1957-1958 The BEPO Release and the New Wigner Group

When I returned from the Windscale works in October 1957, I found that everything had changed. There was an air of dismay, and even panic at Harwell. The news that the No.1 Plutonium production reactor at Windscale had caught fire was met with open mouthed disbelief. Most people did not understand how it could happen. They were unaware of the presence, and danger, of the Stored Energy in the graphite moderator, which had caused the fire. The talk was full of speculation, and even accusation about who was responsible. I was glad that I was not senior enough to attract any of the blame, but I knew that those at the top would be very worried men.

The director, John Cockcroft, called a meeting with John Dunworth, the head of Reactor Physics. They decided to resurrect the Wigner Group, which had only been shutdown at the end of 1956! John Simmons would head the new group to study, **again**, the effect of radiation damage on the graphite moderator of Nuclear Reactors.

Cockcroft was also very worried about the BEPO reactor at Harwell. The Stored Energy in BEPO had not been released for several years, and he considered it to be a potential danger. A fire at Windscale was one thing, but a fire at Harwell would be a disgrace! He decided that, as a matter of urgency, we must measure the Stored Energy content of BEPO, and plan for its safe removal.

John Simmons and I were immediately removed from the 'Shielding Group', and told to set up apparatus to measure the Stored Energy. I think John was quite relieved to hand over the reins to his deputy, Ron Dugdale. He was not very happy about the direction the Shielding Group was going, and was quite pleased to be out of it. I could not have agreed with him more!

We then found ourselves in a curious situation. All the Stored Energy equipment that John and I had developed since 1952 had been given to The Windscale Physics Group. The last part, the 'Linear Rise Calorimeter', which I had completed in April, was only removed two months before the fire. This meant that we had nothing! We did a quick search, but all we could find were a few drawings and sketches.

John now had to decide which apparatus to construct. After some discussion we decided on the 'Adiabatic Calorimeter' which we had first built in 1955. This was quite complicated, but was quick and easy to use. It was also suitable for the large amounts of Stored Energy which John expected to find in the BEPO graphite. While I was making a list of things that we would need, John went to see Dunworth to discuss how we should proceed. When he came back I was in for a big shock.

The government had set up an enquiry into the 'Windscale Fire' to be headed by Sir Alexander Fleck. John Simmons, as the leading expert on Stored Energy, was to be secretary to the enquiry. This did mean that John would not be available to help me with building the new equipment. John Dunworth had decided that I was not in a senior enough position to push through this urgent project, and that he would find someone else to take charge. At first I was very concerned about having to work with a complete stranger, and one that, most likely, had no experience in this field. I need not have worried; Dunworth was smart enough to appoint George Kinchin, someone that I knew very well, and who was well versed in the properties of reactor graphite.

I had known George for over eight years, and had worked with him before. He was now a very senior member of the Reactor Physics Division, so I was a little surprised but highly delighted to have him in charge of the project. At our first meeting, George was very surprised, and even dismayed, that there was no formal description of the apparatus that we were required to build. John Simmons had been a bit negligent in not ensuring that this was recorded. The fact that the work had been 'classified' was some excuse, but not any longer. After the 'Fire', a much more open attitude was required, and much was published in 1958.

I had built the 'Adiabatic Calorimeter' only two years before, in 1955, so it was still very fresh in my memory. I went through the design, and described all the parts in detail. I was surprised how quickly George absorbed the information; I did not have to repeat myself once. At the end of my description, George asked the big question: What have we got? Unfortunately, as the equipment had been given to the Windscale works, we had very little. I had found a few engineering drawings of some mechanical parts, and very rough sketches of the electronics.

[On reflection, it is interesting to note that no one suggested asking Windscale to return the equipment! However, at that time the fate of the Windscale works had not yet been sealed! Following the fire both reactors were shut down. They were never restarted.]

George asked me to make a list of everything that we needed, while he went in search of a lab. where we could build and operate the new equipment. I was rather sad that my lab. on the second floor of B10.5, had been taken over by the Shielding Group, so I was now homeless. Eventually we were given a lab. in the basement of B10.5. This was dark and gloomy, but otherwise very clean and already furnished.

When discussing the operation of the calorimeter, George had been concerned about the time 'lost' before and after the measurement. As the calorimeter was operated in a vacuum, it took some time after inserting a new specimen for the high vacuum to be attained. Also, following the release of Stored Energy, it took about one hour for the calorimeter to cool sufficiently before we could open the system. To overcome these delays, George suggested that we build two calorimeters and one set of the electronics. This way we could switch between the two calorimeters and increase the number of samples measured in a day.

The first item on my list was two vacuum systems. George found one instantly, and was offered another, which although not complete, was in the final stages of construction. The mechanical parts, including the calorimeter, turned out to be very simple. Having found the appropriate numbers, I managed to obtain a set of the engineering drawings from the Drawing Office. George immediately put these into the engineering workshop with high priority.

The next item was the two Galvanometer Amplifiers. This required ordering galvanometers, some optical lenses, and the special Tinsley 'all copper' thermocouple switches. I did not have a drawing of the wooden enclosure, so I had to guess the dimensions. I made a quick sketch and took it along to the carpenters workshop.

The electronics was a more difficult problem. The rough sketches that I had found were very scrappy, so I had to redraw them all and guess some of the component values. Fortunately my memory was quite good, so most of it worked first time. George had already primed the Electronics Division, so when I took along my sketches, they were able to put them to manufacture immediately. I only had one query from them, and that was regarding the power amplifiers. The calorimeter was in two halves, top and bottom, and each part was powered by 12 beam tetrode valves in parallel supplying a maximum of 300 watts. That is a total of 600 watts driven by a proportional control system. During testing they had found that the amplifier was prone to a parasitic oscillation at about 800 MHz. I had had the same trouble in the original, and the result was disastrous.
The high frequency was picked up by the sensitive thermocouples, and ruined the stability of the control system. The cure was appropriate damping in the screen to anode circuit, but it was difficult to get right. The Electronics division suggested an alternative valve, and this solved what I had found to be a difficult problem.

In a little over a week we had everything either 'on order' or in manufacture.
George was very pleased. It is interesting to note that we could not have arrived at this point so quickly without the immense pressure applied by George, who had absolute priority.

Once installed in our new lab., I began to gather the bits and pieces that we would need to assemble the equipment. Tools, wires, vacuum accessories, and test equipment which I could obtain from our local stores. I also managed to find two reels of thermocouple wire (Nichrome and Constantan), that I had squirreled away before we had lost all our equipment to Windscale. I was very pleased about that, as I had calibrated the wires several years before. It would save a lot of time.

We needed a Potentiometer to measure the thermocouple temperature. I was so pleased when George turned up with a high quality Tinsley instrument, which I recognised. It was the one that George had been using many years before, so he must have squirreled that away!

