Professor Adrian Horridge

Neurobiologist

Professor Adrian Horridge's research interests include the role of the nervous system in behaviour. His particular specialty was in understanding natural visual processing as an engineering problem. He was the Foundation Director of the Centre for Visual Studies at the Australian National University. In addition to his biological research he has published many titles on Indonesian traditional boats.

Interviewed by Professor Bob Crompton in 2002.

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Adrian, you were born in Sheffield, England. Your father was in business there, I believe.

Yes. My father was very mechanically minded, and during World War I he was an instructor in the assembly of small arms – too useful to be sent to France. When he came out of the Forces in 1919 he started a motorcycle business in his backyard, buying old motorbikes, repairing them, painting them and so on, and then selling them. And after a couple of years he rented premises with a partner and became the Sheffield agent for Triumph, Ariel and Douglas motorbikes. He became a substantial motorcycle agent, with repair shops and also a showroom and so on for new vehicles. He used to go down to Birmingham once a year and buy large quantities of motorbikes – 100 or 200 at a time – for sale in Sheffield.

So by then the family business of earlier years had been sold?

Yes. My grandfather was the last of the family horn firm of William Horridge and Company, Stag, Buck, Horn, Wood and Buffalo Handles, and Scale Cutters, which dated back to about 1750. At Pool Works, on the corner of two main streets in the centre of Sheffield, they manufactured ivory scales for piano keys, importing mammoth ivory from Russia in considerable quantities. They also made combs, handles for knives, and all kinds of other things out of buffalo horn and deer horn, shell, tortoiseshell and so on.

After World War I, when plastics began to come in, the firm did not want to retool and start in a completely new industry. Also, because this was an old family company there were too many fingers in the pie – about 10 part-owners were taking out profits all the time – and that was not economic. So in 1921 the factory was sold (for £15,000, when you could buy a house for £100) to the Provincial Cinematograph Theatre Limited, and became a picture palace.

What about your early schooling, Adrian?

I went to King Edward VII School – junior school and then senior school – which my grandfather had also gone to. And when I arrived, there were still three masters who had taught my father at the school, so it was a very stable situation. I remember that in junior school I didn't like compulsory games, and engineered ways of avoiding them.

When I was about 12, though, World War II broke out and so school was very disrupted. Sheffield was bombed several times, with heavy damage to the centre of the town, the factories and so on. Large numbers of people were homeless. Our school, which by then had closed, became a reception centre for bombed-out families. By the time I was about 14, I had become a Scout patrol leader and was used to running camps as a quartermaster, in charge of equipment and so on, so I had to report to the school as early in the morning as possible after an air raid to give out soap, towels, nappies and other things that were needed by people who had come in on the bus. We would do our best for them – even if that just meant being a waiter, taking round breakfasts!

We had no more school lessons until about 1943, when the German bombers were no longer coming. We had some lessons in people's houses, but mostly our schooling was very thin, so I decided simply to go down to the public library and work there, because it had lots and lots of interesting books.

I spent a lot of time running camps. I used to organise Scout camps and then farming camps – potato picking, pea picking and so on. One day when I was a cook, quartermastering, at a pea-picking camp near Bedford, I was sent to the POW camp down the road to get some prisoners as extra workers. So at the grand age of 15, I signed for 30 German prisoners, marched them up to the pea field and supervised their picking. (They didn't mind a bit; they actually got paid the same as the gipsies or us or anybody else.) At 5 o'clock they finished picking peas and I marched them back down to the POW camp and solemnly signed them off, the same number as I had taken out!

In another incident that year, I was cycling through Chatsworth Park when one of the long-distance British bombers just back from Norway crashed about 100 yards away. (It killed a few sheep, there was a lot of blood.) And as the aeroplane broke up, with aero-fuel dripping everywhere, the pilot dropped out of it, walked about 10 steps and lit a cigarette! I quickly moved him off, dousing his cigarette. The rear gunner was trapped in the aeroplane, and outside it I found a guy breathing frothy, rather red blood, so I knew he had broken ribs. I laid him down and undid his collar and so on. At that moment an Army convoy of trucks came along the road. I rushed down and stopped them, telling a dispatch rider on his motorbike to get back to the village – down the road about three miles – as fast as he could and come back with the doctor. Then I got the guys in the front truck to radio back to base and sound the alarm. But I never heard the outcome of it all.

You had about two more years at school when it resumed. What were they like?

They were really exciting and vigorous. I had a friend who was very interested in chemistry, and I did lots of that at home. We used to buy chemicals that you could never buy now – metallic sodium, yellow phosphorus, acetyl chloride, thionyl chloride, concentrated or fuming sulphuric acid – and we made all kinds of interesting things, but mostly dye-stuffs and tear gases. (I was a great synthesiser of tear gases. Bromacetone is the simplest.) Some of these were used rather stupidly, but still… And I made lots of fireworks.

For those last two years of school we had three masters who were superb. They all had PhDs from about 1931–32, but a PhD did not get them a job in that time except in teaching. Barton, the physics teacher, had been a PhD student of Rutherford, in Cambridge. Having worked in the Cavendish he was well up on the beginnings of quantum theory. We learned all about the 'new alchemy': the fission of heavy elements, and bombarding light elements with nuclei and knocking bits off them, and firing neutrons. For example, we learned about calibration of neutron fluxes by using a beryllium target and then a counter, as the neutrons hit the beryllium. So while Heisenberg in Germany was trying to count neutrons, we were learning about it in school. That was very unusual indeed.

Barton had written two books for school physics and Wheeler, the biology master, had written a large textbook for school biology. The chemistry master had been a research chemist, involved in the discovery of the detergents and the sulfonated fatty acids, sulphonated hydrocarbons and so on.

With that background you ended up with a scholarship, didn't you?

Yes. And as well, I had worked in the library a great deal and I had done quite a bit on my own account. Very fortunately, we had a good bunch of bright kids in that year – we were egging each other on to a considerable extent, and I think we got 10 Oxford and Cambridge scholarships. I won a

State scholarship to Cambridge and also a college major award, so in a way that ensured my career. I just had to go with the stream for a few years!

As a child you had a deep curiosity about the natural world. Do you think that was innate, or did it come in some way from your background and early environment?

Oh, I don't think anything like that is innate. It was encouraged. We lived on the western edge of Sheffield, where the sites for new suburbs had been planned but were stopped by the war. Across the road from us was a few hundred yards of allotments for poor people who came from the city, and beyond that Ecclesall Woods stretched for miles. Then it was up to the moors, into Derbyshire. So, naturally, we all turned towards the countryside, not towards the city.

My father had a bright red MG (it was like a Meccano set) and every Sunday we would go out and picnic. I would take a fishing net and come back loaded with frogs and lizards and crayfish – all kinds of wildlife.

Then, when war broke out and rationing came in, I decided to keep rabbits. I had to find food for my 30-odd rabbits, and so every evening I was out with a wheelbarrow, collecting grass, putting food into the hoppers and so on. We lived quite well; we had rabbit meat regularly. My mother organised for the skins to be cured, and went to glove-making classes. Actually, we still have some of the gloves she made out of those skins.

As time went on, I had my own tent and spent a lot of time under canvas. And when I was 15 my father gave me a motorbike. (This gift was not without prejudice, in fact, because I rode my 250cc Ariel Red Hunter to school, and within three months my father had sold 20 of them!) So I was always out in Derbyshire, and then camping in the Lake District, which was only an hour or two away by motorbike, and north Wales. Later, when I went to Cambridge, I joined the mountaineering club and added climbing and mountaineering to my outdoor activities.

