Lessons on beginning a career in biological research. How the publications came about.

In Sept. 1949, with a PhD studentship in my pocket, I returned to Cambridge to find the Zoology Dept deserted. Busy-busy Mr Drury, chief technician, gave me a key to a room, which was empty but for two stools. I was supposed to find my own topic, build or borrow some equipment, carry out a significant piece of research, write it up and submit it 3 years later. Such was the deep end of the pool at the time.

So, I went down to the Plymouth Marine Lab to look for a topic. There by chance I met Dr. Alexandrowicz, a Polish refugee, who taught me how to use the vital dye methylene blue to stain living nerve cells in various invertebrates. At the time he was describing the stretch receptors of the lobster, later an important preparation for physiology. In Cambridge and Naples, Eric Smith was staining the central nervous systems of Polychaetes and Echinoderms similarly, so I absorbed the literature and the technique from these experts. Much of our basic knowledge of invertebrate neuron connections came from methylene blue studies, by Retzius, Sanchez, Orlov, Zaćwilichowski, Zawarzin and others, most of it in French, German, Polish, Russian or Spanish, not summarized at the time, and not familiar to English students.

1951 *Nature* 167, 732-734.
Earlier, Martin Canny and I had been on holiday on the island of Tresco, in the Scilly Is. On the last day I collected a plastic bag full of porcelain crabs to take back to the lab in Plymouth, packed in moist seaweed. At the time, I was staining with methylene blue everything that looked promising. Back in Plymouth, Dr Parkes noticed the seaweed, and with a cry of delight picked out a dirty brown tangle of strands which she said was a species new to Britain. She found the illustration of *Asparagopsis* in Harvey's Seaweeds of Australia and insisted that I write a letter to Nature. Such was the first step on my professional path, in the wrong direction, of course.

1952 Birds on Palma and Gomera. *Ibis* 94, 68-84.
In 1949, when I visited Oxford to look for a place to do research, Philip Guiton and I planned parallel expeditions to survey birds in the Canary Is. David Bannerman lent us his 410 gun, disguised as a walking stick, which we carried illegally through Spain eventually to La Gomera via Tenerife. John , Paul Lloyd and Phil Smith and I (Los quatros Ingleses) spent the summer on Gomera, with a base camp in the hills at El Cedro. It

was culturally and botanically a fascinating place but devoid of intellectual excitement or effort, so I lost interest in bird speciation.

1953 The jellyfish action potential. *Nature* 171,400.
In the summers of 1950-1, I found many Aurelia jellyfish at Brancaster Staithe, on the Norfolk coast, and took some back to Cambridge in large jars on my motor bike to stain. I discovered that the living neurons of the nerve net were clearly visible under a phase contrast microscope that Victor Rothschild had purchased. So I spent a year building amplifiers and equipment to record from these neurons. I also visited John Baker in Oxford, where he and Kempson had developed home-made phase microscopes. I made my own phase plates from cover slips, and built a recording camera, all of which I took by train to Largs to the Scottish marine lab at Millport the following summer. There I camped in a bed of nettles, supposedly the Director's lawn, and was able to show my results to Sir James Gray and Victor Rothschild, who were there working on sea urchin sperm. Later they were instrumental in securing for me a Senior 1851 Fellowship and were useful support when I submitted the work for a Fellowship at St Johns. (They were really in Scotland for the salmon fishing.)

1954-1956 Nerves and muscles of medusae. *J.Exp. Biol*. 31, 594-600; etc.
One nerve impulse in the motor net was sufficient to cause a contraction of the bell, so there must be other nerve nets that carried other excitation that was not accompanied by a bell contraction. That inference was tested experimentally in these papers. I also had a job, working with Jim Gordon at the Royal Aircraft Establishment at Farnborough on the design of reinforced plastic structures and cheap rockets, a Fellowship and rooms in Cambridge, and besides was engaged to Audrey who lived and studied in Oxford. I spent holidays working on Scyphozoa in various labs, extending the earlier work at Naples by Emil Bozler before he fled from the Nazis to Columbus, Ohio. Audrey helped at Millport and Naples. Life was hectic, exciting and very educational.
In 1955, I accepted the offer of a job at St Andrews because the Gatty Marine Laboratory was better than Cambridge for marine invertebrates.

