# Australian Academy of Science - Science education
# Interview with Professor Peter Bishop

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Professor Peter Bishop, a visual neurophysiologist and winner of the Australia Prize in 1993, was interviewed for the Australian Academy of Science's *Video Histories of Australian Scientists* program in March 1996. The interview was conducted by Dr Max Blythe of the Medical Sciences Video-archive of the Royal College of Physicians and Oxford Brookes University in the United Kingdom. Here is an edited transcript.

You can order the videotape from us for $65.50 (including GST), or borrow it from Cinemedia.

List of edited transcripts.

## Going to school

*You were born in Tamworth, New South Wales, in 1917, into a fascinating family with interesting roots. Would you like to tell me about your early life?*

I was the second eldest of five in the family. I don't remember anything of Tamworth but I remember going to the local state school in Waratah. When I was seven my father became district surveyor in Armidale and so we moved there. I went to the demonstration school (the primary school) in Armidale, where I had to spend an extra year, in year 7, to avoid being in the same class as my elder brother. After that I went to the Armidale High School and subsequently to Barker College from '32 to '34.

*You made a brave trip coming down to Barker all alone, very independent.*

Armidale is about 360 miles north of Sydney. I was 14 when I went to the boarding school, travelling down on the train on my own to Hornsby and carrying my bag from the station to Barker College. Hornsby is an outlying suburb of Sydney. 1932 was at the height of the Depression so the school was very small – only 78 students. The schooling that I had was not very good and I sat for the leaving certificate twice to get a Commonwealth scholarship. My family couldn't afford to send me to university unless I got a Commonwealth

scholarship but after an extra year at Barker College I gained the award of an Exhibition, as they were called.

## A medical student in the family

*Let's talk about that family that couldn't afford to send you to university without an Exhibition, and about your background.*

My grandfather, Herbert Orlebar Bishop, came out to Australia in 1870. He got a job in Queensland as a line repairer, repairing the telegraph wires. Subsequently, working for the postal service, he went to places like Cunnamulla, which is a long way west of Brisbane, and Port Douglas, way up north in Queensland. So my father, who was born in 1877, had virtually no schooling at all until, at the age of 12, he went down to the state school at Yeppoon, near Rockhampton. Subsequently he went to Toowoomba Grammar School, but he never had any real education. He had an apprentice type training for entry into the New South Wales Lands Department.

*But he was anxious for you to get a good education.*

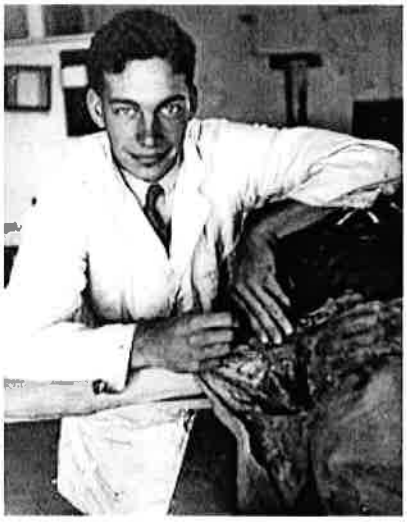
My father was very helpful. But after the age of 14 when I went down to Barker College I saw very little of my family. I think my mother had the bigger influence on me, though.

*She pushed you towards medicine when you might have chosen another career?*

Yes. Maths and physics were my best subjects and I wanted to do engineering, but my mother was keen for me to be a doctor. She had no idea of research work or of what one did at a university. She imagined that I was going to be a specialist in Macquarie Street. It didn't turn out that way, though.

## An awakening interest in neuroanatomy

In 1935 I started off in medicine at Sydney University. Of the subjects at university, the only one that attracted me was not physiology but neuroanatomy. When I dissected a brain, holding it in my hand, I realised that this had belonged to a person who was just like me, had the same thoughts and feelings as I had, even though now it was just a lump of meat. That experience determined me to do something about finding out how the brain worked. From then on there was never any doubt that I would work somehow on the brain.



*As a medical student, aged 18, in the anatomy dissection room of the Old Medical School, University of Sydney*

Towards the end of my medical course I wrote an article called 'The Nature of Consciousness', which had an important influence. It was published in the *Sydney University Medical Journal* and it drew me to the attention of Dr Abbie and Professor Burkitt. I became very friendly with the people in the Department of Anatomy. I graduated in 1940. Incidentally, we graduated early because of the war, which had started in 1939. I was known to be interested in the brain and so when a position of resident medical officer in the neurosurgical unit came up, with many doctors now in the armed forces, I got the job. It was then that I became associated with Gilbert Phillips and with Sir Harold Dew, who was the Professor of Surgery and a founder of neurosurgery in Australia.

That began my work in neurosurgery. Incidentally, since neurosurgery was part of the neuropsychiatry block, I was also in charge of psychiatry.

