

MAG

~~539.422.5~~
~~PHS~~

CA 11681

CA 11681

Fifty Years of Neutron Diffraction

The advent of neutron scattering

edited by

G E Bacon

Smith 26.4-917 353,94



Se 2482 1986

Adam Hilger, Bristol

ISBN - 0 - 85274 - 587 - 7.

Published with the assistance of the
International Union of Crystallography

applicability. Finally, magnetic analogues to liquid and fluid could be pursued in the paramagnetic state of antiferromagnets or ferromagnets above the Néel or Curie temperature. Over the next six years the theoretical structure was considerably strengthened through the work of Placzek and Van Hove, R J Elliott, P G de Gennes, T A Kaplan, W Marshall, A W Saenz and G T Trammell—to name only a few authors of works of especial prominence. And over the same six years the greater part of the programme outlined in this paragraph had been in essence accomplished and the work published.

The experiments on aluminium and vanadium were each worked on at two laboratories: aluminium by Carter, Hughes and Palevsky, vanadium by Eisenhauer, Pelah, Hughes and Palevsky, using the Brookhaven Filter-Chopper (or Cold Neutron) facility; both metals were studied also at Chalk River by Alec Stewart and myself, using the triple-axis instrument as well as the filter-chopper method. The results of the different experiments were substantially concordant. The dispersion curves and frequency distributions obtained were crude by modern standards but left no room for doubt about the general correctness of the theory or the practicality of the experiments. With these results, published in the *Physics Review*, the *Canadian Journal of Physics* and *Reviews of Modern Physics* over the years 1955 to 1958, we have the validation of a new tool of physics and chemistry: Slow Neutron Spectrometry and the emergence of a new discipline—the study of the neutron inelastic scattering by material specimens: slow neutron spectroscopy.

3.5 Neutrons Re-cross the Atlantic

3.5.1 Reminiscences

G E Bacon, University of Sheffield, UK

1936, the year in which the neutron was first diffracted, was the year in which I first entered the Cavendish Laboratory as an undergraduate. In the following year Lord Rutherford died and was succeeded as Cavendish Professor by W L Bragg. By my third year Bragg, Bradley and Lipson were well established with the original Metro-Vick continuously evacuated X-ray machines and I was able to take my first powder photographs. I was destined to become an X-ray crystallographer and in June 1939, supervised by Henry Lipson, I embarked on my PhD course—an investigation of FeCr alloys. This activity was short-lived, for World War II started and, like so

many of the Cavendish staff and students, I found myself at the Air Ministry Research Establishment working on 'R.D.F.' (radio direction finding), diffracting a much longer wavelength.

As the end of the war came into sight, some of the staff were surreptitiously acquired by Dr J D Cockcroft for work on atomic energy. I escaped until February 1946 when I went to Chalk River for 3 months, to prepare to study the Wigner effect in graphite by X-ray diffraction. On the way there I called at Montreal University and met John Warren, whom I had known at Malvern. He suggested that I should also get involved in neutron diffraction, about which I now heard for the first time.

At Chalk River I saw ZEEP and the preparations for NRX and then, in July 1946, arrived at Harwell with the task of producing a spectrometer to do neutron diffraction. All was very primitive and the mud, described by Peter Egelstaff in the following article, was very prevalent: there was a security fence guarding the main road but it was some time before it encompassed the other three sides of the Establishment.

GLEEP (graphite low-energy experimental pile) operated in August 1947 and, with John Duckworth, I was able to get my first feeling for neutron diffraction by carrying out some simple measurements with alkali halides as monochromators. We gained an inkling of the interplay of absorption and mosaic spread by showing that although (as reported) the reflectivity of LiF could be increased by roughening the surface with sandpaper, this did not happen with NaCl. For anything more elaborate we awaited the completion of BEPO (British experimental pile—pronounced Beppo by Cockcroft but called Beppo by everyone else). Meanwhile I continued X-ray work on graphite, read the papers which were beginning to come from the USA and considered what topics we should work on when our neutrons were available. We were much attracted by the possibilities of single crystals, which the Americans had set aside in favour of more rapid advances with powder, and we turned our thoughts to the problem of extinction. Following a most illuminating co-operation with Ray Lowde, 1948 saw the appearance of the paper 'Secondary Extinction and Neutron Crystallography'. About the same time Ron Dyer became my assistant and began his thirty years of service to the neutron community.

