Autobiography chapter: Ferodo

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I heard of the vacancy at Ferodo during the Easter vac in 1943 (actually during the two or three weeks of the vacation that I actually took "off") in a letter from Mr Spiller, the Lecturer in Mineralogy. Mr Hancock had just been appointed Research Manager at Ferodo and found that they had X-ray equipment on order and no-one knew how to use it – it had actually been allocated by Turner and Newall to Ferodo because that was the only one of the four companies which had a research physicist! Hancock had known Spiller well when he (Hancock) was an undergraduate, so he wrote to him to ask if there was a possible candidate in the Department. Normally Spiller would have passed the enquiry on to ("Tiny") Powell, the lecturer in crystallography and my supervisor, but he had had an accident on his bike and was out of action, so Spiller wrote directly to me. I therefore applied for the job, was invited for an interview, confirmed that I would attend, and duly went by train from Derby to Chinley. I imagined (or presumably had been told) that I would be picked up there, but I waited some time and nothing happened. Then a car came to pick up someone who arrived by another train and I asked if it was going to Ferodo. It was, and they took me along when I explained the position.

It turned out that the problem was that my acceptance did not reach them until the afternoon. However Mr Hancock quickly got the physicist Dr Parker to join him and they interviewed me, and Dr Parker spent a long time showing me around his domain – the Test House and Physics Lab. A young assistant (who had been Hancock's assistant in Sales before he became Research Manager) was deputed to take me to lunch at a café, and the showing around and discussion extended into the afternoon. I was offered the job on the spot but asked for time to think about it, as I was still hoping for a favourable result from an interview I had had at ICI Dyestuffs which I thought would give me scope for doing crystal structure determination. However Hancock said that before I could be appointed I would have to be interviewed by the Managing Director (Mr William Smith) so to save a second visit I had better see him anyway. I was warned that he always barked at interviewees "what's your religion?". He also asked me whether I was waiting to see which job would pay me more, but I said I was more concerned with which would be more interesting. Evidently he did not find anything wrong with me, so I left with the offer of a job. The salary would depend on what class I got in my degree. With a second it would be £275 basic + 20% cost of living bonus + 15% overtime bonus (because working hours had been increased on account of the war), amounting to £371 in total; with a first it would be £300 basic and £405 in total. For comparison ICI had talked very vaguely in the region of £350. A few days later, having heard nothing from ICI, I wrote and accepted the job at Ferodo.

At that point it seemed rather unlikely that the job at Ferodo would give me the chance to do crystal structures (though in fact it did) but to compensate for this there were two considerations. The first was the very pleasant personal approach of Mr Hancock and Dr Parker – very non-institutional. The second was that having said that one of my hobbies was Egyptology I was told that the local vicar was an Egyptologist. This turned out to be misleading: it was the vicar of Chinley, not of Chapel, and in any case he left between the time of my interview and when I took up the post!

During the following term Mr Hancock came to Oxford to interview for further jobs, and invited me to meet him again one evening at the Randolph, and again he was very friendly. On that occasion, he recruited another chap in my year, Geoff Weekes, as an organic chemist.

My employment started on 1st August, but as that was a Sunday and as the 2nd was August Bank Holiday I was not required to attend until 3rd August, and was told that I need not arrive till the 10 o'clock train at Chinley and Dr Parker came to meet me at Chinley station in his car. I was also told in advance that he had obtained digs for me with friends of his in Buxton, and after work he took me there by car and introduced me to my landlady. He also arranged for me to be collected at 7.45 every morning and taken to work by car: because of petrol rationing he and three senior colleagues (the works manager, the export sales manager and the technical sales manager) used their cars in rotation and I was taken to work by this august company for the four or five weeks that I lived in Buxton. This was absolutely extraordinary VIP treatment to give to a new graduate joining the firm, but I was Dr Parker's first graduate assistant and he went out of his way to give me the best possible treatment.

The first week in August was the works shut-down which was a remarkably good week to start, as I was able to get orientated while everything was quiet.

I really got in "on the ground floor" of the research set-up, which was only just taking shape. There had been some sort of Research Department for at least 6 years of so, but it had been mostly pretty empirical development work that it did. Up to about 1942 there had been only chemical research, but in 1941 Dr Parker had visited Ferodo on behalf of the Admiralty Research Lab where he was then working, and he was entertained in the Managers' Dining Room and the then Managing Director took a liking to him and offered him a job as manager of the Test House, with membership of the Managers' Dining Room – a very select category consisting of the two resident Directors, the Research Manager, the Works Manager and two senior Sales Managers, and they enjoyed food and facilities at a different level from everyone else. (Later when the number of directors increased it came to be confined to directors.) He took the job on these favourable terms to develop the Test House, which up to then had merely had a foreman in charge (a Mr Billington) who had little direct supervision. He did not take kindly to having his independence taken away, and he sought and obtained a transfer to a job in one of Ferodo's regional depots. Dr Parker appointed a new foreman from among the staff of the Test House, Bert Balkham, a young man of about 27. He then asked to be allowed to start physics research and was given permission provided that he became responsible to the Research Manager instead of reporting direct to the Managing Director. He accepted this, and recruited three junior technicians, put some benches in the office of the Test House foreman, and set them to work. When they had a fire Bert Balkham decided that he couldn't stand sharing his office with three young lads of about 17 or 18, so he had one end of the workshop partitioned off to make himself a new little office, and the big office became the physics lab.