*What did you think of the medical course at Sydney?*

Unfortunately, medical courses are not very inspiring to do, because there are so many subjects and you have to study them at a fairly elementary level. All you really learn during the medical course is how to use the terms. You really don't know much until after you've graduated and started working in hospitals. The anatomy department was my main interest as far as the medical course was concerned.

*Would you say that nowhere was there any course or tutor to prepare you for what was actually to come? You did it alone, gripped by your own ideas and following your own intuition?*

That's right, for my whole career.

**Marriage and a naval episode**

*In surgery you met a rather special nurse.*

Yes, Hilare Louise Holmes, who was a theatre nurse in the neurosurgical theatre. We met afterwards and frequently later on. We were married in February 1942. I had been called up to the Navy in January 1942, as a surgeon lieutenant, and so immediately after we were married I went to sea.

*Surgeon Lieutenant, Australian Navy, 1942*

First of all I served on a cruiser, HMAS *Adelaide*, mostly in the Indian Ocean. Then I served on the destroyer *Quiberon*. I was with the Royal Navy then, in the Far Eastern fleet. We used to convoy ships from the Atlantic round the Cape and up to Mombasa. After about 18 months with the *Quiberon* I came back to Sydney and then went up to Madang, on the north coast of New Guinea.

*During those war years at sea, your fascination with the brain continued and you kept a brain under your bunk, I think.*

That's right. I found that time during the war very tedious, just convoys down the Atlantic and up in the Indian Ocean, so I did a lot of dissections of brains while I was still in the Navy.

*So the brain was your field? You got to know the brain pretty well?*

Extremely well, but that's all I really did know. I knew relatively little about the other parts of the body but I knew quite a lot about the structure of the brain – not too much about how the brain worked, though. That's quite a different thing.

**To England on a postgraduate fellowship**

*You applied for a fellowship with a postgraduate medical group, but in the meantime you worked with Dew and Phillips.*

I got a fellowship to study in Oxford under the neurosurgeon Sir Hugh Cairns, but I couldn't get back from Madang until about May 1946. Then, for a very short time, I was in Dew's department at Sydney University. With my wife and

children we sailed for England in July 1946.

*With two children by then, so you had your hands full.*

By then we had two girls, aged two and a half and one. We went non-stop from Fremantle to Southampton. The men and women returning from service in the Far East were segregated in the ship. I slept in a 14-berth cabin and my wife had a cabin to herself because she had these two small children. When we got to Southampton, we had to get to Oxford. I had never been in London before and I thought I'd go across country, but that was a horrendous trip. The ship berthed at 10 o'clock in the morning and we didn't get to the hotel in Oxford until after 11 o'clock at night.

**Beginning in clinical neurology**

Cairns imagined that I was there to train in neurosurgery and he said to me that I should do a bit of clinical neurology first. So he arranged for me to go down to Queen's Square, and I became clinical clerk to Sir Charles Symonds.

*Did you have time to get to know Cairns?*

Not really. He and Lady Cairns seemed not too pleased about bringing two children as young as they were to Oxford, which was full of returning servicemen. We couldn't get suitable accommodation there and they seemed to feel that we were a bit of an imposition. Anyhow, we got a cottage high on the Downs, in Wiltshire. On the survey map it was called Bishop's Barn. I gather it may have been a tithe barn, possibly connected with the Bishop of Winchester, I presume, although I don't really know.

*Once settled, you found yourself working at Queen's Square.*

Yes, I had nine months at Queen's Square, an absolutely fascinating place. There were people I'd read about but never imagined I'd be associated with them. I was a clinical clerk, with the job of seeing the patients immediately they came in and writing up their history, so as to be ready to tell the honorary, Sir Charles Symonds, all about the history when he came. I learned a lot about clinical neurology.

*Symonds would have been a rather prestigious figure.*

Oh yes. A large crowd would follow his ward rounds.

**A close encounter with neurophysiology**

It was at Queen's Square that I met George Dawson. I went down into the basement one day and saw him working with electronic equipment to record EEGs, electroencephalograms, from the scalps of myoclonic patients. I asked

if I could go in to watch what he was doing, and it wasn't long before he said, 'Well, why don't you be the patient?' He was trying to see whether it was possible to stimulate peripheral nerves and make extra-scalp EEG recordings. He had electrodes stimulating my ulna nerve at the elbow and wrist, and recording electrodes on my scalp. It got a bit painful.

George Dawson was a very remarkable person. He was one of the very early people doing electroencephalograph recordings in a clinical setting. He was interested in myoclonus because people with myoclonus have tremendous jerks, or muscle spasms, which cause big potentials in the brain that can be recorded from outside the skull. But my evoked potentials were sufficiently large to be recorded too. He was a specialist in averaging techniques. He would average a whole lot of cathode ray tube traces: of course, the more you average the more you build the trace up. But my recordings from the skull were sufficiently large that they didn't need averaging. It was the first time in the world that anyone had stimulated peripheral nerves and recorded from the scalp of normal subjects. When he wrote this up in 1947 he used the recordings from my scalp for his paper.

