

1955-57 Harwell - The Wigner Group (continued)

The First Day Back.

On completion of my National Service I was very keen to get back to work at Harwell. I had heard stories of people returning being delayed by what seemed to be over zealous administrators, who treated them like new applicants for a job. I remembered that Alan Perks, when he returned, had been placed in a new position in a different Division! He remained there for several weeks until Tom Fry found out and insisted that he was returned to our group. As far as I was concerned, I had never left Harwell. As soon as I got home, I phoned John Simmons to let him know that I was coming back, and arrived at the gate on the next working day. John had informed the D.A.O. (Divisional Administration Officer) and that was that. I was back!!

John was very pleased to see me, and welcomed me back. We spent the rest of the day in his office, while he brought me up to date with the changes that had occurred during my absence.

The big event was that the newly formed 'United Kingdom Atomic Energy Authority' was taking over responsibility for Harwell and some other establishments. While we were still technically employed by the Ministry of Supply, we would soon be given the choice of staying at Harwell and joining the UKAEA, or remaining with the MOS and having to move to other work at another site. John thought that most would wish to remain at Harwell, and advised me to do so as well. The UKAEA would have almost the same conditions of service as the MOS, but there were some differences. The most significant change was the introduction of a contributory superannuation scheme, for which we would receive an equivalent pay rise. The MOS, of course, had an in built civil service scheme. There were other minor differences, but none that seemed to be a disadvantage to me at that time.

The Wigner Group was still in existence, but the number of staff had been reduced, and John Simmons was the new group leader. There were a few new faces, but many old colleagues had moved to other work. Tom Fry, George Kinchin, Beryl Bird and Alan Perks were still in the Reactor Division, but now otherwise engaged. The group had also moved from Hangar 8 to Building 10.5 which was at the western end of Hangar 10, and adjacent to the BEPO reactor.

John seemed a little sad at what had happened, and it appeared that he had been left with depleted staff to oversee work in which there was a declining interest. However, he was very enthusiastic about my return, and wanted me to continue the work on 'Stored Energy' that I had started in 1953.

I was quite surprised to find that little progress had been made in the field of Stored Energy measurement. It seemed that while I was away, no one had been allocated to do the work, and John had tried to pursue the work himself. As one would expect, after his duties in running the group, he did not have much time left over for detailed innovative work. He did use a 'Vacation Student' during the Summer of 1954 to construct some new apparatus, but the work was so poorly implemented that nothing worked! John had struggled to try and make sense of it, but had really given up all hope.

I had the greatest respect for John Simmons; he was a first rate physicist and full of ideas. However, he was the first to admit that his large hands were a considerable disadvantage when it came to constructing small and delicate apparatus.

John took me to a small lab. to see the new system. I was quite shocked to find that the room was untidy, dirty and covered in dust! He admitted that he had not been in the room for some months, which I could have guessed as it took him a long time to find the key. The vacuum system was familiar, and one that I had used before. This contained the new calorimeter which was surrounded by an untidy mess of tangled wires. The 'electronics' which had been constructed by the 'Vac Student' was an unbelievable jumble, a 'birds nest' of wires! John said that he had tried quite hard to make it all work, but there were wiring errors and he had failed. He suggested that it may be more productive to strip out all the wiring and start again. When I asked for the circuit diagrams for the wiring, John admitted that there were none! The system had been built from rough sketches, and the student had managed to loose them.

Back in John's office we settled to try and formulate a plan of action. The 'Linear Rise' experiment, that I had worked on in 1953, required further development to improve the accuracy. As this required a considerable amount of work, John had decided to shelve this project for the time being, and to construct a simpler apparatus. What emerged was the 'Adiabatic Calorimeter'.

The Adiabatic Rise Calorimeter.

This was complex, but a much easier project to get into a working state. It also had the advantage of being a 'fast' experiment, so that results could be obtained more quickly than by other methods. The principal was very simple;

The graphite sample would be placed inside a copper calorimeter, which would be controlled to be at the same temperature as the sample. An offset would be introduced into the control system to raise the temperature of the sample until Stored Energy started to be released. At this point the offset would be removed, so that the calorimeter would then follow the rise in temperature of the sample until the Stored Energy was all released. The system was to be contained in a vacuum to reduce heat loss, and prevent problems which would be caused by oxidation. The total temperature rise would measure the energy released.

During lunch we talked about my time in the RAF. John was somewhat amused that I had been trained on CHL Radar, and completely amazed that I had used the DU5 Radar display console. CHL was long obsolete, and John had been involved in the design of DU5 in the early days of the Second World War. We both agreed that my National Service had served no useful purpose, and that it had been a foolish waste of time. However, there was just one plus - John thought that the basic training that I had received in Electronics would be a benefit to my work.

During the afternoon we discussed the various parts of the new equipment, and John proceeded to draw the circuit diagrams that we would need. It was all pencil on paper and straight out of his head. I was duly impressed, but that was not unusual when faced with John's above average memory. Of course there were some grey areas which would need attention later, and an almost complete lack of component values. However, John seemed to think that I would be able to sort this out as I built and tested each circuit.

While I was away John had solved two thorny problems which had caused us some trouble on the apparatus that we had built in 1953.

The first was the 'Galvanometer Amplifier' that was necessary to amplify the microvolt level signals from the thermocouples used to measure the temperature. We had previously used a design suggested by Tom Fry which used an optical grating to amplify the galvanometer movement. This was an elegant idea, but had a disappointing performance, and proved to be difficult to use. John stripped out the complex optics and replaced them with a much simpler arrangement. This consisted of a small light source which projected an image of a slit on to a split electrode photocell, via the galvanometer mirror. The output from the photocell was a very small current, and required a high impedance, high gain electronic amplifier to produce a usable output voltage. It was so simple but very functional; It was a great success.

