's what people nowadays refer to as low-hanging fruit. As you were developing the techniques...**

MB: Yes, somebody said just that, that that's what John Vane did. He went into an area and he got quick results and then got out again. But it wasn't quite as cold-blooded as that. The thing was that he had the technique, you see, he had a technique, which enabled things to be done. And the way you maximise that technique is by using a lot of substrates. Because if you use just one substrate and follow that the way through, what you have to do is you have to start getting new techniques as you're going through.

**TT: That takes time.**

MB: A member of the department, Helen Payling-Wright, whose name you may remember. Yes, she was a pathologist and one of the few people, one of the only people I know who was LMSSA [Licentiate in Medicine and Surgery of the Society of Apothecaries]; that was her qualification. She said, 'Oh, he's a prisoner of his technique.' And I thought, 'Well, you're probably right.' But in fact, so was Gus Born. He happened to be at that time a one technique guy. But my goodness me they were generating results. We were actually finding out stuff, which wasn't wrong, it was just we could do it and that's why you see that John Vane took, if you like, almost every transmitter you could think of and put it through his system. And in fact he found out new things about it. And in a way that's what I tried to do, but was much less successful with the perfused lung, turning my mind to different substrates, and then to different pathologies.

**TT: You first single author paper in *Nature* on angiotensin was the cell free extracts of dog lung. Was that the first time you'd worked on lung? Was that the beginning of your interest in lung?**

MB: Yes, we did dog lung, because that was where John had shown conversion. Because in those days he was using the blood-bathed organ technique to look at the pulmonary pharmacokinetics of natural substrates. And it really was very interesting, because, for instance, at least 50% of noradrenaline was lost on a single passage through the pulmonary bed, but adrenaline survived. Histamine survived, but prostaglandins were almost totally entirely wiped out. I was very impressed, I kept thinking, 'My God, how sensible of the dear Lord to organise it like that!' Because there are only two organs, as we know, in the body that receive the entire blood volume. One is the heart, and one is the lung. And the heart is volume without area, because it's a pump, whereas the lung is area without volume, because it's a thin smear, because of the gas exchange. The other thing, of course, was that in those days heart and lung operations were still - and they still are - a little bit, dodgy, because you can get what is called "post-pump syndrome". People who had a very good operation, everything was fine, and you put everything back, reconnected them, and four or five hours later, they were not at all well. And nobody quite knew what all this was about.

And we had our private opinion that what had happened was in fact that those membrane oxygenators and other devices were excellent at exchanging gas, but they had no biochemistry. So all the biochemistry that had been in the lung just wasn't there anymore, during bypass. So you essentially had a very dirty blood, which alright, some metabolism happens in the periphery, but you didn't have that general cleansing which went on in the pulmonary circulation. Because it was cleansed blood, which normally goes into the coronary circulation. When you plugged people back in again after the heart lung bypass, there was all this crummy blood which probably overwhelmed the first pass of the pulmonary circulation, and a lot of the dirty blood went on. So the coronary circulation got a lot of it, got all the rubbish into it, which I think it suffered from.

Anyway, that was my private opinion, but nothing happened about that. It was very difficult to get a handle on it and to devise a system which one could use practically. I would liked to have a diagnostic substrate. As you see from my CV, I did quite a lot of work on lung oedema, and this diagnostic substrate would have pulmonary metabolism that changed particularly during pulmonary oedema, *i.e.* some form of lung injury. But it never worked out - I never was able to find that substrate.

**TT: This is your introduction to the lung, but you're still working on lung extracts, you're still a biochemist almost. Then you publish the joint paper with Reynard and Vane, on isolated perfused tissues, so you're learning more biological techniques?**

MB: I think it was two things. One is that I remember John saying, 'We've done it in the whole animal, but we don't know if it's the blood or whether it's the lung,' I said, 'Well, you can either do the animal without a lung or the animal without blood. It's all the rage to do operations without lungs, so why don't we try that?' He said, 'I don't think that's such a good idea.' [Laughs]. So I said, 'Well we'll have to do it without blood,' and that was the perfused lung. And it in fact was a positive experiment. Because you didn't have any blood, all the biochemistry had to be in the lung. And of course with substrates like a nonapeptide (bradykinin), the substrate has to be in the vascular system. It has to be in the vascular lumen. And then we got Una Ryan interested, and she was very good. I don't think we've ever published together. Una, she's a good scientist, a very good scientist. She used antibodies and electron microscopy (EM), because one of the problems about the pulmonary endothelium is you can't see it under light microscopy, it just doesn't exist. So you need the EM. In 1970 or maybe the late 1960s, the EM was still very much experimental; people were developing different techniques to get good results. And she was very good in that she was able to demonstrate angiotensin converting enzyme activity on the surface - the luminal surface - of the pulmonary endothelial cells.

She (Una) may have been one of the first people to demonstrate either caveolae or the fact that you could get the angiotensin converting enzyme staining in the caveolae. And that it wasn't on the abluminal side; it was never stained on that side. So you could see immediately that this was a sensible functional arrangement and you'd think, 'Well, does it actually make much difference?' and 'Why, for instance, in the lung does angiotensin II go through without change?' Angiotensin I is converted in the periphery, but angiotensin II is destroyed; you get a net loss, at least 50% in the leg. So there is something in the endothelium in the periphery that is different from the endothelium in the pulmonary circulation. And the same difference applies, if you take almost any peripheral circulation and you put any natural substrate through it, it nearly always suffers loss. The activity is destroyed. I think histamine is cleared. John did all those experiments a long time ago. So there is something about pulmonary endothelium, which apart from receiving the entire cardiac output, which is different. But then, of course, people are turning this idea inside out and using these biochemical properties as markers for endothelium. But Una Ryan was one of the first people to successfully culture pulmonary endothelial cells.

**TT: When you were at Royal College of Surgeons and you started doing this work, was this the beginning of you doing biological experiments yourself? Was that when you got licensed, a Home Office licence?**

MB: Yes, it was my first time doing animal biology. Previously, the biology that I'd had to do was with red cells, and maybe some sort of incubation or prepare extracts of tissues from animals in Yale. But I didn't actually do any, as it happens, *in vivo* experiments, so my expertise, such as it was, was just the perfused lung. And very, very much later, I did some perfused hearts as well.

**TT: So someone else did the surgery?**

MB: No, no, I would take the lungs out.