*The influence of George Dawson was pretty seminal.*

Absolutely. When I went in there to watch the experiment and then became the subject of the experiment, I realised that I wasn't cut out to be a neurosurgeon but that this was the sort of work I wanted to do. I could see then that this was a way of getting a lead on what the brain did. So I decided then not to go back to Cairns in Oxford but to do laboratory work.

**Applications and referrals**

I went and saw Professor Carmichael, who was the director of the research at Queen's Square, and asked him whether I could get a job possibly working with George Dawson. Carmichael asked me, 'How old are you?' I said I was 30. He asked, 'What research work have you done?' and when I said I had never done any, he said to me, 'I think it is rather late to be considering a research career at the age of 30.' He should have given me a job: I was being paid for from Sydney, so it wouldn't cost him a penny, and I was keen to work. Anyhow, he didn't see any way in which he could take me on.

He referred me to Lovett Evans, who was Professor of Physiology at University College, London. He was very nice and friendly but he said to me, 'You want to do neurophysiology, but I'm going to retire next year. Neurophysiology these days is all valves and electronics and I know nothing about that.' Because I told him that neurophysiology was what I wanted to do, not neuroanatomy, he told me to go and see J.Z. Young, who had just come down from Oxford to take the chair of anatomy at University College, London. So I duly went and saw J.Z. and he took me on immediately.

## A first neurophysiology project

J.Z. Young gave me a big empty room up on the 4th floor of the anatomy building, which seemed to me to be part of A.V. Hill's biophysics research unit, and he suggested the project I should work on. Apparently he had read that you could train rabbits and get electroencephalographic changes in the brain as a result of the training, and he asked me to do something about confirming it. I was 30, I had an empty room, I knew nothing about electronics, and that was my project.

*You started to do something about electronics, though, quite quickly.*

Oh, yes. I obviously had to do something about it, so I went to Northampton Polytechnic three nights a week for two years to learn electronics. But of course I couldn't wait – I had to start building my equipment. Fortunately I had the help of Eric Harris, in the biophysics research unit, who used to come in each morning, spend half an hour or so with me and then whiz-off. He was remarkable. He was building a mass spectrometer, even making all his own valves. But unfortunately mass spectrometers became commercially available almost immediately after he got his instrument to work. I learnt a lot from him. In fact, as a result of all that I wrote seven papers about electronics, some in collaboration with Harris, and they were published in the leading journals. But I worked pretty damned hard, as you can imagine.

*There was a bit of that engineering background coming out.*

I suppose so. By that stage I'd realised that the project that J.Z. Young had set was beyond me and it was not very suitable, so I thought I'd use the rather primitive DC amplifier that I'd made to see if I could record potentials in the frog's tectum (mid brain). I started by introducing electrodes to try and see whether there are different steady potentials between the cellular layers in the optic tectum. But as soon as you damage the nervous system you get big potentials, and the big potentials I recorded were due largely to damage of the nervous system rather than the function of the optic tectum. Also, the electrodes I used were metallic, being steel electrodes, and you get big electrode potentials because of the difference in potential between the saline solution and the metal. Anyhow, that was a flop but it did enable me to start using the very primitive DC amplifier that I had developed.

## What to study next

I was determined to go back to Sydney, but I thought that the hospital there would not be very thrilled if I worked on frogs so I had better work on a mammal. The mammal of choice is the cat or the monkey but very little work was being done then on monkeys. Because I'd worked on the tectum (which is concerned with vision) in the frog, I thought I would start working on the visual system in the cat. I looked up the literature to see what people were

doing on the visual system in the cat and I found that at that time nearly all the neurophysiology work in the world was concerned with nervous conduction (conduction along nerves) and synaptic transmission (transmission through nuclei). There was virtually no systems physiology, looking at how the brain as a whole worked.

In the literature I looked at the work of two people, Bishop and O'Leary, who came from Washington University, St Louis. They were using electrical stimulation of the optic nerve. Their interest in the optic nerve was not how it worked in vision but rather what sort of nerves it contained, what their conduction velocities were, what their fibre diameters were.

*So they were charting the properties of a mixed nerve?*

That's right. Bishop and O'Leary were colleagues with Gasser and Erlanger, who had got the Nobel Prize just a couple of years before that, in 1944, for their work on the conduction in different fibres in a peripheral nerve in the frog. What Bishop and O'Leary were doing now was trying to see what different fibres there were in the optic nerve, which is the main nerve from the back of the eye into the brain. The optic nerve is a central tract, not a peripheral nerve, and this was the first study of conduction velocity in a central tract. So that's why I started using electrical stimulation of the optic nerve.