1957. Nervous system of the ephyra larva of Aurelia. *QJMS*, 97, 59-74.
In 1955, I accidentally found that scyphistoma polyps were budding off baby jelly fish in a tank in the reserve aquarium in the lab at Plymouth. They stained beautifully with methylene blue. The motor nerve net and the sensory nerve net were both displayed, with their meeting points at the marginal ganglia, but could not be fixed, so they were drawn and described from the living state.

1957, Co-ordination of coral polyps. *Phil. Trans. R.S,* B, 240, 495-529.
In 1955, the Royal Society gave me a grant of £100 to study a giant polychaete worm, supposedly found in coral in the Red Sea, of which there was a specimen about a 2 metres long in the Zoology museum, Cambridge. In the summer of 1956, I put my equipment in a rucksack, found a ship at Marseilles, and bought a return ticket to Port Said. The Shell Oil Company lent me a car and driver, who took me to Ghardaqa, on the coast of the Red Sea, where I stayed in the Egyptian Marine lab for 2 months. There I discovered that the coral polyps were expanded at night, so every night I worked with my electric stimulator, plotting the sequences of polyp contractions when a wave of excitation spread across the colony, using every type of coral that I could find. The coral specimens were dried and packed into the baskets in which I had purchased live pigeons from the Nile Valley for my dinners. Because I could not find an expert on coral systematics, I identified the corals in the basement of the Brit Mus of Nat Hist in Kensington by comparison with the original types collected in the 18th and 19th centuries. I never saw the giant Polychaete at all, but the RS did not mind.

1961. Discrimination of pitch by neurons of the locust nervous system. Nature 185, 623-624. and PRS, B 155, 218-231.
We had a culture of locusts in the Gatty Marine Lab for student practical classes. In 1959, I had spent a year at the Centre for Advanced Study in Behavioral Sciences, at Palo Alto, writing the book with Ted Bullock. While there I had the idea to train locusts to associate a sound with an electric shock *à la Pavlov*. Back at St Andrews, when I recorded from the locust auditory nerve to calibrate the range of the ear, I found obvious signs that different groups of fibres responded to different pitches. There were large neurons in the central nerve cord that also responded differentially to different pitches, so, obviously the locust was interested in the pitch of sounds. When I published this, Hans J Autrum sent me a letter saying that I must be wrong, because he had published a paper on the lack of pitch sense in Orthopteran behaviour.

1962. Postural learning by headless insects. PRS 157, 33-52.
As said above, I tried to train locusts to sound. However, the locusts ignored the sound and learned to avoid the shock by standing on one leg, so that the shock circuit could not be not be closed. So, next I held the locust over a dish of salt solution so the legs hung down and got a shock when they touched the surface. Then I found that the animal learned very quickly to hold up its legs at exactly the right level. Astonishingly, the learning persisted when the head was cut off. John Pringle, critical as

always, rudely suggested that it was no more than muscle rigor cause by the current, so I yoked together two animals, one of which had the opportunity to learn from its own movements, while the other in series with it received the same shocks no matter where it held its leg. The learning depended on the animal making its own trial and error movements, what Skinner called “operant”. The concept of local learning in the ventral cord was quite new, and it was taken up by comparative psychologists. The real significance, however, was that all insect, and perhaps many animal, postures are determined in the same way because the central nervous system has to deal with an unpredictable environment, as designers of walking robots later discovered for themselves.

These early efforts defined my interests and way to progress in understanding the nervous basis of behaviour. The trick was to travel, either mentally or physically, to the heart of the problem and there use the latest technology, or sometimes no technology at all, to attack problems that became accessible by using invertebrates with simple nervous systems. Later, this was applied to the attenuation of light guides in insect retina, the basic mechanism of piloting by the bee, and the use of trained bees to analyse the mechanism of recognition of place.