The second problem was the heater element that was necessary to supply heat to the calorimeter body. We had previously used 'Nichrome' tape wound on a cylindrical copper calorimeter. A thin sheet of mica was used to insulate the winding from the copper, and provide good thermal contact. However, when a current is passed through such a winding, the heater tape expands away from the calorimeter, and thermal contact is degraded. In the closed loop control system that we were using, the effect was disastrous to the stability, and effective control was easily lost. We had tried clamping the heater to the copper, but it was not easy to get good thermal contact over the whole area. At very high temperatures, platinum windings were often fixed using an alumina based cement, but this did not provide the necessary heat transfer in the temperature range at which we needed to operate.

John's brilliant idea was to cut radial slots in the calorimeter wall parallel to the cylindrical axis. The heater tape was then pushed into the slot with a thin layer of mica insulation on each side. With this arrangement the thermal transfer is improved as the heater tape expands in the confined space of the slot. This proved to be a very satisfactory solution to this tricky problem. The only disadvantage was the difficulty of construction. It was necessary to split the mica insulation to just the right thickness so that when the tape and the mica was pushed into the slot it was a tight fit. The tape was then 'wound' on to the calorimeter body, up and down the slots, in a zigzag fashion.

This first day back at work proved to be stimulating and quite exhausting. I went home that evening full of wonder and anticipation. I could hardly wait to get back into harness. I did not get much sleep that night!

The Second Day

The next morning I went straight to the lab. I must admit that I was a little dismayed by the untidy mess that confronted me. The room was so dirty that my first thought was to get it cleaned. I managed to find one of the women cleaners and asked her to put it on her list. She told me that the lab. was always locked and had not been cleaned for several months. I promised to sort out the mess and tidy the lab. so that it could be cleaned the next day.

The lab. was quite large and contained what seemed to be an adequate range of furniture. There was also a lot of junk which was unrelated to the work that I was about to pursue. I had to ask John, and he agreed that I could dispose of anything that I did not need for the work on hand.

The first job was to identify the various parts of the new equipment. I cleared one table and collected the essentials in one place. I then sorted the rest of the junk. Anything that I thought may be useful I stored away in the many drawers and cupboards which were available. The rest I dumped outside in the corridor ready for disposal. When I had finished the room looked quite different; It was tidy, but of course it was still very dirty. Only then could I see that it was a very pleasant room.

The desk sat in front of a large window with a splendid view; It was on the second floor and overlooked the lawns in front of Building 220.

Building 10.5 was a little unusual. It was the first building to have central heating using 'Atomic Energy'. Part of the output air flow from the BEPO reactor was passed through a heat exchanger, which heated water for the heating system. I think that this was an afterthought, and not intended in the original design. However, I believe it was a first! Because the reactor worked at a low temperature, it was possible to suck the air out of the reactor shell before blowing it up the 200ft. chimney. The consequence of this was that the air pressure in the reactor vessel was lower than the external pressure, so air leaked into the reactor rather than leaking out! This had the advantage that holes in the reactor shell did not have to be sealed, and open 'beam' holes were permitted. An unfortunate consequence of this was that there was an air flow into the building from the outside. As the door of my lab. led to a corridor which was connected to the Hangar 10 building, there was a stiff breeze through the lab. when the door and a window were open at the same time! In the hot summer months when all the windows were open, anyone opening the door would be faced by a deluge of papers from my desk which would end up in the corridor. The solution to this problem was to use paper weights!!

The Resurrection

Whether it is possible to raise something from the dead that has never been alive, I do not know! However, I was determined to have a good try. After three days the lab. was clean and tidy. I had acquired a few essential small tools, a stock of stationary, and I was ready to start work.

I began to look at the problematic apparatus in some detail. John Simmons was quite right; The construction was diabolical. The wiring was like the web of a demented spider; No order, no organisation and not working! At first I tried to salvage something from the mess, but it was to no avail. What it required was a hatchet job! I took a pair of wire cutters and chopped the wires away until I was left with just the bare bones of the electronic hardware. The calorimeter was in a better state, but the thermocouples were damaged and would have to be replaced. The vacuum system appeared to be all right, but when I started the pump there was the sound of an obvious leak. This was quickly located and proved to be a broken Covar seal where one of the many wires entered the system. The only item that seemed to be in good condition was the 'Galvanometer Amplifier'; The bare bones of the original system were still there, but the new photocell amplifier had been added. This was quite neatly constructed and obviously built by someone else.

The next morning I decided to make a start with the vacuum system. I thoroughly cleaned it, inside and out, and greased the seals. The damaged Covar seal would be difficult to replace so I made a temporary repair using 'Black Wax'. I switched the system on and was pleased to see that it now appeared to be all right. I decided to leave it running for the rest of the day while I tackled the next item on my list.

The Galvanometer Amplifier

The galvanometer amplifier seemed a good place to start on the electronics and I was very keen to see how the performance had been improved. As this was housed in a light tight box, the interior was quite clean. However, I did clean the optics and the photocell. The input wiring was very

messy so I removed this together with the feedback connection from the amplifier output. As I was curious to find out the 'open loop' gain of the system, I connected a voltmeter to the amplifier output and a voltage source to the galvanometer input. It was so sensitive that I had great difficulty in adjusting the output voltage to zero. When the light spot was on the photocell the output would swing from +50 to -50 volts and back again in an almost uncontrollable way! At first I thought that there was a fault, but it proved to be that my input to the system was far too coarse. However, I remembered that I had a 'Potentiometer' which I would use to measure the thermocouple output, and hence the temperature. This device included a microvolt source built in to the same box. Using this and a simple resistive divider, I managed, with some difficulty, to set the output of the photocell amplifier to zero. After taking a few measurements I reached the staggering conclusion that the basic voltage gain of the system was 100 million! When I connected the negative feedback loop from the output to the galvanometer input, the gain was very stable and a more manageable 250,000. Another clever feature added by John was a diode limiter, in the feedback path, which reduced gain outside the central region. Hence an output of ± 25 volts was produced by an input of ± 100 microvolts. Outside this range the gain was reduced so that the full output of ± 50 volts was obtained with an input of ± 4 millivolts. This gave good protection to the sensitive galvanometer.

An interesting demonstration of the stability of the system could be shown by disconnecting the feedback loop, and allowing the light spot from the galvanometer to swing undamped across the photocell. The output would swing violently between ± 50 volts. However, when the feedback connection was made, the light spot would lock on to the photocell and was 'glued' to the split in the two sections.