Tell me about your undergraduate days in Cambridge.

I did four subjects, which meant a 9 o'clock lecture and then a practical, a 12 o'clock lecture and an afternoon practical, and sometimes a 5 o'clock lecture – every day, with some on Saturdays also. The only time to do anything, or meet or talk to anybody, was late in the evening or at breakfast time. We had four essays to write every week, and a tutorial which ran between 6 and 7 or 8 in the evening.

Life was extremely hectic, but in a way it was all very easy, because we were young and we had a long concentration span. We had nothing to do but study – if necessary, we could work all night. And the terms were short. In the vacations I went off expeditioning and camping. In 1947 I went with a friend to the Ecole de Ski Française, in Briançon. Learning to ski there was an incredible experience for young chaps like us, because the instructors were straight out of the French Maquis and had spent the war years on skis annoying the Germans in the mountains.

Only 10 per cent of the students in Cambridge in my year had come straight from school. The other 90 per cent were ex-servicemen who had come back from Japanese POW camps or from India or France, or who had desert experience and so on. If you are surrounded – almost squashed – by ex-servicemen comrades who are five or 10 years older than you are and who have war experience, then you mature quite quickly.

There were special concessions to enable people with war service to get a university place, but not necessarily at Cambridge or Oxford. Some of those chaps were very rusty after not having done any

studies for four years, but they had to go straight into chemistry lectures and I remember explaining lots of things to my friends.

Were your interests turning towards biology in those undergraduate days?

Well, I already knew the chemistry so thoroughly I didn't have to go to organic chemistry lectures at all, although I found the physical chemistry practicals quite interesting. My tutor, Colin Bertram, was a biologist with a lot of experience in different places in the world – in the Persian Gulf throughout the war, and previously in Antarctica for three successive summers. He advised me to concentrate on biology, taking advantage of the fact that I could do the chemistry, and so I did zoology as a full subject. But in the first year I was interested mostly in biochemistry, physiology and animal function.

Once you had got your First Class Honours, what happened next?

I organised an expedition to the Canary Islands in the summer of 1950. Four of us went by train to Cadiz and hunted around in the shipping offices until we found a ship to take us as deck passengers to Tenerife. Then we took the small inter-island steamer to the island of Gomera, where we spent the whole summer doing a survey of birds.

I got back to find that I had an offer of a PhD studentship in Cambridge. But when I turned up at the lab, not knowing exactly what to expect, things were rather quiet. (This was September, and the full term hadn't started.) I went to the chief technician's little office and was given a key to a room on the second floor – and that's all I got. The room was quite empty. My key would open one of its two doors, and another PhD student, Brian Schaffer, had the key to the other door. This room was next to Carl Pantin's room, by the way. I was quite interested in the sort of things he did.

As a research student you had to find yourself a topic and also find somebody who would supervise you in it. Then you had to collect your apparatus. You built it yourself or scrounged it from somebody who had just finished or some other member of staff who had got some stuff in his cupboard, and collected things from various places in the lab such as the chemical room in the corridor, the workshop and the electronics workshop. So eventually, having assembled everything you thought you wanted, you might start some experiments.

How did you find yourself a topic?

Well, I had been on a course in Plymouth, so I just got out my motorbike and went down to the Plymouth laboratory of the Marine Biological Association of Great Britain, which in those days was an independent body but was funded by the government. This laboratory was large and well known, and had two good-sized ships with which it explored the western fringes of Europe, the Atlantic Shelf and the English Channel, kept an eye on fisheries and ocean productivity, and supplied marine animals to all the universities in England – a big collecting set-up. The laboratory published its own journal and a lot of interesting people worked there, including at least three or four Fellows of the Royal Society.

The director allowed me to work at the lab for a bit while I looked around, and I discovered that Berrill, a famous Canadian zoologist, had worked there in the '20s on chopping up marine worms and letting them regenerate the head or the tail, or whatever, until the whole worm eventually came back to normal. I thought that if I could go back and look at the problem again, using modern techniques invented since those early days, I could work out how worms regenerate their nerves. I could make a

cut in the spinal cord of the worm and let it grow across, and watch the redevelopment of the nervous system. So I did quite a bit on that.

One of the new techniques involved staining nerves, and in another lucky break I met an old chap called Alexandrowicz who had been a professor in Poland but had been given sanctuary from the war in the marine laboratory in Plymouth. As part of a complete revision of the nervous system of the crustaceans, he was staining – with methylene blue and other rather nice methods – the neuron structure of the nervous system. This is very much an art, with lots of little tricks in the technique, but he taught me how to do it. He taught me very well how to handle nervous systems in invertebrate animals, and showed me also where the literature was.

Then I went back to Cambridge, where I was able to use a phase-contrast microscope for the first time to look at transparent jellyfish, *Aurelia*, which I had collected by going on my motorbike to Brancaster Staithe, on the Norfolk coast. (The phase-contrast microscope was another recent invention at the time, and (Lord) Victor Rothschild had purchased some microscopy equipment out of his own private money.) The nerves of these jellyfish have a different refractive index, so although they are completely transparent you can see the nerves under phase. There they all were, living nerves. A very lucky break, I thought.

I spent the whole of that first year learning electronics – building electronic equipment and playing about, recording from snails and odd things that had some nerves in them. I built first of all a power pack, then a multivibrator stimulator and a variety of other stimulators with neon lamps in them, which flashed very slowly, and then a DC amplifier that gave me enormous trouble because it was totally unstable. After that I got a radar oscilloscope, which had a blue screen and fast time bases, and produced circles on the screen, but I changed the time base and the amplifiers inside so I had a new oscilloscope with a green screen that gave a long fluorescence. I had to learn a tremendous amount of stuff for the complete physical techniques for analysing nervous systems, but one result was that later whenever we had a problem in the lab I was able to solve it. Once you've made all the equipment and discovered the little details of exactly how to record nerve impulses, I suppose you become more confident.

I had help from several people in all this hard work. For example, there was good physiology across the road – Hodgkin and his assistants – and I got lots of instruction from Willie Rushton about how to make microelectrodes. Within our own lab, John Pringle was the Reader and he recorded nerve impulses.

What was the level of the signals that you had to measure?

Well, if you are extracellular you get about 10 microvolts, up to 50 microvolts if you're lucky, depending on the amount of insulation you can get round the nerve fibre. If you can get intracellular records it is quite a different story. You have 30 to 50 millivolts at about 100 hertz or so. Extracellular you've got to look at 1 to 10 kilocycles, but that helps you to eliminate hum from the mains. If you are recording slow potentials intracellularly, the enormous problem is the very high impedance, about 10 Megohms, and the hum from the mains, which is going at 50 cycles, right in the middle of the frequency range you're interested in. Not nice!

At about this time you went to Farnborough, surely a very unusual place for a biologist. How did that come about?

Well, I was doing my PhD. I had done my recording of the nerve impulse from jellyfish and that was successful. In about 1952 I wrote a thesis for a fellowship for St John's College, where I was an undergraduate. Brian Schaffer had gone to Porton Down and worked on bacteriological toxins in a Defence laboratory, but I didn't really want to do that. Mark Pryor, however, was in Cambridge after working the whole of the war at the Royal Aircraft Establishment (RAE), Farnborough, in the remarkable group who had built the Mosquito aeroplane out of balsawood. (It turned out to be radar transparent and, because the frames were negligible in weight, extremely fast in acceleration.) He rang his friend Jim Gordon at Farnborough, and eventually I went down there on an appointment to the Scientific Civil Service in lieu of military service.