At the end of 1949 our first powder diffractometer, then called a spectrometer, was ready. It was designed and manufactured by John Curran Engineering of Cardiff, a firm who had earlier assembled mobile radar equipment for me. The spectrometer (shown in figure 3.6) was unique in being mobile. The reason for this was that it had been hinted by the reactor engineers that we might have to remove our apparatus at short notice. However, this dire possibility never happened and the spectrometer remained permanently in place. It was by no means automated and the main requirements for an experimenter were a stop-watch, two stout arms for turning the handles and a lot of patience. Our first study was of

graphite and was the forerunner of what came to be called the X-N technique. We were able to show that the reflection intensities for neutrons were in accordance with calculation and that the long-acknowledged anomalies in the X-Ray pattern were due to an asymmetrical electron distribution with a concentration on the C-C bonds.

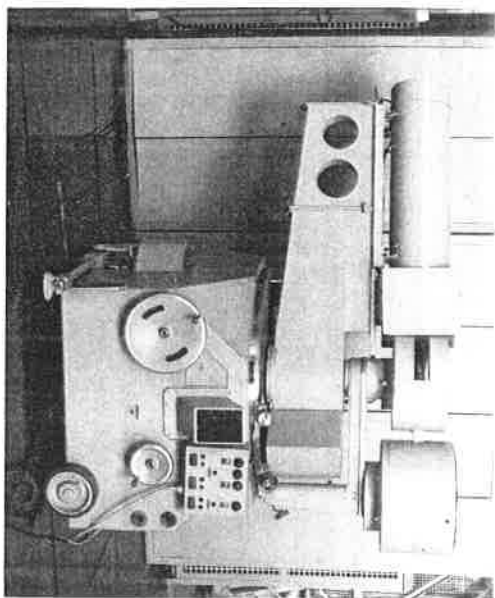


Figure 3.6. The first Curran spectrometer, installed at BEPO in 1949.

Meanwhile Ray Lowde had set up a single-crystal spectrometer to use white radiation and, inspired by a paper by Moorhouse, turned his attention to magnetic inelastic scattering. We heard for the first time, of 'spin waves' but in relation to the neutron intensities which were available this work was in advance of its time.

1951 was a year of change which ended our initial period of isolation. It was the year of the Stockholm conference of the International Union of Crystallography and in advance of this we had a visit at Harwell from Cliff Shull. In a session of the conference held at Uppsala University many crystallographers heard for the first time about ferromagnetic scattering and the importance of secondary extinction. By chance the two neutron papers in the session were combined (figure 3.7) with papers on ferroelectrics, an unconscious portent that during the next few years three independent groups at Brookhaven, Harwell and Oak Ridge would be using neutrons to unravel the role of the hydrogen atoms in KH_2PO_4 .

Among crystallographers generally in the United Kingdom there seemed to be a strange reluctance to become interested in neutron diffraction. This

..ry Morning, June 29th

Neutron diffraction and ferroelectrics

Lecture room X

Chairmen: I. Waller

E. Wood

| Paper No. | Authors | Title |
|-----------|--|--|
| N-1 | G. E. Bacon | Neutron diffraction at Harwell: measurement with single crystals. 20 minutes |
| N-2 | C. G. Shull | Magnetic crystallography and neutron diffraction. 20 minutes |
| F-1 | I. Nitta, T. Watanabe, S. Seki and R. Kiriyama | Thermal transition in penterythritol. 10 minutes |
| F-2 | R. Kiriyama and H. Igarashi | Dielectric phenomena of $\text{K}_2\text{SnCl}_6 \cdot \text{H}_2\text{O}$ and $\text{K}_2\text{HgCl}_6 \cdot \text{H}_2\text{O}$ single crystals. 5 minutes |
| F-3 | R. Pepinsky and B. C. Frazer | X-ray studies of ferroelectric crystal transitions. 10 minutes |

Figure 3.7. The neutron diffraction session at the 1951 meeting of the International Union of Crystallography, held at Uppsala University.

was perhaps due to reaction after the war to any activity which involved security fences and the Official Secrets Act. There were of course exceptions. J D Bernal and Kathleen Lonsdale were keenly interested at an early stage—I like to think that it was because they had been the examiners of my PhD thesis—but it was not until we had done a good deal of work on hydrogen bonds that interest was really aroused. One of our earliest supporters was J C Speakman, around 1955, who inspired a paper on potassium hydrogen bisphenylacetate and who remained a neutron disciple among the chemists for the rest of his life. More striking perhaps was the influence of Shull and Smart's 1949 paper on MnO which impressed the workers in magnetism. Notably there was Bob Street who, in 1952, began a long collaboration on magnetic materials and Terry Willis, whom we first interested at a Physical Society Exhibition and who forsook the General Electric Company to join us in 1953.