It was at this stage that Turner and Newall Ltd decided to invest (at no net cost in the days of 100% profits tax) in some modern scientific equipment at each of its companies, and Ferodo's share was to be X-ray diffraction equipment because it had a physicist and a physics department. This was presumably at the end of 1942, and then early in 1943 the Research Manager was suddenly sacked (a fairly frequent occurrence in T&N) and Mr Hancock was appointed in his place, and that was when he set about recruiting me. Meanwhile I suppose he was also reorganising chemical research, and by the time I started, work was organised as follows. There were two labs – the Upper Lab and the Lower Lab. The latter (downstairs) was entirely concerned with what was later to be called "experimental production" and was given over entirely to "bucket chemistry". It was in the charge of Mr Walter Hall who had two or three assistants. The Upper Lab was considerably more scientific-looking with lab benches and "glassware" but it was divided into several departments each with a departmental manager. Brake and clutch lining development in the charge of Mr Burnett; Ferobestos and rubber products (V-belts and rubber-based brake linings) in the charge of Mr J G Robinson; "Making Particulars" (detailed instructions for the preparation of everything in the works) in the charge of Mr A Butlin; and also the Research Secretary (Mr Haworth) who was in charge of all the records of experiments, the then very small library, and the typists. Of these Mr Burnett was a graduate with experience in the paint and varnish industry who had been at Ferodo for about 6 years. "JG" likewise was a graduate with experience in the rubber industry and (I think) a similar length of service. The research secretary was quite a young graduate in his late 20s, also I think from the rubber industry. Mr Butlin on the other hand had been at Ferodo, man and boy for above 20 years and had an encyclopaedic knowledge of the production processes but little scientific background. In addition to his development responsibilities Mr Burnett was also responsible for testing of all incoming chemical supplies: curiously this responsibility lay in the Research Department and not with "Works Lab" which did all the chemical testing of products and was responsible to the Works Manager, not the Research Manager. I suppose the thinking was that since Research retained responsibility for instructions as to how to make what it had developed, it retained responsibility for the quality of what went into those products.

Apart from the Departmental Managers there were only two graduates in the Chemistry Lab, both of whom were recruited by Mr Hancock at the same time as me: Geoff Weeks from Oxford and May Heath from Hull (but a local resident). Apart from these there was an MPS (directed to Ferodo under war regulations), a girl who had failed to complete a chemistry degree at Manchester, and one or two people with National Certificates or just possibly Higher National Certificates.

The Test House was a large hall filled with an earsplitting noise. It contained the Standard Continuous Test Machine (SCTM) dating back to 1920 or perhaps earlier (according to Bert Balkham it had been due for replacement ever since he started work about 1931). It consisted of four wheels, each about 2′6″ in diameter and about 2″ thick mounted at intervals on a shaft driven continuously at constant speed (a few hundred rpm) by a 30 hp motor. Freely mounted on the shaft and surrounding each wheel was a frame which permitted a pair of brake linings, fore and aft, to be pressed on to the circumference of the wheel, the pressure being applied by rotating a control wheel on the frame. The frictional torque was measured by the effect on a piston actuated by the frame producing oil pressure in a pressure gauge. The machine had to be continuously supervised by a man who had to adjust the pressure on each pair of brake linings to keep the frictional torque constant. The pressure required to do this was recorded on a chart. Thus, whatever the brake lining, the same amount of work was done and the temperature of the wheel and brake lining rose in the same way during a one hour test. Then the linings were removed and weighed (to determine the wear), the wheels were cooled with water, and another hour's cycle began. This machine ran night and day with a three shift system. I think the machine continued in use till about 1957.

There were also two inertia machines – the Large Intertia Machine (100 hp) and the Small Intertia Machine (40 hp). These revved up a bank of flywheels to a pre-set speed and then a standard brake assembly was actuated at a fixed pressure. The torque generated was recorded, and the stopping time was measured by the operative using a stop-watch. Wear was measured by weight or thickness loss over a specified number of stops. The large machine also incorporated a railway wheel so that railway brake blocks could be tested in this way under both dry and wet (sprayed with water) conditions. In some ways this was a more realistic braking procedure (bringing something to rest) but it lacked realism in that the time taken varied with the friction of the material, whereas in a vehicle one would necessarily compensate for friction level by pressing harder in order to stop at the required place.

There were two other large noisy machines running V-belts under 100% overload to see how long they lasted. There was a tensile strength machine (used occasionally) and the Pad Test Machine. This was new. It had been designed and built by Dr Parker to measure the friction of two ½″ square specimens bearing on the top surface of a horizontal steel wheel rotated by a motor belt below it. The load was provided by a dead weight and the frictional torque measured by the deflection of the arm carrying the pads which was restrained by two wires attached to conical tungsten weights immersed in mercury, so that as they were pulled out of the mercury the restraining force increased. This was used for both continuous running (to measure wear) and stopping tests to assess speed sensitivity.

The staff of the Test House consisted of three shifts of two or three "hourly paid" operatives, together with a cleaner (an elderly man of considerable sagacity who was a parish councillor) and a fitter who had recently completed his apprenticeship (Arthur Levick(?)).

The three boys in the Physics Lab were all about 17-18, and were doing part-time courses (during war-time these were largely on Sunday). The one I liked best was Gordon Shearer, but he wanted to be a mechanical engineer so he was not to be my personal assistant. John Fasham was wanting to be an electrical engineer, but he was assigned to me to help me in a variety of ways from time to time. Marginally the eldest was Dennis Matthews who had no particular specialism in mind and was lined up to be my general assistant when I got a programme of work started, but he was regarded as an awkward type and a trouble-maker, so about three months after my arrival Dr Parker took an opportunity to get rid of him. This arose because a 16-year-old recruit to Works Lab contracted dermatitis and had to be removed from regular contact with resins. His name was Don Hatch and he was just 16, and he was swapped for Dennis Matthews. What eventually happened to the latter I do not know – he was around for a few years but what happened then I do not know. Gordon Shearer stayed for about 4 more years and then moved to Dunlop where he was reported to have done well. John Fasham also stayed for about three years and I subsequently heard of home when he worked for a time with Dr W A Wooster at Cambridge and got his name on a paper as a joint author. Don Hatch on the other hand stayed about 37 years and finished up as Technical Director!