**TT: So you did have a Home Office license at this stage? At the Royal College of Surgeons?**

MB: Yes. I had a licence, yes. Not before, no.

**TT: Let's go back to your work on angiotensin.**

MB: We started doing it because of Alan Reynard's visit and it worked out so well; the success of those early experiments of converting enzymes, more or less coincided with the end of my MRC fellowship. About the end of 1967. Actually probably later, the fellowship ended in 1968. So I went into see the headmasters, John and Gus, and they said, 'What are you going to do?' I said, 'Well, ...'; I did not have a project. My only project at that time was something very ill-formed about ATP [adenosine triphosphate], in those days it was fairly new that ATP was co-secreted with noradrenaline, in granules at nerve terminals. The granules in fact contained a lot of ATP. And I thought, 'The body spends a lot of time making ATP and then it just throws it away - that's crazy.' I just thought it would be interesting to find out or somehow analyse how that ATP was made, where it came from, does it come from the metabolic pool, or is there a different place for making it? And John said, 'Hmm, I think you should just come and work for me for a bit, will you?' So I went to work for him and this was the start of the converting enzyme and the lungs business. That's really why these things all, sort of, coincided. And as I said the angiotensin converting enzyme business worked so well, I stuck with it. Then, during Alan Reynard's time, the perfused lung preparations started to work and give really very interesting results, so I stuck with them. And at that time I had my first PhD-student, Val Alabaster, and she was such a good student it was frightening, it really was. I really had to run to keep up with her, she was so good. And she did so much great work, all with the perfused lung and all sorts of things, converting enzyme, bradykininase, and 5-HT as well. So it was a very productive time. And she was so together, she was so good. She was a mature student. She'd been out of Chelsea for four to five years, been working with Mike Barrett up in Macclesfield doing beta agonists I think, anyway, something to do with the  $\beta$  adrenoceptor. And she only came down to London because she married a technician, and he wanted to get a degree, and so he went to Chelsea as a mature student and somebody - probably maybe Mike (Barrett) - said, 'Oh, why don't you go and do a PhD with John Vane?' And so we got funding from the Wellcome with the help; John knew a female administrator at the Wellcome.

**TT: Edda?**

MB: Edda Hanington, right! He was great chums with her and so again maybe through discussion or whatever, but anyway, Val Alabaster got funded for a PhD through the Wellcome. And when she came down, I didn't even see her, John interviewed her, and he more or less told me that she would be my PhD-student. She didn't know, she was expecting to work with John [laughs]. But, anyway, she got lumbered with me. But we stayed friends. And then, of course, she went to work for Pfizer and she did very well in Pfizer.

**TT: How were you funded at this time, Mick?**

MB: Then I was on John Vane's programme grant. As a Senior whatever it's called, Senior Lecturer. I was first on John Vane's research grant as a sort of Senior Research Fellow, and then only after that I became a University Senior Lecturer. I was then paid by the University, the Institute, yes. So I had three years on John's grant, and then I was transferred.

**TT: And during this period were you also carrying out an independent line of research?**

MB: Yes, I think it was independent insofar as, John had sort of worked his way through the blood bathed organ technique, and was beginning to get into the prostaglandin business through Priscilla's PhD Thesis.

**TT: This is Priscilla Piper?**

MB: Yes. I suppose that was about 1970. So I would have finished the converting enzyme business, and then I went on to do 5-HT with Val, and also the converting enzyme and bradykininase with Val. There's one paper in *Nature* about the constituent peptides of bradykinin potentiating factor, BPF, which is Ferreira, Alabaster, Lewis Greene, me and John.

That's 1970. Val Alabaster must have been there already, yes. So, why did I do 5-HT? I did 5-HT because Duncan Thomas had done the work *in vivo*, with John. Duncan Thomas who was actually a colleague of Gus Born and he, eventually, ended up in the National Institute for Biological Standards [and Control] in Holly Hill, NIBSC. But I think he was a heparin man, really. He was in the lab working with Gus but, of course, 5-HT is a platelet thing, so there was the connection. Anyway, I remember that Duncan Thomas and John did the work showing 5-HT clearance in the lung *in vivo*, and it was really so powerful. It just went, it was gone, nothing came out the far side. I thought, 'We'd better have a look at this.' And John Hughes, at that time, who had been in the Department as John Vane's PhD-student, John Hughes had gone to Yale, he'd taken the path to Yale again and he was working, not with Bill Prusoff, but with a Canadian pharmacologist, C N Gillis, who was there. Anyway, Hughes was at Yale and he started working on perfused lung. For goodness sake. I thought, 'What do you mean? How dare you? This is my thing! Get off my patch!' and, thank goodness, he chose noradrenaline. So he did that, and we stuck to 5-HT. But I do remember that what happened next was that John Vane got sucked into this prostaglandin-anaphylaxis area. He started to concentrate on that new area, and he came away from the work that he had been doing, which was the blood-bathed organ technique in whole animals. So he moved across into prostaglandins, and then Priscilla finished her PhD Thesis, and I remember thinking to myself, 'If I'm going to stay with perfused lung and endogenous substrates, although Sérgio Ferreira had already shown clearance of prostaglandins in the lung *in vivo*, I'm not going into the prostaglandins.' I just said, 'I'm not going to do anything about prostaglandins, because that will be treading on Priscilla's toes and everybody else's toes. I'm going to get out of the prostaglandin area, I'm going to stay with this particular set of substrates...' So I did 5-HT, and then we did some work with Moussa Youdim on MAO (monoamine oxidase) and 5-HT, and then we started branching out into different conditions; for instance, the oestrus cycle.

**TT: The oestrous cycle is something I want to talk to you about later.**

MB: Well, we had a look at the 5-HT and phenylethylamine metabolism in lung in the oestrus cycle, and one of my PhD-students did some work on arachidonic acid, the disposition of arachidonic acid. It might have been obvious to other people, it certainly wasn't obvious to me. But if you put arachidonic acid through the lung, it's all retained. None of the biological activity comes out. Very little of it even comes out as prostaglandin. I thought, 'What's going on?' We had a look, it's all in phospholipid. It goes into phospholipid within minutes. It's sucked up in there and then it just gets converted straight into phospholipid; a part of it is triglyceride, most is phospholipid. And so that was a bit of fun, and then we had the influence of the oestrous cycle on that aspect. Then I did some work with diabetes and the effects it had on the lung, because we know the vasculature goes bad during diabetes, type 1 diabetes. Type 2 wasn't really recognised in those days.