The first item to arrive from the workshops was the calorimeter. In machined pure copper, it looked beautiful. Almost a work of art. It was two identical short cylinders which fitted together with a tapered joint. This enclosed a central cavity which would contain the sample to be tested. Each half would have separate heater consisting of Nichrome tape wound into 72 slots in the curved cylinder face. I knew from past experience, that winding the heater was a very difficult and tedious task. It required cutting Mica sheet to critical dimensions and precise thickness, wrapping it around the heater tape, and pressing it into the slot. It did need to be a tight fit. I had expected that I would do this job myself, but George said that I had too many other jobs to do, so he would find someone else. I was worried about this because it was a critical part of the system. [The reason for this elaborate construction was simple. When power is applied to a normal heater winding, it expands and contact with the surface is lost, so reducing the thermal conduction. In the slot the tape expands and becomes tighter, so increasing conduction. John Simmons and I had long experience in trying to overcome this problem.]

As more parts arrived I began assembling the various components. The calorimeter enclosure was fitted to the vacuum systems, and the whole system thoroughly tested. Next came the parts for the two Galvanometer amplifiers. It was a joy to put these together, they both worked first time. The

light beam reflected by the galvanometer instantly locked on to the split photocell, and was perfectly stable. This was a very elegant piece of design by John Simmons.
[Today one may well ask why we used a seemingly clumsy device like a Galvanometer amplifier in an electronic control system. However, it was the only device available at that time, able to amplify microvolt level signals with low drift and very low noise levels.]

The rest of the electronics had already been tested by the Electronics Division. However, I tested it again, just to make sure that it was all complete. At this point I decided to make a small modification to the control circuit. I was not sure that John Simmons would approve, but when I explained it to George he thought it was an improvement. It was only a minor change to the circuit of the twin DC amplifier, which could easily be removed if it proved to be a problem.

In the original system the bottom half of the calorimeter was controlled to be at the same temperature as the enclosed sample (i.e. an adiabatic system), The top was then controlled to be at the same temperature as the bottom half. When the temperature is rising rapidly, the bottom would lag the sample, and the top would lag the bottom. I thought that it would be more sensible for both parts of the calorimeter to be controlled by the same signal, and the top to bottom signal just used as a correction.

As soon as we received the calorimeters with their completed heater windings, I fitted them into the vacuum chamber on each system. I had forgotten a few things, but these were only screws and nuts required for the assembly, and easily found in the local store. I then linked the heaters to the Covar glass to metal terminals that I had fitted into the base of the enclosure. The external end was already connected to the power amplifiers.

The next task was the most difficult; this involved fitting the thermocouple wires into the calorimeter. The wires were very fine (36 SWG) and, inside the vacuum enclosure, had to fit into tiny twin bore silica tubing. George realised that this was a critical and very tedious part of the assembly. He suggested that we should build both systems at the same time. I would build one, and he the other. He just said "you tell me what to do, and we will build them together". George had long slender fingers, and I knew from previous experience, that he was quite capable of doing delicate work. (Quite different from John Simmons, who had large hands, and found delicate work a problem).

We worked well together, and progressed quite rapidly. We had to fit two thermocouples to the calorimeter bottom, two for the graphite sample, and two single Constantan wires to the top and bottom parts of the calorimeter. The two single wires were bridged by the Copper of the calorimeter, so giving the temperature difference between the top and the bottom. (Another elegant piece of design by John Simmons). Altogether ten fine wires were threaded through glass capillary tubes to outside the vacuum enclosure. The wires were then sealed into the tubes using Black Vacuum Wax. Outside the enclosure the wires were threaded through PVC insulation, connecting each wire to an independent 'Cold Junction', which would be immersed in a vacuum flask full of a water/ice mixture. During this last part of the assembly, I discovered something about George that I did not know; he was colour blind, and could not tell the difference between the red and green PVC sleeves that I had chosen. I had to label them so that he would know which one to use.

This assembly took two days, and at the end we were quite exhausted. George was quite keen to test the system straight away, but as it was late evening, I persuaded him to wait until the next day. I anticipated problems (there was so much that could be wrong), so I thought that we needed a fresh start in the morning.

I had prepared a graphite sample, that had not been irradiated, and placed this into calorimeter A. The two thermocouples were attached to the sample, the vacuum enclosure sealed, and the vacuum pump started. It took about half an hour before we reached a good vacuum.

I them raised the temperature of the calorimeter to 100 degC, using manual control. Then, with some trepidation, switched to automatic control. I expected to see a small damped oscillation in the control system, due to a mismatch caused by the switchover. **The result was disastrous!** The system oscillated violently; the output from the power amplifiers swung from zero to full and back to zero every 3 seconds. Fortunately the output was feedback limited to 100 VDC so no damage was caused. I quickly switched the system off.

George was quite shocked, and even dismayed. We did a quick check to make sure that we had the correct polarity of the control circuit. It was all right. We took a break, sat down and discussed what could have gone wrong.

The reason for oscillation in a closed loop control system, is when the phase lag around the loop reaches 180 degrees while the loop gain is greater than unity. I had checked the gain of each amplifier, and all were as expected. This meant that the phase lag was greater than we had anticipated. I thought that the culprit was the calorimeter heater winding. I had been worried that this was not tight enough, and this could cause the delay.

We discussed the cure for this problem. (1) Rewind the heaters; there are four, and this would be tedious and take a long time. (2) Reduce the loop gain; this would increase the temperature difference between the calorimeter and the sample, and increase the errors. I suggested a third possibility: (3) Advance the phase in the electronic control system.

George did not think that we could produce a phase advance big enough to cure the problem. However, I thought that it was worth a try; there was a part of the control circuit which had a high impedance which may make this possible.
I went to the stores to find a range of low leakage capacitors that we could use for this purpose.

When I returned I found that George had summoned up some help. He had been joined by John Simmons, Tom Fry, and the deputy director Basil Schonland. They were all discussing the problem, but with no agreement. John wanted to rewind the heaters, but George thought that it would take too long and wished to reduce the gain. No one thought much of my suggestion, which was discarded. John even went as far as to say that my solution would make things worse.

While they were arguing, I went ahead and made the change to the electronics. I had to guess the value for the time constant, but much to my amazement, and joy, **it worked!** The oscillation was now damped and became steady after a few seconds.

When I demonstrated the result to the gathering, it received a mixed reception. George was all smiles and over the moon, Tom seemed highly amused, and John seemed rather embarrassed and grumpy. Schonland, as far as I remember said nothing. The meeting soon dispersed, and George and I were left to decide on the next step.

We thought that it would be prudent to run a series of tests to check the stability at various temperatures, and to select the optimum value for the time constant. It did take us another whole day, but it was well worth the effort. We managed to improve on my first guess, and found that the system was stable over the whole temperature range.

It was now late evening, but George was very keen to do a real test on an irradiated graphite sample. He had already obtained a sample from the BEPO engineers; it was a short cylinder, ½ inch in diameter and ½ inch high. I drilled two very small holes in the sample to take the thermocouples, and placed it in the calorimeter. The thermocouples were fixed in the sample by using the fine point of a pencil. This was pushed into the hole then broken off. This was a technique that I had used many times before. The vacuum system was then closed and the pumps switched on.