I was working on new materials for the rockets which were being invented at that time – Blue Streak and so on. Rockets for space were being made out of anything from steel to aluminium to glass fibre to asbestos, in experiments with all kinds of new materials. There were big efforts on the guidance, but very big efforts on the materials. One of the things I did there was to invent the material for the venturi, the bit at the back end of the rocket where the high velocity gases come out. In fact, I was invited to join a big company making rocket parts, simply on the strength of my invention of the right way to make that.

We used to do all kinds of things. We built helicopter blades out of fibreglass, linen fibre or silica. We pulled our own fibres, we persuaded glass companies to make peculiar weaves of glass fabric. We spun helicopter blades until they bust – which makes a lot of noise! – and we filled rockets with all kinds of explosives and then fired them to see what the rocket material would withstand.

It was a very practical kind of science, and it was another major learning experience. Jim Gordon, who led the group I was in, was a remarkable man. He had done his PhD in Glasgow University Maritime Department on the construction and design of wooden ships, and had then gone into the RAE and arranged all the material strengths and done all the testing for the Mosquito. He had stayed on to make new materials, and eventually, about the time I was there, he was into the carbon fibre story. We were looking very hard at ways of using the covalent bond to maximise the strength of materials, because if you use the covalent bond then your specific strengths, dividing by the specific gravity, jump by a factor of about 10.

Among other things I learned about at Farnborough from this group of amazingly competent engineers were sandwich structures and honeycomb structures, which I think were invented there. They are still used. And we used to wind rockets with glass fibre and silica fibre, winding at a particular angle so that when you filled the rocket with explosives and set it off, the tension in the direction around the body of the rocket was half what it is in the direction of the long axis of the rocket. (The ratio of the forces is 2:1 in the wall of a cylinder.) We had to wind them at exactly the right angle so that when explosive was put inside – and so the pressure increased, as it did enormously – the thing did not change shape, and shear stresses were not a problem.

While you were at Farnborough you also had your fellowship at St John's. Were you doing any biological research?

No. There was no time for that. But because of the fellowship at St John's I had rights there: I could have a room with a bed in it, I could sleep in College, and I could have free meals whenever I was there. That was very useful and I was in Cambridge most weekends. The working week was spent in Farnborough, living in digs.

This must have been about the time you met Audrey. How did you meet her?

Well, some people would find this embarrassing, but for me it's a normal process. I decided I needed a wife, and so I purchased two tickets to the whole season for the Cambridge Arts Theatre, which put on excellent shows – some undergraduate shows but some which came up from London. Every week I had to go round and find somebody to take to the theatre, and quite a number of young ladies jumped at the chance of a good theatre ticket! (I can recommend this to anybody who wants a wife.)

One day I went with Martin Canny – a good friend who is now retired to Canberra – to a party in Girton College. Meeting this girl Audrey, I told her I 'happened to have' a ticket to the theatre and so we went to a play. Perhaps I shouldn't tell you the next bit. Anyway, there was a great hit line in the play in which some coarse woman said, 'Ah, men, they's lovely.' Audrey sort of quivered at this, and I thought, 'Gosh, there's a girl who likes men.' Oh, and I had a motorbike as well. There's nothing better than a motorbike for taking the girls for a ride.

I met Audrey in 1951 but we were not married until 1953 because she had another year as an undergraduate, reading English, and then she went to Barnett House, Oxford, to do a postgraduate degree in public administration.

When you married Audrey, did you live in Oxford or at Farnborough?

We rented a room at the back of a little house in Farnborough. The landlady was always grumbling at us and we didn't get on at all. Her room smelt. She never cleaned the place. It was awful. I left a kipper nailed on the underside of her kitchen table, and I've often wondered what happened to that.

And what after Farnborough? You had an 1851 Senior Scholarship, didn't you?

That got me back to Cambridge. And from there I started going to Naples at Easter, when it was cold in England. In fact, all the people in Europe who wanted to work on eggs of sea urchins and on development would come into the lab in Naples and fertilise sea urchin eggs furiously. At lunch – served in the lab – you could hear six to ten languages being spoken around the lunch table. I got to know quite a number of people from European labs while I was in Naples.

The Naples laboratory was supported by universities all over Europe who paid a sum and had table rights in exchange. (That is, they could send people to work there.) The lab was founded by Anton Dorn in about 1880 as a private organisation. The next director was his son, Reinhardt Dorn, and then the next was his son, Peter Dorn, so it was a family bailiwick – one of the great laboratories of the world until quite recently, with a magnificent library dating from the 1880s onwards, holding every possible journal and book and monograph you could hope to find. I spent a lot of time working in that library, reading through old literature.

A lot of fundamental work was done there. The main work that I saw was on the fertilisation story, the way that the sperm enters the egg and additional sperms are kept out, and then the process of formation of the first cleavage and second cleavage, and egg development. All of this was done on sea urchins, which was by far the most convenient material. You can get them in thousands at 9 o'clock in your lab every morning, fresh fertilisations. You can't do that, of course, with most animals.

The laboratory seems to have faded in recent times, partly because that kind of zoology no longer attracts funding and partly because universities are no longer prepared to spend money on a laboratory somewhere else.

What were you working on in Naples?

I went to work on coelenterates, on jellyfish and hydromedusae – especially the magnificent Ctenophores, a group of animals that are hardly known. Called 'comb jellies', they occur in all seas around the world. They have the most primitive nervous system of any group of animals. There is no major concentration of nerves anywhere, no 'brain', yet their behaviour is well organised and they do a lot of interesting things.

For example, *Beroe* is carnivorous and has big teeth composed of cilia multiplied up a million-fold. It puts a million sperm tails into one cell, all packed tight side by side, and makes them into teeth which are operating all the time. Another example is a little herbivore which is the food of *Beroe*. This one has an interesting behaviour: when you pass a shadow or any disturbance over it, instead of swimming upwards in the sea it turns over and swims down. And that's nervously controlled somehow.

A bigger Ctenophore, about a metre long, is called a Venus' girdle. It has waves of cilia activity that run all the way along from one end to the other as it slowly propels itself through the sea. Another one, called *Mnemiopsis*, has little bits sticking out. If one of the cilia on the ends of these detects a vibration, it expands by contracting circular muscles. It leaps out and sticks on to any copepod or anything similar that approaches it.

There are many, many kinds of Ctenophores, many of them deep-sea. I did more or less everything with them that one could think of, and published many papers.

Did that work lead to your move to the Gatty Marine Laboratory, at St Andrews?

Not directly. I was put on to that job by Pantin, who was by then Professor of Zoology in Cambridge. He had been Reader for many years and had worked in Naples for many years, especially before the war, leading to the publication of his great works in 1935–36.

The Gatty Marine Laboratory.

Adrian Horridge (front row, 5th from left) with the Gatty Marine Laboratory staff, 1962.

In the spring of '55, I was in Naples while Pantin was there. He came along one day and said, 'I have a letter from a friend of mine in St Andrews (Scotland), Mick Callan. They have a marine laboratory and he's looking for a lecturer who will work with marine animals. It seems to me that you have to find yourself a permanent appointment somewhere, so would you like me to write to Callan and suggest that he appoints you?' I said, 'Oh yes, by all means do that.' I did write a letter myself to Callan, and learned afterwards that he'd thrown it in the wastepaper basket, but when Pantin's letter arrived he'd changed his mind and decided to invite me to St Andrews. And so I was eventually installed as a lecturer in the Gatty Marine Laboratory.

