

Staff in Charge
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**A REVISED TRANSCRIPT OF THE PROCEEDINGS OF THE BOARD OF ENQUIRY
INTO THE FIRE AT WINDSCALE PILE NO. 1, OCTOBER 1957**

UKAEA

1989

Staff in Confidence
DECLASSIFIED

Foreword to the revised transcript of the proceedings of the Penney Enquiry into the fire at Windscale Pile No. 1, October 1957

The Board of Enquiry met under the chairmanship of Sir William Penney from 17-25th October 1957. Its report was submitted to the Chairman of the United Kingdom Atomic Energy Authority and formed the basis of the Government White Paper submitted to Parliament in November 1957 (Cmnd 302). The report itself was released at the Public Record Office in January 1988.

A transcript and taped recordings of the proceedings of the Board of Enquiry were prepared at the time and have been deposited at the Public Record Office. They will remain under embargo for at least ten years on the grounds that the evidence of witnesses was given to the Board in confidence. Examination suggests that the transcript was prepared from a direct shorthand record of the proceedings, and shows that it differs in some details from the taped recordings. The differences arise from omissions and misinterpretations in the shorthand record, and some gaps in the rather poor quality taped recordings. In 1987 the then Authority Records Officer, Mrs A V Martin, undertook the considerable task of improving the quality of the record by listening to the taped proceedings and amending a copy of the transcript. This revised transcript is derived from Mrs Martin's amendments.

The revised transcript still contains some omissions and possible misinterpretations. It might be further improved if a technically qualified person who is familiar with the background to the Windscale reactor operations were to listen to the tapes (copies of which are held by the present Authority Records Officer) and make final amendments to Mrs Martin's version. At present the effort for such improvement is not available and the revised transcript is being placed on record with a limited distribution.

R G Bellamy

ANMCO 10.23
Harwell Laboratory

18 April 1989

Board of Enquiry

Thursday 17.10.57

Evidence heard from:-

Davey pp 1.1 - 1.2
1.9 - 1.11
1.13 - 1.14

Davey and Ross were also present at the other interviews and interjected on occasion

Ross pp 1.11 - 1.13

Gausden pp 1.2 - 1.8
1.28 - 1.33

Hughes pp 1.8 - 1.9

Tuohy pp 1.14 - 1.17

Bell pp 1.17 - 1.28

Hardy pp 1.33 - 1.38

Wright pp 1.38 - 1.40

Board of Enquiry

17 - 25.10.57

Terms of Reference

CHAIRMAN Our terms of reference are (I have a telecard here) to investigate the cause of the accident at Windscale no.1 pile on 10 October and the measures taken to deal with it and its consequences.

It involves not only the measures taken to deal with the accident in the pile, but with the consequences of the accident. Mr. Ross and Mr. Davey, who were going to run this Inquiry until the thing was changed, have done a great deal of planning on how the thing might proceed and I have heard from them exactly what it was that we are going to do, and I think it gives us a good basis to start with. Their suggestion was that we begin by looking at the various records and the log books and the models, all of which are displayed here, so that we begin to get a feeling of at least what our scientific records have shown us. We take as long on that as we please and then, when we are satisfied that we have got an idea of these matters, we proceed to call some witnesses. The first witness would be Mr. R. Gausden who is an Eng.I in charge of the pile and we would get from him what happened from the time when the pile was shut down. The pile was shut down in order to relieve the Wigner energy and there were several days of that before the accident happened. So we would start with Mr. Gausden to bring us through that period.

Because this operation was one of Wigner release, I think the next witness should be Mr. J. Bell who is the practical man here in the R&D department in the Industrial Group who deals with release problems, and we get from him the background of what was done and why, from the point of view of the Wigner release.

Then the next witness would be Mr. Hughes, the Assistant Works Manager, who was called in very soon after something was seen to be wrong; then Mr. Davey who was brought in, and then Mr. Ross.

Now to listen to the evidence of these gentlemen is probably going to take us all day, and I think that is as far as we need think about at the moment.

ROSS or DAVEY May I just interrupt? I think we have left out Mr. Tuohy, who was in the pile through the night.

CHAIRMAN Oh certainly: Mr. Tuohy would come after Mr. Davey. At present then we start with Gausden, Bell, Hughes, Davey, Tuohy, Ross, and possibly Davey, Gausden

or any of these men as we need them. All of these gentlemen are under notice I believe and are standing by.

Well as far as I see then, that is the way we proceed, but we would be glad of any comments or suggestions.

KAY I can see no other alternative to it. We must take these statements and collect data.