I soon realised that no provision had been made to set the zero of the amplifier. The omission was soon rectified by the addition of a two way switch in the input circuit. This allowed the galvanometer to be connected to the thermocouple input connector, or to a dummy consisting of two resistors which matched the thermocouple resistance. Using this, the output could be set to zero using the set screw on the galvanometer suspension. This adjustment was rather coarse and was soon improved by the addition of an optical deflector. A 1mm microscope slide was placed in the light beam and could be rotated to provide a fine zero set. The switch and the resistors were carefully chosen to avoid the generation of any stray thermal input voltage.

This was a splendid amplifier, which I used with little modification, on many different experiments over a period of some seven years.

The Electronics

The electronics were housed in a standard 19 inch rack. This consisted of four units. A Power Supply, a twin DC Amplifier and two Power Amplifier modules.

The power supply was just a box containing standard propriety stabilised power supply units. It provided +300, -300, +100 volts DC and 6.3 volts AC for the valve heaters. This unit was all right but I did replace the low voltage wiring to the output connectors, as it was undersized.

The twin DC amplifier unit had to be rebuilt. I had already stripped out the messy wiring which had given so much trouble. Each section consisted of a pentode amplifier driving a 'Cathode Follower' triode. The gain was about 100, but negative feedback reduced this to a gain of just four. An oddity of this unit was the connection from the pentode output to the grid of the cathode follower. A large voltage difference was required here to provide the correct level to drive the

following power amplifier. The voltage divider, which would normally be used in this situation, did not give sufficient swing to drive the following stage. This was replaced by a 150 volt dry battery, which satisfied both requirements. In subsequent years I received much criticism for this unorthodox connection. However, the load on the battery was negligible, and in a low frequency DC circuit it was the perfect solution.

Each power amplifier was just one large 'Cathode Follower', consisting of 12 beam tetrodes in parallel. Feedback was applied from this output to the DC amplifier input, giving a gain of four. That is an input of 25 volts would provide an output of 100 volts to drive the 33 ohm heater on one half of the calorimeter. The two power amplifiers together could therefore provide a maximum of 600 watts to the calorimeter, for an input to the galvanometer amplifier of 100 microvolts. This input was equivalent to a temperature difference of less than 2 degC.

When the wiring was completed, I fitted dummy loads to the outputs, and checked the operation. It seemed to be perfect and all going to plan. However, there was a problem here which was not obvious at that time. It was to give much trouble later!

The Calorimeter

The calorimeter was made of copper, and composed of two identical cylinders about 1.5 inches in diameter and 2 inches long. When placed end to end they are connected by a tapered joint, and form between them a cavity in which the sample to be tested is placed. Each part has a heater winding, consisting of a Nichrome tape with mica insulation, placed in longitudinal slots in the outer cylinder face. The lower part is supported on stainless steel rods fixed to a platform within the vacuum system. The upper part is 'free' so that it could be removed to expose the interior cavity. This posed the first problem - there was no where to put the top section when it was not sitting on the bottom! I added a small pillar to the platform with a mica disc at the top. Here the top could rest, out of harms way, while the sample was placed in the cavity. A small mica handle was also fitted to the top section to avoid touching the calorimeter body. Holes in the end of each section provided pumping access and a means of getting the temperature measuring thermocouples into the sample chamber.

Three thermocouples were required, two on the graphite sample, and one fixed to the inner wall of the sample cavity. These were damaged and needed replacing. The 36 SWG Nichrome and Constantan wires were from the same spools that I was using in 1953, and had calibrated several years before! I stripped out the damaged wires and threaded the new ones into the calorimeter. The insulation used was a very fine twin bore silica tubing. While the heater wires passed through standard Covar glass to metal seals, the thermocouple wires left the vacuum chamber unbroken. Each wire passing through a glass capillary tube and was sealed with black vacuum wax. From here the wires were terminated at the 'Cold Junctions' and connected to copper cables. The cold junctions were placed in an ice bath at 0 degC.

One thermocouple from the graphite sample was connected to a 'Potentiometer' in order to measure the temperature. The other two were placed in series and connected to the galvanometer amplifier input. This was so that the power output to the calorimeter heaters was driven by the difference in temperature between the sample and the calorimeter body. If the polarity was correct the calorimeter would be maintained at the same temperature as the sample. If the polarity was wrong the power output would swing to a maximum and the temperature would surge upwards! I carefully checked the polarity again. The system was now complete and ready for a real test.

I had already prepared a graphite sample which was 3/8 inch in diameter and 1/2 inch long. Into this I drilled two tiny holes to take the very fine thermocouple wires. The sample was placed at the centre of the calorimeter cavity and supported by a mica cradle. The two thermocouples were then inserted into the holes and pinned using the point of an ordinary 'lead' pencil. This formed a minute wedge which firmly held the wires in place, and was a technique that I had used many times before.

With the top placed on the vacuum chamber I switched on the pumps and waited until the internal pressure was reduced to an acceptable level. A pressure of 10^{-4} mm Hg was usually easily obtainable, and this was reached after about 30 minutes. I next switched on the electronics. I had been warned by colleagues of the many perils of a closed loop control system, so I had taken the precaution of fitting a switch on the DC amplifier input. This enabled me to switch on the power with the loop open. The galvanometer amplifier output was close to zero and the output from the power amplifier was exactly zero. When I closed the switch there was a small surge in power output which slowly reduced to zero, and there it remained. At least it was stable and nothing nasty had happened. The output from the GA was now slightly negative which explained why the PA output was zero. This meant that the temperature of the calorimeter was higher than the sample. The next step was to decide how to force a rise in temperature. As a temporary measure I tweaked the zero set control on the GA until the output was positive. This offset fooled the system into believing that the calorimeter was at a lower temperature. The output from the PA increased and settled at a low level. The temperature was now slowly rising and the control loop seemed quite stable. Using the 'Potentiometer' I measured the temperature, and followed the rise until it had reached just over 100 degC. It was still rising slowly and all still very stable. Next I opened the switch at the DC amp input, so braking the control loop. The temperature began to fall very slowly. As the calorimeter temperature fell more rapidly than the sample, the GA output became more positive. When it had reached 50 microvolts I switched the control loop on again. There was a huge surge in the PA output, but after about 10 seconds it had returned to its previous level and the temperature was again rising slowly. It had shown a very classic damped oscillation and was quite stable! This I decided was a very simple way of checking the stability, and I resolved to run similar tests up to the expected maximum of 400 degC. While I was not quite 'there' yet, it now all seemed very possible. I went home that night feeling very pleased with myself.