The laboratory was founded in 1887, originally in the fever hospital on the golf links on the East Sands of St Andrews. In 1896 a man called Gatty presented £2000 for a new building and the laboratory was called the Gatty Marine Laboratory. (A brochure was published in 1996 for the centenary of the new building.) It has since been taken over partly by the European Union, with a big extension to house work on all kinds of marine mammals, such as seals, whales and walruses. It is now permanently supported by the European Union, but in my day the university alone supported it, and we had to find lots of funds to support our research.

You were at the Gatty from 1956 to 1969, but I think you made a side trip to the Red Sea before starting to teach there.

Yes. I had written from Cambridge – on College notepaper – asking the Shell Oil Company whether they could help me to go and work on the coral reefs in the Red Sea. I had a very helpful letter back to say yes, if I presented myself at the Shell office in Port Said, they would look after me and take me down the Red Sea coast to the Egyptian marine station at Hurghada. That is about 200 miles south of Suez on the Egyptian coast of the Red Sea – lots of reefs everywhere, and a very good place to work on corals.

By June '56, Audrey was installed in a house we had bought for £2000 in a little village outside St Andrews (later we moved into a bigger house) but I did not need to start teaching until September or October. So I hitchhiked to Marseilles, found a Greek ship and went fifth class on it to Port Said. The ship was home-based at the docks a bit out of Athens, and we rested three days in Athens while the ship was reloaded and all the supplies – more than I realised – were replenished. I had not been to Athens before. Then we sailed for Cyprus, where there was a war going on – [Archbishop] Makarios and all that. We came in to Limassol at night. Being fifth class I had been allocated a bunk in the very bottom of the ship, although I slept on deck, so I went to see what was happening: they were taking up the plates at the bottom of the ship and unloading ammunition boxes for the rebels, the Greek patriots, in Cyprus. The ship was gunrunning!

When all the plates had been packed in again, we went off to Port Said, where a message awaited me: Would I meet the Shell representative in the first-class lounge? (He thought I had come first class, but actually I had to barge my way into the first-class lounge to meet him.) They provided me with a car and took me right down the Red Sea coast to Hurghada. On the way I stayed a night with Shell's manager at Suez, not an Englishman but a New Zealander who would be neutral if war broke out there – as it did a few months later, by which time I'd done my work on corals and left for the Gatty.

I gather that shortly after you returned from the Red Sea and took up your appointment in St Andrews, you received an invitation to write a book in the United States. What led up to that?

When I was in Cambridge, Pantin had asked me to read a chapter of a book being written by Ted Bullock, who was quite a powerful professor at University of California at Los Angeles and was on many committees in the States. (He's still alive, by the way.) He had started to write a book which eventually had the title *Structure and Function of the Nervous System of Invertebrates* – a two-volume work of enormous proportions, with many hundreds of illustrations and thousands of references – to replace a much smaller book written in German by Hanstrom in 1928. That volume was very much out of date because an enormous amount of work had since been done.

The chapter I read was on coelenterates, the group of animals I had been working on for five or six years. I knew the literature on that, and also a great deal of unpublished stuff. Because I had worked on coral reefs and at Naples, and also at Millport and Plymouth, I'd seen lots of material that, simply, Bullock had missed. So I rewrote that chapter completely, and sent it back.

The result was that Ted Bullock invited me to be a co-author in the great work, and he arranged for us both to have fellowships at the Center for Advanced Studies in Behavioral Sciences, on the Stanford campus in Palo Alto, California. The fellowship meant full return fares, plus a full salary from the Center for Advanced Studies, which was a private foundation that funded people to go for a year and write. There were many Americans there.

We went in 1958, with our two children. At first we were offered a house by a friend of mine in Berkeley, up the hill in Euclid Street, above the campus. So for three months, a whole summer, I worked in the Berkeley library while Ted Bullock was still in Los Angeles. Then for the 12 months of the fellowship he and I went to Palo Alto, and my family and I lived in Menlo Park. I was still a lecturer at St Andrews and was still supposed to give my courses, so somehow I managed to shuttle to St Andrews, give my course and get back to California.

Those 15 months of solid writing didn't mean the book was finished, though. I remember that at the end of 1963, back at the Gatty, we had all the students doing the index, with cards spread around the big laboratory in rows on the floor. Writing, reviewing and indexing those volumes was so much work that they said I never need work again – which was totally untrue!

You had a very large number of research students during your time at the Gatty, didn't you?

Yes. When I went back to St Andrews after California, I found that the director, Jimmy Dodd, was just in the final process of leaving. There was about 15,000 square feet of lab space for me, in effect totally vacant. I went in one day and found only one person there, an Israeli who said he worked on ticks. And when I asked what sort of animals his ticks lived on, he said, 'Camels.' He turned out to be one of Callan's students who was working on the chromosomes of camel ticks.

Anyway, it took me about a year to get the lab going again, but within nine years we were the largest research department in St Andrews. We produced more students, more PhD theses than any of the other departments. I was appointed director in 1960, and I was elected to the Royal Society in 1969 as a result of that nine years' work at the Gatty, plus a few other things I had done before.

Such as a large number of publications during that period.

Well, in 1966 I published 26 papers. That's one a fortnight. We were going like the clappers, I can tell you. We had a great deal of money coming in, lots of different government departments supporting us, because in a marine laboratory you can apply to the White Fish Authority and the Nature Conservancy and the National Development Commission, and the various research councils. The

Medical Research Council supported us for work on nerve fibres, and the Diabetics Association supported us because of a very interesting fish that only has one islet of Langerhans in its pancreas. If you take that out, it becomes diabetic. And you can see the pituitary through the roof of the fish's mouth. If you take that out, it can no longer control its blood sugar. So you can do diabetic research on fishes.

Getting money from all these sources was learned from Ted Bullock, who schooled me very carefully in 1958–59 on how to apply for money, and how to build a lab, American-style. It was a great experience. He made me go to labs all around America and give seminars, and he really showed me how to do it. As a result, the Gatty just boomed. Everything we touched seemed to turn to gold. Everybody was very happy, and students were excited by the work. If you have a marine laboratory, you have enormous resources for a biologist.

There were a lot of good students there. I was fortunate in getting the PhD studentships funded by different government bodies and by the university, and we worked, for example, on crab eye movement. One student starting on that was David Sandeman, who became Professor of Zoology in Sydney and I think is retiring this year. Another was Malcolm Burrows, who is now Professor of Zoology in Cambridge and a Fellow of the Royal Society as a result of his work on crab eye and, later, the locust neuromuscle system.

So began my great collection of students who have passed through my hands over the years. They have been an enormously entertaining and productive lot. It was my custom in the middle years not to publish with my students, but always to insist that they publish their own papers in their own name. There must be a couple of hundred such papers. That would be unusual now, because these days no professor can afford to do that. To keep up the flow of grants, he has got to put his name on the papers. But I think it is an iniquitous system.

But you also have more than a hundred single-author papers yourself. To write so many papers while mentoring all those students is some achievement!

Your next double appointment, so to speak, was an appointment in Yale, in 1965.

