Kenneth Burton FRS (1926–2010)

Professor Kenneth Burton was elected a Fellow of the Royal Society in 1974, an honour shared by two other scientists who worked with Sir Hans Krebs in his Sheffield days: Sir Hans Kornberg (1965) and Rod Quayle (1978). Luck plays a part in our lives. The American golfer, Arnold Palmer, famously remarked that the more he practised, the luckier he became. There is, of course, an element of luck in science as well as in sport. Many scientists have one stroke of luck; geniuses like Sir Hans Krebs strike lucky over and over throughout their lives. Ken Burton had three occasions when he struck lucky.

The best known of these was his citation classic of 1956 in which he described a sensitive and reproducible assay of DNA which proved so popular that it is still being cited 50 years later and is known as 'the Burton method'. Over the years, that paper has been cited by other scientists 17,000 times, about 1000 times more often than the average scientific paper. It came about as follows: Ken, on a Fulbright Fellowship in the Chicago laboratory of E.A. Evans, was using a traditional Dische diphenylamine reaction to estimate DNA. His first stroke of luck was to leave the half-completed assay tubes on the bench overnight instead of staying late to finish the experiment according to protocol, because next morning Ken was surprised to observe that the colours had developed overnight much more intensely than by the traditional boiling procedure. His second lucky break was when he observed that the colours that developed with Chicago acetic acid did not develop similarly when he returned to Oxford. Ken’s real achievement in this case was having the curiosity and the prepared mind to interpret and exploit these rather accidental observations. Thus he guessed that the Chicago acetic acid might have oxidized on standing, and showed that the 30°C assay could be made sensitive and reliable by adding a small amount of acetaldehyde to the acid (“probably as a chain initiator”).

Much more important, scientifically speaking, was Ken’s paper of the previous year in which he showed that viral DNA synthesis (in bacteria) required viral proteins to be synthesized first. This is a very fundamental point in understanding viruses, and of course antiviral medicines. Virus reproduction requires viral proteins.²

It is arguable, however, that Ken’s greatest achievement was his publication of the following year, 1957³. He wrote an appendix to a massive paper by Krebs and Kornberg, in which he calculated and listed the free energies of formation of all of the compounds of energy metabolism, from the sugar we eat to the CO₂ we breathe out, and most things in between. Things fall downwards, in biochemistry, as in every other field. Ken’s tabulation allows us to see which direction is down (in this metaphor). It allows us to understand why things happen, everything from the contraction of muscle to the synthesis of protein. Countless students of biochemistry learn and quote values from that table (for example the midpoint potential of NADH/NAD⁺, or the Gibbs free energy of ATP hydrolysis) without realizing that they are quoting Ken Burton. Were it possible to count citations in this case, I think it would run to millions.

Some of this may be news to Ken’s friends and even to his departmental colleagues, for I never heard Ken talking proudly about his successes. He was a singularly unboastful man. Most of us are motivated, at least in part, by self-love, prestige and scoring over contemporaries. This can get so bad, in a mediocre department, that one person’s success is generally bemoaned, rather than cheered, because it implies failure for the rest. Ken rose above that. His objectives seemed more remote, more absolute; he was motivated, I think, less by the glory of doing science, than by the fun of it, or a simple need to know. Characteristically, he liked to tell not of his fame, but the story of a failure, or of bad luck; of how, when a PhD student with Malcolm Dixon, he had missed identifying the role of coenzyme A in citric acid synthesis, thus allowing Fritz Lipmann to announce that discovery a year or two later. Ken had isolated an acetylated compound containing adenine, ribose and pantothentic acid. He even wondered whether that might be how carbon entered oxidative metabolism, but the conventional wisdom of the time was that material entered metabolism as a 3-carbon unit (e.g. pyruvate) not as a 2-carbon acetyl unit. Ken’s mistake on that occasion (as he later said), was in paying too much respect to the opinion of colleagues.