From my own personal point of view I recall particularly my visit to the 10th Pittsburgh Diffraction Conference in 1952, followed by my first visit to Oak Ridge—with the bonus of obtaining early single crystals of copper and lead which made much superior monochromators, because of their greater mosaic spread—and then to Argonne and Brookhaven. I remember also a discussion in a Boston café with Dick Weiss and others at which we agreed that p would be a good symbol for the magnetic scattering amplitude, rather than the original D of Halpern and Johnson. These were the years when the circle of workers in neutron diffraction was growing rapidly, although it was still sufficiently small for practically all to be personally

acquainted. Another particular memory is a discussion with I V Kurchatov when he visited Harwell in April 1956. He did not mention any neutron diffraction work going on in the USSR and, indeed, I enthusiastically suggested to him that he should start some.

In retrospect the most remarkable characteristic of work in these early years was the simplicity of the equipment and the tools of the trade. For example, figure 3.8 shows the wooden collimator which was used for the BEPO powder spectrometer and figure 3.9 shows the very simple spectrometer used to study single crystals of chromium potassium alum. Likewise, figure 3.10 shows a very early monochromator, cut from a lead crystal given to us by Oak Ridge. It can usefully be contrasted with figure 8.6, a highly sophisticated double-focusing monochromator in use at the Institut Laue-Langevin more than 30 years later. There were no computers and our first single-crystal problems, with KH_2PO_4 and sodium sesquicarbonate, were solved by Fourier syntheses performed with Beavers-Lipson strips. We had no step-scanning of the spectrometer, although hand-turning and the stop watch had been superseded by slow continuous rotation, using a pen recorder connected to a ratemeter. The pens were very temperamental and the whole system was very much at the mercy of electrical interference from electric drills and overhead cranes operating in the vicinity. Nevertheless there were advantages: for the operator who was in possession, there were no grant-application forms to be filled in and no necessity to wait, say, twelve months for two or three days of reactor time.



Figure 3.8. The wooden collimator used in BEPO, 1949.

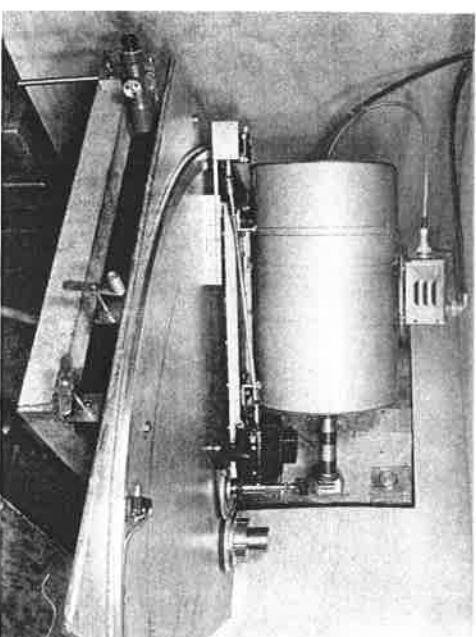


Figure 3.9. A very simple single-crystal diffractometer used at BEPO, 1955.

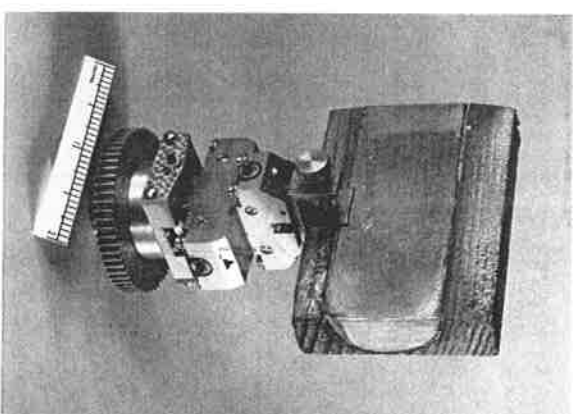


Figure 3.10. A very early monochromator, cut from a lead crystal.