While I was at the Gatty I got a telegram from Clem Market, chairman of the biology faculty at Yale University: 'Our member of staff' – and he gave the name – 'has unfortunately run off with someone else's wife and is not likely to come back. The course on comparative nervous systems is due to start in one week's time. Would you please come and stand in to give the course? What are your terms?' I consulted Audrey, who pointed out that we needed to build a new house, we needed the money. I replied to Clem, 'Yes, I'll come if you pay me $10,000 plus expenses' – basically, the air fares in order to commute – and he immediately agreed.

At that time a professor's salary at Yale was about $20,000 to $25,000, so $10,000 for teaching one semester of about four or five months with Christmas back home in the middle of it was quite good. And the American and British tax laws at the time meant I got it tax-free as a capital transfer back to England. So I was able to build a house in St Andrews which was half paid for by the extra job I did at Yale. I rented a room, a digs, in a house at Yale and commuted about every three weeks back to St Andrews. It was quite hectic.

If you teach American students they demand extensive class sheets and literature. They really work on it, and at Yale they were especially keen in having it all laid out for them and written out. So since I

had to write the class sheets, I wrote it up as a book – *Interneurons* – which became for a time a course book in America and did fairly well. But it went out of print.

I understand that you also went to Russia a couple of times, firstly in '63. Where were you based?

In Moscow. It began when the Foreign Office telephoned me and said, 'We have a cultural agreement with Russia to send scientists on visits around labs. We have a problem, though: they are wanting us to send a scientist to Russia whom we don't want to go. We would like a substitute, and would you like to go instead?' So they arranged it, basically. The Russian Embassy sent me a visa, and the British Council lent me clothes – a big Russian greatcoat, with double fur inside and out, and a big fur hat and double boots, fur-lined inside – so I was dressed as a Russian.

My companion, Jones, had been working on the Russian wartime research on how to organise the troops efficiently – operational research. He was whisked off to visit physics laboratories, and I was to go to physiological laboratories. They turned out to be very dull, so I tried to change my program as much as possible. I asked the Russians if I could go to Tbilisi, in Georgia, to see a famous Russian biologist, Beritashvili, who had worked with Pavlov, and they agreed. In fact, a young medical doctor took me off in his motorcar and we had quite a good time looking at Orthodox monasteries in the Caucasus mountains.

In Moscow I found the labs very bad. In one lab I was shown some electron micrographs and a Russian electron microscope, but afterwards some research students told me, 'You know, those electron micrographs were not taken with that electron microscope. We took them with a Tesla electron microscope which we imported from Czechoslovakia, because the Russian one wouldn't work!' A lot of their research was very old-fashioned. I reported all this back to London later, and had a nice debriefing with the Foreign Office about the relative backwardness of the Russian research at that time. Mind you, at the same time they were preparing space research and all kinds of things. They would not allow me to go to Novosibirsk, although I tried very hard.

Adrian, a couple of very interesting topics in your research have been summarised as 'Headless learning' and 'Insect pitch discrimination'. What about those?

These were some of our early discoveries. When I was at Palo Alto I had talked with experimental psychologists about how learning might occur, and the necessity of this or that for learning. When I went back to St Andrews and was working in 1962–63 on a very interesting story about locust eyes – using locusts which I had got my old lab in Cambridge to send me – I remembered those discussions.

I remembered that Pavlov had done lots of experiments with learning, such as sounding a bell and then feeding a dog, so the dog learns that at the sound of the bell it can expect food. Or you can train a dog to look behind something where there is a noise. So I thought, 'Let's see if we can teach locusts to avoid a shock at the sound of a noise.' A note has to be high-pitched, above 10 kilocycles or so, to excite the locust's ear, but even so the idea was to make a noise, stimulate the locust's ear (which the locust has on its thorax, at the base of the back leg) and see where the learning occurred. I didn't believe that the learning would be in the locust's head.

Well, try as we might, we could never get the locusts to associate the sound with a shock or anything else. The sound never taught them to expect the shock. Instead, they learned to stand with only one leg on the metal plate which gave them the shock. The circuit was arranged so that the plate was divided up into segments, and if a locust had a leg on one and the other leg on another, then it would

get a shock. So the locusts would stand on one leg and support themselves on the roof of the cage or in some other posture such that there was no return circuit and they didn't get the shock

Then I found that if the locust was simply held on a clamp and you gave it a shock, it pulled its foot up so that it wouldn't get another. If its foot dropped, it would get a shock and it would learn to hold its leg up. And if you suspended a locust above a water surface and gave it a shock whenever it touched the water, in five minutes it would learn to hold its foot above the water. What's more, if you cut the head off after it had learned, the learning persisted. And if you taught the locust – headless – to hold a particular leg up, and then tested it on the other leg, the learning was transferred quickly to that leg.

So here we have a very simple learning preparation in an insect ventral ganglion – admittedly, with a few thousand nerve cells, but very simple. I published this in '62 as 'Headless learning in insects', and it got into all the elementary psychology textbooks that were published thereafter as an object lesson that learning does not require a large brain; it can be done by a very simple preparation. A number of other scientists took this up, particularly in America, although I don't think they ever got very far. But our experiment caused a big stir. For a psychologist, headless learning is quite something!

What about 'Insect pitch discrimination'?

When I read the literature in order to teach insect nervous systems to the class, I just could not believe what I was reading. Although many insects have ears – crickets, cicadas and so on sing to each other – the story was that insects did not have pitch discrimination: they detected only the amplitude of the wave-form, not its frequency. And they didn't detect either the original carrier wave or the square of the wave.

To me, however, an insect ear was so nonlinear that it would have to detect something more than just the amplitude. So I set to work recording from nerve cells in the central nervous system, and I showed the differential responses to high pitch, low pitch and so on, at the different frequencies of the carrier wave. Different nerve cells were responding in a very simple way to show that the thing had pitch discrimination.

To somebody used to doing experiments on a slightly larger scale, the idea of trying to pick up signals from an insect's ear sounds remarkably finicky. How do you do it?

Oh, it's not finicky at all. The resolution of the microscope is about half a micron and the nerve cells are about 20 microns. You've got a micromanipulator probe, with a fine screw. It's no problem at all, except for the impedances.

One of the topics you researched at the Gatty was the compound eye. What does the compound eye look like, and how did it evolve?

A compound eye is an eye of many facets. It is like an array of detectors, each with its own lens. The array itself is like a thistledown spray, with the axes pointing in all directions, and each unit acts as a miniature camera with one silver grain. In effect, it is one axis of detector. And you can have a great many of these. Some species of insects have very few facets, but some have tens of thousands.

Insects can have eyes like knobs on sticks. A nocturnal dragonfly, which catches mosquitoes at night, has its eye almost completely enveloping the whole head. And a mantid shrimp has three visual axes in each eye, so it has six visual axes looking at an object at the same time.

Compound eyes occur in the very first animals. Down in the Burgess Shale at four or five hundred million years ago there are animals with compound eyes. And this has evolved because it is extremely efficient for panoramic vision – 360° around, and over the top and underneath. It is impossible to make a panoramic eye with a single lens. Even if you make a wide-angle lens, the aberrations build up very quickly as you increase the field size.

At about the same time there is the first mention of light guides in insect eyes.

That too came out of teaching, and also I had done the vision of the compound eye in the book with Bullock, covering all the literature on compound eyes. In 1962, two scientists in Britain published a paper saying that light comes in through several facets at the same time, and interferes behind the facets within the eye. Like one or two other scientists around the world, I just did not believe that eyes of things like bees, flies and grasshoppers work in this way. So, with microelectrodes, I recorded from locust eyes and showed basically that each facet has its own field, the field is generated by the interaction between the lens and the end of the light guide, and the rod-shaped structure containing the visual pigment acts as a light guide.

