

Peter Mitchell—a remembrance

Peter Mitchell, who received the Nobel Prize for Chemistry in 1978 for developing the chemiosmotic hypothesis linking mitochondrial metabolism to transmembrane proton gradients, died on 10 April. V. Sitaramam provides a personal glimpse of the man. Mitchell created his own laboratory at home in Cornwall and shunned the traditional scientific establishment. Nevertheless, he achieved remarkable success in introducing broadly unifying concepts into the murky field of oxidative phosphorylation. Interestingly, the chemiosmotic theory was propounded in detail in the sixties in two privately published monographs, referred to as the 'grey books'.

—Ed.

Peter Mitchell died of cancer on 10 April 1992. To me, the man and his hypothesis have always remained an enigma. His story is probably the only instance wherein the development of the subject itself came under a concurrent, detailed sociological scrutiny¹. There are so many aspects of his hypothesis that are so clear and so many aspects that elude definition that it has become an obsession, magnificent no doubt, for those who have encountered it in their professional lives as biochemists. I happen to be one and I wish to remember the man and his work, which absorbs me immensely.

I met him not too long ago. It was a detour to Bodmin on my way to the European Bioenergetics Conference at Aberystwyth, Wales, in 1988. I had a specific purpose: If I object to the chemiosmotic hypothesis as many still do, would he consider the specific reasons I put forth as trivial? How would he react to an opposing view? I am glad that I felt compelled to visit him for a couple of days. The Mitchells were very kind and hospitable. Prof. Mitchell was deeply concerned about the state of science and the status of British science. He quipped about the problems I was facing with the chemiosmotic hypothesis since he already got the prize.

The real business started the next day. I found him quite up to date with the little that I had published and proceeded to object to most experiments I had ever done. The nearly three

decade difference in our ages was almost immediately forgotten and we had a roaring argument. He was clear with regard to two issues: there is no way one can question the hypothesis unless a single compelling argument is put forward to explain why so many experiments involving proton fluxes have succeeded; and secondly, so many new observations could well be there



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(like variable porosity of the inner membrane as we have shown) and yet, without a singular hypothesis that is insight-based, these would not add up to much. While the merits and demerits of all our arguments are not relevant here, he was very receptive, candid and insistent but not patronizing. He was also very fair. He was not convinced that our experimental results (at that time) showed that the hypothesis is

untrue and yet he would accept that the experiments themselves were a serious effort and cannot be dismissed either. I came back with the distinct impression of the master being more accommodating than the disciples.

It was said rightly that the Mitchellian hypothesis gave a universal menu for transduction mechanisms. Is the menu ideational or is it factual? Is it the grammar or the language? If it could be that distinctly different models and hypotheses come to pass, would it make his contribution any less? Probably not. The Mitchellian message is that of the individual, the art of the possible. One hopes that what was a heterodox view itself does not concretize into an orthodox view. Any model that replaces the Mitchellian hypothesis has the absolute requirement of being more beautiful. He provided a reference standard hard to match.

Only later I remembered and wrote to him that I had not actually read his 'grey' books, though I had been referring to them. He promptly sent an autographed copy by return mail to Pune.

1. Gilbert, G. N. and Mulkay, M., *Opening of Pandora's Box*, Cambridge University Press, Cambridge, 1984.

V. SITARAMAM

*Biotechnology
University of Poona
Pune 411 007
India*