College. After gaining Higher School Certificate he was awarded a Kitchener Scholarship that enabled him to go to King’s College London to take a BSc. He then went to Edinburgh on a scholarship from the Agricultural Research Council (ARC) to do a PhD with Catherine Hebb. Though offered a junior lectureship in Edinburgh, he opted to move to The Physiological Laboratory in Cambridge where he remained for the rest of his working life, first as an ARC Fellow and then as a member of the academic staff.

Bill was elected a Member of The Physiological Society in 1955 and served on the Committee from 1980 to 1984. News of his death prompted a flow of emails from his Cambridge colleagues and ex-students. Woven together here, these highlight his standing as a physiologist, a teacher and a good and wise friend.

Bill’s lab was a solitary affair, untidy, full of antique equipment with reprints all over the place. Although constantly at the bench his publication list is short. He was always reluctant to publish unless he felt he had something worth writing about. His preference, when he did publish, was for a letter to Nature. He enjoyed discovering novel, important, and frequently counter-intuitive phenomena that would act as a stimulus to others to head off in new directions. He preferred, in his own quiet way, to lead the pack rather than to follow, and a letter to Nature is a time-honoured way of doing just that. Having cracked a problem, or demonstrated something important, he was content to let others sort out the boring bits while he moved on to new but always fertile ground.

Bill worked largely alone though early collaborative work included studies on the synthesis of acetylcholine in brain, carrier proteins for thyroxine and triiodothyronine, the role of the carotid body in the regulation of erythropoiesis, the secretion of progesterone by the adrenal gland, and absorption of colostrum in the newborn calf. Later, his main interest was in the regulation of blood volume. As a willing source of updated and critical wisdom on all aspects of kidney function and endocrinology he was invaluable to his colleagues who appreciated what one described as his high standards and sceptical integrity about science.

A trademark of his experiments was their simplicity. This is exemplified by an experiment in the late 1970s. It had been established that when a muscle-derived preparation of ATP was used in measurements of Na⁺-K⁺-ATPase the enzyme activity was less than that obtained with ATP prepared from yeast. The suspected inhibitory contaminant was eventually identified as vanadate, then attracting interest as a possible mediator of the diuretic effect of atrial distension. However, when sodium orthovanadate was injected into rats its diuretic effect was small and transient. On being consulted by the colleagues involved in this experiment, Bill’s reaction, to the surprise of all concerned, was ‘What a huge effect!’ He set out to prove that the apparently disappointing result was attributable to dehydration. An anaesthetized rat fitted with an intravenous cannula was put on an old-fashioned balance and a primitive but ingenious feedback system set up to keep the rat’s weight constant by intravenous infusion. Vanadate induced a diuresis comparable to the animal’s weight in a few hours. A beautiful experiment, with the simplicity only profound expertise can deliver.

Bill was a dedicated teacher and an excellent and caring Director of Studies in King’s College. His lectures on endocrinology are remembered for their clarity, logical progression, and the vast amount of fascinating information conveyed at a speed that allowed copious note taking (although, according to one student, this was at the expense of an aching hand).

Progressive ill health meant that in his later years Bill became increasingly reclusive but as a young man he had a passion for exotic cars. These included a red Gordon-Keeble – a British car of which only 100 were built, two of them being owned by fellow physiologists in the Cambridge Lab. Another enterprise was making his own trousers and shirts which he did with some success.

Our sympathy goes to Bill’s wife Margaret, his three daughters and his grandchildren.

Ann Silver
(using contributions from Hal Dixon, Alan Findlay, James Fitzsimons, James Hickson, Arieh Lew, Miranda Potter (née Harrison) and David Tolhurst).

Paul Lauterbur 1929–2007

Paul Lauterbur, who has died aged 77 from kidney disease, published the first magnetic resonance image in a short letter to Nature in 1973 (242, 190-191). Though the name he coined for the technique – zeugmatography - never caught on, the method has revolutionised medical imaging, particularly of soft tissues. An estimated 60 million or more magnetic resonance imaging (MRI) scans are now carried out each year. The invention of MRI won Lauterbur many prizes and awards, culminating in the 2003 Nobel Prize for Physiology or Medicine jointly with the British physicist Peter Mansfield.

Paul Lauterbur enjoyed being something of a scientific maverick. As a teenager he built his own basement chemistry laboratory, and would recall as an early inspiration a chemistry teacher who let him get on with doing self-designed experiments ‘while the rest of the class got a lecture’. The lifelong independence of mind led him to give up MRI research in his 70s, as he joked, ‘just in time for the Nobel’, to work on the possible pregenomic chemical origins of life.

Paul Lauterbur was born in Ohio in the American Midwest, and got his Bachelor degree from Case Institute of Technology (now part of Case Western University) before being drafted into the US army. As he described in a 2003 interview with Physiology News (55, 12-15), it was in the Army that he first got to use an NMR spectrometer, publishing several papers.
Following return to civilian life and completing graduate school, Lauterbur became an Associate Professor at the State University of New York (SUNY) in Stony Brook. Here he carried on pioneering work with multinuclear and particularly 13C NMR, including the first 13C NMR studies of proteins. In the summer of 1971 he got involved with an NMR instrument company in Pennsylvania, and the seed that led to the idea of MRI was planted when he watched living tissue experiments there. He often recounted the story of how he made the first notes of the basic concept of MRI on a napkin in a hamburger restaurant later that night. Back at Stony Brook that autumn he did the pioneering work single-handed, often using the NMR machine at night, and he did the pioneering work single-handed, making early images of simple ‘phantoms’ (capillary tubes of water and heavy water) and of seashells collected by his elder daughter Sharyn. Lauterbur liked to point out, in discussions on the beginnings of MRI, that Nature only published the seminal 1973 paper after he had argued long and hard with their original rejection of it, and also that SUNY’s patent department thought the whole idea of MRI too far-fetched to patent.