CHAIRMAN Yes. Diamond, do you think this is right.

DIAMOND Yes.

CHAIRMAN Schonland, are you satisfied with that?

SCHONLAND We shall have information about loading of the isotope channels from these witnesses, shall we?

CHAIRMAN Yes.

(SOMEONE INTERJECTS) I think we will be able to cross question them all the way through.

DAVEY We had in mind to go through the sequence of events and in fact after that is complete, we would then go back to the loading of the reactors, the types of cartridges, with exhibits, but that may be tomorrow.

CHAIRMAN You all have papers?

ROSS There is a copy for each but not in the right sequence I'm afraid.

CHAIRMAN I think we should now have the secretaries in.

I think the best way to proceed is to ask Mr. Davey or Mr. Ross to tell us about these records which are in front of us. We shall want the graphs.

SECRETARY Do you want us to take down everything you say?

CHAIRMAN I think that is what we should try to do. Is that going to be possible do you think? We shall have a tape recorder here to help you.

Well, will you take us through this, Davey?

Chairman: Sir William Penney
Members: Professor J. M. Kay
Dr. Schonland
Professor Diamond
Mr. D.E.H. Peirson
In Attendance: Mr. K.B. Ross
Mr. H.G. Davey

In opening the Court the Chairman asked if Mr. Ross or Mr. Davey would give an outline of events.

DAVEY I think the first thing is the Pile was shut down on 7th October to carry out a Wigner Release and to do certain maintenance work. The Wigner Release proceeded and the records in front of you are detailed records of the way the release proceeded. These are essentially temperature charts and I think you will see that a normal release proceeded and in fact a very good one. It will be described as one of the best Wigner Releases that we have had. The charts in themselves will enable you to follow the course of the release and you will see that, as usually happens, there was an occasional temperature rise and the normal procedure was carried out for introducing cooling air to control that rise and to bring the temperature back to approximately 400°. I think that is a fair summary of what those graphs show.

CHAIRMAN The different records are different thermocouples.

DAVEY They are different thermocouples distributed throughout the reactor.

CHAIRMAN There are, is it seven, in all.

DAVEY These are not the complete records. I think you will find that when the release started there were something like 92 thermocouples installed, some in the graphite and some on cartridges.

KAY Have we any other records available of what might be termed a normal or typical Wigner Release on another occasion.

DAVEY These records are not available but could be produced.

ROSS There are pictures of how effective other releases were.

KAY But they do not show actual temperature records?

CHAIRMAN These could be provided later.

DAVEY The other document which is available is a log which gives the salient points from the 7th October right through to the morning of the 11th, and that log I think is fairly comprehensive. It will certainly pick out the important items.

CHAIRMAN The Chairman suggested that the members spend two minutes to read through the paper, and suggested they read this in two parts.

The first part, up to the minute that something seemed to be going wrong, and second, after something was seen to be

going wrong and I think we might start now by going over the first part, and asking Mr. Davey to summarise it.

DAVEY I think I have to be careful at this stage not to give evidence.

CHAIRMAN Yes, just take us through this statement.

DAVEY If you want this very briefly, and it will be quite difficult to do some of this now in the light of what happened, but I will do my best to look at the thing in terms of a Wigner Release with nothing happening, and if you go all the way down to 0540 hrs on the 10th, which is about $\frac{2}{3}$ of the way down the page, then the detailed examination of the evidence of which this is a summary will show that this was a normal Wigner Release. Apart from the satisfactory general rise in temperature throughout the lattice there was the usual difficulty in initiating in some parts of the lattice. That was dealt with in the usual manner by injecting nuclear heat, then taking it off, and letting the temperatures rise. Again, as is typical in a Wigner Release, small pockets go off very successfully and the temperature will start to rise rather steeply towards the 400° mark, then the normal remedial action is first of all to open a few dampers and let a small amount of air go through to bring the temperature down reasonably quickly. It is true to say that a normal release was proceeding with high temperature being shown at one or two points and the normal method carried out for dealing with these hot spots.

KAY On the 8th at 0200 hrs the report reads, "Usually during a Wigner Energy Release temperatures are maintained by the energy being released but on this occasion the release continued slowly, temperatures fell and it was decided to boost the release by more nuclear heating." Is there nothing unusual about this?

DAVEY Nothing unusual. It was perfectly normal.

DIAMOND The Pile is made to diverge to generate nuclear heat. Is there a temperature to which it is normal to go?

DAVEY I cannot give a specific answer to that because the experts might bring out another point during the Enquiry, but the idea is to inject nuclear heat to a point where the temperature starts to rise.