By that time I had developed the DC amplifier to a much more sophisticated level and I determined to keep on working on the amplifier design, and finally the paper was published in America in the *Review of Scientific Instruments*. But during this time, as well as publishing these papers about electronics, I did manage to do a bit of neurophysiology research. I worked on the synaptic transmission in the lateral geniculate nucleus, which is a way station between the optic nerve and the cerebral cortex, and the recordings that I made from the lateral geniculate nucleus using microelectrodes were subsequently published in the *Proceedings of the Royal Society*.

*During the three years of your fellowship in London, you changed direction dramatically from what you went to do, yet no-one ever questioned that. You went your own way and did anything you wanted.*

That's right. I was in London for nearly four years and I had this fellowship for the first three years. The postgraduate committee in Sydney had imagined that I was being trained as a neurosurgeon. I decided not to do that. Off my own bat, without referring it to the committee in Australia, I went to Lovett Evans and subsequently to J.Z. Young, and then I reported what I had done. And they accepted all this. They didn't query it, which is very surprising considering that I could have stayed in England all my life and never come back to Australia. They were very tolerant and sympathetic. I don't think that would ever happen now.

## Researching in Australia, with student assistance

Towards the end of my time at University College, the National Health and Medical Research Council, in Australia, gave me £1000 to buy bits and pieces to bring back to Australia. When I came back to Australia early in 1950 I went into the Department of Surgery at the University of Sydney, because Sir Harold Dew had been one of my main mentors in getting the fellowship in the first place. He gave me four empty rooms and I started building everything all over again. I had brought some equipment from University College, London, but it was not much, mostly bits and pieces.

What happened then was very important. I realised that I would have to have people to help me. A new degree called the Bachelor of Science (Medical), BSc (Med), had just been introduced, in 1949. Arriving back in Australia I thought this was wonderful, and although I wasn't a member of the Faculty of Medicine I put up a proposition to the faculty that I could take some of these students. In the first year, 1950-51, I took four, and of course I had to work enormously hard to get some equipment made for these people to work on.

The first work that we did was the same electrical stimulation of the optic nerve as I'd been doing already in London, because it gave me the opportunity of studying the properties of the fibres in a central tract, which could be quite different from a peripheral nerve. Incidentally, one of those four students became a neurosurgeon and subsequently dean of the faculty.

One of the other students was working with me on a stereotaxic map of a particular part of the brain, namely the thalamus, using cadavers. As soon as bodies came in to the mortuary at the medical school, whenever they came in, even on Sunday, I would go in there. We'd take the bodies out, bore little holes in the skull and put needles through the brain. We'd x-ray the skull so that we could see where the needles were, and then perfuse the corpse with formalin to harden the brain. A day or two later we'd take the brain out, and we could locate the particular part of the brain because of where the needle holes were. We never published that. It was not as professionally done as it was subsequently in other parts of the world, but that was the first such work that was done anywhere in the world at that time.

*You gave your new unit a rather impressive name.*

Yes. Without asking anyone we simply put the name on the door: Brain Research Unit.

*These students, who were doing this fourth year study for one year, for the Bachelor of Science (Medical), were a tremendous help to you.*

That's right. They were a tremendous help to me. They worked very hard. I couldn't have done all this on my own. All this time too I had to build the

equipment. I had a technician but I had to tell him a lot about the electronics.

*You were soon publishing.*

Oh yes. Every year from then on I was publishing something in international journals, mostly in the *Journal of Neurophysiology*, an American journal. Nearly all the work I did for the first four or five years after I came back concerned electrical stimulation experiments of the optic nerve.

*And you were looking at signals in the geniculate nucleus and charting that in a more and more refined way. Then you got promoted and everything changed.*

**Professor of Physiology**

I had become a senior lecturer in 1951, and then in 1954 I became Professor of Physiology, beginning work as head of the department in 1955. That was a tremendous change for me. Administration never worried me but I had a tremendous teaching load to cope with. The department consisted only of myself and two others, and about 800 students. We had courses in the Faculty of Medicine, Faculty of Dentistry, Faculty of Science, Faculty of Veterinary Science. We had courses for physiotherapy, speech therapy, occupational therapy and all the different postgraduate courses – diploma of dermatology, obstetrics and gynaecology and so on.

*1957. Staff of the Department of Physiology, University of Sydney, on the occasion of a visit by Professor Bernard Katz. Professor Bishop is front left, next to Professor Katz.*

Being responsible for all these courses, and with just two other academic staff members, I engaged a lot of fairly recent graduates who were starting out in medical practice. We had a large number of them as part-timers. At the beginning, there were over 200 students in the second year of the medical course, quite apart from all the other students, and every year after that another 100 students were added. At one stage I had 620 students in the second year of the Faculty of Medicine and about 400 in third year, as well as students in dentistry and all the other courses. We finished up in about 1962 having 1500 students doing physiology. I still did my research work but I had to do it at a somewhat reduced level than before.