**TT: And this was the rat model?**

MB: Yes, mostly. But we also were doing at this stage some human lung, because we managed to get some human lung samples, from operations. A lot of this work was done by a young woman who came from Iraq, this was Firial Al-Ubaidi. Somebody had been out to Iraq to teach a course, and she wanted to get out of Iraq and do a PhD in this country. To a great extent this was because she had married across boundaries, one of them was a Shia, one of them was a Sunni, and even in those days, this was not considered to be correct. So she came out of Iraq and went to George's, St George's Hospital Medical School, to work with a urology surgeon, whose name escapes me at the moment - a very competent urology surgeon. Because she was going to use the same bioassay method we used - the cascade bioassay - in her assays, she came up from George's, at Tooting, to learn the bioassay with us. And I was asked to teach her about this bioassay technique. Then she went back to Tooting.

And I don't know, a year, 18 months later, she appeared in the lab and said, 'Mick, can I come and do my PhD with you?' And I said, 'Oh, crumbs! What is going on?' And she said, 'Well, he's never there, he's always busy surgoning and cutting people open.' And he really wasn't fit to do a PhD with.

**TT: These things happen.**

MB: It happens. He was a very good surgeon and that's exactly what he should be doing, he shouldn't be a PhD supervisor. So finally she did her PhD with me. She picked up her tent and came up to the Royal College of Surgeons, and she did some very nice work on human lung and the differences of arachidonic acid between human, guinea pig and rat. This is another story of inadvertence, if you like. If John Vane had put arachidonic acid through the lungs, perfused lungs of humans or rats, he would never have seen the fancy thromboxane activity that he called RCS (rabbit aorta contracting substance), because these lungs don't make it; well, very little compared with the guinea pig lung. In those two (species of) lungs, you can't see it.

**TT: That's rat and human?**

MB: Not through a rat and human lung. If you put it in a guinea pig lung, all you ever see is thromboxane. And it's very difficult to get an anaphylactic reaction in rat lung, because rats - I believe - show most of their anaphylaxis in their gut. That seems to be their organ of anaphylaxis. Although humans, of course, show anaphylaxis in the lung, and I think I did some human passively sensitized lung, and you get really minimal amounts of this RCS material. When you do anaphylaxis with the guinea pig lung, you must stand back as this RCS leaps out at you in a great wave; it is so obvious and so potent. If John had used some other species for lung anaphylaxis, the results would have been very different. So that was another variation of the perfused organ: the species business, which sounds a bit boring, but I always said, 'Look, you need to know if there is or if there isn't a difference, because most of us aren't actually interested in the life of a guinea pig or the life of a rat. We want to know what happens to people.' And so I had another two or three PhD-students; I remember there was one student, Ramani Chelliah, who worked on adenosine, and ADP [adenosine diphosphate] and ATP in the pulmonary circulation. And there, when we looked at human, rat, guinea pig and hamster lung, there was a different pattern. The human and guinea pig were closer than human and rat. And the hamster, of course, was quite different.

**TT: So by this time you're a well-established member of the team.**

MB: I was stuck in there, staying away from prostaglandins completely and looking at all these other substrates, natural substrates, in the lung; how they were handled, the systems, how all the enzymes are arranged. And then going into certain conditions, and I was thinking very seriously, and I had put out a couple of grant requests for doing this work in perfused heart, because nobody had ever looked at the effects of the coronary circulation, what happens to luminal transmitters in the coronary circulation? Do they persist or what happens to them? And the other thing was, there were conditions quite frequently where, if you put especially high amounts of 5-HT, say a microgram per millilitre of 5-HT through the pulmonary circulation, all the 5-HT was mostly destroyed, but you got prostaglandins out at the far end, in the pulmonary vascular outflow. A sort of injury type of reaction. I remember talking to Merton Sandler about this, and Merton said, 'Mick, I think that's got to do with migraine.' And so we tried a little bit, but we never got anywhere. I think we probably weren't doing the right sorts of experiments to analyse, organise, this release as it were, the release of prostaglandins, because it was prostaglandin-like materials from the lungs when they had a high dose of 5-HT. But that project never got very far. But Merton Sandler always thought there was something there trying to emerge, an idea trying to break out somehow and be properly formed.

**TT: And this period we're talking about is the mid to late 1970s?**

MB: Yes. My last attempts to get a grant were trying to apply the metabolic approach to the coronary circulation, but none of those attempts actually succeeded. So I never did the experiments, which is rather sad.

**TT: When you went onto the University's books, as it were, as a Senior Lecturer and then a Reader, did that involve you in teaching very much? It was an unusual set up at the Royal College of Surgeons?**

MB: Yes, the critical difference was we did not have any undergraduates. But in my early days, even before I joined John Vane's grant, I used to teach undergraduates, going as a demonstrator down to King's College,

Strand. But that never went any further. But there was always teaching to do at the College, because the College required us to teach pharmacology to surgeons and to dental surgeons, neither of whom really wanted to hear about it. And we also taught pharmacology to anaesthetists, who were much more receptive and much more with it, and needed it much more, of course.

**TT: But were these doing Membership?**

MB: They were doing Membership, or Fellowship as it happens, yes.

**TT: Oh yes, of course. It's different in the College of Surgeons from the Physicians, yes.**

MB: It was called the Part 1 Fellowship. And eventually, I don't know, late, mid-1980s something like that maybe, I was actually in charge of organising the whole of the pharmacology, and eventually the whole of the timetable for the Part 1 course that the Institute (IBMS, Institute of Basic Medical Sciences), as it was called, put on for the anaesthetists. Because they got physiology of course, they got physiology and they also got a bit of biochemistry, and they also got anaesthetists to come in and talk about, for instance, gas exchange and so on. There was John Nunn who came to teach, from Northwick Park, very nice man. He would come and talk to them about respiratory physiology. I remember after one of his lectures I was talking to one of the students and he said, 'Why didn't we have lectures like that about respiratory physiology?' I said, 'You did actually, but they didn't mean anything to you, because you hadn't seen a patient.' I said, 'You listen to him and you see what he's doing, he's illustrating what he says by what you see. Now if you've never seen somebody puffing, panting, whatever it is, you don't know what it means. But once you've seen it, you've seen somebody go a little bit blue, and you've done something and it's changed and you think 'Oh!' Now that's why and you now see the sense of what he's talking about.' And then, I would ramble on to another project I used to have about revising medical education [laughs]. They should go out, like nurses, they should be put on the wards as soon as possible to observe. And then they come back and you say, 'Right, we've seen this. This is what is happening to make that happen to the patient.' And it means so much more. Otherwise, for instance, kidneys and renal function, there's too many numbers, it doesn't mean anything.