When I arrived at Ferodo I was given a second desk in Bert Balkham's office, and he was away for the works shutdown, so when he returned there was I! It was intended that I should pursue two lines of work – X-ray crystallography when the equipment arrived, and the "powder research". The idea of this had already been started by Dr Parker; it was to make miniature blocks (2″×½″×½″) of various powders bonded with various resins in order to explore and understand the effects of these ingredients on the frictional and wearing properties of the blocks. I was told to plan a programme of work to this end, and when I outlined it all at a meeting Mr Hancock said to Dr Parker "What is Mr Whittaker's expectation of life?"! I planned to investigate primarily the effect of hardness and particle size of the components, but also as many other physical properties as possible. We investigated calcite, fluorite, quartz and mica as the minerals, each in a range of particle sizes, and two resins, a soft cresylic one and a hard phenolic one. This amounted to only a fraction of the plan that I drew up in late 1943, and it lasted until 1948. During the 4½ years that it ran I had a total of four juniors working on it with me. From early 1944 till about mid-1947 my assistant was Don Hatch, and for a time in about 1946 another chap of the same age, Ken Alcock, was transferred to me from the Chemistry Lab to help speed things up by working alongside Don. By this time Dr Parker had taken a fancy to Don's abilities and made him his personal assistant, and a girl, Pat Beckett, was transferred to me from the Chemistry Lab to take his place. Apparently, unknown to me, she had for some time sought a transfer to the Physics Lab, but this had been regarded by Miss Pickford (who had the final say regarding the employment of all female staff in the offices and the labs) as unsuitable. However in 1946-7 when John Fasham left Miss Pickford had actively promoted his replacement by a girl (Rosemary Milne) whom she knew, so she was unable to maintain this position, and Pat was transferred. Unfortunately, for some reason I never discovered, she hated it when she actually got the job – probably she got teased by the boys and she was not the tough character that Rosemary was. Anyway, she only lasted a month or two and then, when her previous boss Mr Burnett became short of staff for some other reason she got him to get her transferred back. A new 16-year-old recruit straight from school, Gordon (Kip) Heron was assigned to me in November 1947 for 6 months to complete and wind up the Powder Research; he was then to go to a much more boring job in Chemistry mainly on mechanical testing of the strength of experimental variants of Ferrobestos, and asbestos reinforced plastic. Both Kip and others in the lab wanted me to save him from this fate, but there was nothing I could do as it had been made clear from the start that this job with me was purely temporary. However about two to three years later when I had a vacancy (in quite a different line) he managed to return to me till about 1956. Towards the end of this time he was distinctly under a cloud because his brother worked for a competitor (Small and Parkes of Manchester) and Dr Parker even contemplated sacking him. Then he was away for two years of national service, which had been postponed repeatedly while he was training and failing (but eventually succeeding) in getting his ARIC. I thought he was quite good, unlike Dr Parker who was very surprised I wanted to take him back in a much more senior job that was vacant when he came out of the army. In fact he worked very well for me for the next 4 years 1958-1962 or thereabouts, and I was sorry to lose him when he got a job at TBA (another T&N company). In fact he did so well there that by about 1970 he was Technical Director there and actually succeeded in staying in post to retiring age! So of the juniors, Dan and Kip, it was the latter who made it to the Board first by about 5 years, in spite of being 4 years younger. However all this was far in the future when I planned the Powder Research in 1943-4. The results of this research itself did not really come up to expectation, in that they did not lead to a science of formulating moulded brake linings, but they did indirectly lead Don Hatch in later years to devise a revolutionary new type of railway brake block which was very successful.

To return to 1943. After I had been at Ferodo a week or two Dr Parker and I visited Metropolitan-Vickers to find out when we were likely to get our X-ray equipment. The answer was that there was no real prospect at all: they could only deliver things which were certified as having priority for the war effort and Ferodo had not produced any evidence in this direction. So the next thing we had to do was produce pretty specious arguments about how it would (might?) assist in solving problems related to various war contracts – I think one of these was better control of the properties of "9418" – super charger disks for aircraft engines, which were a perpetual problem with people from the Ministry of Aircraft Production permanently in residence at Ferodo to test them.

To assist my planning of the Powder Research Dr Parker lent me books about statistics and the statistical design of experiments, which I spent quite a lot of my working hours studying. I also suggested we should make friction measurements on single crystals, as Dr Parker had designed and built an apparatus for measuring the friction between a small hemisphere a few mm in diameter and a specimen on comparable size. To this end I spent quite a lot of time growing crystals (a favourite occupation of mine) partly from solution in a cellar, and partly of alkali halides from the melt in a platinum apparatus that I devised. Unfortunately these friction measurements were never made. I was encouraged to read widely on anything of interest: fundamental theories of friction and the structures of asbestos minerals in so far as they were known or hypothesised at the time. Dr Parker even recommended me to go to Manchester one day a month to read such things in the public library and (without permission, since his experience was that it would be refused) in the university library. Within a few weeks Dr Parker also gave me the job of assessing the results of the Standard Continuous Test Machine, to which I had to append an assessment and sign it, and also a similar job on the railway brake block tests. These were mostly run overnight. I also joined in with Dr Parker in suggesting new mineral ingredients to try out in them. I always remember when we tried pyrites for this purpose. Fortunately the machine operatives had a sense of humour, and the report left from the overnight test was quite amusing for the reader though it could not have been very funny for him at the time. Anything of note had to be reported on each of the 100 stopping tests, and it read something like: *5th stop strong sulphurous smell. 21st stop smell much worse. 29th stop operator retired to be sick. 35th stop operator sick again; test discontinued.*

The effect of heat and pressure on pyrite and a cresylic resin must have been to produce some pretty nasty organic sulphur compounds.

Just before I started work at Ferodo on 3rd August I received details of a week's crystallography course at Cambridge which appeared to suggest that I was to attend it. When I arrived at Ferodo I found that this was in fact true. It was a fortnight's course on which they had enrolled Dr Parker for the first (basic) week which they assumed I had covered, and me for the second week on techniques (which I had largely not covered) and I was booked into the University Arms. This took place about the beginning of September, and while there I met for the first time a number of people who were to figure considerably in my professional life later on. Another consequence however occurred during the week that Dr Parker was away, when there was some kind of problem going on about a complaint on the characteristics of railway brake blocks. Mr Robert Turner, the T&N Technical Director, heard about this when visiting Ferodo and he told Mr Hancock that Dr Parker should not have gone away at such a juncture, but Mr Hancock told him that Dr Parker now had an assistant who was quite capable of dealing with the situation. He was astonished to hear this, and asked to meet me, so I was summoned to be introduced. I apparently made a good impression on him from the start, and for the best part of 20 years (till he retired) I was always in his good books. He would quite frequently come to see what progress my work was making, he made me secretary to the T&N Joint Research Meetings, he insisted on occasions that I travel to them as his navigator, and he eventually told Dr Parker that he was not to allow me to leave Ferodo.