It took about 20 minutes to reach a low enough pressure. I raised the temperature manually to 60 degC then switched on the control circuit. It was perfectly stable, and the temperature was constant. The technique was now to raise the temperature slowly using a small radiant heater, which was in the cavity between the sample and the calorimeter. This was just a small coil of Platinum wire, fed from an external power source. When I switched the heater on the temperature slowly began to rise. I took the temperature measurements, using the potentiometer, and George began to write down the readings. When the sample reached 100 degC, I switched off the heater. The temperature continued to rise, slowly at first, then accelerated suddenly as the stored energy started to be released. I think that George was quite surprised at the rate of rise; the temperature rose to 300 degC in a few minutes. It proved to be quite difficult to keep up with the measurements, and to write them down. The rate of rise then decreased, and the temperature levelled off at 330 degC. While I was still taking measurements, George was plotting a graph, showing an 'S' shaped curve of the rise in temperature. From the total rise, and the known specific heat of graphite, it would be possible to calculate the total amount of energy released. The system had behaved perfectly and was stable throughout the experiment, despite the enormous power surge into the calorimeter.

It was now quite late, but we went home feeling very pleased with ourselves.

The next morning we sat down to write some instructions on how to use the apparatus. George thought that this was necessary because a large number of people would have to use the system.

George had already assembled a team of volunteers! who would help to operate the equipment. It was intended to run three shifts a day, but not at night. As each measurement would require two operators (one to measure the temperature and one to write down the result), it required a lot of people. It is interesting to note that when I had first built this calorimeter, in 1955, I was working alone. To take the measurement, and write down the result, when the temperature was rising rapidly was quite difficult.

The BEPO engineers had to take graphite samples from a wide range of positions in the reactor. They devised a machine to cut the samples from the roof of a fuel channel. The hole had a Vee shaped bottom, where the fuel element lay, and a square shaped top. The machine, which could be pushed along a channel to the correct position, had a trepanning cutter at the top which could cut a cylindrical core from the roof. It was a fantastic machine and it worked very well. We soon had a large number of samples to measure.
During the next few days George and I trained the 'volunteers' in the use of the new apparatus. However, most were very experienced and needed little help. It was not necessary to show them twice! I cannot now remember all their names but they did include Bob Loader and David Martin.

By the first week of December the measurements were in full swing, averaging 4 to 6 samples a day. It was a fantastic achievement to reach this state in such a short time; only two months after starting from nothing! This could not have happened without the high priority given to this project by John Cockcroft.

It all ran very smoothly and required little supervision. Once a rota had been set up the work continued with few problems. I organised the samples to be measured, and George began to analyse the results. The work continued through the Christmas period without a pause; two of the team even volunteered to work on Christmas day!

There was great interest in this project. We had a lot of important visitors to our very gloomy and untidy lab. in the basement of Building 10.5. Cockcroft, Dunworth and Schonland came to give encouragement, and to stress the importance of the work. Even the chairman of the UKAEA, Sir Edwin Plowden, turned up one day without warning.

At the beginning of 1958 John Simmons called me to his office to discuss the future. He had been charged with resurrecting the Wigner Group to study, again, radiation damage in graphite. So far the only members of the new group were John and me. He had to recruit new staff and to find new premises. I was very sad to hear that we could no longer stay in Building 10.5. I was very fond of my lab. on the second floor, which had a magnificent view across the lawns in front of the Radiochemistry building. The next shock was that we were to be transferred to the Metallurgy Division. Cockcroft thought that as the work came more under the heading of Solid State Physics, it would be better to be supervised by an expert in that field. Alan Cottrell was to assume that role; at that time I think that he was the deputy head of the Metallurgy Division.

[It is interesting to note that, although Cottrell's name occurred frequently on subsequent reports, I don't ever remember seeing him visit the lab., or take any interest in what we were doing!]

As the new group would be about a dozen people and require 3 or 4 labs., it was difficult to find spare accommodation that was all in one place. It was then decided to convert an old RAF Nissan hut, which I believe had been a small aircraft hangar. I went to see S37 (the S was for shed!) before the conversion was started. It was derelict and an unbelievable mess. I could hardly believe that it could be converted into suitable accommodation. However, the conversion was completed very quickly and proved to be acceptable. The inside was quite modern but, as the windows had been cut into the sides of the shed, the outlook was horrible – just looking at the black walls of the adjacent Nissan hut.

I learnt later that they had spent £20,000 pounds on the conversion, which was at that time about ten times the cost of a modest house.

The Bepo Release

Early in 1958 George, John Simmons and others were discussing with the BEPO engineers the best way to release the stored energy from the graphite core of the reactor. The method previously used was to switch off the air cooling, and to use nuclear heating to raise the temperature to the trigger point, at which the energy would be released. This had some disadvantages: The fact that the fuel temperature would be higher than the graphite, and that the extremities of the graphite core would be cooler than the centre. Factors like this, and the fact that the Windscale reactors had insufficient instrumentation to record the temperature, had probably led up to the conditions that caused the fire.

It was decided not to use nuclear heat but to use electrical heating in a low air flow. This would mean that the whole core could be brought up to the correct temperature. It was also decided to add a large number of thermocouples to the core, in order to record the temperature distribution.

The release of the stored energy from the BEPO reactor took place in March with little ceremony; in fact although there was a huge build up to the event, they kept very quiet about the exact date when it was going to happen. It was a great success. Most of the energy was removed, and the graphite core reduced to a safe state. After this event BEPO was to continue operating successfully for another ten years.

Shortly after this event, I received a letter from Alan Cottrell thanking me for my part in this successful endeavour. I should have been pleased, but I was annoyed that it came from a man that I had never met, and one who seemed to take little interest in the physical effort required to produce the results that made it possible. The letter also began with 'Dear Henson', the only time in my entire working life that I had been addressed in such a superior manner.

After the dust had settled George Kinchin returned to his work in the Reactor Division, and was moved to Winfrith that year. I don't think that we ever met again. I really enjoyed working with him. He was a very clever man, and despite his seniority, did not mind getting his hands dirty in the lab. In 2003, a few years before he died, we had a long telephone conversation, and exchanged letters.

Alan Cottrell left Harwell to become Professor of Material Science and Metallurgy at Cambridge University.

The New Wigner Group – 1958 - 62   Building S37

We moved into our new building in February 1958. It was a converted Nissan Hut, which in the RAF days had been a small aircraft hangar. The inside was pleasant and very new, but because it was in a line of similar huts, the outlook from the windows was horrible, just onto the black walls of the hut next door. At the front there was a workshop, with the M & F toilets on each side, then on the left a temperature controlled room with no windows for the dimensional measurements, and then an area with three offices. On the right there were two large rooms, and one small one at the back of the building. The first large room was to be mine, for the Stored Energy laboratory. It had an office at one end, which I was to share with other members of my group.

At first the group consisted of just John Simmons, Bob Loader and myself. We had been transferred from the Reactor Division to the Metallurgy Division; I was not too happy about the transfer, but there was no choice. The director, John Cockcroft, thought that our work was more in the field of 'Solid State Physics', so would be better served (and controlled!) in the new Division.

The next shock was that we would not be taking the Stored Energy equipment that we had just built in Hanger 10! That would be left with the Reactor Division so they could continue to monitor the BEPO reactor. We were to start from nothing, and build new equipment. I was not pleased, but it did have the advantage that we could now improve, and add new features, which were lacking in the original system.