It is exactly the same in our eyes. Our rods are light guides. But in 1962 this was not a popular subject. There were perhaps two or three people in the world interested in the optics of rods and cones; nobody was interested in the optics of insect light guides.

I had a couple of good students at the time. One of them, John Scholes, used to get up so late that he had to work at night. And so he discovered that the locust eye sensitivity increases about a thousand-fold at night, and you can then record single photon arrivals. So one day he comes in and says, 'Look at these things I've recorded. You turn down the light to nothing, and this is the response to my white shirt!' This was in the dark room, with nothing but a glow from an oscilloscope indicator light to provide a bit of light. The facet of the locust eye is, say, 25 to 30 microns, so the area to catch photons is pretty small. The photon flux cuts down to about 10 a second with a very dim light.

Scholes said, 'What are all these little bumps?' They were random in height, and randomly spaced. I suppose someone said, 'They probably are photons.' Someone else probably said, either then or within a day or two, 'Are they Poisson distributed?' and of course they were. Then we did very careful measurements and showed that it was first-order Poisson system – there was no summation between photons required in order to get a response, and if two photons are required, then it is a second-order Poisson. And so on. We were onto that in 1962. You could do exactly the same thing with a photo-multiplier tube, of course, but we had an insect eye.

That caused a big stir. John Scholes discovered it, there's no doubt, and I sent him to a big conference in America to read his paper. He did very well out of that.

Wasn't it during yet another period overseas while you were still with the Gatty that you began to get overtures from the Australian National University?

Yes. I was working in Woods Hole, Massachusetts, at the biggest American marine laboratory on the east coast of America. I had a Grass Foundation fellowship to work for the summer on recording from dragonfly eyes. We all went, including our four children and my mother-in-law to look after them, and we rented a house on Cape Cod. My assistant, Steve Shaw, had a Lalor Fellowship; we had fixed it so that we had coinciding fellowships. And he took his wife with him as well.

That was the summer of '67, and I was using some of the money that I had earned at Yale two years before. While I was there I got a cable from Canberra: CONSIDERING YOU SERIOUSLY FOR CHAIR STOP PLEASE VISIT CANBERRA OUR EXPENSE STOP FIRST WEEK OCTOBER. So I wound up my effort at Woods Hole and came straight to Australia, where I was interviewed by a committee consisting of John Crawford, Doug Waterhouse, Frank Fenner and a few others, and I was offered a Chair as a founder professor for the Research School of Biological Sciences. At about the same time they offered similar appointment to Ralph Slatyer and Denis Carr; and David Catcheside was already here as a professor. The four of us had the job of setting up a new school.

I sent Ian Meinertzhagen as my agent about eight months ahead of me, to supervise the acquisition of furniture and microscopes and benches and amplifiers – all the things we would need. Our building was not yet up, so for about four years we occupied the old Nurses' Home, which became Property and Plans and then Earth Sciences, up on the hill opposite Physics. We moved into the RSBS building in '73.

Arriving at the ANU in 1969, did you continue your Gatty lines of work for which you had been so recently elected to the Royal Society, or did you begin anew?

We came with about eight scientists and set up immediately, with a bang, doing the jobs that we had been doing before. I brought with me several students who were in the middle of their PhDs, and two postdocs. Ben Walcott was appointed here, having come to work in my lab after hearing me give a seminar in America. (He became professor at the University of New York in Stony Brook and now he's retiring to Canberra, to come back and work with me here.)

I decided we would have a variety of things operating. I myself started out doing the compound eye, which I was already deeply into. We were looking at nocturnal eyes, we had a lot on the light-guide story and the photon story, the origin of noise in vision. The signal-to-noise ratio was determined by the shot noise of the photons, basically, which is limiting: as you increase the intensity, the signal-to-noise ratio changes as the square root and so on. There is a lot of physics involved in all of these things.

Is that the only source of the noise – nothing within the system itself?

Oh, there is intrinsic noise too. There is synaptic noise, linked to the irregular bursting of synaptic vesicles at the synapses. The release of transmitter is not uniform or linear.

What is the contribution of the two?

Roughly fifty-fifty, as Laughlin later measured. And the efficiency of the visual pigment is above 50 per cent. It's much better than a photo-multiplier. You can get 60 per cent efficiency out of a locust eye. So it's very good.

We worked all these things out, and I handed all that side to Simon Laughlin, whom I had appointed here as a research student from Cambridge. I handed on to him virtually all of the signal-to-noise ratio stuff and the function of the first synapse, and he in turn, with his students, worked out all the contributions of synaptic noise and shot noise. He was elected to the Royal Society for that in 2000.

I started also a group working on development of the nervous system of insects. Meinertzhagen, who had been working on that, was very keen on working on the development of the nervous system behind the eye. Working with him was Mike Bate, the first person to work out the cell lineage of the

nervous system in the development of *Drosophila*, the common genetics fly which they all use. Mike Bate was elected to the Royal Society in 1997 for his work on embryonic cell lineages.

Alan Snyder worked on the mathematics of the light guide story – which we had brought with us – and eventually generated the principles by which modern long-distance light-guide transmission is used for communication. He was elected to the Royal Society for that in 1990. So you can see what a high level our work was at.

Barry Ninham, of Physics, had appointed Snyder, but as soon as he was appointed he turned up in my lab and started talking about light guides in insect eyes. He worked a great deal with Laughlin and later on with Joe Howard, Stavenga and others – there was a lot of interaction with Physics.

Ninham and I made some joint appointments as a defence arrangement when there was talk of cutting down on appointments. Joint appointments would require approval of both faculty boards and would be locked in, we thought – not easy to chop.

One such appointment in my department was Jacob Israelachvili, who worked on photo-pigment molecules. (His election to the Royal Society was actually on his atomic force microscope work, though.) I interviewed him in Cambridge, and he was a joint appointment with Srini [Mandyam Srinivasan].

A lot of the work of your department at that time was on vision. Perhaps you could say something about the developments in that regard which are taking place now.

The team at the core of the Visual Sciences Centre (left to right): Professor Bill Levick, Professor Allan Snyder and Professor Adrian Horridge.
Photo: ANU Reporter*, 1987*

Well, in the late '80s Bill Levick, Alan Snyder and I got together and decided we would have a new centre and call it Visual Sciences. (It was another political defence measure: we knew that if we had a collaborative venture between three Schools we would be likely to fund it better than if we acted independently.) We applied to the university and got excellent funding, and established postdocs and students and so on, and the Centre for Visual Sciences is still running. Much of the work that went on in my department after 1987 actually was labelled as coming out of the Centre. It was a very good move. I think we were the second Centre within the university, followed by 10 or so others.

Meanwhile, I had been working on how insects – specifically, mantids – measure range. A mantid on a stick will walk along until it comes to the end and then stand, moving its head from side to side to measure the range to anything beyond the end of the stick. If then you tickle it on its behind and it's

anxious to get onto something else, and if there is something else near, it will reach out with its arm. But it won't reach out unless there is something within range. So it can measure range.

To do this, I showed, mantids use relative motion. They are not triangulating and they are not using parallax as one thing goes behind another, they're using relative motion – just as we do. When you move one eye, things 'move'. Things that are near move a lot, and things that are far away move much less. The mantid is doing what the person with one eye would do in order to pick up something: just move a bit.