*You were concentrating still on these Bachelor of Science (Medical) people but you started to appoint research fellows as well.*

In 1952 I had two research fellows. One was Jim Lance, who subsequently became Professor of Neurology at the University of New South Wales, having started the first department of neurology in Australia. He came to me as a research fellow and worked with me on the properties of the fibres in the pyramidal tract, which is the big motor tract coming from the cerebral cortex down to operate all the muscles. In addition I had all those BSc (Med) students, one of whom was Jim McLeod. He subsequently became Professor of Medicine and is just now at the stage of retiring from the University of Sydney. Another one, Bill Levick, is now a Professor at the Australian National University, in the John Curtin School of Medical Research, and a Fellow of the Royal Society. He became an expert in dissecting single fibres in the frog or toad sciatic nerve.

*So in the 1950s you made enormous progress from those four rooms to a massive teaching load and a total kind of development of research that was against all the tides and the pressures of teaching.*

I just liked doing it all. I worked damned hard, of course, but that never worried me.

*During that time you started to appoint one or two significant senior lecturers.*

That's right. I got the chair in 1955, and in 1956 I appointed two senior lecturers. One was Paul Korner and the other was Dr Liam Burke. Both subsequently became professors. They were both in London at that time. Paul Korner was working with Professor McMichael; Liam Burke was working with Bernard Katz. Bernard Katz had the laboratory opposite me at University College so I became very friendly with him during my time there. In Australia those two people helped me enormously, but the student numbers grew even faster than the number of staff members. The university wasn't terribly sympathetic. I tried to get a quota introduced – after all, to finally have 1500 students doing physiology, something had to give.

But towards the end of the 1950s Prime Minister Robert Menzies set up a committee of inquiry into tertiary education in Australia, and this committee recommended that there should be another medical school in New South Wales. As soon as the second medical school was started at the University of New South Wales, the whole question of introducing quotas at Sydney University could be tackled. The big problem then, especially with a Labor government in power at the time, was that to be turning away students from a university was not very acceptable. So that at first the university wasn't too keen on introducing quotas in the medical school.

**The neurophysiology of vision**

*With a fairly burgeoning department and all kinds of research interests going along, you took a world trip.*

In 1958 I was invited to go to a conference in Paris in honour of Henri Piéron, who was the leading psychologist in France at the time. Going to Paris became a trip round the world. From Paris I went to Denmark and Sweden, across to England and up to Edinburgh, and then over to America. I went to Johns Hopkins University, in Baltimore, especially to see Steve Kuffler, who was an Australian and well-known for his very important work in vision. It was at Johns Hopkins that I met Hubel and Wiesel.

Hubel and Wiesel were just starting out on their career together (subsequently they got the Nobel Prize for their work) and I watched them doing an experiment using a multibeam ophthalmoscope. That's a very complicated instrument that Kuffler had used in 1952-53 to study the cat retina. He was the first person to determine the receptive fields of retinal ganglion cells in the cat. The multibeam ophthalmoscope was an instrument with which you could look into the cat's eye and direct a microelectrode under direct vision to a particular part of the retina.

To see Hubel and Wiesel using the multibeam ophthalmoscope to stimulate the cat's eye with spots of light made a considerable impact on me, because for the first time I realised that the visual system was there for seeing things, not for stimulating the optic nerve electrically. It was for seeing objects in the external world, so the thing to do was to work with the intact eye. You can study important properties by stimulating the optic nerve electrically but the more important and interesting thing is to study how the visual system works, how animals are able to actually see things. That's why Hubel and Wiesel used the multibeam ophthalmoscope. When I went back to Sydney I determined to stop doing the sort of work I had been doing and build a multibeam ophthalmoscope. It took me about 18 months to develop this equipment, for which I had to learn optics and all about lenses.

When we finally got the multibeam ophthalmoscope to work, I realised that although it was good for what Kuffler used it for, it was very constraining and just not suitable for the sort of thing I wanted to do. In fact, what Hubel and Wiesel did was to throw the multibeam ophthalmoscope away and actually wave things in front of the cat and record the impulses in the brain. Very simple!

*But they did that after you had left and brought the idea of the multibeam ophthalmoscope back with you.*

Yes. By the time they published their paper in 1959 (a tremendously important paper at the very beginning of their career) I had realised that the multibeam ophthalmoscope was an unsuitable instrument for the sort of work I wanted to do. All you do is put an anaesthetised cat in front of a screen, move objects on a tangent screen and then record from the impulses in the brain that the animal is actually seeing the objects. At about that time I had moved from the

Department of Surgery, where I began all this work, to the Department of Physiology, in a different part of the university. I had new laboratories built in the Department of Physiology, and by then I knew that I should use these screens and not the multibeam ophthalmoscope.

The paper that Hubel and Wiesel published in 1959 was virtually the beginning of all work on the neurophysiology of vision. Before that time brain research was largely done neuroanatomically - you would cut out a part of the brain and see what could be done without that bit of brain. Visual neurophysiology effectively began in 1959.