The new building was, of course, completely empty so John had ordered a large amount of new furniture. When this arrived, Bob and I spent several days directing where this was going to be located. We did not have much of a plan but managed to sensibly spread it around the building. In this process I did manage to rescue my own desk from B 10.5 and place it in my new office.

As we were starting from scratch, John had to recruit a lot more staff. I was rather sad that Bob Loader would not be working with me, as he was to undertake other measurements.

The groups objective was to measure the changes in -   Stored Energy, Dimensions, Thermal Expansion, Thermal Conductivity, and Young's Modulus. And to chart how they changed with Neutron Irradiation. Of course we had done all this before, but now we were able to irradiate samples at higher temperatures, and to much higher neutron doses. The new Dounray Fast Reactor in Scotland, had a neutron flux one thousand times higher than the Bepo reactor at Harwell, where our measurements began in 1949.

The first new recruits to arrive were Richard Russell and John Caro, two very young Scientific Assistants. They were allocated to me for the Stored Energy work. They had no experience, but had been through the SA Training school, so they had been taught some laboratory skills.

These were quickly followed by W.N.Reynolds (Bill) the deputy group leader, and John Polling who was to organise the irradiation of the samples.

I had a long chat with John Simmons, to ascertain what was required. He wanted two new Adiabatic Calorimeters, to be labelled, Systems C and D, to distinguish them from the A and B systems that we built in Hanger 10, and one other system for special purposes. The Adiabatic Calorimeter was very flexible, and could be used in other modes. When the stored energy in the sample is too small to get a good rise in adiabatic mode, it is possible to use a Pseudo Linear Rise mode where the calorimeter is controlled to be at a small temperature above the specimen.

This is done by inserting a small constant voltage into the thermocouple loop that measures the difference between the sample and the calorimeter. The system then ceases to be adiabatic, and causes the sample temperature to rise steadily at a rate depending on the difference. When stored energy is released the temperature rises above this curve. A second run is then necessary to confirm the baseline. The difference between the two curves is a measure of the release of the stored energy. This of course assumes that the heat transfer characteristic remains the same for both runs. This is a good approximation when the amount of stored energy is small.

My first job was ordering all the necessary equipment. Because of the systems that we had recently built in Hangar 10, we now had a good set of drawings for most of the components. All that I needed to do was locate the reference numbers, and send them for manufacture.

I was pleasantly surprised to find that that the required Vacuum Systems were now available – 'off the shelf'. I remember that George Kinchin had been very scathing that people were building their own systems from scratch! Now we had a standard design made from standard 'Edwards' components. It consisted of a four inch oil diffusion pump with isolating valve, a backing pump, all the necessary pipes and valves, a Piranni vacuum gauge, all mounted in a purpose built steel frame. All one had to do was plug in the mains lead. I am sure that this was all down to George.

In discussions with John Simmons, we decided to made some small modifications to the system, in order to improve the stability of the heat transfer characteristic. The 'Top Hat' cover for the vacuum chamber would be water cooled, and the calorimeter, which had previously been bare copper, would be electroplated. It took a little while to find a suitable plating to use, however I was surprised to find that Rhodium plating was an easily available option. Rhodium is a noble metal of the Platinum group - It is chemically inert and corrosion resistant.

We had good drawings of all the electronics, so all I had to do was submit them for manufacture. However, I did make a few small modifications. I added some additional switching to the input of the control amplifiers. The use of 'rate control' was made optional, and a manual control (test) input was added to simplify the 'startup' procedure.

I ordered two precision 'Potentiometers' from H Tinsley & Co. also the Galvanometers and the all copper switches for the Galvanometer Amplifiers.

Most of the other required items could be ordered through our normal stores system.

We also needed a calculating machine. Of course in 1958 there were no electronic machines. They were either hand wound like Brunsviga or Facit, or electro-mechanical like Monroe and Friden.

I was horrified by how much they cost. We eventually chose a 'Monroe-Matic' machine that cost £650. This had less capability than a £5 modern pocket electronic calculator. It was considerably slower, made a lot of noise, and was a huge machine weighing about 15 Kg.

Setting up the new Stored Energy Laboratory

I now had two assistants Richard Russell and John Caro. To keep things simple I will call them RR and JC. They were both very young, but enthusiastic and willing to work hard. RR was a little 'laid back' and needed a prod sometimes to get things done quickly. JC, on the other hand, was very confident, but was apt to do things too quickly and make mistakes. He also talked so fast that it was often difficult to keep up with him.

I had already decided on the layout of the room. It would be a symmetrical 'U' shaped arrangement at the far end of the room away from the office and the door to the lab. At the centre a long table would hold the two measuring 'Potentiometers, then a table in each corner to support the Galvanometer Amplifiers, next the 6 foot high electronic racks, and finally the vacuum systems with the calorimeters.

While waiting for the new equipment, we prepared the room as far as we could. We also placed two long tables at the other end of the room, where we could assemble and test the new items as they arrived. The room was well set up. A shelf around the walls provided all the services: Gas, Water, Compressed Air, and all the Electrical Sockets that we would need. It even had sockets providing 200 Volts DC, which was required to supply the power amplifiers.

To the edge of the shelf we attached a Cable Trough. This was made from Dexion, a construction aid like large Meccano, with slotted holes. This would keep all the cables (and there were many) off the floor. This had been a big problem in the hastily assembled set up in Hangar 10.

The first items to arrive were the three vacuum systems. These were beautiful and just what was required. We put these on test immediately. RR and JC had been given some knowledge of vacuum technology at the training school. However, I went over the details of the new systems to ensure that they knew the purpose of every valve and switch, to make sure they made no mistakes.

Next to arrive were the two calorimeters. They looked perfect with the new Rhodium coating. I now had to make a big decision, whether to allow the new assistants to install the heater windings. This involved cutting the Mica insulation into the precise dimensions required, and pushing the Nichrome heater tape into the slots in the calorimeter wall. It had to be a tight fit, or it would give problems with the control circuit. I gave them some instruction on how to cleave the Mica into thin sheets of the desired thickness, and how to cut this into the strips required to insulate the tape. This was a very delicate operation, as the Mica needed to be only a few thousandths of an inch thick. Surprisingly they were quite interested in this procedure, so I let them get on with it. I just had to check occasionally, that they were doing it correctly.

This now gave me time to do other things. The components for the Galvanometer amplifiers had now arrived, so I set about putting them together. Once again this proved to be quite straightforward. This was a good design, and when tested, worked perfectly. I just made one small modification.

Setting the zero was quite tricky, so I introduced a beam shifter in the light path to the photocell. This was just a half mm. thick microscope slide mounted on a simple piece of Meccano, so that it could rotate and move the light beam across the face of the split photocell. It worked very well, and served as a fine adjustment of the zero setting.

While this was happening the parts for the two vacuum chambers arrived. I interrupted our work to assemble these on the vacuum systems. The base plate was bolted onto the top of the isolation valve, and the 'top hat' placed on this using an 'O' ring seal. We were then able to give the two systems a good long test to make sure everything was working as required.

Next to arrive was the 'electronics'. We placed the two 6 ft. racks in position, and bolted the modules into the rack. Each rack contained two power amplifiers, one control amplifier, and one unit containing all the required power supplies. After checking that the two assistants had received instruction on how to connect mains cables, I left them to install these on the racks, while I went to a meeting, after warning them not to switch anything on.