Another byproduct of this trouble with railway brake blocks was that it seemed desirable to check on them with X-ray diffraction. Dr Parker therefore rang up Dr W H Taylor, head of Physics at Manchester College of Technology (later UMIST) whom he had known when he worked in Manchester in the late 30s, and arranged for me to go there to take some X-ray photos on their equipment. As a result I received and enormous amount of help professionally as a crystallographer both from W H Taylor, and from his successor Henry Lipson, as my contacts with that Department continued.

Thus the non-delivery of our X-ray equipment had a very beneficial effect for me. It was also fortunate in another way, because we had at that time nowhere where it could have been installed and safely used. However during succeeding months the physics lab was extended. The area was doubled, the new part being subdivided into an X-ray room (with some lead lining), a dark room, and an open L-shaped extension to the original lab. Finally the X-ray set was delivered about November 1944, 15 months after my arrival. Even then it gave persistent trouble for another 15 months or so, which led to much recrimination between the makers Metropolitan-Vickers and us – they obviously thought I was not maintaining it properly. But I eventually convinced them that the trouble was that the high voltage insulator section was not vacuum tight when hot, although it was when cold. This meant that on maintenance visits they could always apparently demonstrate that they could get the vacuum right but when I was actually using the X-ray tube, and switched off when it had heated up, the vacuum failed overnight and of course also caused trouble while it was actually running.

Once we had the X-ray set working Mr Hancock asked me what I was going to do with it! So I said that the first thing was to take diffraction patterns of all the works chemicals. He obviously thought this showed I had my feet on the ground, so this is what I did first, and it was very useful to me subsequently to have got to know all the works chemicals – or at least the solid ones. Before long three lines of work opened up. Mr Forbester of Turners Asbestos Cement Company sent, via Mr Hancock, twelve samples of various kinds of asbestos for me to X-ray, and of course this led on to the main field of research for most of my working life. Off my own bat I started doing amorphous/liquid type diffraction patterns of the various phenol formaldehyde resins produced in the works.

To my surprise these turned out to be remarkably different from one another, and changed when they were "cured" to the solid infusible state. I envisaged the possibility of deriving quite new information about them by Fourier transformation of the intensity pattern to radical distribution functions. About this time the design of an optical Fourier transformer was described in the literature and I envisaged building one, but failed to persuade the firms involved to sell us one of the essential components as they wanted to make and sell the whole machine at a large price. As a result I looked for a way round the problem and devised a numerical method for doing it, and thus provided the subject of my first full-scale published paper, in *Acta Crystallographica.* This work gave rise to some intriguing results but no real conclusions unfortunately. The third line of work was on crystalline phenol-formaldehyde intermediates, to which I looked for help in interpreting the resin structures. Mr Hancock had contacts with Dr Megson who had a team of three working on the synthesis of these substances and they provided us with about ten of them. I felt I was doing real X-ray crystallography when I determined their unit cells and space groups about 1947, and subsequently did a structure determination of the easiest of them. I hoped to take this work much further, but it got crowded out – there was not time to do any other pure research in addition to that on asbestos by the time I had done the necessary "bread-and-butter" applied work.

The Ray-Max X-ray generator that we bought cost £1500 in 1944, and we thought it was very sophisticated because it had a lot of automatic controls and a working diagram that lit up progressively as various things happened. The X-ray tube was of metal, with a large porcelain insulator to insulate the high voltage lead to the filament. There were four seals that had to be made with plasticine and apiezon grease – tube to target, tube to insulator, and tube to two window assemblies. The windows were of aluminium foil sealed to the assemblies with bitumen. The tube was further joined to an oil diffusion pump by a viscous oil seal (J-oil) and this was further joined to a rotary oil pump via another such seal and a phosphorus pentoxide moisture trap. All these seals had to be made and maintained vacuum tight. In addition the target tip had to be soldered (water and vacuum tight) on to the target holder, and the filament had to be mounted at exactly the right level in the filament holder to focus the electrons on the target. All the internal parts had to be de-greased with ether whenever they had been touched. The vacuum was checked by a Pirani gauge to control the interlocks and visibly by a discharge tube that would show no air or vapour "striations" when the vacuum was good. There were good automatic relays which prevented the diffusion pump switching on until the fore vacuum produced by the rotary pump was OK, and to prevent high tension being applied unless the vacuum in the tube was OK. There was also interlocks to ensure that the cooling water flow was adequate and that the high tension enclosure was securely locked. **But** there were no safety precautions regarding the emission of X-rays! There was a window on each side of the tube measuring about 15mm$\times$15mm, so very large beams were emitted and there were no built in shutters at all – there was simply a peg on which one could hang a roughly bent piece of lead sheet. When the machine was first installed and run up to full voltage (100 kV) we were invited to peer cautiously round the corner to see a cone of blue light arising from each window where the X-rays were ionising the air! Gradually over the years I improved this set up by restricting the size of the beams with lead apertures sealed to the tube, and with close fitting metal caps to slot over them. But never in the 20 years that I was responsible for the equipment did we have any remote control over these shutters, let alone any automatic cut-off of the beam. Yet we never had an X-ray accident and the two of us who used the equipment regularly over that time have reached the ages of 68 and 71 without any known ill effects.

But to return to the more general history of Ferodo from 1944. In the autumn of that year a metallurgist, Peter Marshall, was recruited to the Physics Lab. He was the same age as me, but had just about completed a PhD at Newcastle in two years. As I had got a share in the only available office he had to have a desk with the juniors in the lab, so it was decided to build on to the store-room at the end of the Test House to give us both minute offices in which we both had some research equipment, and he had to come through my office to get to his. We both disliked clocking in and out, and made our views known on this, and were eventually (as a compromise) excused from clocking out. Then in about 1945 J G Robinson the senior man in the Chemistry Lab left for another job and Mr Hancock promoted his two assistants Harry Robinson and Geoff Weekes (my contemporary from Oxford) to the rank of Departmental Manager, each taking half of his job. I heard subsequently that he had some difficulty getting this promotion through for so young a person, but it was the only way he could see to overcoming the latent disaffection amongst us young graduates about our lowly status exemplified by the clocking in business etc. However it did not work, and Geoff Weekes left only a year after he got his promotion, and this did not do Mr Hancock any good in terms of the confidence of his superiors in his judgement. Soon after this he also became aware that Peter Marshall and I were applying for jobs (he for promotion, I for an academic career) and early in 1947 he offered us promotion to the rank of Departmental Manager provided we undertook to stay for at least three or four years, to cover him. This undertaking we gave, and we were duly promoted, Marshall being put in charge of the Physics Lab under Dr Parker and I having the same rank but “without portfolio” as it were. In fact Peter Marshall was absolved from his undertaking after only two years because he was advised on medical grounds to move from Chapel for the sake of his wife’s health.