**TT: Yes, it's soul-destroying for them. Going back to this entire period, so you're actually at the Royal College of Surgeons for nearly 30 years?**

MB: 28 yes, essentially, nearly 30 years. I moved several times, yes. I moved down the corridor in fact towards, but I never got really into, the John Vane end, but [laughs] the closest I got was at the edge of John Vane's end. Because you see there was this rather peculiar time, there was an interregnum when Gus went to Cambridge. We'd been to a meeting and I was in a cab with Gus, John, me, and we were driving back from a lecture, near Holland Park, probably the old Queen Elizabeth College. And Gus said, 'Oh, Mick, I've just been offered the chair at Cambridge.' And I said, 'But you're not going, are you?' He said, 'Why not?' I said, 'You'll hate it, Gus.' 'Oh,' he said. Anyway, then he said something which was difficult, because John (Vane) was there and they were friends, but there was always a little sort of rivalry, and he said, 'Of course they only offered it to me because I publish in *J Physiol*.' And I know that John as an act of principle stopped publishing in *J Physiol*. Stopped submitting because he had submitted one paper and they'd said, 'This isn't suitable for *J Physiol*.' and he was so incensed he said, 'I'm never going to publish in this journal again...' For a scientist, at times, he had the most subjective responses. And he never did publish in *J Physiol* after that. So I thought, 'Oh, that has the ring of truth about it.' Because it wasn't just that he didn't publish in *J Physiol*, it was the fact that not publishing in *J Physiol* was a sign that John was a sort of an outsider. And I'm pretty sure he felt that. A lot of what he did was trying to get to the other side of that barrier, which was a sort of scientific snobbery barrier, I think. Obviously, it was nothing to do with achievement; he just didn't know the right people. Here was Gus - mind you it's something he must have carried as a burden - the son of a Nobel Prize winner. But at least he had the sense not to stay in physics and to get out and do medicine, where the comparisons would not be that easy to make. But I'm sure there were lots of people, people of his parents' time, and maybe later on, who expected him to succeed to a Nobel Prize like his dad, and all that. So I think that for Gus, when he went to Edinburgh and then he went to Oxford, all these places where his father's reputation would have made it easier, it wasn't a hindrance. Whereas John Vane was much

more a working class man trying to make his way amongst all these toffs. And I think that was always a part of his life, his scientific life. It probably actually just incited him to do...

**TT: Do you think that drove him on.**

MB: That's it. The fact that he would frequently get into areas in which he had no background and make a good discovery and people would discount it really because he was a newcomer. 'Where does he come from?'

**TT: Going back to the conversation in the cab...**

MB: Anyway, this was the comment Born made, and I just said to him, without thinking, 'Surely you're not going,' because I couldn't imagine anybody at the time less capable of handling a full-sized Department. Already four years, five years may be, I'd known him, and it was quite clear that this guy had ideas like other people had dandruff. They would come out of him all the time; ideas. Most of them were absolutely crazy, we'd never, ever, have the time to follow all of them, some weren't even testable. Gus had a second-in-command, David Mills, D C B Mills, related to the Bertram-Mills Circus somehow, maybe Bertram was his uncle. David had come out of Glaxo or Allen Hanbury's, and joined up with Born's group. He was Born's second-in-command and David would act as filter, saying, 'Yes, yes, yes, yes,' and noting the 1% which was testable and filtering all this stuff. And the thing about Gus, he never actually resented that. He just generated ideas all the time. You had to ride with that, you had to develop that in order to make things work in his lab. And most people did, because they realised that it was worth waiting for the real cracker. But you had to be able to sort the ideas out. But he, he was completely unsuited to running a Department, and he didn't actually run this (Royal College of Surgeons) Department in any real sense. Born was the Vandervell Professor, so he was the titular head. And John became Professor by title, but he was not the official person responsible. But I think, in fact, between them they ran it, and I think John Vane ran it much better. He probably did most of the organisation. We had a highly competent, old fashioned, Chief Technician in: Geoffrey Langston, who had come out of M&B (May & Baker) in the old days, from Dagenham. And he would commute in every day from Upminster and keep us, and the finances, more or less straight and organised. Because Gustav had no idea about money. He would just say, 'Oh, yes, we have it or we don't,' without any reference to the reality. So when he told me he was leaving, all this was in my mind. I don't know how many other people said that to him, but I was talking (very many years later) to a chap who was his technician at the time, and he said, 'When Gustav told me that he was going to Cambridge to be Head of Department, I said to Gustav, 'Prof, do you really want to go? All the organisation, those committees?'' And Gus said, 'No, but you can't refuse the job.' So he went and he was a disaster, absolute disaster as a Head of the Department. And, in fact, he didn't get back to normality until he came back to be Professor at King's College, Strand, but that's a different story. But it was very clear.

**TT: So Gus left...**

MB: Gus left and then everybody, at the end of that, said the Royal College of Surgeons would advertise, because they had to, but then they really have to appoint John, John Vane. Well, they never advertised and everybody thought the Royal College of Surgeons were scoundrels. And it was said - rumoured - that this was because everybody who could possibly have taken it, none of them were medically qualified and they (the Royal College of Surgeons) couldn't stand the thought of a non-medically qualified person holding a Chair at the Royal College of Surgeons. The fact that Gustav had, the only clinical practice that he had, had been his House Officer years, but then he'd gone out on National Service, but I don't believe he'd ever seen another human patient after that.

**TT: But it didn't matter, he'd got the right bit of paper.**

MB: He'd got the right bits of paper. Humphrey Rang is another example of an excellent pharmacologist who was minimally medically-qualified; he didn't even do his house jobs. He told me one day that he actually hated the idea of patients. He just wasn't interested. And in fact he didn't want to be a doctor. I think he wanted to read biochemistry. He could not do biochemistry at UCL for some reason, so he did medicine,

instead. This came out at the 80th birthday celebration for him.

**TT: So what happened to Gus' Chair, with the Vandervell Chair?**

MB: Well, it remained empty. It remained empty and everybody could see that John was fairly not pleased about all this.