When I returned I found that the work had not been completed. When I asked why, Richard looked terribly upset and ran out of the room. He did not come back until the next day. John Caro explained to me that Richard had cut through a **live** mains cable with a razor blade! A narrow escape! He was not hurt, just shocked at what had happened. The arc caused by shorting the mains voltage, had melted the centre part of the steel blade. This made a 5 mm. hole which was surrounded by a layer of melted copper from the cable.

[I must explain that the razor blade was a 'Ever Ready Corrux' blade. This had only one sharp edge, was thicker than the more familiar 'Gillette' blade, and had a metal bar on one side. It was ideal for cutting, and we had been using them to cut the mica sheets]

No harm done. However, I gave them a lecture on safety, and showed them how to use more efficient tools to cut and strip cables.

By the time they had finished winding the calorimeter heaters the metal parts for the mounting had arrived. It took only one day to assemble these into the vacuum chamber. The two heaters were then connected to the 'Covar' seals which passed the wires through the wall of the vacuum chamber. I then left RR and JC to connect the external cables to the output of the two power amplifiers. They also installed the various cables that connected the 4 boxes of electronics together.

The next job was installing the thermocouples. This was a very delicate task, so I did this myself, but helped by the two assistants who were keenly interested in the construction. Fortunately I still had the two reels of wire (36 swg. Nichrome & Constantan) that we had used on the Hangar 10 calorimeters. I had calibrated these some years before, so this would not have to be done again.

This required 10 wires. Two thermocouples on the fixed lower half of the calorimeter, and two for the Graphite sample to be tested. The remaining two wires were both Constantan; one to each half of the calorimeter. The copper calorimeter bridged the two wires and provided the difference in temperature between the top and bottom.

This did take a lot of time as the wires had to be passed through glass capillary tubes, in the wall of the vacuum chamber, and sealed with 'Black Vacuum Wax'. On the inside the wires were insulated with fine twin bore silica tubing, and outside covered with PVC sleeves. Outside the wires went straight to the 'Cold Junctions' which would be immersed in a one litre vacuum flask filled with ice. From there the wires were connected to copper cables. These were led to the Galvanometer Amplifiers and the Potentiometers. Note that the 'Hot' junction of the thermocouples were arc welded and never gave any trouble at the high temperatures experienced in the calorimeter; the 'Cold' junctions were connected with soft solder.

Last on the list was the heater to start the 'Adiabatic System'. This was a small coil of Platinum wire installed inside the calorimeter, between the sample and the cavity wall..

It was over a week before we had completed both systems. I think that RR and JC were very interested in the construction, and were a great help.

The two new Tinsley Precision Potentiometers had arrived. They were beautiful instruments with a resolution of one Microvolt. We soon got these installed on the bench between the two systems. They both came with a galvanometer, and a 'Standard Weston Cell' for calibration. However, I had forgotten one thing - they required a two volt 'Lead Acid' battery to power the potentiometer. It was another two days before we had found the correct batteries and had them charged.

After a few more connections, we were ready to go. I placed an unirradiated sample in the calorimeter of system 'C', sealed the vacuum chamber and waited until the pressure was low enough. I then raised the temperature of the calorimeter using manual control to 100 degC, then switched to automatic control. I breathed a sigh of relief – after a short damped oscillation the system was stable. This had been a problem on the Hangar 10 systems, but was cured by using rate control to enable a small phase advance. Applying a small voltage to the Platinum heater, caused the sample temperature to rise at about 4 degC per minute, as required to start an 'Adiabatic' measurement.

It was now time for a real measurement. I found a BEPO sample that I knew contained .about 100 cal/gm of stored energy, and placed it in the system. I had given the boys some experience of using the potentiometer, so I got RR to take the measurements and JC to write down the readings. One thing that I had forgotten – a clock! A quick dash to the stores to get a timer. It was not ideal, but it would do for now. I asked them to take one reading each minute, while I raised the temperature using the starter heater. When the stored energy began to be released, I switched off the heater. The temperature now began to rise very steeply. I think that the boys were a bit surprised at the rate of rise, and found it difficult to keep up. When it had finished I got them to plot the curve on a graph. The temperature had risen over 200 decC in only a few minutes. I then showed them how to calculate the amount of energy released. A very successful first measurement on our new system.

The next day we repeated the measurement on system 'D'. Again it worked perfectly. So now we had two new systems in good working order. It had taken us over four months of hard work to reach this point.

It was now obvious that we needed a new clock to make it easier to take the measurements. I could not find a suitable one off the shelf, so I decided to make two new clocks. I gave the job to the two new assistants. As it was only required to count minutes, it was very simple. A mains synchronous motor which rotated once per minute, and a four digit mechanical counter which was advanced one step per minute. The counter could be reset to zero at the start of a run. This was all mounted in a steel box, and mounted on a shelf above the potentiometer.

We were now ready to start real measurements. We had a backlog of samples from the BEPO reactor, so after some training on how to operate the adiabatic calorimeter, I let the boys get on with it. I was surprised how quickly they managed to operate what was a very complex system.

The problem with 'Air Cooled' reactors

The early reactors like BEPO and Windscale, were air cooled. This was mainly because it was simple and quick to build, but it was a bad idea. The air inlet was at ambient temperature, and no heat was recovered. It was just blown straight up the chimneys and wasted. This meant that the Graphite moderator at the inlet end of the reactor was cold, and this was where the problems like stored energy and dimensional changes slowly built up. We had learnt that the hard way back in the early 1950's.

The new breed of reactors, like Calder Hall and Chapel Cross, were gas cooled using Carbon Dioxide in a closed loop. These also had a heat exchanger that could remove the heat and generate electricity. This meant that the inlet temperature could be raised to remove some of the problem Wigner effects. Of course the outlet temperature was limited by the maximum that the Uranium fuel could tolerate, so raising the inlet temperature reduced the power output. It had to be a compromise. Subsequent Graphite moderated reactors like 'Magnox' were all built in this way.

The Pseudo Linear Rise method.

When the amount of stored energy was small, the sample was unsuitable to be measured by the Adiabatic rise method. By injecting a small voltage into the control circuit of the apparatus, the calorimeter could be controlled to be at a higher temperature than the sample. This caused the temperature of the sample to rise at a steady rate. The rate of rise was not linear as the release of the stored energy caused the rate of rise to increase. The energy was therefore teased out of the sample. A second run, when there was no energy left, was necessary to calibrate the system. The stored energy released could be calculated from the difference between the two curves.

Of course there was an issue about the change in heat transfer characteristic as the energy is released, but it proved to have a negligible effect. Unfortunately this method required two 'runs' for each sample, followed by lengthy procedure to calculate the result. A graph was then plotted of the rate of release of energy against the temperature. The area under this curve was the total amount of energy released. This area was measured using a 'Planimeter', a very clever mechanical instrument, which took a great level of skill to use.

