

ESSAY 21: Graduate Studies

I regarded graduate studies (1971 - 1974) as my reward for the top first of 1971. The Dr Samuel Williams studentship was riches for me - more than six hundred pounds a year. My undergraduate grant was a standard one of about 282 pounds a year, with fees paid. So that restricted me to chips, green peas and salad cream (because like the salt it was free). Nevertheless such small sums of money taught discipline, especially as my parents could not pay anything. If not exactly glad to get rid of me, they were at the same time not interested in paying for education in order to get rid of me. This is not to say that relations between them and me were ever bad, it was just that they had never had the experience of someone going to college from the family and did want to deal with the boring affectations of the middle classes of a small, damp town. In the village we were more cultured than the small town. When I first entered room 262 in the Edward Davies Chemical Laboratories (EDCL) I felt an exhilarating sense of freedom, although all I was offered was part of a wooden bench across the way from an interferometer and high pressure cell. Room 262 was a laboratory specially designed for infra red research, and I felt immediately that this was the life for me, not cleaning out a cowshed. I was immediately on my own, my Ph. D. Supervisor Mansel Davies was well past it, embroiled in perennial committees, and had not prepared for my arrival in any way at all. I remember that he scribbled a few ideas on a piece of paper and ushered me out of his office. In room 262 however there was a sense that things could be done at last and that I was no longer entirely the anonymous servant of a system. Mansel Davies was fundamentally of the same background as myself, and was fundamentally a kindly and highly intelligent man. However by 1971 he was trapped in a system which made him act as a member of the middle classes. Even his accent would drift from broad Aberda^r to pseudo cut glass and back again. However, he did have the gift of insight, and fortunately for me allowed me to go my own way.

The start of my Ph. D. work overlapped with the last few weeks of the M. Sc. work of Arnold Baise, so he taught me the use of the interferometer and the Elliot Brothers 4130 computer. At the same time I learned Algol, and the use of the computer to solve difficult problems. Up to that point I had done everything by log book, and there were no hand calculators in 1971. The Fourier transform was done with paper tape and a pack of cards. Each time one had to go up the hill to the computer unit to hand in the pack of cards, hoping that the computer was functional. Fortunately, my father had kindly lent me his old car, so I did not have to walk. At the EDCL there was also a library self contained within the building. So all the ingredients were there for the methods I still use now, of producing scientific papers to the best of my ability.

In taking the spectra there was often a profound sense of excitement, the same kind of excitement as in the discovery of a new continent or island. In this process of discovery I could share things with Mansel Davies as an equal, we were both scientists in the purest sense of the word. If science consisted of pure discovery it would be a much healthier pursuit. However, being part of human society, the discovery is clogged into the exigency of the machine as R. S. Thomas so memorably wrote. I soon began to think of what would happen once my three years of Ph. D. were up, and discovered that one had to win a post doctoral to stay in research. This is the point at which pure science becomes corrupted with careerism. In order to win a free post doctoral one had to write papers and get them published. So I began writing. At that time I had to give the paper first to Mansel Davies for his approval, then submit it. As usual in the academic system the referees= reports were interminably slow in arriving, but despite all these obstacles the published papers were produced in time to be bound into my Ph. D. Thesis, now in the National Library of Wales.

There were frequent apparatus outages because the interferometer was an old machine on loan from the National Physical Laboratory. In those intervals I began to develop original theoretical methods for the first time, and from the outset to learn the discipline of self checking of theory and its comparison with data at each stage. I found quickly that in order to code a computer the theory has to be understood perfectly. One of Mansel's ideas was to have me leave Aberystwyth within months of starting my Ph. D. In order to work with his friend Brot in Nice and his friend Rivail in Nancy. The visit in 1973 lasted a few months and despite the chaotic conditions at Nice I did learn the use of the correlation function and Laplace transform. Nothing was prepared for me at Nice, and Brot just asked me what I wanted of him. At Nancy things were more organized, but on return to Aberystwyth I really got going with the use of Algol to transform spectra into correlation functions. The great advantage of the method is that theory could be brought to bear on the far infra red. These early papers are now in the Omnia Opera on www.aias.us and in the National Library of Wales and led to the Harrison Memorial Prize and Meldola Medal of the Royal Society of Chemistry. All Aberystwyth had to do was let me work in my own way, but it could not even do that. So now it has no relevance to me and little to Wales, it is just an insignificant college in world rankings while AIAS is number one. I may tread heavily but write the truth.

This golden time lasted a few brief months only (from summer 1973 to autumn 1974) before the system forced me out of Aberystwyth on an SRC post doctoral in 1974 at Oxford. When I arrived there, Rowlinson was again totally unprepared for my arrival. I was again asked what I wanted. The answer on both occasions is that I wanted nothing but to stay at Aberystwyth, but did not voice this truth at the time. I won three free post doctorals in open international competition in 1974, all under my own initiative. It was clear that I should have been promoted to tenured lecturer at that point, but the exigency of the machine (the workings of human society) prevented that from happening, causing lifelong problems with Aberystwyth. What is really needed in research is stability of environment, and that has been available in the years in which ECE theory has flourished. Mansel again wanted me to go back to work in 1974 for his friend Brot on one of the post doctorals I had won, an ICI European post doctoral fellowship. That was a non starter, and for the first time I stood up to Mansel and insisted on going to the least disruptive locality, Oxford. I realize now that I should just have switched to another department at Aberystwyth because my SRC Fellowship was transferable. The acquisition of knowledge at Aberystwyth was embroiled in incredibly bad management, so ultimately one had to move entirely out of the system in order to be a scientist.