Progress seems to have been fast, in spite of what would now be regarded as laughably small neutron intensities, but this belief may be



Figure 3.11. G. E. Bacon with E. O. Wollan at the Gattinburg conference, June 1976.

deceptive. No doubt, many of the physical and chemical studies which we made were relatively simple. When many virgin fields are available for picking it is possible to make important progress with a minimum of data.

3.5.2 Early days at Harwell

P. A. Egelstaff, University of Guelphi Ontario, Canada

Neutrons re-crossed the Atlantic when Sir John Cockcroft left Chalk River to take up his appointment as Director of Harwell. During the early days of Harwell there was great emphasis on the design and building of the new reactors GLEEP and BEPO, and on reactor physics experiments. In those days Otto Frisch and then Robert Cockburn ran the 'Nuclear Physics' Division which looked after nuclear and reactor physics. Both they and Sir John were strongly interested in the application of slow neutrons to solid state physics, a subject in which rapid progress was being made at that time in the US laboratories. In the General Physics Division George Bacon and Ray Lowde were asked to build up neutron diffraction and were designing new instruments from scratch. John Duckworth was asked to make neu-

tron scattering from matter an important part of his new slow neutron group, although its main emphasis was neutron cross sections for reactor design. Duckworth decided to build both a crystal spectrometer and a Fermi chopper for the cross section measurements, and two new scientists (Alec Merrison and Peter Egelstaff) who joined him were given the responsibility for each one respectively. Experimental progress was slow at first, as the laboratory was being built from the ground upwards and work on the reactors had the first priority. However some work on the crystal spectrometer was made at GLEEP in Hangar 8 during 1947, while the Fermi chopper was designed for the new reactor BEPO in Hangar 10 for late 1948. At that time the first item issued to new recruits walking between Hangars 8 and 10 was a pair of gumboots. There was mud everywhere. This seemed to come from an array of trenches which were being built in every direction. They were to become a system of ducts for heating and power, but during the first two years their mud dominated life at the lab.

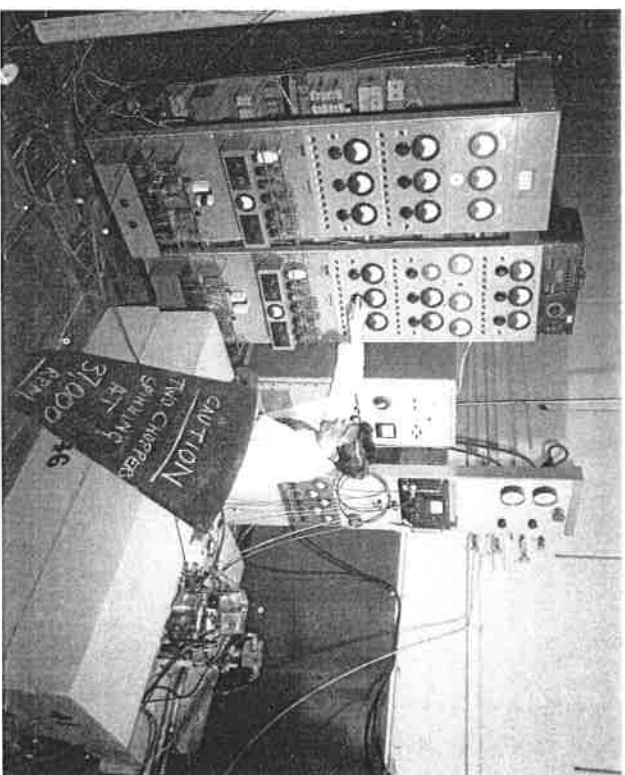


Figure 3.12. P. A. Egelstaff testing an early chopper at Chalk River in 1957.