Of course this all took far too much time, so it had to be simplified. The first step was to eliminate the second 'run'. This was possible by defining the heat transfer characteristic as a standard. This was proved to be very stable as long as we precisely controlled the position of the sample in the calorimeter. This did speed things up, but we then found that, because the calculation process took so long, we could not keep up with the measurements. This required another step to the new technology of 'Digital Computers'.

Digital Computers

By this time we were getting on quite well with the measurements, but it was slow progress and we were finding it hard to keep up with the stream of new samples from the new breed of reactors.

We now had a new addition to our group, David Longstaff, who was an AEO (Assistant Experimental Officer). He was not very bright, but soon became a useful member of our team. Despite the fact that we were over stretched, John Simmons asked me to do some calculations on our Mechanical calculating machine. The job was given to David. This was a calculation of the progress of a thermal wave through the Graphite moderator of a nuclear reactor. Its sounds complicated, but it had been reduced to a simple iterative step by step process. It was very tedious, and David was not happy.

By sheer chance David had joined the Harwell Hockey team. There he met Bob Hopgood who was working on the new Harwell Digital Computer. He suggested that we use the new system. After some disscussion with John Simmons, Bob wrote a programme to do the calculation.

The computer was a Ferranti Mercury, a very early computer containing thousands of glass thermionic valves, and using punched paper tape to input the programme and data. The 5 hole paper tape was prepared on Creed Teleprinter machinery which was the standard, at that time; that the Post Office used for sending telegrams! It was all very clumsy and difficult to use. It used the Murray code, but because it was only 5 hole tape, it had only 32 combinations, not enough for the alphabet and numbers etc. So it had to use a special code to shift between 'Figures' and .'Numbers'. If you made a mistake in preparing the tape, it was a sticking plaster job of repair, or starting again. The programme was written in 'Autocode', which was developed at Manchester University for the first digital computer.

However, it all worked and produced satisfactory results. John Simmons was very pleased. This prompted me to suggest that we use the computer to calculate the Stored energy results. John though that this was a good idea, and approached the head of the computer department to get advice. We were given the name of Denis Rimmer who was a member of the Theoretical physics division. I went to see Denis who was only too keen to help. However when I suggested that we used the Ferranti Mercury, he was not at all keen. He suggested that instead we should use the new IBM 704 computer which was located at AWRE at Aldermaston. This was available to us, and had the additional advantage that it could be programmed using FORTRAN. This was a much more powerful language and very suitable for this task. The Name Fortran is derived from Formula Translation, which was a language most suited to numeric and scientific calculation.

After much discussion Denis wrote the programme which turned out to be a stack of 80 column punched cards about two inches thick. All we had to do was put the raw data onto punched cards. The punched card had many advantages over paper tape. A single column had 12 holes so was able to represent any character or number. Also a single card could contain a complete data reading at one single time. Another advantage was that when punched on an IBM machine, it printed the contents on the top line of the card. All this made it very easy to prepare the data and also to make quick modifications to the programme. When I wanted to buy an IBM card punch I got a shock; it cost £2000, There was no way that we could afford that, so we would have to go to the Theoretical Physics building to use a punch, but that was a long walk and was inconvenient. The solution was an inexpensive hand punch. This could punch numbers by pressing a single key, but it required one to press keys 11, 3 and 8 simultaneously to get a decimal point! We made a simple addition to the punch so that it could do this by just pressing a simple lever.

Of course all of this took a long time to organise. However, we soon got into a new routine which proved to be very satisfactory. We required two operators, one to measure the sample temperature and one to measure the difference in temperature between the sample and the calorimeter. Readings were taken once per minute, and immediately transferred onto the punched card by one of the operators. It was hard work, and required a great deal of concentration to keep this up for over two hours. In this way we managed to get in two measurements a day, and three if pressed.

It is interesting to note that we measured the temperature in Millivolts (the thermocouple output) and this was converted to degC using a cubic transfer equation. This was now all done effortlessly within the computer programme.

At the end of the day, the punched cards, and the programme were bundled together and taken to a dispatch point. At first the only one was in the Theoretical Physics building. From there they were taken to AWRE and processed overnight. The following morning we were able to collect the returned pack of cards, and the printed results.

This new method worked very well, and we were now able to keep up with the steady stream of samples as they came in from different reactors.

I think that John Simmons was very pleased with the rapid progress that we had made.

For me it had been a hectic but very stimulating time.

The Group expands

Other members had now joined the group to assist with the other measurements. John Polling had left (I don't know why) and had been replaced by Alan Perks. Alan was an old friend who had been with us in the original Wigner group. He now arranged the supply of irradiated samples and also supervised the measurement of the changes in - Dimensions, Young's Modulus, and Thermal Expansion. Bob loader was measuring Thermal Conductivity, a very difficult task.

The New Wigner Group 1958 – 62 continued

With things now running smoothly, I was now able to pay attention to some minor problems which had been bugging me for the last year. The Zero of the galvanometer amplifiers was set at the beginning of every run. However it did drift and I was determined to find out why. After some investigation it proved to be sensitive to the 12 volt power supply that powered the lamps. I could not think why, but the supply was not stabilised.

Also the Potentiometers had to be calibrated against a 'Standard Weston Cell' several times a day. They were powered by a two volt 'lead acid' battery, which had to be frequently recharged, and did drift as it discharged.

Both these problems required a stabilised power supply, but this was difficult to provide at low voltage. The solution turned out to be the use of Transistors.

Transistors

Transistors only appeared on the market in the mid 1950's. I had been to some lectures about them, but had written them off because, at that time, they were all low impedance and low voltage devices. They were also Germanium PNP devices, which in my opinion, was an upside down world. However, they seemed to be just right for a low voltage stabiliser.

When Bill Reynolds arrived in the group he had given me a catalogue of 'Newmarket' transistors. This was a name that I had not heard before, as the main manufacturer of transistors at that time was Mullard. However, I found in the catalogue a power transistor capable of supplying 2 amps at 12 volts. Two of these in parallel would be just right to provide the 12 volt at 4 amps required for the galvanometer amplifier lamps. However, when I looked at the low power transistors, I did not like what I saw. They may have been suitable for 'Radio' applications, but they were not what I wanted.

After some searching I found that Texas Instruments made a range of NPN Silicon transistors. These were much more suitable for the application, and were available from the stores at AWRE Aldermaston. I obtained detailed information about these devices from TI, and was soon able to design a comparitor circuit using two 2S004 transistors. Using another as a driver, I found that it interfaced nicely to the PNP power transistors. A reference voltage was provided by five Mercury cells, giving 6.75 volts, which proved to be very stable.

Before connecting the complete system, I tested the output stage on its own, to make sure it could do what was required. With the two power transistors mounted on a large heat sink, as required by the specification, it all worked well and provided 4 amps output at 12 volts.

However, when I connected the rest of the circuit – disaster – the two power transistors were destroyed! I was not sure what happened, but it transpired that they were subject to 'Thermal Runaway'. This is a condition where increasing temperature lowers the resistance and so increases the current, so leading to destruction. I believe that this happened because of the transient condition caused by switching the stabiliser on. Rather than waste time on solving this problem, I decided to use four Mullard OC35 transistors in parallel. These were much more stable and never gave any trouble.

The new stabilised supply proved to be very stable. The output changed by only 0.1% when the load was varied from 1.2 to 4.0 amps. At 4 amps output the drift over the period of a day was only 0.03%. A very satisfactory result.