Having got this promotion in 1947, with the end of the powder research programme in sight, and with steadily increasing work on X-ray diffraction I thought it would be good to have an assistant on this work. This proposal was accepted and two serious applicants presented themselves, Alan Gard and Dave Swinburn. I had met Alan Gard briefly when he was working at Turner’s Asbestos Cement at Trafford Park, and he had moved from there to Washington Chemical company (also T&N) but was not happy with his superiors there either. His father had been chief chemist at Washington under whom

Mr Hancock had worked before he came to Ferodo, so he wanted to give him a break. Alan was two years older than me and we subsequently became good friends, and he eventually climbed to senior lecturer status at Aberdeen, but I did not see him as the sort of assistant I wanted. He had a better academic background than Dave Swinburn, but because of this and his age advantage and the fact that his thought processes seemed very different from mine I could not see him as effectively assisting me. Dave had worked as an analyst at Rolls Royce, got married and then tried to qualify in minimum time at Leicester, and had just failed finals, but I felt he had potential and would be on my wavelength, so I chose him. Mr Hancock gave Alan another job as assistant to Mr Burnett on brake lining development; he never really liked it and left after about four years to be a research assistant at Aberdeen University where he progressively worked more and more on my sort of interests. Dave on the other hand, after doing very well as my assistant for many years on crystallography gradually moved over to development work and finished up as the senior manager of brake lining development. It is an extraordinary double switch, but I still think I made the right choice!

It was about a year later, when I was working on the structure of Bolivian crocidolite I needed a complete chemical analysis of it, so in view of his background experience Dave was given the job and a bench in the chemistry lab. It was obvious immediately from the way he set about the job that he was a born analyst.

A week after Dave Swinburn joined me in November 1947 I had another new assistant, "Kip" Heron aged 16, recruited and given to me for 6 months to finish off he powder research. He was then transferred to very boring work, but eventually in about 1951 managed to return to my group. After a two year absence doing national service from 1956-8 he returned to me (in spite of doubts by Dr Parker who thought my faith in him mistaken) and stayed till 1962 when he applied for and got a better job at Turner Brothers Asbestos, Rochdale where he eventually rose to be Technical Director and (unlike most such holders of that position in T&N) survived to retirement age.

In 1948-9 a major extension of the factory was in hand, and the far end of it was assigned to the Research division for new labs. It was a far from stylish set-up. Apart from three or four labs (pure physics, dark room, X-ray room and infra-red room) the walls finished at a height of about 10 feet, and so the tops of the labs opened to the factory roof, communally as it were. The walls were of cement bricks and painted directly without any plaster. However it was a great advance in terms of space and office space. With a view to our move into these premises Hancock planned some reorganisation, and (prompted by the finding of Dave's skill in analysis) suggested to me that I should manage a new department to be called the Analytical Department. This would take over from Mr Burnett the routine analysis of raw materials and analysis of competitors' produces, but would also include my own interests of X-ray analysis, the development of new methods like infra-red spectroscopy (which had in fact already been assigned to Dave and me in the Physics Lab, and which we were just starting on), and indeed very general things like mathematical analysis of problems (I being just on the verge of getting my maths degree). The date of moving into the new labs (which I think was in September 1949) was getting quite close when one morning Mr Hancock came into my little cubby-hole of an office and said he wanted to tell me, first of his staff, that he was leaving that day. The directors had made proposals to reorganise the Research Division which he could not accept, so he had had to look for another job and had accepted one as general manager of Gandy Ltd, one of our smaller competitors. He had just told the managing director and had been told to remove his personal effects and go within the day. I was quite appalled and depressed. I had always got on very well with him and comparatively my relations with Dr Parker had deteriorated from the early days. He had become very right-wing politically and he obviously despised my outlook, and had become very friendly with my co-equal Peter Marshall who was much more of a kindred spirit to him (Peter and I could argue these things good humouredly on a level, but a lack of rapport had developed between me and "Doc" as we called him to such an extent that I sometimes found myself beginning to stammer when I spoke to him). My future looked bleak. The nature of the unacceptable proposals I never found out in detail, but it seems fairly certain that they involved Dr Parker becoming Research Manager with Mr Hancock stepping down to a subordinate position (Doc had in the preceding few weeks made several remarks which seemed to imply some imminent major change in his position). Anyway he immediately became Research Manager and after a hiatus of a few weeks he called us in one by one to tell us of the new organisation that he was setting up. My role was actually expanded; instead of an analytical department I was to be manager of the Organic and Physical Chemistry Department, with the same space as had been envisaged but with an immediate staff of five rather than two or three. The only reduction would be that I would report, not directly to the Research Manager but to a senior manager, brought in from outside. He would be one of 5 senior managers reporting to Dr Parker, and would have charge of the 4 different chemical departments. This was fine by me, giving me both expansion and sufficient insulation from Dr Parker. The other departmental managers however were far from pleased, as they both lost functions (to me) and relative status level in the organisation.

The new senior manager was Mr Whitehouse, and when he arrived in the following February (1950) my future looked brighter than ever, as we got on excellently. We had the same sort of approach to science in many ways and he approved of my work to such an extent that he progressively transferred further functions (on resin development) to me from the other development departments, physically doubled the size of my lab at the expense of Mr Burnett's by moving a wall, and changed its title to Chemistry Department. And over the nine years that we were in those premises my staff expanded from 5 to 10.

