once it had been shown that neutrons were emitted after the fission. One was awed by the thought of a new weapon of terrifying power, but Frisch, like others who had understood the paper by Bohr and Wheeler on the mechanism of fission, and Bohr's argument that the fission caused by slow neutrons was due to the rare isotope ^{235}U, were reassured that no violent explosion could happen in ordinary uranium. The thought of separating uranium isotopes in large quantities seemed to belong to science fiction. Then one day Frisch came to me and said "Suppose one had a large amount of separated ^{235}U, what would happen?" One could make a guess at the fission cross section from the ideas of Bohr, and I knew how to estimate the critical size, given the cross section. The answer staggered us by being smaller than we would have guessed. Next we had to estimate how far the chain reaction would proceed before the energy it released would drive the uranium apart. The answer again staggered us by being a reasonable fraction of the total energy available. If that was so, we said, it would be, as a weapon, worth its price, even if the isotope separation plant cost as much as a battleship (this turned out to be an underestimate). What if the Germans got there first and a nuclear bomb was in the hands of Hitler's Germany?

We wrote down our arguments, and that was the start of serious interest in atomic energy work in England. Frisch first tried to explore the possibility of isotope separation by thermal diffusion, but did not get far, and we now know that the thermal diffusion coefficient in the only gaseous uranium compound happens to be practically zero. Instead Frisch moved to Liverpool, where in Chadwick's laboratory there was a cyclotron and other facilities for nuclear physics, and worked on the nuclear physics aspects of atomic energy until the end of 1943, when it was decided to discontinue work in England and move to America those who could be useful to the American work.

So Frisch went to Los Alamos, the strange atom-bomb city in the middle of New Mexico. He was not attached to any specialist team but had a roving assignment and in that way was able to give essential help to many different groups. One of the experiments he set up himself was a typically original scheme known as the dragon. Two pieces of fissile material which together would make a critical mass and so cause a violent chain reaction, were allowed near each other for only a short moment, by dropping one piece past the other. The speed was so arranged that the chain reaction did not develop to any dangerous degree. The name indicated that one was tickling the tail of a dragon. The experiment made it possible to observe details of a near-critical situation which were otherwise hard to get at.

After the war, Frisch returned to England and became head of the Nuclear Physics Division at Harwell. Administration was not his favourite pastime, but he found that his deputy Dr (later Sir) Robert Cockburn delighted in running things and did it well. Frisch left Harwell in 1947 to become the Jacksonian Professor of Physics in Cambridge and a Fellow of Trinity. At that time nuclear physics in Cambridge was overshadowed by cystallography and other lines. It was not in Frisch's character to fight for more support. Besides, nuclear physics had now become big physics and less to his taste. While he kept a lively interest in nuclear physics and in the newly developing particle physics, he spent more of his time teaching and writing. Many students profited from contact with his way of doing and looking at things. He still liked gadgets, and made many ingenious instruments. His writing included *Meet the Atoms*, a very readable popular introduction to modern physics. Only a few months ago he published his recollections under the appealing title *What little I remember*,[1] and it is pleasing that he was able to leave this lively record of his personality.

Shortly after moving to Cambridge he married, and there were two children. His wife shared not only his Austrian origin, but very many of his attitudes and his love for music.

Shortly before retiring in 1972 he invented an apparatus for measuring and evaluating bubble-chamber tracks, and he later became a partner in a firm set up to manufacture this. I do not know whether this commercial activity made him a wealthy man, but he clearly enjoyed this novel position and seeing his gadget being produced.

His was a full life, but he just failed to reach his seventy-fifth birthday with its warm messages from his many friends.

Rudolf Peierls

1. Frisch, O.R. *What Little I Remember.* (Cambridge University Press, 1979) See review by Rudolf Peierls in *Nature* **280**, 257-259 (19 July 1979).

Martin Lüscher

IN THE midst of his fruitful work, Martin Lüscher's life was taken on 9 August 1979 by a tragic accident, at the age of 63. He was Professor of Zoology at the University of Berne, and directed its Department of Zoophysiology with scientific foresight and a fatherly concern for his colleagues and students. His name will be remembered by zoologists throughout the world.