The next day I spent some time thinking about how the experiment would be organised, and in particular, how to get the calorimeter up to the operating temperature. Fiddling the GA zero was not a good idea. As the GA output was a measure of the temperature difference between sample and calorimeter, this was an error that we may wish to record. An additional input to the DCamp was quite easy to arrange and would provide the required offset. However, the insertion and removal of this offset would produce a transient and disturb the control system. While this was not a problem in a stable system, it was perhaps undesirable. A third way would be to add a small heater in the calorimeter cavity which could be used to heat the sample. The calorimeter would then follow this temperature rise. This seemed to be a good compromise, but it was essential that the heater should have a low thermal capacity, and should have a negligible effect on the heat transfer between the sample and calorimeter. A small coil of fine platinum wire was used, and supported in the cavity by the twin bore silica tube that carried the power leads. It was fortunate that there were spare Covar seals to access this from outside the vacuum chamber. I quickly located a small variable power supply to feed the heater and I was ready to go.

This method was very successful and easy to use. After a short time I was able to set the temperature rise at any rate between 1-10 degC/min. When the heater was switched off, the temperature rise quickly fell to zero, and the control system was not disturbed by any transient. I was very keen to try a real measurement using an irradiated graphite sample. However, common sense prevailed and I spent the rest of the day checking the stability of the control system at various temperatures up to 400 degC. This was done by applying a voltage step to the DC amp input, which produced a severe transient to the PA output. It was all good news! The system was slightly less stable at high temperatures, but the damped oscillation always settled within 20 seconds.

The next morning I went to John Simmons office to report my progress. He was over the moon and quite excited at the prospect of a real measurement. John quickly found some graphite samples which had been irradiated in the Windscale reactor. We then discussed how to organise the measurement, and in particular how to choose the 'starting temperature'. We needed to raise the temperature of the graphite sample to the point at which the release of 'Stored Energy' was just enough to produce a self sustaining rise in temperature in the almost adiabatic enclosure of the calorimeter. At this point the radiant heater would be switched off, and the temperature of the sample would increase until all the energy was released. From the rise in temperature, and the known specific heat of graphite, we could calculate the total energy released. If the starting temperature was too low, the release of energy would be too slow, or not be enough to overcome the heat loss from the sample. If the starting temperature was too high, some energy would be lost before the start! This required a certain amount of judgement, and a knowledge of the irradiation temperature of the sample would be a good guide. John suggested that a starting temperature of about 100 degC would be right for these samples.

My first trial was very tentative. I switched off the heater at 90 degC and the temperature levelled out and became constant. I tried again and switched off at 100 degC. This time there was a very small rise in temperature but it was very slow. I decided to go up to 110 degC, however before it reached 110 it was obvious that the rate of rise was increasing. I immediately switched off the heater, and concentrated on measuring the temperature at 1 minute intervals. The rate of increase took me completely by surprise, and I had a job to keep up with the measurements. In 15 minutes the temperature had risen to over 350 degC! I continued to take readings until the rate of rise decreased and the temperature again became constant. The whole experiment was all over in less than 1 hour. I must admit to being completely overwhelmed by what happened! I think that my pulse rate must have increased during that fantastic rise in temperature. I did not know when, or even if, it would end! When I had recovered I plotted the results on a graph, and was presented with a beautiful 'S' shaped curve. This was marvellous, a magnificent experiment!

I was so excited that I ran along the corridor to John's office, graph in hand. He was so pleased that his face positively beamed. I think that he was highly relieved that his idea had at last born fruit, after languishing over 18 months in the doldrums. John looked at the graph, did a mental calculation and pronounced that the sample had contained about 100 calories per gram.

One thing that did worry me was the magnitude of the stored energy in the sample. If it were much higher than 100 cal/gm the temperature would exceed the operating range of the calorimeter. We had considered 450 degC to be a cautious maximum, and were reluctant at this stage to risk anything higher. Also the thermocouples were only calibrated up to the melting point of zinc, which was 419 degC. We decided that the only course of action was to switch the power off if the temperature exceeded 450 degC.

I was very keen to measure more samples, but John was cautious and suggested that we should first learn more about the system. He then planned a series of tests to measure the thermal parameters of the calorimeter. I spent the next two weeks measuring heating and cooling rates, and the heat transfer between the sample and calorimeter under many different conditions. This was all useful information that would help in the calculation of results, and give an estimate of the errors involved in any particular experiment.

Before I embarked on this long series of tests, John asked me to check one thing that we had overlooked; the temperature difference between the top and bottom sections of the calorimeter. I must admit that I had given this little thought. I just assumed that the two large lumps of copper were in good thermal contact, and that the temperature difference would be small.

The base plate of the vacuum system only had room for one more thermocouple, so I decided to measure the difference directly. I fitted a single constantan wire to each of the copper sections of the calorimeter. These were fixed to the inside surface of the sample cavity. The copper calorimeter would provide the bridge between the constantan wires, and directly give the difference in temperature. This worked quite well, and I was able to make a series of measurements under widely different conditions. When the calorimeter temperature was static, it was easy to adjust the power levels so that there was little difference in temperature. However, if the temperature was moving, up or down, a difference of several degrees could be measured. When I showed these results to John he was very unhappy, and decided that we needed to control this difference. He suggested another galvanometer amplifier, driven by the differential thermocouple, to control the top section of the calorimeter.