**TT: I think that's probably putting it mildly.**

MB: The other thing that was perhaps even a bigger smack in the eye for John, and I don't know why they did this, I'm pretty sure he applied for the UCL Chair, to succeed Schild. And the electors in fact chose Jimmy Black. As it turned out, that wasn't a very good choice. I don't know the basis of that choice, except that he too was medically-qualified, Jimmy. But I did wonder, UCL generally speaking isn't quite so hide-bound, they always prize themselves on being "modern and more God-less" than Kings College - that kind of thing, and therefore looking to the future and not traditional. But, anyway, Jimmy Black got that job, and John must have been fairly, fairly fed up, because it was quite clear that the Royal College of Surgeons would not move a finger to give him official command of the Royal College of Surgeons' Department, although he was running the Department now, because there was nobody but him. And it was booming. Everything was going well. The aspirin papers were just published, and everything was up and running, and things were happening. And then he got offered the job at Wellcome, and he went. He was clearly going to be given *carte blanche* and he probably said, 'I am going,' because when he went down there, he took Rod Flower of course, and he took several other technicians, some of them who weren't from his section at all. Geoffrey Blackwell was, in fact, Gus Born's technician.

**TT: But he'd stayed in the College, and not gone to Cambridge with Gus?**

MB: Yes, he stayed in the College when Gus had gone. And there was Gerry Higgs, who went to Wellcome also; probably about half a dozen people. And, of course, at that time Salvador Moncada had just arrived, and Sérgio Ferreira was there as well. They all went with JRV.

**TT: And they went to Beckenham?**

MB: They all went to Beckenham. In fact, John said to Sérgio, 'I want you to come to Beckenham with me, and essentially set up a Department of Prostaglandin Research, which will be you and all the guys from the College who are coming down, and we're going to do some prostaglandin research which is going to be basic science, nothing to do with drug discovery necessarily. It's going to be basic science.' That was the Unit which eventually did the prostacyclin work. But that was set up as completely separately, because the Pharmacology Department of the Wellcome drug company was run totally separately from this private, JRV enclave. And the first person in charge was Sérgio Ferreira, and then Rod I think was temporarily in charge for a short while. And then Salvador came back from Honduras, because after he finished his PhD he went back to Honduras to try to set up something like this in Honduras. And he was there I think for at least two years, but it was impossible. So he wrote to John and said, 'Can I come back.' John said, 'Sure.'

And he became chief of the Prostaglandin Research Department, and I'm not sure at what stage, but very soon after that I think, Rod went to Bath. But I stayed at the Royal College of Surgeons, John did actually come and talk to me (when he was going) and he said, 'Mick, I haven't asked you to come down to Beckenham, because I don't think you're coming.' I said, 'You're right, I'm not coming.' I stayed.

**TT: So what happened to those of you who were left, because suddenly you'd lost both the big chiefs, as it were?**

MB: As soon as John said he was leaving, they advertised the post.

**TT: The Vandervell Chair?**



MB: Yes. And who did they appoint? They appointed a non-medic. G P Lewis. But he came out of industry, he came out of what was then Ciba-Geigy at Horsham. G P Lewis was appointed, and it really was very clear that the Royal College of Surgeons did not want John Vane. That was it, you couldn't have made it clearer. They couldn't fire him, but they just didn't want him around. And John was immensely, he was surprisingly loyal to those people (in the Royal College of Surgeons) in his own way. He called his mystery prostaglandin substance "RCS", he called it "rabbit aorta contracting substance", but he did it purposely. And I just thought, 'The Royal College of Surgeons are mean-minded and completely stupid,' but that's what happened.

**TT: What kind of atmosphere did that leave behind when Lewis arrived?**

MB: Lewis arrived. Well, Priscilla and me and Lawrence Youlten were left. Lawrence had come when everybody was there. He was essentially a physiologist; he was a medic but he'd been doing physiology for years. He was interested in the microvasculature. He came from the London. He'd qualified in Guys, gone to the London. Lawrence came and worked with us, and he must have been there for some time. He came when Gus was there and stayed, he didn't leave until after Graham Lewis had arrived. And, of course, when Graham came, he built up his own group. Tim Williams was one of the people who came with Graham, or very soon after Graham, and there was a new nucleus, which was formed around the various people that Graham had brought. But Graham and Priscilla worked together for some time, and then she got involved in the SRS-A [slow-reacting substance of anaphylaxis] story much more, and that project developed quite separately from G P Lewis. But she obviously stayed there till the end.

**TT: What about you? How did this affect you? Did you just carry on?**

MB: I just carried on, you see. Now I was more or less self-funding, and I became a Reader during his time. As far as Graham was concerned, we didn't really get on, but we could keep out of each other's ways. He was, well, he wasn't a difficult boss, but he wasn't anywhere near as inspiring as the other people. He was very different. So I just kept on doing my bit, and he did his bit, and we just kept out of each other's way.

**TT: How long was he there for?**

MB: Well, John went in 1973/74 to Wellcome, and so Graham came very soon after that. Yes, well Graham was there until 1990 or so. He had two heart attacks. One while he was in India with the British Council, and then he came back and he was told to take it easy, and then he fell down on the street, Portugal Street, right outside the College more or less. And luckily, because he was buying something from a fruit stall, the person behind him in the queue, happened to be to his good fortune, a theatre sister. And she saw him fall down and she leapt on his chest and kept him alive until the ambulance came. And he then said, 'Well, two attacks means I really am too ill; I really have to retire.' And the College at that time was going through all sorts of financial convulsions, and they were essentially looking to close the entire Institute down from end to end. They had already not reappointed two or three Professors, and it was highly likely that they would not reappoint another one in Pharmacology when Graham left. At that time, they'd also - slightly in desperation - had hired Stan Peart to be the head man of the Institute, to try to sort out the future of the Institute: who should stay, who shouldn't stay, which Department should have the funding, what should the funding level be, and so on.

**TT: Do you have any insight as to why they appointed a senior physician to this?**

MB: No, absolutely not. Because the previous Head of the Institute had been an anaesthetist. That was at a time when the anaesthetic Faculty was still a Faculty of the Royal College of Surgeons. And no, everybody was extremely surprised. I can only surmise that they did it to show that they weren't against the fact that there was no surgical research going on in the Institute, which was true. It had always been true. There was very little surgical research, because physiology, biochemistry, pharmacology research don't immediately apply to surgical problems. There was a time when the Physiology Department had a Professor who was interested

in biomaterials. Now that you could see would have an interest for the surgeons. But that didn't happen - that was never taken up by the College. And he had to do nearly all his biomaterials work outside the Royal College of Surgeons, in collaborations. So I don't know why they hired Peart. Perhaps it was the fact that he was a famous name, FRS, medically-qualified, he had run a Department before, a research Department and all that sort of stuff, so they thought maybe he is appropriate. And, perhaps, he was available. It may be that they asked somebody else and he said 'No,' because it was fairly clear that the College were looking to close as much as they possibly could of the Institute, without shutting it down completely.