Initially when we moved my staff numbered three actually, Dave Swinburn, Pat Beckett (transferred to me once again from Mr Burnett and bringing her job of raw material analysis with her) and Martin Mikschik (who later changed his name to Milne) transferred to me from Poole's section for my resin development section. Also in line for me was Allan Rea, a rubber and polymer chemist already appointed by Hancock, again diverted to me as my main lieutenant on the resin side. He was a FRIC by examination, whereas Dave had just failed his BSc again, so Allan had to become my deputy and share my office instead of Dave – as the latter had hoped. The fifth post was someone to operate the infra-red spectrometer, and Laurie Bridge (an assistant with Poole) applied for this and I accepted him.

Pat Beckett soon got married, became Pat France, and left to start a family, and she had a whole series of successors: Ken Alcock (who had for a time been my second junior with Don Hatch as the powder research); Einstein (a distant relative of the great man who never managed to arrive less than an hour late so we soon asked him to leave – and not long after that we saw in the paper that he had been killed on his motor bike); there was also a young Dutchman married to an English girl who did not last long as he was pretty hopeless; Kip Heron who was overjoyed to get back into my department at his own request; and finally Bryan [?] a young boy who turned out to be epileptic. His parents were divorced, he had been kidnapped by his father from his mother as a small child, and while with me he re-found his mother and I got quite involved with both parents as well as advising the boy. Eventually the raw material analysis was transferred to Works Lab from Research, which was more logical, so that particular post came to an end about 1955, I think. Subsequently I got another assistant for Dave on the analytical side – Margaret Davies who became quite the most lively character in the lab. Meanwhile we recruited Frank Smith to double up the effort on the infra-red side – he was much better qualified than Laurie Bridge and a better practical scientist too, but he only stayed about three years or so, and was not replaced.

In 1952 we decided to make a major effort to understand better the chemistry of cashew nut shell liquid (CNSL) which became a major raw material. We appointed Barbara Dennis who had worked in a food lab. She was about 20 and lived in Chapel where her father, a former golf pro, was managing a pub. She worked directly under me mostly rather than being assigned to Dave's section or Allan's. Eventually I was to supervise her writing a thesis on her work for which she got her PhD.

Another additional post was created about 1952 for a manufacturing plant oriented chemist, and we appointed Bob Carruthers, a new graduate from Glasgow who had been a Bevin boy in the mines. He was a great character, and although so young he was an elder of the Church of Scotland. Sadly he only stayed for one year and then went to ICI Explosives, back in Scotland. In his place we appointed Dharam Sood, an Indian aged about 26. After a year his father demanded that he return home to get married, and started sending him photographs of potential wives, and he needed a lot of help and "advice". Eventually he decided to go but said that he wouldn't get married unless he liked the girl. In fact he did marry and brought Usha back and they bought the house next door to me, and we got on very well with them. But after one more year his father required him to return to India to join his business. He did return but instead of joining his father's bristle business became a chemist at a paint company.

In the middle 50s, probably as a result of Dharam's departure, Dave Lewis was transferred to me from Works Lab. He was approaching his qualification as ARIC for which he had been working very hard, when he started having epileptic fits. They were largely under medical control when he came to me, but he used to have "brown-outs", when he did not lose consciousness or fall down but was temporarily our of touch with what was going on. The crunch came when this happened while he was holding a beaker of meths, and as he came round he impulsively drank it. This caused him to be immediately sick and no harm was done, but it could easily have been something deadly. He was accordingly transferred to a clerical job in Test House, but there he fell down an iron staircase and had to give up work altogether. Eventually he had to go to live in a special "colony" for epileptics and went further and further down hill. After his mother died about 1980 I lost touch with him completely, as he could not write.

In place of Dave Lewis, about 1957, Alec Sanderson was appointed. He was an interesting character with wide interests – at his interview he tapped his head and said "home's in here" when asked whether he minded where he lived. He was a good worker, but not very bright really, and it was difficult to explain to him why he didn't progress as well as he thought he should. He stayed about 10 years and then became a school teacher in Scotland. Curiously enough he kept in touch with me till he died of cancer in the 80s and his widow still does.

In 1957 Allan Rea left for a job in Leicester with Bostik Chemicals, and his place as my number two was given to Dave Swinburn who had at last qualified with a BSc in 1956. So I put him in charge of the whole lab, resin development as well as analysis. The result was the immediate resignation of Laurie Bridge. He had eventually acquired Associateship of the Plastics Industry, and had apparently done this in the belief that it would ensure that he would eventually succeed to Allan Rea's job. I had had no idea of this belief on his part, and would never have contemplated giving him the job – he had much too dull an intellect. He was completely shocked at Dave's appointment and complained bitterly, but I never had any doubt either then or subsequently that my choice was the right one. Anyway he quickly found another job, and his place was soon filled (advantageously to us) by Kip Heron just returning to us from national service, and he stayed with me for about 5 years till he went to Turner Brothers.

With Dave's appointment to take charge of resin development I was deprived of his assistance on the X-ray work, so we appointed David Elias. He had a PhD from Cox's department in Leeds, and was totally deaf but very good at lip reading. He should have been much better at crystallography than Dave (who only knew what I had taught him), but in fact he was much worse. He found it impossible to reconcile himself to the fact that crysoltile obeyed different laws from normal crystals (he kept saying "it's very peculiar") and I found him quite useless and after a reasonable trial we wondered how to tell him (humanely) to find another job. Fortunately just when I had decided it must be done he asked to see me and said that he had found out that Dave, who was graded above him, only had a general BSc degree, and unless he was promoted he didn't think he could stay. So we encouraged him to go! From what I heard from Leeds it seems probably that he had anyway got his PhD more on compassion than merit.

At this stage we decided to try to capitalise on our hard-won understanding of the structure of chrysotile by exploring its surface chemistry. To this end we appointed (as a replacement for Elias) Ron Davey, who had done a PhD in surface gas absorption. He built an apparatus to do that sort of thing, but got nowhere. Again he seemed to have a very narrow expertise and no nous to lead him out to new ideas. He only stayed a year or two.