Insects are – like us – incredibly sensitive at that operation. They have sub-pixel sensitivity; they can resolve movements that are smaller than the width of a receptor field on a single receptor. So clearly it is very important.

I was then able to extend that work with Srini, who, when his research fellowship had come to an end had gone off to a laboratory in Zürich run by Rüdiger Wehner, to work on the behaviour of honey bees and ants. He was not particularly happy in Switzerland, so he wrote saying he'd like to come back and I secured for him the last tenured appointment in my department. (There was already talk that I would be retiring in six years' time or so.) He came back in '86 or thereabouts.

In Zürich one of the best bee-trainers in the world, Miriam Lehrer, ran all the bee training experiments for the laboratory, and Srini learned how to work with trained bees, marking them individually so that you can run a protocol on individual bees and test them to see what they have learned. And since you can work with trained animals which learn very quickly, you can ask the animal all sorts of questions. I saw immediately that this was going to be a real whiz, because we had so many things we wanted to ask the bees. Srini first of all worked on the resolution by behavioural methods, and then he worked on the motion resolution and how the bees can measure the speed of a motion.

So we did the experiments that came out of my mantid work; we used bees and trained them to measure range. We found that they clearly do that, and they do it by measuring the relative angular velocity of contrasts that cross the eye. As the insect moves, every part of the visual field moves, and every part moves differently because it's all cos functions and sin functions relative to the motion of the animal. It's not a simple matter to try to measure velocity when no single point is moving at the same speed as any other point. There were many, many experiments to be done, and from about 1987 or '88 I handed all that to Srini, to use his mathematical background on that visual flow problem.

Srini worked for about three years, very hard, and we showed that bees see parallax and they measure angular velocity. He flew bees down tunnels and showed that they measure the distance they have gone, by integrating the optic flow. He showed that this was independent of pattern, and many other things – a very rich research field, for which he was elected to the Royal Society in 2001. Since then he has won one of the senior awards for senior professors, and he has quite a reasonable size group putting all that now into hardware for applications.

That is a very interesting development out of some biology, isn't it?

Well, in '87 we had the basic theory. We got a GIRD [Grants for Industrial Research and Development] grant from the Department of Employment, Education and Training, appointed Nagle and Sobey, and built a gadget in conjunction with Guide Dogs for the Blind. We had to have a company to work in collaboration with, and they had a company for that. We were building gadgets to put on a blind person's hand so that they could measure range using a hand movement, with a touch output on the wrist. These gadgets worked a treat, and could easily have been manufactured in large

numbers. We tried to sell it to industry, but there was no interest at all because most blind people are poor and can't afford gadgets, and also the numbers are not terribly great in wealthy countries. There are only 2000 Braille printing machines in all the world, so technology for blind people is not a moneymaking field.

How did your work on insect vision come to be applied to robots?

In 1987 the Chernobyl reactor blew up. The Russians sent in men, many of whom died, either very soon or subsequently. The Japanese, having about 30 nuclear power stations, were very frightened, and the Japanese government gave more or less free rein to their big manufacturers to build robots that would go into nuclear power stations. Although they were offered tax breaks and so on to do this work, they lacked a system of vision which could control a freely moving robot that was self-managed, with a brain. So they sent engineers around, and eventually they found us.

The Japanese offered the Australian National University $5 million for our know-how, and there was some talk that ANU would get a supercomputer from them and that we would put some work into it as well. But that deal fell flat, and eventually Fujitsu simply gave ANU $10 million and took all our know-how. They went away and put a team on it, building a hardware box that did exactly what we had planned it to do. It functioned very well so the robot had information about the range of every object around it at all times, in real time, which is what they wanted. They sold a few, but I don't think they made much money.

Fujitsu used the software, however, playing the equations backwards. They were inventing a simulated environment, virtual reality, in which you have all the objects around you and if you make a movement everything 'moves'. In our system, you have the animal moving forward and it has to measure the range from the movements of all the objects around it. In virtual reality, you know the ranges and you have to calculate the apparent movement. The same set of equations govern the two things, but in reverse. The variables that were known are now unknown, and vice versa. So it was the same software, basically, and I think they made their money out of virtual reality.

There were no patents; I don't think we could have patented it. It was so simple that no-one had ever thought about it, and yet it is glaringly obvious, once you have seen it, that as you move around, everything 'moves' – and you can get the whole world from that.

And now there are aircraft applications as well.

Somehow the American Air Force picked up that we were working on this, and in about 1992–94 they wrote to Srini that they wanted to put insect vision onto helicopters. It seems that the Americans want their infantry to have pocket helicopters. Suppose you land your force by parachute or something and when they come down they don't know what's immediately around them. What they need is a nice little spy helicopter to go off, search the district, see what's behind buildings and so on, and broadcast photographs back. So Srini went ahead and appointed some people. They purchased three or four nice little model helicopters with a range of a few kilometres, and they have now put insect vision onto them. They fly very nicely and they hold stable in a wind.

The next thing was to have a low-flying aeroplane, and low-flying aeroplanes are now built here, in the Research School of Biological Sciences, to fly at 150 kilometres an hour. They've got insect vision downward looking and they have another form of insect vision upward looking to stabilise against the horizon as they fly, to give them a general view of the whole horizon.

NASA then picked it up, perhaps through the American military, and now supports a similar program for Mars landing. If you want a vehicle to land on Mars, or to fly on Mars – and several of these are now being developed – since you have a half-hour gap, you can't control it from Earth. You need to have vision on board, with a brain installed, and it needs to control the vehicle. So all of that has gone ahead.

It's a most amusing development to come out of insect eyes. It wouldn't have happened if I hadn't worked at Farnborough, where we were aware of the massive requirements for technological advance and we knew about the pace and the funding that would become available when anything was obviously saleable and obviously could be done.

Let's go back to 1975, and your participation in the expedition on the Alpha Helix. *What was the background to that?*

After I came to Canberra I had to go back to Europe every year, to conferences or to find staff and students. And in 1971 I stopped off for the first time in Bali, staying at a little hotel in Denpasar where the son of the house wanted to learn English, to get a scholarship to Australia to study hotel management. In my good English accent I read all his English books into his tape-recorder, and he must have heard those tapes over and over again. He won his scholarship to Australia with no trouble. Eventually he went back to Bali and became the head of a fine hotel.

Bali is a very convenient stop-off on the way back to Australia from Europe or Japan, and eventually I learned Indonesian and even some Balinese from my young friend. He lent me his motorbike and I travelled all over Bali. In his gratitude for my help with his English he provided me with a home from home – I could go to Bali, dump my suitcase in his hotel and go backpacking round the Indonesian islands. By '75 I knew quite a bit about Indonesia. And my friends, including Jim Case, who was Professor of Biology at Santa Barbara, and Ted Bullock, who was professor at La Jolla, were aware of this.

The Alpha Helix.

The Americans had a ship called the *Alpha Helix*, a fine vessel. (Being all top and no keel, however, it's terrible in a rough sea.) Run by the National Science Foundation, it has about three labs on board, it has an electron microscope, an electrophysiological set-up, deep-sea equipment, photocopiers, charts of anywhere in the world, 12 useful American sailors and a huge freezer full of pre-cooked food – and Orange Julius on tap at all times of the day and night. The ship went through the Bering Straits and up the Amazon, and in 1975 it came across the Pacific, with three different groups of scientists. It was refurbished in Cairns and then went up through Indonesia to the Philippines and on into the sea north of Japan. And the operators asked me to be chief scientist for the Indonesian leg of the trip.