So that was the background when Graham said, 'I'm sorry, I'm packing it in,' and so they appointed Priscilla as the next Vandervell Professor. And you could see that she was well qualified for the position and so she became the third, only the third, Vandervell Professor. Because Gustav was the first, the second one was Graham, and then there was Priscilla. And very soon after Priscilla became Professor, she - well - admitted to having cancer. She was a very private person, and it must have cost her something to admit to being infirm, and being infirm enough to interfere with her professorial duties. She asked me to go with two of her students to a BPS (British Pharmacological Society) meeting in Rome, where they were presenting data, because she wasn't fit to go. And she was so punctilious - punctilious sounds bad - she took her responsibilities so seriously. She would never have done that unless she was really not fit. So you knew she was, by that time she was quite seriously ill. And she got worse. I have a suspicion that the College always thought, 'Well look, she's cracking up. This would be quite a good time to close the whole Department,' which is what happened.

**TT: And how many of you were in the Department by then?**

MB: Probably about 20 people at that time. We had reduced in size, yes, but there had been quite a burst of activity in Graham Lewis' time. There was Tim Williams and he did some very nice work, and he went on to Northwick Park and then Imperial, and John Westwick was there and he did a lot of work on chemokines, and he went on to Bath. Tim Williams went up to Northwick Park to that rather ill-fated Clinical Research Centre, which was meant to be the answer to all our problems. We had Luke O'Neill from Dublin, who has just now (2016) got his FRS, who did his PhD with Graham. And so things were still happening. The time with Graham - although Graham's own research didn't really get anywhere - he managed to attract some good young people, which is as far as I'm concerned, is one of the functions of a Department Head. Not necessarily to be the leader himself, but to get people in who will do the good work, and he certainly had the ability to bring in a whole team of people like that. So it was still quite a live place, but, of course, by the time Graham left, I think Tim Williams had gone already, John Westwick left, so there was a shrinkage and the whole of the Department was smaller than it had been. Then Priscilla got ill and it was clear that she wasn't going to go anywhere except unfortunately downhill, and the answer to that was essentially to hive off everybody they possibly could, wherever they could. So Priscilla's unit was more or less empty, I think there was only one person who remained in science (Tony Sampson), and he went down to Southampton to work in Stephen Holgate's Department. And there was Helen Cox who came in from Cambridge with her own programme, but she and her little group and her money went to Tommy's, and she's still at Kings doing very well. So she should. For me, I was of an age when they said, 'Well, what about taking early retirement?' And I wasn't at that time funded, and I was having real difficulty in finding funding.

**TT: Your appointment was a University appointment still?**

MB: Oh yes, I was still a Reader then, and it became clear that the Royal College of Surgeons were going to close up the Institute anyway. At this time (circa 1993), Tim Williams had come down from Northwick Park and the CRC [Clinical Research Centre], and was at the National Heart and Lung [Institute] as Professor of Pharmacology there. And Tim said, 'Well, why don't you come and spend some time with us? I can give you an honorary appointment,' and make it all, sort of, semi-legal. So I decided, because it looked as though, I was 58 or so, I was not a very good prospect to be employed by anybody else. As an academic I didn't have any money and I was getting towards the end. And then I talked to John Vane about this impending event, and he said, 'See what you can do and if you can't do any better, come along and be a consultant for three days a week.'

**TT: At the William Harvey Institute?**

MB: At the William Harvey. And that's what I did for four to five years. I was there and then I was two to three days at Tim's Department at the National Heart and Lung [Institute]. In both places I wasn't really able to do any work, physical scientific work, but I helped out, teaching, all sorts of things. In the William Harvey I was editing people's manuscripts, trying to make sense out of them. So that's what I did and I also, at the same time, joined a rather curious group of external consultants who was headed by a chap who had been many years ago a physiologist, but had translated himself into a "medical educationist" - somebody who essentially worked for drug companies, producing reports and analyses and sometimes arranging meetings and all that. I can't remember what was the phrase was he used. It wasn't all MedEd (medical education), there was also some amount of promotional work. That's right, and he'd also help with the launch of a new drug and suggest ideas like that. But it was mostly a science input; he wasn't really into the PR [public relations] side of it so much, although that of course happened. So I worked with him; he was already, I suppose, about 70-years-old at the time.

**TT: What was his name?**

MB: Jim Nurse, and he had a company, yes. Well, he said what he found particularly difficult, the way life is, so many bio-scientists come to the age of 65 and they just say, 'That's it, finished, bye.' And he said, 'All that knowledge, all that experience, and nothing happens with it. Just at the age of 65 it disappears. It's not accessible to anybody because there's no way to access it. It's gone.' And he said, 'That's why I like to employ people who look as though they're about to retire, because they have all this information there, they know how to write a report and things like that, and they very often have the time to do that. They've been teaching so they know how to explain things. There are lots of reasons to use all that resource.' So I did that for quite a few years.

**TT: For those years you had what nowadays is called a "portfolio career"?**

MB: Yes, you're right. That is in fact a portfolio career from 1993 onwards. And one by one my other portfolios closed up, and the company folded, because it became too old; Jim Nurse's contacts became old and so he no longer had the introductions that he used to have, and it was difficult to get contracts and so on. He had himself other sources of income, so he wasn't starving, but he couldn't pay us essentially. And John Vane's arrangement came to an end after four to five years. I said, 'It's fine.' I appreciated very much the fact that he had done it to begin with, because in 1993 I was mentally fairly, not disturbed as depressed, because being made redundant academically is a fairly unpleasant experience.

**TT: I know, it's facing me in six months' time. Yes, I can understand it, it is most depressing.**

MB: But, anyway, he was very good. And mind you, I did actually give him his penny's worth, so I wasn't just freeloading. And so I stayed with Jim Nurse and I've retained a title at Imperial and a share in a desk and a chair, and most of the work I do now is for the journal (*British Journal of Pharmacology, BJP*). And that occupies me adequately. In fact, my wife keeps saying it occupies me too much. Apart from the actual physical editing of manuscripts, it's also about trying to change perceptions of people of what and how to write a paper. It is getting a bit more like that. So it occupies my time.

