

# Microbiology and me in 1952

John Postgate

Former SGM President and distinguished writer John Postgate looks back at the state of microbiology in 1952, the year when Jim Watson made a presentation at an SGM meeting.

In that year the microbiological scene was buzzing with activity. As usual, progress was driven by new experimental techniques. New ways of disrupting bacteria without denaturing their constituent proteins had greatly facilitated the preparation of active cell-free enzyme preparations. The traditional Warburg manometer was obsolescent, albeit still useful in special contexts, and cumbersome analyses by microbiological or enzymic assays had been widely replaced by paper chromatography. This could be rendered semi-quantitative by ingenious gimmicks such as photometric or densitometric scanning of chromatographs, or weighing cut-out spots, but the truly quantitative partition chromatography, and its ion-exchange analogue, were coming into use. Many biochemicals labelled with radioactive isotopes for use as tracers were now available from dealers; centrifuges had diminished in size and improved enormously in quality; optical colorimetry and spectroscopy were being displaced by electronic devices.

In consequence, papers exploiting such techniques to determine the minutiae of bacterial and microfungus metabolism had been dominating the contents pages of *J Bacteriol* and *J Gen Microbiol* for 3 or 4 years, and many pathways of catabolism, respiration and fermentation had been elucidated. 'Simultaneous adaptation' was illuminating more specialized degradation pathways, such as the breakdown of aromatics by pseudomonads. Pathways of biosynthesis of carbohydrates, of amino acids, haems and micronutrients, for example, were hot research topics, the major research tools being radioactive tracers, metabolite analogues (to block pathways), and examination of syntrophies among related auxotrophic mutants. At a more holistic level, electron microscopy had come of age and was giving reliable information about the structure of viruses and the interiors of bacterial cells; the existence of an osmotic barrier, somewhere beneath the cell wall and associated with a semi-permeable surface zone, was clear, and strong evidence was accumulating for sub-cellular compartmentation. Despite its featureless appearance under the microscope, the bacterial cell could no longer be regarded as an amorphous bag of enzymes.

I was then a newly

fledged full-time research microbiologist, 4 years after completing my doctoral research at Oxford, employed by the Department of Scientific and Industrial Research (DSIR) at the Chemical Research Laboratory, in the enclave of the National Physical Laboratory, at Teddington outside London. I was part of a little team of four scientists headed by a desk-bound K.R. Butlin, one of the handful of groups in the world who studied sulphate-reducing bacteria. It may seem odd that a tiny microbiology unit existed among all those chemists and physicists, but that is how things were. Microbiology may by then have gained recognition as a distinct discipline in academe, but not among the administrators of the DSIR. To them it was a fringe pursuit, an aberration of medicine, or perhaps something to do with brewing. They did not know quite how to fit microbiologists into their scheme of things, and rather wished they would go away.

I worked with traditional equipment for growing strict anaerobes: tubes with plugs soaked in pyrogallol, and Knight & Fildes anaerobic jars. I calibrated my Warburg manometers. I used paper chromatography backed by old-established chemical analyses, a 'modern' but jittery centrifuge suspended on rubber coated springs, and so on; my main concern was how sulphate reduction was linked to substrate catabolism. Viruses, protein synthesis and nucleic acids were remote from such smelly pre-occupations, so I was more an observer and exploiter of the great developments that were taking place (a few years later I was able to tip in my own two-pennyworth, but that's another story). Yet I was well aware of the big questions of the day: what were viruses?

RIGHT:  
Practical microbiology ca 1952. The author inoculates specimens of iron and steel with a laboratory culture of sulphate-reducing bacteria before burial. They will be exhumed in later years, as part of a field study of anaerobic corrosion. COURTESY J. POSTGATE



*'Luria's absence thrust upon me the job of describing the recent work of the American phage workers. There was no need to put together a speech. Several days before the meeting, Al Hershey had sent me a long letter from Cold Spring Harbor summarizing the recently completed experiments by which he and Martha Chase had established that a key feature of the infection of a bacterium by a phage was the injection of the viral DNA into the host bacterium. Most important, very little protein entered the bacterium. Their experiment was thus a powerful new proof that DNA is the primary genetic material.*

*Nonetheless, almost no-one in the audience of over 400 microbiologists seemed interested as I read long sections of the letter... Moreover, when it came out that I was an American, my uncut hair provided no assurance that my scientific judgement was not equally bizarre.'*

Those are the words of J.D. Watson in *The Double Helix*. In fairness to his audience one must add that his diction (head down, reading passages from a letter in not the easiest of mid-West accents), and the hall's poor acoustics, had much to do with his lack of audience response; but even in 1952, scepticism about the genetic role of DNA was still widespread...