I must admit that I was not very pleased at the thought of building another galvanometer amplifier. This would delay and interrupt the flow of work that I had already planned. However, it was not a big problem as John said there were some spare parts which could be made into a new unit. A quick search of the lab. revealed: A Tinsley galvanometer new and boxed, a lamp projector, and a photocell amplifier. The amplifier was without valves and photocell. The valves were easily obtained, but the photocell was not a standard item. I spent some time trying to locate a source, before John found a box of photocells in a cupboard in his office! All we needed now was a base plate, on which the components would be mounted, and a wooden box to contain the completed system. I made a quick sketch of the required box and took this to the carpenter's workshop. I was promised the box within a week and it was on time. In the meantime I had assembled the components onto a steel base plate, and done some preliminary testing. When the whole system was fitted into the box, I was relieved to find that the performance of the new galvanometer amplifier was identical to the first one! I needed to make some small changes because the thermocouple was different, and I was ready to go. This had delayed me only two weeks.

I spent the next few days testing the calorimeter, which was now fitted with the additional control system to keep the top and bottom sections at the same temperature. It all worked perfectly and was very stable. However, we now had two control loops and I had to make some modifications to the switching at the input to the twin DC amplifiers.

Eventually I settled down to the programme of testing that John had planned several weeks before. At first everything seemed to be going well, and I was halfway through the series of tests, when disaster struck! The control system suddenly became erratic and wildly 'out of control' for about 30 seconds. It then just as suddenly returned to normal, and remained all right for the rest of the day. In the next few days this happened several times. All I could do was to try and determine the conditions

under which this instability occurred. I soon got the measure of it, and could reproduce the problem. It happened at specific power levels, but it did not always happen instantly. I believed that the instability required some sort of trigger. When I asked John Simmons for advice, he was quick to suggest that I looked at the power amplifier outputs with an oscilloscope. When I did this, there was little to see, just a slight fuzziness on the trace when the instability was present. It looked like a very high frequency, but the 'scope' was a very simple device and was not able to display the problem. I went to our 'Equipment Store' and borrowed the only high frequency instrument that I could find. It was a sensitive high impedance valve voltmeter. I fitted a loop of copper wire to the probe to act as an aerial. Using this it was easy to show that when the instability occurred, I was picking up a signal on the meter. It was a very strong signal, and was all over the room! Using the aerial, I traced the origin to one of the power amplifiers. John was very quick to realise that the strong radio frequency signal could be picked up by the sensitive thermocouples, and cause trouble with the control system. It only required a very weak 'detection' of the signal to give false readings to the galvanometer amplifier input.

It took me several days before I located the cause of the radio frequency radiation, and just one day to effect a cure. The source of the radiation was the 'cathode follower' in the output stage! A very unlikely source! The anode and the screen, of all 12 valves, were connected together to the positive rail of the DC mains. The unwanted oscillation was occurring between the anode and screen! The valve had an anode top cap, so all the anodes were connected to a common rail running along the top of the array of valves. The screens were also connected to a rail on the under-side of the chassis. The two rails were then both connected to the DC power input. It seemed that the rails formed a classic high frequency 'tuned circuit' and caused an oscillation between the anode and the screen! I did not get an exact measurement of the frequency, but it was somewhere in the region of 500 MHz. The cure was simple; I removed the anode top rail and connected the anode of each valve to its own screen via a damping resistor. This completely cured the problem, and at last I was able to get back to testing the system.

By the time I had finished all the tests it was mid November. It had taken me almost 4 months to reach this point, and I was now ready to start on a real series of measurements. John had accumulated a large backlog of samples which needed to be measured, enough to keep me busy for at least two months. It took me a little while to get into a routine, but I soon acquired the experience to avoid false starts, and managed to fit in two or three samples per day. I must admit that after a while I became quite bored with making the routine measurements, but I did feel that I was doing something worthwhile.

During the Christmas holiday I fell ill and was diagnosed with Bronchitis. When I returned to work at the beginning of the new year, I was still far from well and was hardly able to work. Eventually, after about two weeks, I was sent to a specialist at Battle hospital and was given a chest X-Ray. By this time I was in a terrible state, and I genuinely thought that I had Tuberculosis! The specialist diagnosed Pneumonia. I was sent home and told to go straight to bed and stay there. No one thought to ask me how I got to the hospital! I had arrived on a bicycle, and returned home the same way in the pouring rain!! The next day a nurse arrived to give me an injection, and this happened everyday for a fortnight. I suppose that I was lucky to be given Streptomycin, which was a new drug. I lay in bed for 8 weeks before I was well enough to go back to the specialist. Another chest X-Ray showed that the infection had gone, but I was advised to take it easy for the next 3 months. I celebrated by going back to work!

Bob Henson My Life in a World of Physics

Missing second part of Chapter 5 Please insert between pages 142 and 143

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1955-57 Harwell - The Wigner Group (continued)

Back To Work

After being ill for almost 3 months, I was very weak when I got back to work. However, John Simmons was very sympathetic and allowed me ample time to get back into harness. Although there were some samples still waiting to be measured, John wanted me to think about a new experiment. This was to be an advanced form of the 'Linear Rise Calorimeter' that we had started in 1953.

The Linear Rise Calorimeter

One of the disadvantages of the Adiabatic Calorimeter was that it was not very good at coping with samples which had a slow rate of release of energy. This particularly applied to samples which had been irradiated at high temperatures, and contained less total energy. The Linear Rise method would slowly tease out the energy, as the calorimeter was forced to rise at a constant rate.

The 1953 experiment was very simple, not so much a calorimeter as a method. The graphite sample was hollow, and contained at its centre a small heating element. Power was supplied to this heater so that the temperature of the sample rose at a constant rate. When stored energy was released, the power had to be reduced to keep the rate of rise constant. The experiment was terminated when it was considered that all the stored energy had been released. After cooling a second 'run' was carried out using the same sample, which provided a base line power curve when no stored energy was released. The difference between the two power profiles then directly gave the quantity of energy released. Of course, if the rate of release of stored energy exceeded the 'specific heat' of the graphite sample, the power would be reduced to zero and control would be lost! The details of this experiment have already been described in chapter 3, so no more will be said here. However, there were many problems and sources of error in this very simple apparatus, which would need to be addressed in the design of the new calorimeter.