Also about this time (about 1958) we appointed an 18-year-old junior to help Barbara. She was named Valentine Critchlow. She was very little and very decorative and knew it. She stayed about 4 years by which time she had married an engine driver and left to start a family. Also in the early 60s we had another very small girl named Gwen Cottrell. She was quite good and had two sessions in my department and an intervening period in another one. Valentine was replaced by a lad of similar age whose name I quite forget – he outlasted my time. There was also a young man aged about 20, unqualified but from a public school, who was being put through a training involving periods in each of the research departments – it was said that he had been assessed as "Board material". In fact he turned out to be completely hopeless at what he was given to do and I think the whole project eventually faded out.

Over the 14 years from 1949 to 1963 we also employed for short periods various "vacation students". The most memorable was Paul Jackson who came three times – in 1949-50 between school and national service, in 1952 between national service and Cambridge, and again while at Cambridge. He was quite extraordinarily intelligent, and picked up the whole of a job before one had half explained it. After school teaching in India he eventually got a job as a lecturer in statistics at Aberystwyth on the basis of the statistics I had taught him in 1949! Two others that I remember were a girl who was coming to do chemistry at Oxford, and a Dutch physicist Northolt, who eventually became an academic crystallographer, whom I met again at one or two international congresses.

A watershed in the development of the Research Division (or Technical Division as Dr Parker re-named it in 1949) was the move to purpose-built premises in 1958. These were really quite impressive. I was asked to design the decoration of the wall at one side of the entrance hall, which I did with a repeating wallpaper pattern of the electron-density map of the structure of chrysotile with an enormous photograph of a model of the same structure superimposed across the middle of it. The opposite wall was designed by Dr Spurr (the pure physicist) with reproductions of Leonardo da Vinci drawings of friction experiments, mechanical designs, and mirror writing. The building was opened by the Duke of Edinburgh in September 1958, and we spent most of the preceding three months mounting an exhibition of our work for the occasion. For this purpose I built two big structure models, one of chrysotile and the other of amphibole. After the event was over I found he latter very useful indeed in elucidating the crystal chemical relationships of the numerous amphibole species which until then had been very obscure – the resulting paper was one of my most successful and eventually made it possible for me to be regarded as a geochemist! After opening the building the Duke toured the labs and in demonstrating two or three of the exhibits to him I was filmed in animated discussion and laughing with him.

In the new building only the senior managers and the director, whose offices were on the front corridor, had proper walls to their offices. All the partitions elsewhere between corridor and labs, labs and labs (except for the X-ray lab and of course the darkroom), and labs and offices were of glass. I now had an office to myself and Dave had an adjoining office next to it.

Soon after the move we installed a gas chromatograph which was put with the infra-red spectrometer under Kip Heron. This involved me in quite a lot of supervision time, as did the work on gas adsorption on chrysotile by Ron Davey, and a much increased output of newly developed resins – all of which I used to supervise personally when they were first made on a factory scale. All this meant that I had less time than before for fundamental research, though I did continue to produce a few papers of a theoretical nature and had some electron diffraction patterns of chrysotile obtained by outside contracts, and did some more optical diffraction work at UMIST, but the great days of my experimental X-ray work were over. Commercial pressures were beginning to crowd in and my fundamental work was less encouraged from above.

The surrounding organisation was changing too. Whereas in 1949 the sections reporting to Mr Whitehouse had been: brake linings (resin based) – Mr Burnett; Ferobestos – Mr Poole; brake linings (rubber based) and belting – Mr H R Robinson; organic and physical chemistry – me, all these being Departmental Managers, changes gradually took place. Two new sections were added: railway blocks under Don Hatch and sintered metals under Alf Jenkyns, but these two were only Senior Technical Officers. The Ferobestos was moved from Ferodo to TBA Company, and Poole went with it. Residual work on resins was taken from Burnett and given to me and my department renamed Chemical Research. His responsibility was further reduced to woven brake linings only, and a new manager, George Walker was appointed for the up and coming moulded brake linings. Harry Robinson was despatched to take charge of the new Ferodo factory at São Paulo and replaced for a short time by Vennels but he never made the grade. Walker did not last long either and both their departments were transferred to Don Hatch who was promoted to Departmental Manager. When Burnett retired about 1961 woven linings were transferred to Don, so that by then Whitehouse's section comprised: brake linings – Don Hatch; sintered metals – Alf Jenkins; chemical research – me, and we were all Departmental Managers. By this time also Dr Parker was making it explicit that Don Hatch was to be his nominated successor as Technical Director.

Then as time passed it became increasingly clear that Dr Parker was showing a lack of confidence in Mr Whitehouse. Mr Butlin, Senior Manager in charge of administration of Technical Division and of “Making Particulars” (i.e. control of factory production methods) was within 2½ years of retirement, so he was detached from his duties to study and assess the whole organisation for the future, and Whitehouse was transferred to his job. Simultaneously Don and I were promoted Senior Managers responsible direct to Dr Parker. Thus Don had all Whitehouse’s functions except supervision of me. Although Whitehouse’s status was unchanged it was obviously a smack in the face for him to be given a purely administrative job, and while my face was saved by not being responsible to my former junior, it was clearly anomalous that so small a section as mine reported directly to Dr Parker. Within 6 months Whitehouse found another job (at Scott Bader) and left, his required three months notice being commuted to one month. Simultaneously and quite independently Mr Webber the Senior Manager in charge of Mechanical Sciences also resigned. His responsibility covered: applied physics – Keith Mackenzie; pure physics – Dr Bob Spurr; statistics – Dr Knowles; Test House – Ellis Goddard; test cars – Reg Adams.

Dr Parker asked me to take over this position instead of my own. I had considerable doubts whether I should take the job, though I did not doubt my capacity to do it based on my experiences from 1943-49 and my maths degree. It seems that he so much wanted me to take the job that I would really forfeit the interest of him and the company if I did not accept. So with considerable misgivings I accepted, with an unsaid proviso that if I did not like it after a year I would have to look for another job.