With a grant of something like $25,000 from the Minister for Science we took on quite a group of Australian scientists. Some boarded the ship in Cairns and some flew to Indonesia and picked up the ship in Ambon. We based ourselves in Banda, living at a fine house belonging to an Indonesian politician (Des Alui). We spent three months in Indonesia with a ship, inflatable runabouts and so on, and a full scientific backing.

What was the scientific thrust of that research?

The Banda Sea is one of the few places where you get extremely deep water close to land – a mile offshore it is at least a mile deep, and it goes to four miles deep – and you can work with upwelling currents. So, although I did some work on eyes of land insects on Banda, I was working mainly on eyes of deep-sea animals. For example, you can get magnificent deep-sea fishes with photophores all along them, and luminous jellyfish. We had a lot of scientists on board, coming and going. I don't know what the others discovered, but I came upon the fovea, the concentrated part of higher resolution, in compound eyes. There were cases described here and there in the literature, but this yielded much more information.

Is the fovea just an element within each element of the compound eye?

No, it's a concentration of axes. The visual axes are compressed together, which means there are fewer of them in other parts of the eye. The compromise is that the bigger the fovea, the worse the other parts of the eye. It's a sampling density problem. If you want a good picture of something, you have to have a high sampling density. To have that, do you sacrifice the other parts of the eye, which then lose receptors and lose sampling density? The compromise is determined by a whole set of interactions, and different animals have different compromises. We have a fovea in order to look at things, but a cow doesn't, beyond a visual streak or so.

Is there some reason why the animals you were looking at would need a fovea?

Well, as an example, a mantis shrimp has six visual axes looking at the same place, and it has huge foveas. The animal has a dagger and a club, but in order to stab or club its prey it needs to know exactly the range. I discovered that it has a very complicated set of visual axes by which to measure the range very accurately – by triangulation, of course, not by movement. I measured a large range of sampling densities in many eyes, and that was all published in 1978.

Wasn't there another extremely interesting development from your Alpha Helix *trip?*

The hull of a traditional Indonesian sailboat.

Yes indeed. I happened to walk into a village on Banda where they built boats. I am interested in boats and construction – woodwork and stresses and aeroplane frames and all kinds of things like that. But they build these big boats by a most curious method. They cut relatively short planks in the forest to shapes that are in their heads, and make the boat out of these 10-centimetre thick planks, solid hardwood, cut with an adze and curved two ways. They build the shell first, putting the planks end to end, and side to side edge-joined with dowels. They then fit in the ribs, which look exactly like the ribs of boats built in the North Sea by the Dutch or the English in about 1880. Then they put in the deck supports, and build up the boat.

To us the basic structure is totally wrong. Western Europe mostly builds boats by building the frames first and then bending the planks over the frames. But these people do it the other way round. (It turned out that Europeans used to build shell-first till about Elizabethan times, and in Scandinavia they continued even into quite recent times to build shell-first and put the ribs in afterwards. But I didn't know that when I walked into that Banda boatyard.)

A West Sulawesi sailboat.

They called themselves Binonkos and didn't speak Indonesian. They had come in from Sulawesi and had put down their village on a stretch of sand which was nobody's else's coconut grove, and they lived by building boats and by using the boats for trading with other parts of Indonesia. These were gunter lug ketches, as we would call them these days, sometimes with a single mast and sometimes with two.

I came back to Australia with a wordlist for all the parts of the boat, photographs of each stage in construction, samples of the wood that was used, notes about the cost of the timber and the economics of the boats – how long they lasted, how much the people got from each trip and what was carried, such as copra to Surabaya. But I didn't know what to do with all this. I looked around in the literature; I asked people in the Anthropology Department. Nobody here knew anything about boats except David Lewis, who had worked in the Pacific on navigation. They just told me to write to So-and-So in Holland, or something like that.

I sent letters off to all the maritime and ethnology museums around the world, asking whether anybody knew anything about Indonesian boats, but when the letters came back they just said things like, 'We used to have an expert, but unfortunately he died in 1929.' Several of the Dutch museums said that since the war and the loss of Indonesia they had no further interest in Indonesia. 'We have 250 models in our store,' they would say, 'but nobody knows anything about them.'

I could see a marvellous opportunity. I had a good knowledge of engineering construction of things like aeroplanes and boats, I could sail, I could speak the language, I could read the languages of most of the colonial powers that had been there. And so, in the ensuing years, each time I went overseas I would go to a different museum – in Holland, Budapest, Hamburg, Berlin, Jakarta, Singapore, and in Salem, Massachusetts, and La Jolla, California, for example – looking everywhere for models of Indonesian boats. I built up an archive of photographs of every model that I could find, with the acquisition dates, the names and sometimes any attached notes.

On my way out of or into Australia each time, I went to a different island in Indonesia and took photographs of living boats. And I was very fortunate that a monk in the Philippines told me that the Atheneum in Manila had a manuscript written by a Spaniard in about 1660 about boat construction in the early Philippines. So, with one thing and another over the course of about 10 years from 1975 when I first walked in to the boatyard, I have had books about these boats published in '81 and '85, and a book about canoes published in about '86. All the photographs are taken by me, and in one of the books I did all the drawings. I have written about 10 papers, as well.

What I did was to bring together all the work on these boats into a cohesive story, encompassing the interesting methods the people use for construction and how they know the measurements and define the boat; the methods of using natural material so as to get optimum performance out of weak material; and how the influence of Western designs affected the Indonesian cultures, how the boatbuilders slowly adopted tricks from Western boat design, rig design and so on, and how different aspects were adopted in first one place and then another.

The story extended then to how the original design and construction methods of East Asian and Pacific boats was a compromise between the weak material and the forces they had to sustain, and how sailing upwind was no problem to the original Polynesians in their maritime invasion of the Pacific.

All this came out of my aircraft engineering experience, and it has been good fun.

Since your retirement in 1992, what have you been working on?

I have been writing another book on the outrigger canoes of the rest of Indonesia, but some places have been difficult to get to. One of them is Sulu, between the Philippines and Indonesia. It's always been a bad area, with pirates and desperadoes, and people getting kidnapped and even killed, but it has interesting boats. Although I couldn't finish that book, I've certainly been to many interesting, curious places north of Australia, looking at boats.

And what research have you been doing since you retired?

First I went to Cambridge again, for a year at Churchill College. I had been a Fellow there on a year's sabbatical in the '70s. Then I came back and started a new line.

On how honey bees recognise patterns, is that right?

Yes, basically to understand how a small brain recognises pattern. Suppose you are trying to draw down a picture on your computer. It takes quite a fast computer to bring down a decent sized picture in, say, one second. A picture may contain 100 million pixels, and if you want to collate that data in real time, say at 50 a second, you've got to do it at about 100 megahertz. Then, in order to process it, you have to look at the relations between every pixel and every neighbouring pixel, and you've got to do that between frames, or in real time – a totally impossible job even for us, with half our brain

occupied by the visual system. So bees have got to be extremely cunning to be able to see any pattern at all in real time. Yet you can teach them patterns.

This poses a magnificent problem, a solution to which would be immensely useful for artificial vision, or even for indexing pictures so that you can look for a particular feature in any group of thousands and thousands of pictures. This is what interests and occupies me at the moment: how does a bee not only see pattern but remember it, learn it and remember it, and recognise it when it sees it again?