Early days - J B West

I joined Geoff Marr's UV spectroscopy group at Reading University as a post-doc in the autumn of 1970, to work on his SR programme either at the 330MeV synchrotron at Glasgow University or on the new project at the Daresbury 5GeV synchrotron. I chose the latter as it seemed much more exciting and was entirely new, although I did play a minor part in his work at the Glasgow machine, together with an Australian post-doc Mervyn Lynch. This machine used to run at 4Hz at a current around a tenth of a milliamp, but even so the Reading group carried out the first experiments on photoelectron angular distributions and partial cross sections on the rare gases. The data were not conclusive but showed what could be done if more intensity were available. Geoff Marr had obtained some funding to build a beam line on the East Kilbride 100MeV linac, using a superconducting magnet, but this was abandoned when it was discovered that Ian Munro at Manchester was developing SR activity on the Daresbury synchrotron, and the two groups were encouraged to work together on the Daresbury SR project. I entered the project when this collaboration was well under way, though I do remember going with Ian Munro and Keith Codling to Glasgow, and Ian saying "Gee, if I knew that this facility existed I'd never have got involved with Daresbury!" In retrospect, it's as well that he didn't know, since the Glasgow machine had nowhere near the development potential that Daresbury had.

By the time I arrived the new Synchrotron Radiation Facility (SRF) bulding was under construction, and two beam lines, the North Line and South line, were being designed. We had to become familiar with a new way of working, ie remote control of all equipment in the experimental areas since these were locked up and therefore inaccessible when the SR beam was in use. Manchester and Reading universities were responsible for developing instrumentation on the North line, and Oxford University, with Bill Hayes and his post-doc John Beaumont in charge, had the responsibility for the South line. My task was to develop a grazing incidence monochromator for the North line, and to assist Manchester with a normal incidence monochromator on the same beam line. Reading's research programme was in atomic spectroscopy, and together with Manchester worked on the development of heat pipes for measurement of total photoionisation cross sections of metal vapours. Manchester also continued with their work on organic scintillators, as well as making measurements on molecules trapped in rare gas matrices. Bill Hayes was mainly interested in solid state research, and hence wanted a normal incidence monochromator for work in reflectivity of solid surfaces and solid state absorption.

In the early stages of beam line development there was considerable, and at times quite hostile rivalry between the North and South beamlines, mainly due to competition for resources. I particularly remember a heated argument at Reading between John Beaumont on the one side and Geoff Marr, Ian Munro and Keith Codling on the other, over who should have the South line and who the North line. John Beaumont and I did however collaborate in the area of instrument design and manufacture, having found, via different routes, the company Bird and Tole, whose excellence in instrument manufacture soon came to be legendary. By the end of 1971 the first three instruments were nearing completion and the normal incidence instrument on the North line, the so-called horizontal Wadsworth constructed by J & P Engineering of Reading, was already being installed. Its first exposure to SR was traumatic; the replica grating was rapidly destroyed, the gold coating erupting in large bubbles as the underlying resin outgassed when subjected to the X-ray part of the SR spectrum. The search was on for so-called master gratings, where the rulings were made onto a gold layer and no resin was used. It took some time to find a manufacturer prepared to make such gratings. Hilger and Watts supplied the grazing incidence with its grating, and Jobin-Yvon made gratings for the normal incidence instruments. Even then, the gratings did not last long because in the high vacuum of the beam lines residual hydrocarbons cracked onto the surfaces reducing their reflectivity, but at least the experimental programme could begin. At around this time there was a major accident with the grazing incidence monochromator for the North line: a glass plate fitted temporarily to the exit port shattered under vacuum and effectively shot-blasted the instrument. True to type, on hearing this the South line team, mainly John Beaumont dancing on what he thought was the corpse of the North line monochromator, tried to divert all resources to installing their monochromator on the South line. However, after completely dismantling the grazing incidence monochromator and thoroughly cleaning all its bearings and precision mechanisms, this spectrometer was reassembled and was ready for installation within two weeks. The fact that this was possible testified to the excellence of the design and manufacture by Bird & Tole, although they were horrified when they heard what we had done to their spectrometer. There was however some good to come out of this incident, since during dismantling we discovered machining oil between some of the parts pinned together; had it remained there the instrument optics would rapidly have become contaminated, rendering it useless. We came to learn how an oil free vacuum was essential to maintain optical performance, and although this was never achieved at the SRF, with the consequent need for frequent recoating or replacing the beam-line optics, the lessons learnt were put into effect when beam lines for the new SRS were designed.

During 1973 and 1974 the experimental programmes on both beam lines began in earnest. The metal vapour heat pipes produced useful data on the sodium and cadmium atoms but proved unreliable for other species. Soon the Reading programme switched first to measuring rare gas total photoionisation cross sections and then to photoelectron angular distributions and partial cross sections. This work attracted considerable interest internationally since there were groups at Orsay and Wisconsin doing the same kind of work, but it was fair to say that at that time the SRF led the world in this area. The Manchester group were also advancing their work on fluorescence life times using the short pulse length form the synchrotron. NINA was not ideal for this but valuable experience was gained and put to good use in the successful development of the technique on the SRS.