When this was applied to the lamps in the galvanometer amplifiers the stability was improved. The Zero drift was reduced to 2 to 3 tenths of a microvolt per day.

The problem with the potentiometers was much simpler, they only required a power supply to give 2 volts at 25 milliamps. I used almost the same circuit, but used an NPN 2S006 transistor to drive the output. The reference was reduced to two Mercury cells giving 2.7 volts. However, as the supply had to be 'floating' it was still powered by a lead acid battery.

This improved the stability of the potentiometers by a factor of 10, meaning that they only had to be calibrated once per day.

I was very pleased as my first foray into the new world of Transistors was a great success.

Visitors

No sooner than we had the whole system working well, we had a lot of visitors. They came from the new Nuclear Power stations, the Windscale works, and some from research laboratories abroad. They came to study our methods Some even took root in my lab. and helped with the measurements. It was a very busy and stimulating time for me. The only trouble that I experienced was a group from a power station in France. They spoke little English, so this made life difficult as we did not have a fluent French speaker. Fortunately they did not stay long.

Extending the range to 650 degC

As the new Nuclear Power Stations came on line there was a trend to higher maximum temperatures to improve the efficiency. The maximum temperature was limited by what the fuel could stand. Improvements in fuel pin design allowed higher temperatures, so we had to extend the range of our measurements from 450 to 650 degC.

This at first sight did not seem to be much of a problem. However, there were two things that required attention.

(1) The calorimeter could easily stand the increased temperature, but the thermocouples had only been calibrated up to 419 degC - the melting point of Zinc. We would need a new calibration point at the melting point of Aluminium - 660 degC

(2) The stability of the electronic control circuit would have to be improved, as it was already close to its limit. This would mean a new look at the design.

Calibration of the thermocouples

I had calibrated the Nichrome/Constantan thermocouple wires several years ago, at the melting points of: Tin – 231, Lead – 327, and Zinc – 419 degC. With reference to Ice – 0 degC.
A cubic curve was calculated using a 'Least Squares Fit' This had served us well, but we now needed a new point at the melting point of Aluminium.

The first thing on the list was to find the Aluminium in a sufficiently pure form. I quickly obtained a bottle of 'Analar' (extremely pure) Aluminium granules. I was unable to find a suitable crucible, so I had one made from reactor grade Graphite, which is very pure. This fitted nicely into the small tubular heater that I had used before. Connecting the heater to a Variac, I was easily able to melt the Aluminium. I covered the top of the molten metal with Graphite powder to prevent to much oxidation.

I made a new thermocouple, the hot junction was insulated with twin bore silica tubing and placed in a silica sheath. The cold junction was placed in a glass tube and immersed in a thermos jar filled with melting ice. A potentiometer was used to measure the output. With the hot junction placed in the molten metal, it was easy to adjust the power to the heater so that the metal cooled at a suitable rate. After a few trials, it was possible to plot a cooling curve which showed a distinct plateau, caused by the Latent heat released as the metal solidified. I repeated the process several times and got the same answer for the melting point. When compared with the previously calculated cubic curve of temperature against voltage, it fitted so well that no change was needed. A very satisfactory result.

[I digress at this point to talk about the cold junctions used in all these experiments. A quart sized wide neck thermos flask was used to contain the ice. These were supplied with a large cork stopper which fitted closely into the neck of the jar. The cork was pierced with holes to take the required number of glass tubes for the cold junctions. The jar was filled with an ice-water mix, which was refreshed at the beginning of each experiment. The required ice was created by rubbing a large block of ice, (delivered every day) over a fixed jack plane. The plane was a standard wood plane and produced a kind of 'snow' that was placed in the flask with water. It was important to have the correct amount of water in the mix. Too little and the temperature could be below 0 degC, too much and it could be above. It was important to get this right.]

Modifications to the Control circuit

A problem of the existing system was that the output voltage to the calorimeter heaters is proportional to the input error voltage. This meant that as the temperature rises, the output power to maintain that, increases as the square of the input. This of course increases the loop gain of the control system, and can make it unstable at high temperatures.

To cure this problem, a 7 diode function generator was fitted to the feedback circuit of the Bottom output amplifier. This gave an output power approximately proportional to the input voltage signal. The Top amplifier then copied this output via a 'Unity Gain' inverter. It was not exactly unity gain, as it could be tweaked to get the correct balance between top and bottom.

This arrangement gave the opportunity to fit an Integrating amplifier, driven by the Top to Bottom galvanometer amplifier The integrator output was then fed into the unity gain inverter to correct the balance between the top and bottom parts of the calorimeter.

It all worked, when the integrator was switched on, the difference in temperature between the top and bottom was slowly reduced to zero.

I must admit that it all worked very well; we were now able to extend our measurements up to 650 degC. I was very pleased with myself.

New additions to the group

Of course all this development had to be merged into the regular programme of Stored Energy measurements. We could not afford to get behind.

During this time we had some changes to the group. I was pleased to be joined by John Blackburn, who had been in the RAF as a technician. He was a great help in the modification of the electronics. I was also given two new assistants, Roger Griffin and Malcolm Hone, but unfortunately I lost John Caro.

John Caro, who had proved to be a very useful member of our team, decided that he wished to be transferred to the Medical division. He had a burning ambition to become a doctor! I must admit that I thought that it seemed to be a little late to make this decision. However, with the help and advice of one of the doctors, he eventually left Harwell, took his 'A' levels, and went to medical school. It did take a long time, but he eventually became a GP in Norfolk.

In 1960 two new Scientific Officers joined the group. Peter Thrower, a Cambridge graduate, was given the task of task of studying radiation damage using the Electron Microscope. Also Peter Goggin from Reading university, who was to undertake low temperature studies of radiation damage.

The larger group also acquired a number of extra staff. I cannot now remember all their names, but included, Jenny, Aileen, Heather and Dave Clifton.

New Nuclear Power Stations

All the nuclear power stations built at that time were not commercial; their main purpose was either experimental, or dual purpose to produce Plutonium. A new breed of Reactor, the 'Magnox' Advanced Gas Cooled Reactor, was to be the power station of the future. These were to be built by private companies, and had to be commercially viable. The first was being built at Berkeley in Gloucestershire, on the edge of the river Severn. Construction started in 1956, but it did not start supplying electricity to the Grid until 1962. It was then in operation for the next 27 years! More were built in succeeding years, and seven of these are still in operation today (2018).

As the Magnox reactor also used a graphite moderator, it would require staff who were familiar with the problems caused by radiation damage. So it was no surprise when, in 1960, a Physicist from Berkeley asked to come and study our methods. Ray Jenkins liked it so much that he stayed for over a month, and readily helped with our routine measurements.

Automation?