*I think there was also a very exciting work written on what the frog's eye tells the brain.*

That was a very seminal paper by Jerry Lettvin and colleagues. Its impact was that if you wanted to work on the visual system you should use objects that the animal would be interested in, rather than electrical stimulation. It was a big watershed. They didn't follow it up much, but Hubel and Wiesel went on and did tremendous work.

**Studying visual discrimination and stereopsis**

*In the 1960s your whole work was turned over to charting the course of the transmission of impulses in the visual system, through the geniculate nucleus towards the cerebral cortex?*

We started off by plotting the projection of the visual field onto the lateral geniculate nucleus, finding where the different fibres go to in the nucleus. To do that, I realised, I would have to know much more about the eye itself and how it forms an image, and that was the real beginning of my work on the visual system. That's when we started to study the cat's eye in detail and I developed, with my colleagues, the schematic eye for the cat. A schematic eye is a mathematical model of an average eye. That had been done for the human eye by Gullstrand, way back before the First World War, but we were the first to prepare a schematic eye for any animal.

*And that was absolutely essential in showing the relationship between visual input, optical stimulation, and what was coming through to the geniculate nucleus.*

Well, you have to know what the optic nerve gives to the lateral geniculate, because the optic nerve joins the eye to the lateral geniculate nucleus. That was the beginning of the work. In the late 1960s I became interested in stereopsis, which is the ability to see in depth, to see that one object is further away than another object. We started single cell recording from the cerebral cortex - the visual parts at the back of the brain, the occipital lobe. Hubel and Wiesel had already done this as well. What was new was the realisation that the two eyes

send impulses up to the brain that, by coming together on a single cell in the striate cortex, could form the basis for stereopsis. We started by studying the properties of the receptive fields. A receptive field is that little patch in the visual world – the outside world – that each cell keeps a watch on. Each cell is concerned with a little area in the visual world – that's its receptive field. The impulses from the two eyes go back to a single cell (the same cell) in the cerebral cortex, so that in effect that cell in the cerebral cortex looks out through both eyes at a little area we call a receptive field, and its special job is to report to the rest of the brain what is happening in that little area.

*That little view of the world.*

Yes. What the cells in the brain, in the cortex, do at that stage in the visual system is not to record seeing an actual object but rather to report to the rest of the brain the individual features of that object – geometrical properties such as lines and edges, corners and so on. A cell in the brain looks out through both eyes at the two receptive fields, one for each eye, and the cell's job is to report individual features of objects in those two little areas, which have to have exactly the same properties because they have to report the same features of the external object - they must be capable of recording a line at a particular angle, and edges and so on.

What we did in the 1960s was to study what happens when the two receptive fields come together. So, if cells in the cortex are going to report a particular feature in the external world, the two receptive fields have to be in register. They can't be separate because the cell would be reporting different features. What we did was to study how the responses of the cells in the cortex change as a result of the two receptive fields being in register.

Furthermore, in stereopsis or depth perception, a cell has to be able to report that, when the two receptive fields come into register, the feature of the object is closer to or further away from the fixation point, the point that the animal or human is actually looking at. It can do this with extraordinary precision, as a result of a property called receptive field disparity. When the two receptive fields are a bit out of register, the brain can tell the change in the visual angle that occurs. The human brain can do that to about 10 seconds of arc. In laboratory conditions humans can even do it to 2 or 3 seconds of arc. That's quite an incredible property. The human brain can tell when these two receptive fields are in register and when they're out of register even by 10 seconds of arc, and that 10 seconds of arc represents an image difference on the retina of the two eyes of about 1 micron, which is one thousandth of a millimetre and not much greater than the wavelength of light. Light has a wavelength of about half a micron. To do experiments to determine these things required very high precision work.

**Interpreting the visual system and hence the brain itself**

*Were you putting electrodes into single cells?*

We recorded from single cells in the brain. We recorded extracellularly, not intracellularly, with tungsten and glass electrodes. By pushing the electrodes into the cerebral cortex you can get right next to a single cell and record the electrical currents that are actually flowing in the extracellular fluid. They can be quite biggish potentials (most of them are microvolts but they can be up to a millivolt) and by recording from the cell you are able to determine what is happening to that cell – whether it's firing or not firing. When it's firing strongly you know that the two receptive fields are in register and you can tell that the feature must have been either further away from the fixation point or closer to it, and that's how we got the lead on stereopsis.

*Does memory play a part when we're looking at distant objects and so on?*

No. There are various kinds of depth perception. Memory plays a part in some kinds of depth perception but not at all in the kinds I'm talking about now. And that's terribly important, because we know virtually nothing about memory or how memories are stored. No, this kind of depth perception takes place in the very early stages in the visual system. People often think that because the animal is anaesthetised none of this can happen, it can't see anything. That is true, but of course a lot of the early happenings in the retina, optic nerve and cerebral cortex all take place before consciousness. To have consciousness you've got to have memory. When you're conscious of something, you're recognising something, and that means you must be remembering something. So memory and consciousness go hand-in-hand.