At about this time the SRF, now seen to be a promising facility for SR work in the UK, was attracting interest from several groups wanting to work over the whole range of the EM spectrum. Many of these groups had shown some interest in the initial proposal for the facility, but now came forward with definite plans for new instruments and experimental stations of their own. Peter Key from the NPL installed equipment on the South line for making absolute intensity measurements in the VUV, using the calculated SR spectrum as a radiation standard. John Forty from Warwick obtained funding for a substantial programme on photoemission from solid surfaces, including grazing incidence and normal incidence monochromators to be installed on the North line. Malcolm Howells moved from his Manchester post-doc position to design the optical instrumentation for this programme. One of the most interesting instruments to be fitted on the North line was a 5-metre extreme grazing incidence spectrograph, used by David Creek from AWRE to make absorption measurements in the soft X-ray region. The first signs of an X-ray diffraction programme arrived, with Wazi Farouki and John Hazelgrove from Cambridge University beginning measurements on collagen on the South line. This was a foretaste of the future, though few of us realised it at the time; X-ray diffraction, scattering and crystallography would eventually dominate the SR programme in the UK.

It was at that time that the SR programme began to influence the way NINA was run, since in principle the SRF was parasitic and had no say in the running characterstics or scheduling of NINA, this being the prerogative solely of the high energy physics community. Two studies changed this: one, for which I presented an analysis of recent NINA cycles, was to show that if a criterion of “mA.hours” were adopted, ie a measurement of how long NINA ran at a usable current for SR users, the overall efficiency for SR often fell below the 25% level, as distinct from the ~90% efficiency level the HEP users were reporting for their work. The second, pointed out by Wazi Farouki, showed the impact of the HEP programme on X-ray users: for HEP experiments, the electron beam from NINA was extracted at or just below peak energy, thereby reducing the X-ray photon flux to the point where their experiments were no longer viable. All this resulted in NINA beam time being dedicated to SRF users, somewhat to the consternation of the HEP community since they were paying for the running of the synchrotron. This was a clear indication of the future; to put it another way, it now seemed that the parasite was about to eat the host. Although the existing programmes were in fact very productive it was clear that demand was exceeding capacity and a new dedicated facility was needed if the UK was to maintain its growing reputation in the exploitation of SR.

The need for a dedicated facility had been recognised as early as 1974, when it was clear that exploitation of SR was going to grow in the UK as it was already doing in the rest of the world. Recognising that NINA was due to close in 1977, the SRC was receptive to discussions and plans for a dedicated source, and agreed to the setting up of four panels: crystal optics, VUV optics, beam lines and machine design, these panels to decide requirements for the new source in their respective areas. One of the crucial points, at least from my point of view, was access to the experimental stations while in use, since having to remotely control experiments and monochromators was a serious limiting factor in carrying out complex experiments. The fact that the new source was a storage ring promised to make this practical, but persuading those responsible for building the accelerator and maintaining radiation safety standards took considerable effort. The experience gained at the SRF, particularly the need for vacuum cleanliness to maintain optical performance was taken on board by the optics panels, and led to a requirement of UHV in all beam lines and monochromators, including residual gas analysis to determine the level of contamination by hydrocarbons. The reports of these panels formed the basis for the design of the new storage ring, the SRS; the only disappointing feature was the large electron beam size, particularly the horizontal dimension; this was later rectified when the high brightness lattice modification was undertaken in 1984.

As for instruments, very little could be taken from the SRF. The Vacuum Generators ADES system, and the much travelled 2nd grazing incidence spectrometer (returned from Wisconsin; see later) were transferred, but in general the old equipment was unsuitable for this new kind of source, and in any case could not meet the vacuum standards required. For the X-ray region of the spectrum, although the vacuum requirement was much relaxed because in general the experiments were isolated by Be windows, very little instrumentation existed anyway so a considerable fraction of the reources available had to be devoted to this fast growing area. One of the most expensive was the construction of hutches to shield the users from scattered X-ray radiation; this was an area where access to experiments taking beam was simply not feasible. For these reasons it was some considerable time before the SRS achieved its full complement of experimental stations.