Ken’s achievements were significant, but why did they happen to him? Well, he was very sharp; he was well grounded in chemistry, physics and maths (his undergraduate subjects); he had a roving curiosity, a sense of enjoyment in science, and persistence. He studied for his PhD under a master of benchwork biochemistry, Malcolm Dixon; a legendary enzymologist, an intensely shy man, but a very considerable pianist. (It was said of Malcolm Dixon that he was as happy fixing a gas boiler as doing biochemistry, and that his students tried in vain to stop him dismantling the new spectrophotometer out of pure curiosity.) When Ken moved to Sheffield in 1949, he came under the mantle of one of the world’s great scientists, Hans Krebs.
Krebs was hard-working, methodical, but imaginative. He was remarkable among scientific bosses in not letting his name get on to the papers of colleagues merely because he was their boss. Krebs encouraged Ken's independence of mind. Ken was remarkable too, in a similar way, in that a high proportion of his papers are single-author papers. He did his own thinking and his own experiments. Medawar pointed out that to achieve a big answer, you have to tackle a big problem; Krebs gave Ken access to the big question of free energies.

After those three classic papers of the mid-1950s, Ken pursued two other major projects, with equal tenacity and skill, but to less acclaim. He and his students followed up the diphenylamine reaction for more than a decade, developing methods for cleaving the long threads of intact DNA in the hope of finding a way of determining the sequence of bases and thus ‘reading’ the genes. In this, they initially had some success. On receiving the 2009 Nobel Prize, Elizabeth Blackburn recounted using “Ken Burton’s depurination reaction” in her 1975 investigations of satellite DNA (although she wrongly concluded that Ken was a New Zealander). However, much more powerful methods of sequencing were developed by others, and Burton’s depurination method has been forgotten.

In 1966, Ken became the first professor of Biochemistry at Newcastle upon Tyne. Designing the course and selecting staff took over his time and energies, but did not completely prevent active research, nor indeed bench work. Ken soon started an investigation into the mechanism by which adenine is actively taken up by bacteria, a project he pursued almost single-handedly until retirement. It was not fashionable. It was not even surprising (“bacteria take up everything; so what!”). The trend in science was towards progressively bigger research groups getting more and more of the money. With ingenuity, dogged persistence in the face of a crippling stroke, and a ‘shoestring’, Ken eventually brought the subject to a satisfactory conclusion with his last scientific paper in 1994, identifying the purP gene and proposing a proton-linked uptake with intense product inhibition (to prevent the build up of mutagenic concentrations of internal adenine).

I have tried to summarize Ken Burton the scientist, but that is only one aspect of the whole man. I understood that Ken had enjoyed building the department from scratch, but had found the deeply entrenched Medical Faculty frustratingly aloof, if not actually hostile. Physical integration of Biochemistry into the Medical School only came in 1985 when Ken was Dean of Science. In his own department, ‘Prof Burton’ was universally regarded as a completely fair, singularly unaffected by egotism, unusually given (for a head of Department) to working at the bench, and an enthusiastic walker.

Professor George Petersen, one of Ken’s first research students and a lifelong friend, remarked on Ken’s “endearingly impish sense of humour”, which is beautifully displayed in the Biochemical Society Archival video recording an interview between Burton and Petersen in 1990 (www.filmandsound.ac.uk/collections/records/0028-0000-2746-0-0000-0000-0-0000-0-0.html). He and others of Ken’s friends have enjoyed recalling some of Ken’s foibles, which all seem to point to an agile mind that occasionally wandered. Thus, in mid conversation, he could drift off into a kind of trance. Or if you were speaking to him, he was inclined to murmur “yes-um, yes-um” in a mildly disconcerting way. Basil Smith, another DPhil student in Ken’s Oxford laboratory, recalled a distinctive way Ken had of terminating a conversation. Ken was not the most lucid of lecturers, and he knew it. He told a story remembered by both George Petersen and Basil Smith, that on one occasion he woke to find himself lecturing. Laughing, the PhD students asked him what he did then, and Ken said “I just carried on”. If Ken could mystify when face to face, his written prose by contrast was extremely lucid.

Malcolm Page, who did his PhD with Ken in the mid-1970s (and subsequently worked with me), writes: “Having now supervised a few students myself, I realize what a special quality [Ken] had, and I have endeavoured to give my students the same freedom.” Burton could well have said the same of Hans Krebs, and thus we see the baton passed on.

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References