When Ray was assisting taking the measurements, he was appalled at the tedious job of two people taking a reading on the potentiometer every minute, and keeping it up for over two hours. He asked why it could not be done automatically. He showed me an article in an electronics magazine describing a Digital Voltmeter. (Common place in today's world, but very new technology at that time) This could take a reading at the press of a button, and the data recorded. It was only an experimental device, and quite unsuitable for our purpose; it measured volts, and we needed microvolts. However, it did make me think and investigate to see what was possible. My first port of call was H Tinsley & Co. the manufacturer of the potentiometers that we were using. I was surprised to find that they were already developing such a device. They offered to give a demonstration at their factory in London. I quickly made an appointment, and went to London with Ray. We were given a good demonstration of the system, which could take a reading at a set time, and print the result. However, it only recorded to the nearest 10 microvolts, the last digit at microvolt level was then printed on a chart recorder. This meant that we would have to analyse the chart and add this to the printed output. This seemed like too much work after the run finished. Also it was only a prototype system, and they did not have even an estimated price.

However, seeing that their system was mounted on top of a standard potentiometer, gave me an idea; I asked if they could fit a system of switches on top of our existing potentiometers, so that the position could be read and printed. They said that they would look into this and give me an estimate. A week later I received a set of drawings and an estimate of £250. As I could sign for something up to £ 250, I accepted the offer. Our potentiometers had to be sent to Tinsley's to have the switches fitted. Only then did I realise that the estimate was for the switches only; the fitting cost another £250!

When I looked at the estimate it was a bit confusing having the same price for the cost of the switches and the cost of fitting. This got me into trouble with the administration, who normally insisted that orders over £250 should be sent out to tender! After a lot of persuasion I convinced them that Tinsley was the only firm that could complete this order.

Creating the Scanner

With the switches fitted, we could still use the potentiometers in the normal way, so the measurements continued as usual. I was now faced with the problem of providing a mechanism of reading out the switch positions. After considering several different ideas, I settled on a very simple solution; I could use a 'Uniselector', this being an electromechanical stepping switch, similar to those used in telephone exchanges at that time. I chose one that had 25 positions, one for each digit to be recorded. It also had several banks for the data input. I needed to record the two potentiometers and a clock. This with spaces and decimal points took up most of the available positions. This was driven by a simple transistor multivibrator and driver, working at ten steps per second. So one sweep took only 2.5 seconds. This was quite within the capability of the Uniselector. It selected the required data on the forward stroke and printed it on the reverse. The clock turned out to be very easy; I found one in the standard electronic units in our stores, which was normally used in counting applications. It already had a digital readout feature.

The next problem was what to do with the output from the selector. I would have preferred it to directly print the punched cards, which we used to process the data. However, the only punched card system available cost £2000, so it was not possible. After some searching I found a Creed paper tape punch which cost only £200. This had 8 holes, so did not suffer from the same problems as the Mercury computer system, which had only 5 holes. With appropriate drivers, this interfaced very nicely with the output from the selector. It all worked very well. When the operators had taken a reading, one just needed to press a button to record the data. I was greatly assisted by John Blackburn in the construction of this new apparatus.

Of course I had first discovered that there was a punched tape to punched card converter available in the computer centre. So our data processing could continue as usual.
The whole system now worked very well, and took a lot of pressure off the operators. I was just sad that Ray Jenkins, who had proposed the idea in the first place, was not with us to see its completion.

Writing Annual Reports

As I had several people working for me, I was now required to write an annual report on each one. I found this to be very difficult. It was awkward to be honest and sometimes critical, when I considered most of them to be my friends! John Simmons was not at all sympathetic to my dilemma, and no doubt gave me a bad report!

The Controlled 'Linear Rise' system

With the routine measurements now proceeding smoothly, and a team who now required little supervision, I was able to look at another advance in the way we measured the stored energy release. John Simmons was very keen on the Linear Rise method, as it meant that all samples went through the same temperature profile regardless of the amount of energy released.

[John and I had first built a Linear Rise system in 1953, It was very crude, but it did demonstrate that it was a viable method. In 1956 I built an advanced form of this. It worked well and produced good results. The output of the energy release was printed directly onto a chart recorder. However, the system was very complex, and difficult to manage. When the Wigner group shut down at the beginning of 1957, the system was given to the physics group at the Windscale works. It was taken to Windscale in the summer of 1957, just two months before the disastrous fire. For more detail on this see Chapter 7.]

Using the Adiabatic calorimeter system, the concept was to control the calorimeter temperature to be above the sample temperature, at the beginning, and lower than the sample when the energy starts to be released. So that the rate of rise in sample temperature was linear. A way of doing this is to compare the sample thermocouple output with a linearly rising voltage, and use the difference to control the temperature of the calorimeter. This was the way John wanted me to go, however, I suggested another way. To differentiate the sample temperature, to get the rate of rise, compare it with a constant voltage, then integrate this and use it to set the calorimeter temperature.

In principle this method sounded good, but John warned me that I would have two problems; the long time constant required for the differential stage, and also the increased sensitivity to noise. Despite the warning I decided to give it a try.

The real problem was that as the differential stage required a long time constant it required that the Op Amp (Operational Amplifier) would require a very high input impedance. The ones that we had been using had a CV138 (valve) input, which had an input impedance of about 10 Megohms. This was nowhere near high enough. I therefore designed a new amplifier, using the semi-electrometer valve (CV432) that we had used in the photocell amplifier in the galvanometer amplifier. This had an input impedance of 100,000 Megohms, more that adequate for the purpose. The new Op Amp was very stable and had an open loop gain of 6,000, a very pleasing result.

As John had predicted, the system was difficult to control. It was very sensitive to mains borne electrical noise, and also to physical vibration of the galvanometer amplifiers. However, after some refinement, it was possible to make the system stable. After I had added a suitable start up procedure, it proved to be a very good system and worked well.

We made a lot of measurements using this method, so the system, although difficult to manage, gave good results and had proved its worth

The end of the long road

In April 1962, we had completed all the measurements of the samples that were required for the assessment of the Stored Energy in the new Power Producing Reactors. Most of these samples came from either Calder Hall, the first power producing reactor, or were irradiated in the DIDO or PLUTO heavy water reactors at Harwell. Note that the Windscale reactors were long dead and not in any way used.

The result of all our work was finally published in the following report:

Philosophical Transactions of the Royal Society of London
No. 1043 Vol. 254 pp. 361-395 February 1962

Stored Energy in the Graphite of Power Producing Reactors
by
J C Bell, H Bridge, A H Cottrell FRS, G B Greenough, W N Reynolds, J H W Simmons

It is interesting to note that Bell, Bridge and Grennough, formerly of the Windscale Works, had by this time moved elsewhere, Joe Bell had even gone back to Australia. Alan Cottrell, the leading author, as far as I know, never took any interest in how the measurements were made. Not sure why Bill Reynolds name is there.

Two reports listed in the References are as follows:

An Adiabatic Rise Calorimeter for measuring Stored Energy in irradiated Graphite

by R W Henson, J H W Simmons AERE M/R 2564 1959

I first built this apparatus in 1955, but at that time the work was Classified.

A Linear Rise Calorimeter for the measurement of Stored Energy release in irradiated Graphite

by R W Henson, J A Mounsey DEG 328 (W) 1961

This report was also initially Classified. I built this apparatus at Harwell in 1956. It was given to the Physics group at the Windscale Works in April 1957, but not taken until August, just a few weeks before the disastrous fire.

N.B. Both these systems were built by me at Harwell, under the direction of John Simmons.