Improvisation was the greatest asset in doing the experiments, and electronics the biggest headache (see figure 3.12). Neutron detectors were

notable by their absence. Russel Aves ran a group which built the detectors (BF_3); he made them lovingly by hand and when one was finished it was protected and cared for like a bar of gold. Amplifiers, discriminators and power supplies were made locally to begin with, and hence when one finally could detect neutrons it was felt to be an achievement. A monochromator was an asset—crystals from museums were considered highly. All data were recorded by pencil and paper. Since the equipment was not too reliable, many measurements were repeated. Data reduction was done by hand using a slide-rule, although there were a few mechanical calculators (for addition and subtraction) available. One had to apply in advance to obtain one of these for a few hours.

Duckworth's group were developing contracts with the slow neutron group (Havens and Rainwater) working on the Columbia cyclotron pulsed neutron source, when one day Sir John Cockcroft walked into Hangar 8 and said 'Why don't you build a pulsed neutron source based on the electron linac?' This remark changed the direction of our activities and led to the installation of the 3 MeV electron linac in Hangar 8 as a pulsed neutron source for neutron cross section measurements and for solid state physics. By this time Egon Bretscher had taken over direction of the Nuclear Physics Division.

Both Cockcroft and Bretscher were interested in and supportive of collaborative ventures with the universities, and they backed joint activities from the earliest days of Harwell for the remainder of their lives. The large university neutron beam programme in the UK stems from their work. At first one of the best routes was through the introduction of the Harwell University Research Fellowships. Both Jimmy Cassels and Gordon Squires of Cambridge were early holders of these fellowships. Cassels stressed the importance of using slow neutrons for studying dispersion curves of simple crystals and designed a three-axis crystal spectrometer for BEPO in 1950. Unfortunately BEPO was the worst reactor ever used for thermal scattering experiments (due to the low source flux, the extremely long and narrow holes and the fact that the beam holes passed through the thermal column which had a very thin shield, the neutron beam fluxes were lower than elsewhere and the background was higher). For this reason Cassels gave up his programme on lattice vibrations and looked to other areas of physics. Squires followed Cassels and, perhaps because of the new reactor designs being talked about in early 1950s (e.g. HIPPO, which was an improvement on NRX), decided to make pilot experiments on the available sources (electron linac and BEPO) in preparation for the future. His programme led to many Cambridge students being trained in neutron scattering at Harwell, and they have now spread through the leading neutron scattering centres world-wide. The support of Squires' group may be one long lasting result of the HIPPO design, which was dropped after the NRX accident in 1952 and replaced eventually by DIDO and PLUTO

(although unfortunately they had less satisfactory designs for neutron beam work).

The low performance of British reactors led to the development of cold neutron sources. In 1952–3 Heinz London and Peter Egelstaff at the instigation of Sir John Cockcroft began a series of low-angle neutron diffraction and inelastic scattering experiments on liquid helium using 4 Å neutrons. One day London said 'this experiment is going too slowly, why can't you get more neutrons?'. 'In principle it's easy' replied Egelstaff 'sometimes at cyclotrons they use liquid hydrogen as a moderator, and all we have to do is to maintain a few 100ccs of liquid hydrogen at the centre of the reactor. The moderation is fast in hydrogen and we should be able to lower the neutron temperature with a small quantity, but it must be hydrogen itself not a hydrogenous compound.' 'Very well' said Heinz London 'let's do it, I'll build the low temperature side and you look after the rest.' The reactor manager at BEPO was Bob Jackson, who on being approached about the safety of liquid hydrogen in BEPO said to Egelstaff 'you are the designer, engineer and physicist, you are also the safety committee, therefore your head will roll if anything goes wrong'. Careful tests and exploratory experiments were done by two new recruits Ian Butterworth and John Webb and the first cold neutron source was installed in BEPO in 1954. It could be operated so safely and successfully that later its operation was passed to the PhD students in Squires' group who used it for several years. This cold source was used also by Bill Mitchell's group of Reading University in the mid-1970s, in order to study low angle diffraction effects from amorphous and disordered solids. The advantages of high resolution small angle neutron scattering experiments were discussed and designs for DIDO and PLUTO were proposed but not approved. At this time the technology of detectors and electronics was too primitive to support the sophisticated instruments required for this field.