Martin Lüscher, son of the Basle artist Jean-Jacques Lüscher, spent a quiet youth in his family home, with extended visits to Provence. There, the harmony and beauty of the landscape created a lasting impression on him; in the balmy Mediterranean atmosphere with the colour play and diverse insect cacophony in the flower fields he developed a love and interest of plants and animals. In Basel in 1944 he was awarded a PhD in zoology. After his marriage to Noemi Stoecklin, daughter of the Basle artist Niklaus Stoecklin, which proved to be a true life partnership, he began, with her, his lifelong progress as a zoologist. They shared a life of work, pleasures and trials. A great deal of his professional success was due to his close accord with his wife, with guests always welcome at their home.

As a young zoologist, Lüscher's first post was as a research assistant in Berne, where he worked under F.E. Lehmann on the physiology of development of amphibians. Soon afterwards he met his great teacher, the insect physiologist, Sir Vincent Wigglesworth of Cambridge, under whom he was able to work. Martin Lüscher saw Wigglesworth as the founder of a new branch of research: experimental insect physiology.

From England Lüscher moved to Paris, where he was introduced to the biology of termites by the renowned termite specialist T.P. Grassé and found the stimulus for what was to become his principal research — caste formation in termite colonies. He wished to study termites in the field as well as the laboratory, and he soon had the opportunity to take part in an expedition to observe at first hand the highly organised termite colonies of Africa. Lüscher was particularly fascinated by the highly elaborate nest structures built by the millions of termites within the colony. He was the first to understand their design as a well planned respiratory system with temperature and humidity regulation, and an air circulation system functioning through the warming of the air inside the nest. After imaginative and productive field work he spent a very important year of study in the United States on a grant from the Rockefeller Foundation. He was given the opportunity to stay for some time with A.E. Emerson, the expert on termite biology and taxonomy, and was then able to acquaint himself with modern methods of insect endocrinology in the leading laboratories at Berkeley, and at Harvard with C.M. Williams. Afterwards he worked at the Swiss Tropical Institute in Basel and in 1954 was appointed professor at the University of Berne. Under his direction the new Department of Zoophysiology was formed.

In 1965 he was appointed to the four-year directorship of the Zoological Institute. During the academic year 1967/68 he was Dean of the natural science faculty and from 1969 a member of the Swiss National Science Foundation. In his last important undertaking, in Nairobi as project leader at the newly founded International Centre of Insect Physiology and Ecology, he set up an enthusiastic research team.

Lüscher carried his title with quiet

modesty. His lectures were simple and clear, and his criticism was always constructive and helpful. His motivation as a researcher derived from the pleasure of observation, an urge for knowledge and a keen understanding of relationships. He was intrigued, like many earlier researchers, by the secretive world of the termite colony. How do all the different castes including the workers, the soldiers and the reproductive individuals develop from the same origin in such numbers? How is this ratio maintained and regulated? His understanding of the termite colony as one organism, which he had gained in Africa, and the methods of experimental physiology he had learned, led to further discoveries. He found that all the castes of the termite colony interact continuously, as though they were parts of a single organism. The presence of reproductives in lower termite colonies inhibits the development of that caste, whereas their absence induces the formation of further reproductives. In a similar way the ratio of developing soldiers is regulated by the proportion of soldiers present in the colony.

Lüscher became aware of the importance of 'social hormones' as agents of regulation and communication within the termite colony, and with the biochemist Peter Karlson he introduced the now well-known term pheromone. He could demonstrate by means of ingeniously simple experiments that pheromones are released from the anus of the reproductive individual and are ingested orally by other termites and then distributed throughout the colony by oral exchange, the so-called process of trophyllaxis. Pheromones regulate the make up of the social structure, whereas hormones determine the process and direction of development of the single individual, and the latter are controlled by the former. It was established experimentally that the inhibition of reproductive caste development could be simulated by application of 'juvenile hormone', which in general inhibits the metamorphosis of larval insects into adults. The same hormone applied in higher doses to the termite society was shown to stimulate soldier production. With these findings, Lüscher made a breakthrough towards a new physiological understanding of termite caste regulation. The antagonistic effect of reproductives and soldiers that inhibit the development of their own caste and open the way towards development of the other could be interpreted physiologically: the reproductive castes transmit, by means of their pheromones, information causing the developing larvae to produce their own juvenile hormone, whereas the soldiers, probably also by means of specific pheromones, transmit information to inhibit juvenile hormone.