No account of the early history of SR in the UK, centred as it was around the Daresbury SRF, is complete without including the personalities involved. We were mainly physics experimentalists whose way of doing things did not always go down well with the civil service methods and mentality of Daresbury Laboratory. Ian Munro was the User Coordinator, in a joint appointment with Manchester University. Being a physicist who enjoyed working on experiments he typified and set the pattern for the pioneering spirit of the SRF, with little regard for those civil service rules which in his view served no useful purpose, and that fitted in very well with the rest of us. Geoff Marr had an academic approach to research, never losing sight of why we were there in the first place; both he and Keith Codling knew what the important experiments to be done were and through that managed to maintain the SRF's lead in their areas of interest. Keith's irreverent sense of humour, as well as his experience and expertise in the use of SR, were of great value to the facility and to me in particular. Those who joined the facility after it began, although individually different, shared the same enthusiasm for research, were universally hard working and brought to the facility new ideas, broadening its scientific basis. Many of these went on to build up foreign facilities after the SRS closed; Malcolm Howells went to Brookhaven and to the ALS, developing soft X-ray optics and VUV interferometry. Gwyn Williams, working with Colin Norris on solid state photoemission, also went to Brookhaven and developed a world leading programme in far IR spectroscopy. Hugh Huxley, working with Wazi Farouki and John Hazelgrove, began a muscle diffraction programme at the SRF and which became a major part of the SRS experimental programme. Some stayed at Daresbury, or returned to Daresbury after the SRS started: Joan Bordas, who came to the SRF to work on solid state spectroscopy and begin experiments to demonstrate atomic effects in solids, later moving to fibre diffraction at the SRS, then director of the SR division and finally head of the Spanish Light Source at Barcelona. Samar Hasnain, starting as a Manchester student working on his PhD on rare gas matrix spectroscopy at the SRF, changed his field at the SRS to the application of EXAFS to biological molecules and was finally appointed to the Max Perutz chair of molecular biology at Liverpool university after the closure of the SRS.

In the interim period between the closure of the NINA, and hence the SRF, in April 1977 and the start-up of the SRS at the beginning of 1980, some groups and individuals were welcomed at foreign facilities. Geoff Marr and his post-doc David Holland continued his atomic and molecular physics research at the Bonn synchrotron, in which I assisted by helping to install the SRF grazing incidence spectrometer there. Phil Woodruff, assisted by David Norman, carried out an experimental solid state phsics research programme at the Tantalus storage ring in Wisconsin, using the 2nd grazing incidence spectrometer from the SRF; Gwn Williams and I installed the spectrometer at Tantalus, aided by Ed Rowe’s enthusiastic staff there. Ian Munro spent 1977/8 at the Stanford Synchrotron Radiation Laboratory, setting up a facility for time resolved spectroscopy, and I myself spent 1978/9 in Bob Madden’s VUV spectroscopy group at the National Bureau of Standards (now The National Institute of Standards and Technology) 240 MeV storage ring, to take part in a new programme on molecular photoelectron spectroscopy. Keith Codling, whose work at NBS some years earlier had produced classic measurements of the VUV spectra of the rare gases, joined me during the latter part of this period. Joan Bordas and Samar Hasnain went to Hamburg to work at the EMBL and the DESY synchrotron respectively.

Notable amongst the Daresbury staff who assisted the SRF users were Jeff Worgan, who managed to reconcile our way of doing things with the Lab’s methods thereby ensuring progress, always with good humour despite the exasperation he must have sometimes felt. We had a great affinity with Les Naylor; he worked the same way we did and was brilliant at computer automation. Bill Silversides, endlessly patient with us as he set up and adapted the vacuum control system to our needs. Above all Alick Ashmore the Lab director, who welcomed the SRF activity in his Lab, particularly when he saw its potential for the future. He understood what drove us and gave us great encouragement; in my case, against the policy of the SRC chairman Sam Edwards who had instigated a hiring freeze, he secured a permanent post with only days to go before I had to leave, recognising that the new source would need experienced scientists.

There were many others who contributed to the success of the SRF; in my area the students John Hamley, Ray Houlgate and Pamela Woodruff, post-docs such as Malcolm Howells, Iggy McGovern, Gwyn Williams, Manolis Pantos, Tony Bourdillon, Chris Binns and David Norman.

Visitors also took an active part in the programme: Lee Torop and Don McKoy from the university of Adelaide, the distinguished Jim Samson from the university of Nebraska and even Sir John Randall himself working on the South line on the fibre diffraction experiment. In the photo below, taken in 1977, are shown some of those who took an active part in the early development of synchrotron radiation at Daresbury.

The NINA SRF team taken a few hours before the final switch off of NINA on 31st March 1977. At the time there were 10 user groups coming from the universities of Manchester, Reading, Oxford, Coleraine, Durham, Bristol, Warwick, Leicester, Edinburgh and MRC Cambridge who successfully put the case with the wider potential users community to build the world's first dedicated SR source, the SRS. From left to right: Pat Ridley, Iggy McGovern, Bill Smith, Tony Bourdillon, John West, John Beaumont, John Morton, Ian Munro, Paul Brint, Samar Hasnain, Jeff Worgan, Robert Pettifer, Tom Short, Joan Bordas, Ken Lea and Tony Cox