Even in these early days the low flux and intermittent operation of the electron linac made it less satisfactory than BEPO for solid state physics, although the pilot experiments could indicate a brighter future. The bread and butter work of Duckworth's group was neutron cross section data for reactor design purposes. After he left in the early 1960s this was actively continued on the old electron linac and later on the new 10 MeV linac, and also many measurements were made on BEPO using slow and fast choppers. The great activity in pulsed neutron work for cross section measurements, led to the idea that it could be used for neutron diffraction work. In 1954 Egelstaff gave a review of Harwell ideas to that time, for the International Congress on Crystallography in Paris. Among the audience was Dick Weiss who explained that similar ideas had been discussed at BNL. Little was done however other than demonstration experiments, until Bronislaw Buras took up the field seriously in the early 1960s.

Possibly the message of the period 1946–56 is that neutron scattering

work, both diffraction and inelastic scattering, became well established in Britain at Harwell and in several universities. Many ideas were discussed for new fields of study, for new instruments and techniques. The authorities gave this field their support and blessing, but the source flux, the reactor design and the technology of detectors and electronics were all too primitive to allow for many of the developments that the enthusiasts of the time hoped for.

3.6 Recollections of a Research Student 1948–51

GL Squires

Cavendish Laboratory, Cambridge, UK

I became a research student in the Cavendish Laboratory in the summer of 1948. Although nuclear physics was not the pioneering subject it had been in Rutherford's day it was still a major interest in the Laboratory, which possessed two Cockcroft–Walton machines giving energies of 1 and 2 MeV, and a cyclotron with a 37 inch magnet capable of accelerating deuterons to about 9 MeV. I toured the groups to see what they had to offer.

The cyclotron group was run by Albert Kempton and was pursuing two quite separate lines of research; one, in which Kempton himself was mainly interested, was the study of nuclear reactions initiated by charged particles at low energy. The other, an investigation of the scattering of thermal neutrons by metals, was conducted by James Cassels, a research student two years senior to myself. It would be gratifying to say that I was inspired by the subject of thermal neutron scattering and saw its vast possibilities. The truth is somewhat different. The cyclotron group possessed an electronic neutron velocity selector, an elaborate apparatus containing about 300 thermionic valves that generated so much heat that it had to be cooled by a large fan blower. During the Second World War I had attended a crash course on electronics at Cambridge and had developed an interest in pulse generators, ring circuits and other electronic devices. Here was a glamorous electronic instrument—I made my choice.

The velocity selector had been designed by Kathirkamathamby Kandiah, and built without a circuit diagram—or so the story went. At any rate no diagrams were available and, as Kandiah had left the Cavendish

Laboratory for the Atomic Energy Research Establishment at Harwell, it was difficult to make modifications to the circuits without them. Accordingly my first task as a research student was to trace all the circuits, wire by wire, and make a set of diagrams. Some idea of the physical scale of the apparatus may be obtained from the fact that there were six units, each one being so long that when I needed to get it out of its rack to trace the wiring it took two of us to lift it.

Neutrons were generated in the cyclotron by the ${}^9\text{Be}(\text{dn}){}^{10}\text{B}$ reaction. The way the velocity selector worked was that it produced a succession of 36 (or other multiple of 12) top-hat pulses in a cycle, which was then repeated. Each pulse was $270\ \mu\text{s}$ long. (Other values from 10 to $810\ \mu\text{s}$ were available.) The first of the 36 pulses was applied to the radio-frequency oscillator of the cyclotron, which was effectively on only during that pulse. The neutrons produced were moderated by a disc of wax about 20 cm in diameter and 5 cm thick placed just outside the cyclotron, near the beryllium target. About 3 m away was our single neutron detector, a splendidly dignified BF_3 chamber in a brass shield, with its head-amplifier attached. How carefully we handled it. There was no spare. If anything happened to it, our whole programme would stop dead—fortunately nothing did. The output from the detector was gated by 10 successive top-hat pulses from the velocity selector, thereby giving us 10 time channels.

The output from each channel was sufficiently low that, after going through a pair of binary counters with neon indicators to provide a scale-of-4, it could be counted by a mechanical post-office register. So the velocity selector plus the cyclotron provided a pulsed source with time-of-flight analysis, similar in principle to the machines of today—the wheel has come full circle.

The neutron source was too weak for us to detect scattered neutrons. We could only measure transmission ratios, and hence total cross sections. So all our samples were mounted on a little trolley, which was pulled in and out of the beam—by hand of course. All the neutron measurements in my three years as a research student consisted of columns of numbers, the readings from the post-office register and the two neons for each of the 10 channels, with the sample in the IN or the OUT position.