Memory and consciousness are problems that are far too difficult for me to work on at this stage. We know very little about the nature of consciousness. Even though I wrote a paper about it as a student, we know practically nothing about the nature of consciousness and we know very little about the nature of memory. So all these events that I've been describing have happened prior to consciousness. They are hard-wired. You don't need a learning procedure to do it.

*To summarise: stereopsis has been the great core of your work since the later '60s and you're still working on it, in a total commitment of nearly 30 years to looking at units of visual discrimination in the optical cortex areas and trying to trace what is happening from the eye back to the responses on that cortex.*

Most of my work has been done in the striate cortex as the first receiving area in the cerebral cortex, but I have been interested also in the lateral geniculate nucleus.

*And you have never stopped being interested in that particular relay.*

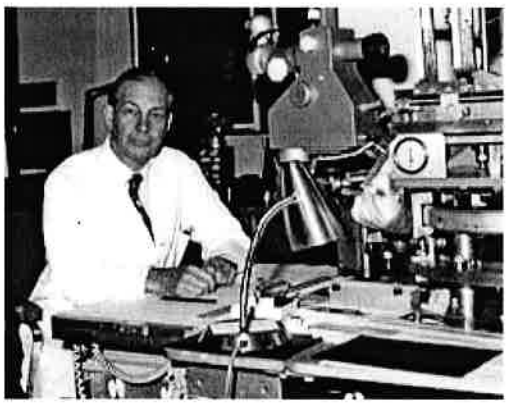
Well, in retirement I still have an appointment at Sydney University and I go

up one day a week to my office there. But I don't do laboratory work anymore; I just do what I call thought experiments. I read the journal papers and then try to work out how the brain must have done these things. Although I don't know much about the higher cortical levels, I certainly know a lot about the earlier stages in the brain, so I try to work this out. I have two papers in press at the moment, for international journals. But the thought experiments that I've been doing may not be very welcome because I've proved that a lot of the work that's being done needs to be changed a little.

**Technical contributions to research**

*In your 30 years of research, there have been great strides in getting the electrodes and working up the techniques. There was a lot of engineering involved, a lot of electronics.*

In 1967 I succeeded Sir John Eccles at the John Curtin School, and I didn't have to do any teaching from then on. That was a very big break. The John Curtin School, at the Australian National University, was in the Institute of Advanced Studies, which had been set up in the late 1950s as a place for training PhD students. Of course, it never worked out that way, but that was the idea. I was very fortunate to arrive at that stage because Canberra in 1967 was still a pretty isolated country town and so the John Curtin School had to have its own workshop to do everything, including electrical wiring, cabinet making, fitting and turning. So there was a very big workshop available to me, with lathes, drilling machines and so on, and I had the opportunity of developing all the equipment that was necessary in the main workshops. I was fortunate too in having two very able technical people who were specialists in electronics and fitting and turning for fine-instrument making. We finished up having seven fully-equipped laboratories. I had the ability then to invite people from various countries in the world to Canberra, and we had quite a number of people coming out each year to work with us.



*Professor Bishop in his laboratory in the John Curtin School of Medical Research, Australian National University, about 1980. He is shown at the plotting table used to record activity of nerve cells in the brains of experimental animals.*

The work, particularly in relation to stereopsis, became more and more technical and complex. We'd already shown how, when the two receptive fields of a cortical cell come together in register, the cell responses change dramatically. It is this change in the response that enables the brain to decide

whether a particular object feature is closer to or further away from the fixation point. It's quite remarkable that the properties of the two receptive fields of each cell in the cortex are so much the same. The receptive fields respond, say, only when an object moves from left to right, or at a particular angle and so on. It is essential that the two receptive fields of a given cortical cell are responding to - reporting – one and the same feature of the object in the external world. It would not be much good if each receptive field was responding to a different object feature. To be responding to the same object feature the two receptive fields of the cortical cell must have exactly the same response properties.

**Spatial frequency**

We also studied in much greater detail what is called spatial frequency analysis. The properties of the receptive fields can be thought of in spatial frequency terms. I will try to explain this a bit more: Gabor, who got the Nobel Prize in 1971 for studying this in hearing, pointed out that if you want to be sure you have heard, say, a middle C, you have to listen to several cycles of the middle C. Then, however, you can't say precisely when that note occurred, so there's a big problem in saying both when a note occurred and what the note was.

You have exactly the same thing in the visual system, which is able to work out the spatial frequencies – visual cycles now, instead of auditory ones. To respond to a line or an edge you need very high spatial frequencies – that is, many visual cycles. If you cut out these high spatial frequencies the object becomes blurred; it doesn't have any high-frequency properties. But of course the more spatial frequencies you're able to respond to, the less certain you are where that object was, so you have exactly the same problem in knowing both where an object is and what it is. You have to make a compromise, and that's what we studied in great detail.