**TT: It is atrocious that all that experience and knowledge is just lost. I'm facing this myself, and you think, 'How crazy. You are in a position where you can still contribute, and you're still getting ideas and getting people talking and being involved.'**

MB: There are the collaborations that I have in Brazil, I have input from them about ideas and the experiments that they do. And I say, 'Hang on, that's an interesting experiment, but have you tried this or something else like that,' and then we argue about what to do next. It keeps me alive.

**TT:** When you're talking to young people you can find they're talking about something that was actually done already, 30 years ago, but they haven't picked up on that PubMed search or something, and it's something you remember, or even did?

MB: [Laughs]. I have only one experience like that. I saw a paper in the *BJP*, this is in the days when it was all printed. I can't remember why I looked at it, but it turned out to be quite interesting because it was talking about bradykinin metabolism. And they came to the same conclusion that I had come to about bradykinin metabolism some, I don't know, 15 years earlier. I had written just a short communication in the *BJP* about the metabolism of bradykinin saying that there were effects of the potentiating agents was not entirely due to effects on metabolism. There was something else that we don't know about, and some other way in which these peptides were interacting with the bradykinin receptor, in order to potentiate the outcome. And this paper 15 years later, in fact it turned out to be one chapter of a PhD Thesis. As I knew one of the co-authors, a chap called Pramod Saxena - worked on 5-HT. And so I just wrote to him and 'I don't wish to be bitchy about this, but I'm just saying that you might just remind the author, if anybody is still involved, that I'm very glad that you came to the same conclusion as I did 15 years later, with a different approach and using different compounds.' So Pramod, in fact, got his own back on me. He said, 'Right, I've told the author about this. By the way, would you like to be examiner of his PhD Thesis?' [laughter]. So there I went, it wasn't difficult to do, but I thought that was a very good counter-stroke on his part. And so I got a trip to Utrecht and, of course, it's one of the European Thesis defences, everyone dresses up in fancy (academic) dress, the whole business. And you semi-parade around and it's all done very formally because, of course, all the real analysis and criticism has already been done. Because the Thesis, the little booklet that you see is the real final version. But, of course, they do have to stand up there and defend their Thesis in a foreign language, which is nothing that I could do. And it really is a public examination. I've only been to two others that are like that, or three, I think - two in Finland and one in Sweden - and it's a really public examination. Everybody's there, mother, father, the dog, the cat, it's not just Professors, everybody's there.

**TT:** Your interest in oestrous cycle. How did that start?

MB: That started because I was working with my chum, Moussa Youdim.

**TT:** Was he a visitor to your lab?

MB: No [laughs], it was a classic piece of Sandler. Merton was a great fixer, and much of what he said, and he behaved, was to fix people up as collaborators, and it worked. He said he felt like an old fashioned Jewish marriage broker, introducing people to each other who he thought would fit or work, whatever it might be. And it was at an Oxford meeting of the BPS; it was, I think, the reception, which took place in the Museum. So there we were wandering around between the dinosaurs with a glass of wine, and all the nibbles were on the ground floor. And Merton breezed up to me and said, 'Oh, Mick, you have to meet this chap, Moussa Youdim. He knows all about MAO and you're dealing with 5-HT metabolism. He'll sort you out, I think you could...' So I said 'Hello' to Moussa, because he more or less invented MAO-A and -B. He was working at Queen Harlot's, *i.e.* Queen Charlotte's - he hadn't moved to Oxford yet. No, on more mature consideration, he was at that time at the Oxford Department with Grahame-Smith and Frank somebody, who went to Sheffield.

**TT:** Woods?

MB: Indeed, Frank Woods. In that Department, Richard Green was there at the same time and they were working on the central effects of 5-HT, and so I talked to Moussa, and I think we started the collaboration by looking at selective MAO-A and -B substrates and inhibitors to dissect a little bit more what was going on in the lung. And so there were a series of papers on that. And then I think, I'm pretty sure, some time in that first set of collaborations, he produced a paper showing the variation of MAO over the oestrous cycle, not just in what you might call sexually steroid responsive tissues, uterus, ovary and so forth, but also surprisingly, to me, in liver. And I said, 'What do you think it does in lung?' He said, 'I don't know,' and that's how it started. A very long standing chum of mine was working in the Zoology Department in Oxford, Harry

Charlton (H M Charlton).

**TT: The neuroendocrinologist?**

MB: Always referred to himself as a sex fanatic [laughs]. I've known Harry ever since he first came up to Corpus. Anyway, I knew Harry was into the oestrous cycle *etc.*, and so I said, 'Harry, how easy is it to do these smears and follow the oestrous cycle in rats?' And he said, 'Dead easy.' So I sent my PhD-student up to Harry's lab and said, 'Can you go up and learn how to do the smears?' And he did. And it was really, thank goodness, quite easy. So we then had a look at all of the substrates we'd done before in normal males, we went through the different substrates, and it was quite clear that, much to my non-expectation, there was an effect of the oestrous cycle, on a totally non-target organ, which was the lung. And this was true in different ways for 5-HT and phenylethylamine. You could measure two things: you could measure uptake and you could measure metabolism. It was clear that these two processes weren't being affected in the same way over the stages of the cycle. The outcome, of course, was a net effect of the two processes. So uptake and metabolism can have different responses to the oestrous cycle.

**TT: How widely accepted or influential were your results from working on the oestrous cycle?**

MB: I have no idea if anybody ever looked at them, and we published one paper - I think it was clearance of prostaglandins - in *J Endocrinol* [*The Journal of Endocrinology*]. That was a disaster because we submitted, we waited, and we waited, and we waited, and we waited, and eventually after some two or three months I wrote and said, 'What's happened? What's going on?' And he said, 'Oh, we're handling it.' Then we waited another two months and then he said, 'Oh, I have to apologise to you, the Editor in charge of this has had some trouble, some ghastly thing that had happened.' Somebody had a brain storm or gone to Kenya or something, I don't know. And our manuscript had been the one that he'd been handling, and it had just disappeared from their knowledge. In those days nobody was fully computerised, it was all done by hand. It must have been submitted at least a year earlier if not more, and eventually they said, 'We'll take it and we won't make any comment about it, we'll just take it.' I think they would otherwise have been minded to reject it [laughs]. Well, of course, we don't know. If I had, for instance, been able, although I wouldn't expect this to happen, show a correlation shall we say with gas exchange or respiratory function of some sort or other, I think people, at least the lung physiologists, might have twitched a bit. But I would be very surprised if that were the case, because I don't think what we used to call the pharmacokinetics of the pulmonary circulation is concerned with what we call lung function. It's concerned with the whole body function. It's looking, if you like, outwards towards the rest of the body, not inwards towards the lung. And that's why it's set up that way. And so I wouldn't expect lung function to be particularly altered by things which change the metabolism, the metabolic pathways. And it was very difficult for me to explain. Now, people accept the fact that there is this type of pulmonary metabolism going on, but nobody really knows what to do with it. And I have a strong suspicion that I don't really know what to do with it, either.