Kempton was my official supervisor and gave me much helpful advice, but during my first year I had in effect a second supervisor in Cassels. From him I learnt the good habit of not letting measurements accumulate, but of working out the results as the experiment is done. Being close in age he had no inhibitions about working me hard. I remember once, at the end of an exhausting day when we had made measurements non-stop from 10 a.m. till 6 p.m., his putting on his jacket to leave the laboratory and saying 'Make sure you work out today's results this evening, so we know what to do when we start tomorrow—I'm going to the cinema.'

I did two separate experiments during my three years as a research student. The first was to measure the total cross sections of magnesium and nickel as a function of temperature for neutron wavelengths in the range 5 to 10 Å. This was a continuation of work started by Cassels and Robert Latham, who had made similar measurements on iron and aluminium. The impetus for the work came from a theoretical paper by Weinstock in 1944, who calculated the total cross section for coherent one-phonon scattering as a function of crystal temperature and neutron wavelength. He gave a rough argument for showing that higher-phonon processes were negligible. By working at wavelengths beyond the Bragg cut-off, and hence excluding elastic processes, we hoped to measure only the one-phonon scattering and compare the measured values with those of Weinstock. Cassels found some discrepancies in the case of iron which he correctly attributed to magnetic scattering, not included in Weinstock's calculations. Aluminium had given results in reasonable agreement with the theory.

It soon became clear that the scattering in magnesium was greater than Weinstock predicted, and we guessed this was due to multiphonon processes, which, owing to the comparative lightness of the magnesium atom, were of greater significance than Weinstock had suggested. I devised a method of estimating the magnitude of the higher-order processes which, when added to the single-phonon cross section, gave satisfactory agreement with the measurements.

I would like to recall one incident in connection with the phonon work. One day in the summer of 1949 Cassels came to me in some excitement. The thought had occurred to him that the two conditions—conservation of energy and quasi-momentum—in coherent one-phonon scattering provided a method of determining the frequencies of phonons in crystals. He went to Robert Frisch, at that time the Jacksonian Professor in the Cavendish Laboratory, to ask his advice about publishing the idea. Frisch advised him to wait until he could provide an experimental demonstration of the method. Cassels, being only a research student, acquiesced. The advice, though well meant, was unfortunate. In fact it was another six years before Brockhouse and Stewart at Chalk River and Carter, Hughes and Palevsky at Brookhaven, working with improved neutron spectrometers and sources, were able to make the first measurements and thus demonstrate the method, which has become the standard one for the determination of phonon frequencies.

The second experiment on which I worked was the measurement of the total cross sections of ortho- and parahydrogen, to determine b_1 and b_2 , the bound triplet and singlet scattering lengths of the proton. It was done in collaboration with Alec Stewart, who had come to the Cavendish Laboratory as a research student from Dalhousie University. Although the cross sections for ortho- and parahydrogen give b_1 and b_2 separately, the main interest in the experiment was the determination of the coherent scattering

length of the proton, i.e. the combination

$$(3b_1 + b_2)/4.$$

This is given (apart from an ambiguity of sign) by the parahydrogen cross section. Previous to our experiment a large team in the United States had measured the parahydrogen cross section and had obtained the value $f = -3.95 \pm 0.12$ fm, which agreed with a value obtained from measurements on sodium hydride, but was inconsistent with the value of Hughes *et al* obtained by neutron reflection from a liquid hydrocarbon mirror.

The American ortho-parahydrogen team had had nine members, so it was somewhat presumptuous for two research students to try to repeat the experiment, but fools rush in.... Stewart and I realised that some complicated glasswork would need to be constructed to handle the hydrogen gas, and in particular to measure the ortho/para ratio. We obtained a supply of glass, and each sat down with a blow lamp. Apart from a little glassblowing as undergraduates (obligatory in those days) neither of us had had much experience, but we set to with a will. After about ten minutes it became apparent that Stewart had some talent in the matter and I had none. Fortunately there was much else to be done in the experiment, so I was happily able to leave the glassblowing side to him. In fact the glasswork became increasingly complicated, and we had to call in the services of the Cavendish Laboratory glassblower, but the fact that Stewart was able to do so much himself meant that we always got prompt and sympathetic service from the glassblower.