Excerpt from *Fifty Years On* by John Postgate (1995), Society for General Microbiology, Golden Jubilee Brochure.

American post-doc then working at Cambridge, UK, deputized for Luria and took the opportunity to read to the meeting a long letter from A.D. Hershey, which described Hershey's and Chase's now famous experiment demonstrating that phage DNA, and not phage protein, was the infective material which initiated virus synthesis. Essentially Hershey and Chase had prepared T2 phage labelled with radio-S (marking the protein component) or radio-P (marking the DNA component), and demon-

How did they originate? And what controlled the replication and heredity of bacteria? Nucleic acids? Autocatalytic enzymes? Specialized proteins?

The SGM's meeting on the *Nature of Virus Replication* at Oxford in the spring of 1952 promised a degree of enlightenment, as well as the prospect of gleanings for my own research – not to mention the opportunity to meet old microbiological chums. The overt theme of the meeting, set up by the distinguished plant virologists N.W. Pirie and E.C. Bawden, was how did viruses multiply when they had, or seemed to have, no metabolism of their own? It was a timely, forward-looking theme, for consensus was badly needed on what a virus was; whether it was fundamentally a rogue protein, a rogue nucleic acid, a degenerate bacterium, or a rogue cluster of genes – whatever the latter might mean in the days when the chemical nature of genes was in doubt. Yet most microbiologists already felt that the rogue gene cluster idea matched up best with the general behaviour of viruses, and that therefore study of their multiplication would yield valuable information about heredity in general. Bacteriophage multiplication and lysogeny seemed to offer a relatively simple model system for virus action; yet even here at least one scientist argued passionately at the meeting that bacteriophages were endogenous lytic products of the bacterium, not viruses, so such studies were irrelevant.

Overshadowing the whole meeting was a wholly non-microbiological affair. An invited speaker from the USA, bacteriophage expert S.E. Luria, was not present, because at the last moment the US authorities, then in thrall to Senator McCarthy's Committee on Un-American Activities, had confiscated his passport – they suspected Luria of holding communistic views. (His written paper was nevertheless published in the SGM symposium volume). Jim Watson, a peripatetic

strated that, when infection of the host bacteria was interrupted mechanically using a blender, the radio-P remained in the bacterial cells, in which phage multiplication continued, and the radio-S washed off the cells and could be precipitated and recovered. The controls were proper, and the work was powerful evidence that phage heredity resided in its DNA. Watson, thrilled by the letter, recorded in *The Double Helix* his disappointment with the Society's response (though for my part I am not sure how well his message got through; see box) and the hurt was still there in 1993 (see Watson's *A Passion for DNA*, OUP, 2000, p. 23).

However, the message certainly registered where it mattered. Roger Stanier took me to a gathering that evening in one of the rooms of Lady Margaret Hall, where the conferees were billeted, and I was able to be a fly-on-the-wall while pundits in the infant field of viral molecular genetics cross-examined Watson, aided by a bottle of calvados which Roger had imported from France. Details of the discussion, and the names of the participants, have long vanished from my ageing memory, but I well recall the sense of excitement in the room, childish pleasure even, as Watson parried the company's interrogation and the elegance and plausibility of Hershey and Chase's experiments were revealed. History has confirmed that it was not just the calvados!

● *Professor John R. Postgate, FRS was President of the SGM 1984–1987. His popular books have helped to enthuse and inform many members of the public about microbiology. 'Microbes and Man' is in its 4th edition. He may be contacted at Houndean Lodge, 1 Houndean Rise, Lewes, East Sussex BN7 1EG, UK. email johnp@biols.susx.ac.uk*