Recent results from Lüscher's team in Nairobi and Bern suggest similar exocrine-endocrine principles of caste regulation for the highly organized mound-building 'higher termites.'

To understand all these interrelationships Lüscher augmented his work with basic research into the mechanism of hormonal interaction. He investigated the mechanisms of action of juvenile hormone and the ecdysteroid moulting hormones. He studied the interactions and regulatory mechanisms of those hormones in the cockroach, a nonsocial insect related to termites.

Lüscher's name as a termite researcher and insect physiologist has international acclaim. He retained, however, his love for the countryside, for plant and animal life and for his fellow men. I best remember the hours I spent with him in the African bush, as we stood looking with astonishment at an opened termite mound. Here lay the motivation and drive for his life's work: the questions arising from his awe and respect at the incomprehensible and beautiful. This ethos together with scientific accuracy and genius gave him and his work its far reaching importance.

Reinhard Leuthold

G.S. Adair

GILBERT SMITHSON Adair, who died on 20 June 1979, was a highly respected and well loved character on the Cambridge scene for sixty years. Many generations of students and research workers have cause to be grateful for his willing help with practical advice and theoretical explanations. This ranged from supplying Max Perutz with his first haemoglobin crystals and instructing people in the art of preparing collodion membranes of varying permeabilities, to simple explanations of Gibbsian thermodynamics. In this way he exerted considerable influence although he was, to the best of my knowledge, only formally responsible for the supervision of two research students; Muriel Robinson (who became Muriel Adair) and the author of this notice.

Adair was born on 21 September 1896, the son of a Quaker family. He went up to King's, Cambridge, in 1915 and remained in that city, with only very brief interruptions until his death. He was a Fellow of his college from 1923 to 1933 and an honorary Fellow after retirement from his University post (Reader in Biophysics) in 1963.

The wide interest of biophysicists in haemoglobin, which extended over the last 60 years, involved several distinguished Cambridge men. Adair had the stimulation of A.V. Hill, Barcroft and Roughton, who not only contributed to our knowledge about the respiratory function of haemoglobin, but also used this important and plentiful protein to develop novel thermodynamic and kinetic methods. The iron contents had given an equivalent weight for haemoglobin of about 16,700 Daltons. Brown and Hill had shown that comparison of calorimetric and van't Hoff ΔH for oxygen binding gave about 17,000g of haemoglobin per mol of binding site. Osmotic pressure measurements preceding those of Adair gave variable results and tended to support the prevailing theory that haemoglobin occurred in solution with a molecular weight of about 17,000 and that there was some tendency towards aggregating of these units. Adair's attention to experimental detail is illustrated in a masterly paper "A critical study of the direct method of measuring the osmotic pressure of haemoglobin" (*Proc. Roy. Soc.* **A108**, 627, 1924) in which he reports a molecular weight of 67,000 and explains the erroneous results of others. It is important to point out that this preceded by about a year, Svedberg's determination of the molecular weight of haemoglobin in the ultracentrifuge.

Further studies of the osmotic pressure of haemoglobin solutions led Adair to detailed experimental and theoretical investigations of membrane potentials. In the 1930s he extended these studies to serum proteins and to the determination of charges on protein molecules from the measured membrane potential at different *p*H and ionic strength.

The above contributions led to his election to the Royal Society in 1939. During the war Adair was much occupied with the teaching of physiology, especially in practical classes. After the war, one of his main concerns was to improve his simple technique for measuring osmotic pressures, so that small pressures of solutions of large protein molecules could be measured. He also found it difficult to believe the results of his second research student, which indicated reversible dissociation of haemoglobin molecules in dilute solutions and at high ionic strength, until he confirmed it himself with his refined methods.

Adair was a shy but very friendly man who was absolutely devoted to his rather specialised, but very valuable work in the laboratory. The essence of his experimental technique was simplicity and attention to detail. Old fashioned alarm clocks and domestic heaters formed the backbone of his wonderful method for making semi-permeable membranes.

Apart from his scientific work Adair read widely and, in his youth, was a keen rock climber. There are a number of stories about his climbing activities at college and into the Physiology Laboratory. From 1931 until her death in 1977, he shared a life of work and simple pleasures with Muriel Adair. Their house near Grantchester Meadows allowed him to pursue his interests in the observation of a variety of plant and animal life.

H. Gutfreund