The measurements had to be made at liquid-hydrogen temperature (to minimise the inelastic scattering due to changes in the rotational state of the hydrogen molecule), and we were concerned at the safety aspects of hydrogen leaking in the cyclotron area with its occasional 10 inch sparks. One of my tasks was to construct a hydrogen detector, based on a heated platinum filament whose resistance changed in the presence of hydrogen. (A commercial detector was available, but it cost £50, which was considered too expensive.) My device rang a bell when the hydrogen concentration in the air reached a certain value, and usually went off when liquid hydrogen was being transferred to the scattering chamber containing the gaseous hydrogen sample. If it did not go off on these occasions, I was not above turning up the sensitivity control until it did so, thereby maintaining the confidence of the assistants that Stewart and I knew what we were doing.

This was rather necessary as we had a certain number of mishaps. On one occasion we were next to the cyclotron when someone switched on the current through the magnet coils. Stewart was holding a large screwdriver which flew from his grasp to the magnet, narrowly missing my head. On another occasion we tried to abort the experiment after Frank Sadler, the chief assistant at the Mond Laboratory which supplied the liquid hydrogen,

had gone home. We tried to remove the liquid hydrogen by bubbling gaseous hydrogen through it. Something went wrong and the resulting pressure blew out the glassware—fortunately it went to the side opposite where we were standing.

Our most spectacular accident arose from my own experiment on magnesium, which I was concluding when Stewart first arrived at the Cavendish Laboratory. I wanted to make measurements with the sample at about 180 K, and found that the melting point of toluene was suitable. Accordingly I made a mixture of toluene and liquid oxygen. (Liquid nitrogen was also available, but we were encouraged to use the former as it was cheaper.) Stewart was worried, rightly as it transpired, about the safety of my procedure. He therefore made a mixture, with very small quantities, of the ingredients I was using and set a match to it. There was a loud explosion. He was severely shaken, and I had to take him to the Casualty Department at Addenbrooke's Hospital. Again, fortunately, it turned out that he had suffered no serious damage, other than to a luxuriant, chestnut moustache with which he had arrived at Cambridge. It was so severely singed that he shaved it off and remained moustache-less for the rest of his stay. I absorbed the lesson and used liquid nitrogen thereafter.

One feature of the hydrogen experiment was that the Mond Laboratory could only provide the liquid hydrogen once a week. Once it was in the scattering chamber and the sample had cooled to 20 K, we made the neutron measurements for as long as the liquid hydrogen lasted—usually about 18 hours. In this time we carried out about 20 runs of alternate sample IN and sample OUT measurements, 30 minutes for the former and 20 minutes for the latter, plus a few background runs with a cadmium sheet blocking the beam. Apart from recording the numbers in the 10 time channels we were continuously monitoring the ortho/para ratio by a thermal conductivity method. The liquid hydrogen usually ran out at about 12 noon after our all night session, and we trundled home feeling tired and virtuous. Our final value for the coherent scattering amplitude for the proton was -3.80 ± 0.05 fm, in good agreement with the liquid-mirror value at that time, and just consistent with the present accepted value of -3.7423 ± 0.0012 fm.

Looking back at those times I have two main impressions of how things have changed. Firstly, we had the equipment in our own laboratory, and we built our own apparatus. It took time, and no doubt we did it inefficiently. But we learnt a lot, especially from our mistakes. We knew the apparatus thoroughly, and if anything went wrong, we were to blame and suffered the consequences. But as time went on we made fewer mistakes, the results assumed a consistency, and our confidence increased. Secondly, we did nearly all the calculations with a slide-rule. When I became a research student I celebrated my improved status by purchasing

an Aristo Multilog slide-rule. It was a de luxe model with several exponential and folding scales, and was priced accordingly—£3.15.0.

I do not hanker after the old conditions: with present-day sophistication of techniques, shared instruments, run by committees, are inevitable. One must use computers to design the apparatus, control it when built, and work out the results ('process the data' in modern jargon). But even today I believe it is good practice to do some of the calculations by hand, to get the 'feel' of the results—before bringing in the computer. But whatever the pros and cons of the different ways of working, my research student days were a productive and enjoyable period in my life, and, I think, in those of my colleagues at the time.