Through the 1970s Lauterbur’s lab at Stony Brook became a focus for others interested in the idea of imaging with NMR, and once MRI machines became a reality in the early 1980s he worked tirelessly to convince radiologists that the technique would provide new data not available from CT (computed tomography) scans. He took great satisfaction from the way that new applications of MRI in science and medicine continued to emerge, stating in 2003 that among developments of MRI that had most gratified him were its use in ‘functional imaging’ of things like brain activity and heart movements.

In 1984 Lauterbur married for the second time, to the American physiologist Joan Dawson (then at UCL), and they moved to the University of Illinois at Urbana. He is survived by Joan, their daughter Elise, and his son and daughter from his first marriage.

A century ago in J Physiol

As many readers will already know, the full archive of J Physiol is now available on Highwire, and can be reached from The Society web site. This allows one to see what was being published 25, 50 or even 100 years ago.

The four papers in J Physiol (35.4) (http://jp.physoc.org/content/vol35/issue4/) come from just two universities, Cambridge and UCL (March 1907). The authors include a future Nobel Laureate, Frederick Gowland Hopkins (Nobel 1929), a brilliant experimental physiologist killed in a WWI flying accident (Keith Lucas), and the Cambridge ur-pharmacologist W E Dixon. Two authors are among the UK founding fathers of sciences that ‘spun off’ from physiology – biochemistry (Gowland Hopkins) and pharmacology (Dixon) - or perhaps even three if one views Keith Lucas as the father of UK biophysics. The papers run the full gamut of the experimental systems of the day, ranging from amphibian and crustacean nerves in Lucas’ work, through frog muscle, to rabbits, cats, dogs and humans.

Papers 100 years ago came in all sizes. When I started in physiology J Physiol had a reputation for printing very long papers. This may well be an ancient tradition, as volume 35.4 opens with a paper by Fletcher and Gowland Hopkins that runs to a hefty 62 pages, albeit the smaller pages of the pre-1994 Journal. Fletcher and Hopkins’ paper on Lactic acid in amphibian muscle is heavy on methodology, with an Appendix even giving the (heroic?) numbers of frogs used in each experiment! Their painstaking experimentation does, however, give a picture of the conditions under which muscle lactate is produced that holds good to this day.

Keith Lucas’ paper includes some delightful diagrams of the self-designed and built equipment he used to deliver brief stimuli to nerves (e.g. his Fig. 2). Lucas worked in a special vibrationally-shielded basement room in the Cambridge Physiological Laboratory, inherited after his tragic early death by his former student E D (later Baron) Adrian. Lucas’ paper employs the amphibian sciatic nerve – gastrocnemius muscle prep still in use (one hopes) in undergraduate physiology labs. It is one paper of a staggering 25 Lucas published in The Journal between 1904 and 1914.

Academics in those days were clearly particular about their affiliations. Lucas does not list any degrees but gives his personal affiliation as ‘Fellow of Trinity College’, while Fletcher and Gowland Hopkins also give their college details.

The volume also includes a paper from the Department of Physiology at UCL (then run by Ernest Starling) by David Henquies de Silva. Silva later worked at King’s College, collaborating with W D Halliburton among others, and seems to have had biochemical inclinations; his later papers cover many subjects, often with a focus on digestive juices and blood.

The final paper in volume 35.4 is the only one not to come from a physiological laboratory. This is W E Dixon’s The action of alcohol on the circulation, detailing experiments on humans, dogs, cats and rabbits (!) to try and determine absolutely whether alcohol is a cardiac stimulant or depressant. The following excerpt particularly caught my eye:

‘If moderate doses of alcohol well diluted with water be administered to animals or men the pulse rate does not alter. I have tried these experiments over and over again and always with the same results. The popular fallacy that alcohol quickens the pulse is clearly derived from the conditions under which alcohol is usually taken. It is well known that excitement of any kind quickens the heart and alcohol is generally taken under exciting circumstances.’

There is a highly entertaining account of Dixon’s career written by Alan Cuthbert in his 2001 W D M Paton Memorial Lecture (Cuthbert AW 2001. Br J Pharmacol 133, 945–950). Cuthbert wryly points out that though Dixon was the founding father of pharmacology in Cambridge, Cambridge never got round to making him a Professor. Dixon was a professor, however, as for many years he held a lectureship in Cambridge simultaneously with a Professorship at King’s! Cuthbert quotes Dixon’s motto as ‘Dire n’est rien , faire est tout’, and tells a fascinating story of how Dixon almost discovered neurotransmission almost a decade before Otto Loewi.

Austin Elliott

Paul Christian Lauterbur, pioneer of 13C NMR and co-inventor of MRI, Nobel Laureate, member of the US National Academy of Sciences (1985) and Honorary Member of The Physiological Society (2004).

Austin Elliott