One way of thinking of stereopsis is to consider the spatial phase - that is, how the spatial frequencies are in phase or out of phase. In sound they have to be in phase for us to be certain of middle C. The high frequencies have to be in phase or out of phase, just like the spatial receptive fields in the visual system being in register or out of register.

We weren't the first people to think of the visual system in terms of spatial frequencies. That had been done mainly in Cambridge, in England. One of the people doing it was Janusz Kulikowski, who came out to work with me and that's how we started doing the spatial frequency analysis in the John Curtin School.

**A life's work**

*You have made a most enormous journey into vision, looking at it from so*

*many angles over so many years. It's become your life's work and day by day you still research it. Yet when for weeks on end you would work slavishly at the bench and on experiments, your family to some extent had to pay a price for that.*

They did indeed. The experiments we did in the John Curtin School would go on for four and five days at a time, when I would work every day from 9 o'clock till midnight. I didn't go on after midnight simply because if I worked all through the night I couldn't go on the next day. So I went home at midnight, had a few hours' sleep and was back in the lab again at 9 o'clock ready to go on with the experiment. But I liked the work.

*Once an animal was anaesthetised and the job was going, the animal was a dying entity and you had to work on, I suppose.*

The big problem with biological work in higher animals is that, if it is intended that they not recover from the anaesthetic, from the time you anaesthetise the animal it is actually dying. You have to keep the animal in as good condition as you properly can. There is quite a lot of technique and work required to maintain the fluid balance and the fluid levels of the animal, keep its temperature up, make sure that it is able to breathe in and out and all the rest of it. But the animal is gradually dying, and the condition of the animal finally determines when you have to stop.

*Let's return to the dedication of your family, particularly Hilare, your wife, who supported you in an incredible way over the years.*

Yes. My wife virtually ran the home for me. She not only managed all the financial affairs – gave me pocket-money, in effect - but also entertained all the people that came from abroad to work with me. She would meet the families, see them into their homes, and have the fridge stocked ready for them. And of course we entertained the whole department – 60 or 70 people – once or twice a year and always at Christmas. She saw to all that.

*It was a family business?*

Yes, but the price was that because I worked so hard they saw very little of me. All my children have done well. My elder daughter married a cardiac surgeon; sadly, he died just recently. My second daughter is a senior member of the immigration department in Canberra. My son Rod, who is now 40, is a Sydney University graduate. He's an emergency medicine specialist at Nepean Base Hospital.

**Beyond the bench experiments**

*You became a Fellow of the Australian Academy of Science in 1967, and a Fellow of the Royal Society in 1977. Then, in 1993, you had the great honour*

*of becoming a winner of the Australia Prize. That must have been a great tribute.*

I guess so. Anyhow, it's very nice to be recognised in that way. Sydney University have also been very kind to me. They gave me an honorary degree and also made me a life member of the Faculty of Medicine. I don't attend the faculty meetings – at my age it would be a bit pointless, I guess.

*Being awarded the Australia Prize, 1993. (From left: daughter Clare, Peter and Hilare, son Roderick and wife Margaret, daughter Phillippa and son-in-law Douglas Baird.)*

We have had many trips abroad. We've lived in Japan twice. I was at the Massachusetts Institute of Technology in '63, and St John's College, Cambridge, invited me there as an overseas visiting fellow in 1986 and we had a year in Cambridge. After I retired I spent six months at the Catholic University of Leuven, in Belgium, working with a former colleague, Guy Orban, who's now one of the leading neurophysiologists in Europe. I subsequently worked in Zurich, Switzerland, with Esther Peterhans, who had also worked previously with me in Canberra. So we've had many interesting trips. I was also on the Council of the International Union of Physiological Sciences, and as a result I attended all the international congresses from 1968 till I retired in 1977. So there were a lot of interesting outside activities as well as working in the laboratory.

**Looking back, looking forward**

*I can't see you ever actually retiring. The thought experiments still go on. Let's recall that third year in medical school when you first handled a brain and felt, 'This has got to be the destiny'. Has it been as fulfilling as you hoped then?*

I think so, but I would have liked to work on the nature of consciousness and the nature of memory. I realised very early in the piece that those are enormously difficult problems. I think they are partially soluble but not in my time, anyhow.

*Those are problems for another year.*

Another century. Certainly I feel I've done something that's on the way towards solving these major problems, but it's only the beginning. The brain is unquestionably the last frontier. We seem to know most things about the physical world now. We haven't quite got to integrating gravity with the other atomic forces, but it won't be long, I think, before that can be done. But the

brain, the nature of consciousness, is a very, very tough problem.

*It's been marvellous for me to have this opportunity to spend time with you to learn something of your interests. For all that you've conveyed to me of your work, many thanks.*

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