The announcement of my appointment some time in October was not popular, especially with Mackenzie who thought he should have got the job, and I did not take over until Webber left at the end of November. I had two immediate tasks: to gain the confidence of my somewhat hostile departmental managers, and to find out how the organisation really worked and how it could be improved. I soon decided that the weakest link was Ellis Goddard, especially in respect of his responsibility for developing new testing equipment and instrumentation. As a cross-check I made a visit to the SAFF[1] in Paris to find out how they organised these functions, and on my return I announced a re-organisation in which Goddard lost this responsibility, and I promoted his assistant to be a Departmental Manager in charge of that work. Webber had always, like the other senior managers, held himself somewhat aloof from his staff, and had his tea break alone in his office. I was certainly too much persona non grata with any of my new department to join them, so I continued to go back to the chemistry department and my own old staff for coffee in the morning, where I was among friends. I also had permission to return there to do any further X-ray diffraction that I might find time for among my new duties – but this was a vain hope. I soon decided that the job was not merely an administrative one: there were many features of testing that cried out for a deeper mechanical and mathematical analysis than anyone had had sufficient

[1]”French Ferodo” *[RW]*

insight to give them and which I felt I could do. Some pieces of the work I started on and it was very interesting mathematically. Indeed I had a year or so earlier realised from circulated reports that just such a job needed doing on the bursting of clutch disks with highly anisotropic mechanical properties (disks based on wound yarn which were **much** stronger circumferentially than radially), and it may well have been my successful analysis of this problem, outside my then duties, that gave Dr Parker the idea of my new appointment.

However, it became apparent in the course of the year that administration was going to be a major part of my job. In total I had a staff of about 90, the majority of whom were skilled manual workers, operating test machines, driving test cars and doing general engineering workshop duties. They were all unionised, and in conjuction with the Personnel Manager I had to negotiate with their unions. This was a dreadful business. What was in dispute I do not remember but we had repeated sessions with the union representatives who would periodically withdraw to discuss in secret what we had said. We eventually reached what we thought was a successful compromise, only to have it vetoed by the Works Director on the grounds of its implications for other workers. When we re-met the unions they were (reasonably) scathing about whether we were authorised to reach an agreement.

I was also landed with another appalling job. The company was plagued financially by its high overhead costs, and the Board decided to set up a working party to investigate the reasons for this. It was to consist of one representative from each division of the company (defined as the areas of responsibility of each director), and Dr Parker nominated me. He further told the board that they needed someone with a scientific approach to analyse the problem, and had me appointed chairman of the working party. My main problem was to get any data to work on, because the Office Director (in charge of finance) was extremely secretive, and I was told from the start that we could not have any actual figures of expenditure! So the first part of the job was to draw up a statement of what information we had to have, worded in such a way as to underplay its significance to get it past the Office Director. We did in fact get a lot more than he wanted us to have and were able to deduce from it more than he realised. Of course I had help from the other members of the working party, but I had to take the lead in all this and eventually write a long report with a pretty tough deadline for completion. We eventually found that the real problem was heating the factory. As it was modernised, and especially when the new second factory had been built at Caernarvon, many processes had gone over from steam heated presses to electrically heated presses. These changes had no doubt been shown to be cost effective in terms of each individual machine but no-one had noticed that all that all the steam costed as part of the pressing operation, had through its miles of pipe, also heated the factory for no evident cost. With the new electric presses space heating had to be installed that this was costed as an overhead. What they eventually did about it I don't know – the information was years too late.

The upshot was that by October 1964 I was feeling very depressed – I did not want to spend the second half of my working life as an administrator, and I realised that I really loved things connected with chemical structures much more than mathematical analyses of other things. I was very doubtful of the possibility of getting an academic job because of the borderline nature of my expertise which seemed to unfit me for any department of chemistry, physics or maths although I had some kind of qualification in all three. However I resolved to try, recruited three referees (Prof Lipson, Jack Zussman and Mr Whitehouse), and started making applications. On the last working day before Christmas Dr Parker came to my office for a chat and asked me how I was finding the job. He was quite shocked when I described all the things about it that I didn't like, and he asked me what I saw myself doing about it. I replied that I saw myself leaving, and I think it gave him a gloomy Christmas having to envisage another major shake-up of his organisation so soon after the last one. I eventually gave my (three months) notice at the end of February 1965, and it was decided very quickly that Keith Mackenzie was to be promoted to the job as from the 1st of June, so I looked forward to a gradual hand-over to him which would give me plenty of time to complete various bits of work I had in hand. However within two or three weeks he died of a heart attack and I had to keep all my administrative duties going to the end. In fact no new appointment was made and Don Hatch took on all my responsibilities as well as his own. From what I heard afterwards he devoted much more attention to "my" job than his own, and did it very well, but I suspect that his consequent lack of attention to the "formulation" work cost him the loss of his expected succession to the directorship when Dr Parker retired – though he did get it for a few years later on, only to be thrown out about 1981 when the company's fortunes hit an all-time low.

It was a big wrench leaving Ferodo after 22 years, but Dr Parker made a very nice gesture in giving a leaving party for me at his house. We had not been on sociable terms with one another except in my first year there, which made it all the clearer that he had valued my work. In his farewell speech at the party he said that it was not to be regarded as a precedent for what he would do when other people left unless they had also stayed for 22 years and taken a maths degree and a PhD during that time! The conjunction sounded pretty unlikely at the time, and the caveat saw Dr Parker out to retirement, though subsequently the record was more than equalled by Don Hatch (38 years, an engineering degree and a PhD) and nearly equalled by Barbara Chapman (29 years and a PhD)!

In retrospect I have always been very glad indeed that I took the job at Ferodo, and the firm treated me very well indeed. The timing, both of my going there and of my leaving, were perfect, because I was in at the start of so many new things I got far better treatment than later recruits. Dr Parker encouraged me to do fundamental work and to publish it, and to take on the secretaryship of the X-ray analysis group of the Institute of Physics for 3 years – during which time I spent at least one day a week on my secretarial duties for that body without any regard being taken on the fact that it was pretty expensive to Ferodo. But above all it gave me a much broader experience of life than if I had spent all my working life in an academic environment. I particularly enjoyed the experience of working in the works production process in producing new resins, and working with the production workers on those occasions. Finally I was extremely lucky that the point at which my job ceased to please me was just right in three respects. It was just when the universities were expanding to a unique extent, when I was still just young enough to take advantage of it, and before the financial decline of Ferodo began to get really serious and to diminish the freedom of its research staff.