The first change was to place the calorimeter in a vacuum. This would reduce heat losses and prevent oxidation problems, so allowing a higher temperature limit. The sample was to be placed inside, but isolated from, a copper calorimeter. The temperature of the calorimeter would be controlled to rise at a linear rate, and the sample would be controlled to be at the same temperature as the calorimeter. This would minimise the heat loss from the sample, and improve the accuracy of the experiment.

John had decided that, in order to save time, we would cannibalise the Adiabatic apparatus, and use the same vacuum system and electronics. This was done on the assumption that we could restore the old system if required. With this in mind I set about designing the new calorimeter. The next two weeks were spent at the drawing board, defining the various mechanical components that were

required. This was punctuated by discussions with John, to resolve the many uncertainties that I encountered.

The sample size was increased to give more effective containment of the heater. A cylinder of 0.5" in diameter, 1.8" long was chosen, with an axial hole 0.06" in diameter to contain the heater. The heater was constructed from Nichrome wire wound into a fine four bore silica tube, and used a four lead connection of nickel wires.

The calorimeter was a hollow cylinder 3" in diameter and 3.5" long with two end plates. This was constructed from 0.25" thick copper, and clamped together with copper tie rods. Slots were cut into the surface of the cylinder, and in the end plates, to allow the insertion of a Nichrome tape heater in the same manner as the Adiabatic Calorimeter.

While I was waiting for the new components to be made in the workshops, I went back to finish the series of measurements on the Adiabatic Calorimeter, that had been interrupted by my illness at the beginning of the year. It was necessary to complete this before I dismantled the old system.

It was some three weeks later that I received the new calorimeter. I was just in the middle of inserting the heater winding, when John came into the lab. and declared that he wanted to make some changes to the design. A few spokes into my wheel I thought! Three changes were suggested:

- (1) To use Platinum/Rhodium thermocouples to increase accuracy and stability.
- (2) To use two samples in the calorimeter; one irradiated and one not! This would enable a direct comparison of the power to each sample in one run!
- (3) The sample heater power supply would have to be isolated from earth!!

I was not happy about the first two changes. The Platinum/Rhodium wires were soft and very delicate compared to the Nichrome/Constantan wires that we used in previous experiments. Also the thermoelectric output was reduced, so we would need to increase the gain of the electronics. I did not like this but I was overruled! The second change seemed like a sound idea, but it required more thermocouples and additional electronics for a third control loop.

The third point however was a good one. Any leakage from the heater to the graphite sample would return to earth via the thermocouple lead. This would almost certainly cause instability in the control circuit. This problem had to be addressed, but it would call for a very novel DC amplifier!!

I was a bit dismayed by all these modifications, but it certainly made the project more interesting and a significant challenge.

Once the calorimeter heater winding had been completed, I installed the calorimeter in the vacuum system. The bottom end plate was mounted on three stainless steel pillars to provide a rigid support with a minimum of heat loss. The cylinder and the top were not fixed, but were firmly seated on the bottom plate and held in place by three copper tie rods. The three heater sections were connected in series and driven from one of the DC amplifiers that we had used on the Adiabatic experiment. Four Pt/Rh thermocouples were installed on the inside surface of the calorimeter. Two on the base, one at the centre of the cylinder, and one on the top plate.

The next step was to run a series of tests using only the calorimeter. This was to check the temperature distribution, and to finalise the drive electronics. The first test was only to check that

the final temperature of 400 degC could be attained with the existing DC amplifier. This was reached easily with power to spare.

To effect a Linear Rise in temperature it was necessary to provide a voltage reference that would increase at a constant rate. John had already decided that we would accept a linear rise in voltage, and not try to be too clever and pursue the more difficult path to a linear rise in temperature! There was not much difference, and at least it was constant. The required rate of rise was between 2 and 4 degC/minute, and this would equate to 20 to 40 microvolts per minute when using Pt/Rh thermocouples. A maximum of 4 to 5 millivolts would be necessary to reach 400 to 500 degC.

My first attempt was to use an electronic integrator. As this needed a high input impedance amplifier and a very long 'time constant', it was not easy to obtain the required stability. It did, however, have the advantage of an easily variable linearity. After some initial testing this was abandoned in favour of a much simpler arrangement. A ten turn helical potentiometer was driven by a synchronous clock motor. The power source was a lead acid battery, and the output smoothed and divided down to the required level. This provided a reliable and reproducible linear rise source.

The next test was to get the calorimeter to rise at a linear rate. The linear rise source was placed in series with the thermocouple on the base of the calorimeter, and the output connected, via a galvanometer amplifier, to the DC amplifier driving the heater. The calorimeter, much to my amazement, slowly began to rise in temperature. It was completely stable and required no form of correction. I checked the temperature of the three calorimeter parts and recorded the differences as the temperature rose to 400 degC. There were significant errors due to the differing heat losses from the three components of the calorimeter. This was cured by the addition of shunts to the upper two parts of the heater, so the power was reduced. It took two days of trial and error before I achieved a satisfactory uniform temperature distribution.

Although I had calculated the power required to drive the sample heaters, I considered it prudent to measure this before I embarked on the design of the power amplifiers. I placed one graphite sample at the centre of the calorimeter, mounted on a mica cradle. With the sample heater in position, measurements were taken of the power required to keep the sample temperature the same as the calorimeter, during a controlled linear rise. The results were quite encouraging, requiring less power than I had anticipated. This would make the design of the amplifiers a simpler proposition.

The Sample Power Amplifier

The design of this amplifier proved to be a bit of a puzzle! How was it possible to have a DC amplifier, and yet isolate the output from the input? Of course it was not possible, but with a compromise we could cheat! The essential element was the isolation, for which there were two obvious solutions. The first thought was optical coupling, but this seemed to be complex and would require new and novel technology. Much more straight forward was AC coupling; this could be achieved by either capacitive transfer, or inductively with the use of a transformer. A quick search of the 'Stores Catalogue' identified a high quality audio transformer. This was designed to connect the high voltage output from an audio valve to a low impedance loudspeaker. It was just perfect, and could easily handle the power level that was required. All I needed to do now was to decide on the 'carrier' frequency. It should be high enough to give a fast response, but not so high as to create other problems; a mid frequency of 5 KHz was chosen.