There was one clinical chap, who actually got one of his PhD-students to look at propranolol - not metabolism, but the clearance of propranolol, because it's not metabolised even though the metabolic enzymes are there. It has a particular clearance pattern. I thought this was great, because this was actually being done in whole animals. He wanted to do it and he was going to do it in humans, but he didn't, and eventually he resorted to doing it in whole animals, which was an approximation to the truth. But then he stopped that line of work and so I said, 'Are you going to do anymore?' He said, 'Mick, I don't know where it's going. I can't see where it's leading me.' And regrettably it's true. Before him a chap called Alain Junod from Geneva, who had been involved in lung metabolism studies, like mine, had said, 'Mick, what's the point of all these systems in the lung?' For instance, if you put ATP into the pulmonary circulation, it is transformed, right down to AMP [adenosine monophosphate] and adenosine. But, of course, if you put adenosine through the lung, 50% of it's taken up into the lung. The two ways you can clear adenosine activity: one is to suck it up into the cell, and the other one is to use adenosine deaminase to metabolise it. Now on thinking about it, taking it up into a cell as a form of inactivation is very good, because you can phosphorylate it and bingo, you've got ATP! So, it's good news for the cell. It's a vacuum cleaner effect. It's actually a very sound way of handling it, much better breaking it down with deaminase, when it becomes a

waste product and you lose it in the urine.

But why? Why should there be these systems? I don't really know but my goodness me, they're there, there must be some purpose to them. And one of the purposes, I suspect, is to deal with platelet aggregation. Because when those platelets aggregate you release 5-HT, but in the degranulation process, gallons of ATP are also released, and we now know that ATP itself is a potent pharmacological agent, as is ADP. So if you want to have a system like an aggregating platelet, but you want to keep that system under control, you want a mechanism that will clear both 5-HT and ATP as rapidly as possible, because if you don't you're going to get more aggregation. And in the pulmonary circulation, as we know, getting lots of aggregating platelets is bad news physiologically. So it may be just that in this particular circumstance, it's a mechanism to protect the pulmonary microcirculation from a microembolus progressing into a macroembolus. That's one possibility. In order to pursue that I should really look at perfused hind limb and see if that is any different. So we come back to an old question: are we seeing something which is specific to the lung and therefore to where it is, it's processing the whole blood volume, or is it just a local effect which depends upon the particular vasculature you're in?

**TT: And is that species-specific?**

MB: The ATP breakdown isn't. There are differences between rat and guinea pig and human lung, but those are at a lower level, the metabolism of AMP. We, Ramani Chelliah and I, actually published a paper on that in the *QJP*, *Quarterly Journal of Experimental Physiology*. But we had a bit of an argument because somebody else, Tom Eling - whom I knew quite well from RTP (Research Triangle Park, North Carolina) - worked in the environmental health section of the NIH [National Institutes of Health] at RTP. We'd met because we'd been working on prostaglandin and the lungs and of course, environmentally, lungs were important. And he'd done some work on ATP or one of the substrates, at this moment I can't entirely remember, but his results were quite different. We even repeated his use of albumin and I thought we did it fairly well. Ramani was a very good experimenter, but we still got a different answer from Tom. I still don't know why we had got such different answers. But there was really a lot of activity towards all aspects of adenosine metabolism in the lung, and it always seemed to be in the endothelial cells, as far as we could tell, because if you had endothelial cells and cultured them, they would reproduce the effects you saw in perfused lung.

**TT: I had no idea that the lung was so interesting. I have to say, I know very little about the lung.**

MB: It really only becomes important because it's sitting over the entire blood volume. The whole of the cardiac output goes through, actually has gone through, the lung. And either you say it doesn't make any difference, or it's a hangover of some other time when it was important and isn't important any longer. That may also be true. I just don't know right now why there is a difference between one particular finding in the lung and, say, the hind leg and it would be probably too boring to get a grant for, to actually work it all out, work out all the possible variations that you should test, because at the end of the day you may not be able to come out with a nice story. All you could say is, 'Hey guys, it's different,' and people would say, 'Of course, it's different, it's a different organ, bloody stupid...' The question is why is it different? It's important that there is a difference. These things, they're still to be done, but nobody really wants to do them.

**TT: I think on that point we probably have to finish. Thank you very much Mick.**

[END OF TRANSCRIPT]

#### Further related resources:

1. Overy C, Tansey E M (eds) (2013) *Drugs Affecting 5-HT Systems*. Wellcome Witnesses to Contemporary Medicine, vol. 47. London: Queen Mary, University of London.
2. Reynolds L A, Tansey E M (eds) (2003) *The Recent History of Platelets in Thrombosis and Other Disorders*. Wellcome Witnesses to Twentieth Century Medicine, vol. 23. London: Wellcome Trust Centre for the History of Medicine at UCL.

3. Tansey E M (intvr); Tansey E M, Zarros A (eds) (2016) *Flower, Roderick: transcript of an audio interview (14-Apr-2016)*. History of Modern Biomedicine Interviews (Digital Collection), item e2016068. London: Queen Mary University of London.
4. Tansey E M (intvr); Tansey E M, Zarros A (eds) (2016) *Flower, Roderick: transcript of a video interview (14-Apr-2016)*. History of Modern Biomedicine Interviews (Digital Collection), item e2016069. London: Queen Mary University of London.
5. Tansey E M (intvr); Tansey E M, Zarros A (eds) (2017) *Bakhle, Mick: transcript of a video interview (10-Aug-2016)*. History of Modern Biomedicine Interviews (Digital Collection), item e2017125. London: Queen Mary University of London.