I next made a quick sketch of the circuit required. At the input a pentode DC amplifier was chosen to drive the pentode modulator. The modulated 5KHz carrier was then amplified by a 6V6 audio

output beam tetrode valve. The transformer was the 6V6 anode load, and the low impedance secondary winding reduced the voltage swing by a factor of 20. This output was then rectified by a diode bridge, and passed through a low impedance filter to remove the residual carrier frequency. The output from the transformer primary circuit was rectified, filtered and returned to the input of the DC amplifier as negative feedback, so controlling the overall gain. N.B. This was the compromise, and allowed the isolation of the final output! The 5 KHz carrier frequency was generated by a simple CR phase shift oscillator with a stabilised output level. Note that the same carrier signal could be used on both sample amplifiers.

Both sample power amplifiers were built into the same 19" rack mounting unit, together with the phase shift oscillator and the necessary power supplies. The input stage contained similar switching to the DC amp. that was used to drive the calorimeter power amplifier. Once the carrier frequency amplitude had been stabilised, there were few problems. The system was tested using a dummy load, and the gain set using the feedback loop. The fact that the transformer secondary was not included in the loop did not appear to be a disadvantage. The output was quite stable, and closely followed the input signal.

Just one point that I had overlooked; this was the inter winding capacitance of the transformer. Despite having an earthed electrostatic screen between the primary and secondary windings, there was enough leakage to place a high voltage, at the carrier frequency, on the whole of the output stage! Fortunately this was easily cured by including an 0.1 microfarad capacitor between the secondary winding and earth.

The final testing was delayed while I assembled the third galvanometer amplifier. Fortunately I had the foresight to order the parts before I commenced the construction of the twin amplifier module. However, it was nearly a month later before I had all the equipment ready for a real test.

Being cautious, I decided to make the first test using only one unirradiated graphite sample. I connected the calorimeter control circuit, and started the linear rise source. I measured the temperature using a 'potentiometer', and observed the slow rise. When I connected the sample heater control, there was a small transient in the output before it settled into a steady state. After this it all behaved perfectly, and I allowed the temperature to rise to 400 degC before terminating the test. During the 'run' I measured the temperature every two minutes, and in between each reading I recorded the power level supplied to the sample heater. It was so smooth and uneventful, that I considered it to be quite boring! Quite different from the 'Adiabatic Calorimeter', when the sudden rise in temperature could be dramatic.

This first test did highlight another problem which had to be solved; the measurement of the sample power level was not easy! To do this I had taken readings of the heater current, and the voltage across the heater. With two samples in the calorimeter, it would be necessary to take four readings. As we required the difference in the two power levels, it would be necessary to calculate the difference in the two products! That is $P = (I_a \times E_a) - (I_b \times E_b)$. This did worry me as it seemed like a lot of work to calculate the results.

As a temporary measure I decided to use a chart recorder to plot the voltage and current readings for each sample heater. A Honeywell six channel multipoint recorder was chosen. This also allowed the recording of the two galvanometer amplifier outputs, which registered the difference in temperature between each sample and the calorimeter.

I was about to make a second test using two unirradiated samples, but John was very keen to see a test using an irradiated sample. He quite correctly pointed out that a second 'run', without changing the samples, would be the same as two unirradiated ones. (Provided all the stored energy was released during the first run.)

I prepared an irradiated graphite sample that I knew contained only a modest amount of energy, as I did not want the rate of release to exceed the specific heat of the graphite. When all was ready to start the experiment, I went to find John as he wanted to observe the first 'active' run. He was not in his office so I decided to go ahead without him.

The experiment went very smoothly. I measured the temperature every 2 minutes, and in between kept a very careful eye on all the meters. The chart recorder worked well and traced the voltage and current levels for each sample heater. As the stored energy was released, the power level supplied to the active sample slowly reduced, passed through a minimum, and then gradually returned to a normal level. It plotted an 'inverted' peak which represented the release of energy from the sample. It had worked perfectly! The run was terminated at 450 degC, and the calorimeter allowed to cool. I was very keen to calculate the result, but I needed to complete the second run first. As it was late this had to wait until the next day.

The second run was completely uneventful. Everything went according to plan, and I was more than a little pleased. John was over the moon and could hardly wait for me to do the calculations. Of course, if everything was perfect, the second run should show a power difference of zero! However, in reality, it would expose all the differences and errors in the system. It would depend on the difference in sample mass, asymmetric heat losses, and of course any residual stored energy left in the sample. The last factor could be exposed by a third run. The power difference, if any, from the second run would be applied as a correction to the first run.

The calculation proved to be more work than I had anticipated! Even after I had got myself organised, it still took over a day to produce one result. This required reading four tracks of the chart at 2 minute intervals, calculating the power difference, repeating this for the second run and applying the correction, measuring and allowing for the sample mass, and finally correcting for the non linear rate of rise in temperature. The end result was a graph of the rate of release of stored energy against temperature. The area under the graph would represent the total energy released.

As it was easily possible to measure two samples a day, it was obvious that it would be difficult to keep up with the results. The main problem was the calculation of the power difference, which even with my 'hand wound' Facit calculating machine, still took a long time. A mains powered calculator would help, but it was not the complete answer.

Of course the ideal solution was an 'Electronic Digital Computer'. However, at that time (1956), such facilities were not available. The Harwell Mercury Computer did not come online until 1957-8.

Perhaps the ideal solution would have been an 'Electronic Analogue Computer', which was all the rage at that time. This could have taken the current and voltage from the amplifier outputs, and provided an output proportional to the difference in power. John and I discussed this in some detail, but it would be a complex device and difficult to obtain the required accuracy.

A few days later John came to me with a copy of the 'Journal of Scientific Instruments'. He had found a paper describing a 'Differential Wattmeter'. I cannot now remember why it had been constructed, but it seemed to be just what we required. It was of course a 'one off' instrument made for a particular purpose. It was not commercially available. It was constructed like a suspension

galvanometer, with two sets of current and voltage coils, and a mirror to measure the deflection. It was far too delicate a device for us to consider making. However, it did lead us to think about similar devices. What emerged was a curious device, which was related to an 'Ampere Balance'. Instead of a balance arm, with its mechanical complexity, we would use a pendulum suspension which would be a lot easier to construct. Of course this would in fact be an electromechanical Analogue Computer!

At this time something happened that almost brought to an end the extremely good relationship between John Simmons and me. It was a hiccup in the system which very nearly led to me leave Harwell.

When I returned to Harwell in the summer of 1955, I was very keen to complete my studies which had been so insensitively interrupted by compulsory National Service. I had one year left to finish the course, and worked very hard. It was certainly not easy to do this at the same time as a demanding full time job! In the summer of 1956 I took the examination, and was awarded a Higher National Certificate in Applied Physics. With this I was well qualified for promotion, and the next step up the ladder was to Assistant Experimental Officer, something for which I had studied and waited for 9 years. I applied for promotion, and was given the customary interview which went well. I had absolutely no doubt that I would be successful. However, two months later I had still not been informed of the result! When I asked the Divisional Administration Officer, he told me that the promotion had been granted, but that it would only be effective from January 1st. 1957! He said that he did not know why it had been delayed. I thought that this was an insult! I was well qualified, and quite capable of doing the job! When I approached John Simmons, he was indifferent, and unhelpful. I remembered that he displayed much the same attitude as he did when I was conscripted to do National Service. I was so angry that I stopped the work that I was doing, and started to look for another position. CERN, in Switzerland, was expanding at that time and were looking for people in all grades. I wrote them a letter and was sent an application form.

While I was completing the forms, I had a phone call from Tom Fry asking me to go to his office. I was rather surprised as I had not had any contact with him for about 3 years. He was very polite and sympathetic, and said that he was very sad about the way in which I had been treated. He must have been talking to John Simmons, as he knew the details of what had happened and also was aware of the work that I was doing. Tom said that he did not want me to leave, and that it was important that I finished the Linear Rise apparatus. He promised that he personally would ensure that I received the next promotion within 2 years! I was rather surprised, and relieved to receive such a positive response from someone as senior as Tom Fry. I agreed to stay, but my relationship with John Simmons was never to be the same again.

The Differential Wattmeter

On reflection, this was an extraordinary piece of apparatus, and caused a considerable degree of interest amongst my colleagues. While based on the familiar 'Ampere Balance', it used a pendulum suspension to avoid the problems likely to be encountered with a traditional balance arm.

The main part of the instrument was constructed from red Ebonite, and looked quite beautiful. The fixed coils were mounted on a solid base, which also supported the top plate on rigid pillars. The moving coils were attached to a rod, which was supported from the top plate by four wires, two at each end in a 'Vee' formation. The rod was therefore free to move, pendulum like, only in a horizontal direction. Three sets of coils were used; one at each end for the differential wattmeter, and

one in the centre as a restoring force from which the measurement was derived. The current to the outer moving coils was via the four wire suspension, but the central coil used slack wires also supported from the top plate. At one end of the moving rod, a diaphragm was fixed which contained a slot. This was used to measure the deflexion of the rod, using a lamp projector and photocell amplifier, identical to the system used in the galvanometer amplifiers. Feedback from the amplifier output to the central moving coil was used to oppose the movement of the rod, so providing a restoring force. The amplifier output was therefore proportional to the power force generated by the wattmeter coils, and the movement of the rod was minimised. The whole assembly was mounted on a thick aluminium plate, and covered with a light tight wooden cover. Levelling screws in the base were used to set the zero of the instrument. It proved to be quite easy to construct, and gave little trouble in use.

One very interesting problem emerged during initial testing. When the feedback loop was first connected, the deflexion of the rod oscillated with an amplitude of several millimetres, and the frequency was an unbelievable 10Khz! It was amazing that the rod assembly with its coils could move so fast! I believe that only the diode limiters in the feedback loop saved the assembly from mechanical damage. A very small amount of phase advance in the feedback loop cured this problem instantly.

The testing and calibration of the wattmeter was exhaustive, and took several weeks. At the end of this time I was happy that the accuracy was better than 0.5%, far better than I had anticipated. One slight reservation was in setting the zero. It did need a good firm base on which to mount the instrument.

In use the wattmeter output was recorded using a single channel pen chart recorder. This device was easy to use and never gave any trouble. It only required a zero check at the start and end of each run. Periodic calibration checks showed little change.

It was in the early months of 1957 before I had everything ready, and was able to make useful measurements using the new Linear Rise calorimeter. The calculations now seemed quite manageable, and less of a chore. It was now easily possible for one person to complete one sample per day, including all the calculations.

After only one month of useful operation this new work was brought to an abrupt end by the intervention of the Division Head; Dr. J.V.Dunworth. A policy decision had been taken which transferred all the stored energy work to the Windscale Works in Cumberland! What happened is described in chapter 6.

Reports

The two pieces of apparatus described in this chapter are explained in considerable detail in the following reports:

An Adiabatic Rise Calorimeter for measuring Stored Energy in Irradiated Graphite.

R.W.Henson & J.H.W.Simmons AERE Report M/R 2564 1959

A Linear Rise Calorimeter for the measurement of Stored Energy release rates in Irradiated Graphite.

R.W.Henson & J.A.Mounsey DEG Report 328 (W) 1961

N.B. Both these reports were published long after the work was done in 1955-56

This delay was due to initial sensitivity to the very existence of stored energy in the graphite of nuclear reactors. Following the Windscale Fire in 1957, and the subsequent public enquiry, a more open policy was required.

Technology Update.

Looking back from the 21st century, the reader may wonder why no semi conductor devices were used in the electronics. Although the Transistor was invented in 1947, it was many years before it was a commercial reality. In fact transistors only became commonly available in the mid 1950's. In the period 1955-56 I did look at what was on offer at that time. I was not impressed! Transistors were low voltage, low impedance devices, with little to commend them. It was not until 1958-59 that the situation improved, and I began to use Transistors in some low voltage applications.