If I may I would say that down to 0450 we were looking at a normal Wigner Release. The next bit takes us from 0540 to 1630 hrs on Thursday. The point there is that there was a high activity reading on the meter in the stack and in the first instance this is not altogether unusual because when shutdown fans are brought on to sweep out the pile, one does normally get some perceptible increase in the reading of the activity on the meter, and what we are saying here is that the position was confused by an incident which occurred on Pile 2. That is, the area survey as distinct from the activity meter, but the confusion arose there because of a possible incident on Pile 2. I think the other thing is that quite quickly after that, the reading on the stack activity meter rose to a point which was quite concerning. From 1630 onwards, again I shall have to be careful not to give evidence, and I think this is a factual statement of what occurred and the action which was taken.

CHAIRMAN You mean up to the end of your record?

DAVEY Yes and, of course, I shall be involved in this.

CHAIRMAN This will be a useful thing to allow the committee to ask their questions when we come to take evidence. We have not seen what these other records are. Are they all temperature records?

DAVEY Yes.

Again, Mr. Chairman, I would suggest that if I attempt to deal with these then I would be attempting to do something which Bell could do much better.

CHAIRMAN The Chairman suggested a recess of five minutes to read papers.

FIRST WITNESS
MR. GAUSDEN

CHAIRMAN For the record would you first start by saying just what your job is here.

GAUSDEN I am Manager of the Pile Group at Windscale looking after both reactors and the pond.

CHAIRMAN We should like you to tell us please the history of this event beginning with the time when the Pile was shut down, the reason why it was shut down and how things developed over the next few days.

GAUSDEN Presumably you would like me first of all to say why the Pile was shut down. I think I ought to refer to the report which was issued by R. & D.B. JGR TNW 586. This discussed the effect of radiation and annealing cycles on the stored energy in the Windscale graphite and was discussed at the recent technical committee, where one of the recommendations was that the Piles could very well go to 50,000 cumulative Megawatt days before an attempt was made to release the Wigner Energy. This was generally accepted by the Technical Committee but I did ask for a half-way step on this and suggested that as the previous period had been 30,000 we ought to take this a little steadily and instead of going straight to 50,000 go to 40,000. This was agreed and we now come to the reasons for shutting down Pile 1. Pile 1 had reached just over the 40,000 cum M'Days and was, therefore, ready for a Wigner Release.

(Programme was in front of members) I prepared a Pile Shut Down Programme, No. 79, laying out the sequence of events during the shut down. This laid down that the Pile be shut down at 2200 on the Sunday evening, the 6th, and would be completely shut down by about 1.15 on the Monday morning. After sampling the charge hoist and installing the proportional counter the next job was to check all the graphite thermocouples that we use for Wigner Release and reload those which were unserviceable. This went on, in fact, until about 10 o'clock instead of 8.30 as programmed, and after this was completed R. & D. people went on to the charge hoist and withdrew certain graphite specimens which they wished to look at before annealing had taken place.

KAY Have you any idea where these specimens were?

GAUSDEN I have not got a record here. Mr. Bell could give you the exact position.

We also discharged one isotope channel, which was not required during the Wigner Release.

KAY You took the opportunity of removing that?

GAUSDEN That was just discharged straight out the back because the temperatures reached were considered undesirable for the specimens in this isotope channel.

CHAIRMAN You are telling us what the programme was and what was actually done at the same time.

GAUSDEN The times are slightly different but the sequence is the same. We also disconnected the control rods from their drive. I ought to explain that there are four banks of control rods interspersed with two sets of fine rods and these go in two vertical planes in the side of the reactor. The object when one starts a release is to get as much heat as possible into the bottom so the control rod drive is disconnected in such a manner that the top rods are left in and you start the reactor up by running the bottom rods only so that you get heat generated at the bottom which can then, one hopes, spread to the top.

CHAIRMAN This is perfectly standard practice?

GAUSDEN This is absolutely standard practice, yes.

We then started to raise Pile power to 100 KW and Mr. Robertson, who has done quite a large number of releases, was in charge at this time. He continued to raise Pile power to about 1.8 MW so that he got a maximum cartridge temperature of 250°. This is a limit which has always been set that we should not exceed in the first instance.

KAY Is it true you work this entirely on temperature measurements?

GAUSDEN Yes.

CHAIRMAN When you say a maximum temperature of 250° does that mean on any thermocouple?

GAUSDEN On any uranium thermocouple.

CHAIRMAN You don't go above 250 on the first development of power in the pile.

GAUSDEN That is so.

We have a total of 19 uranium thermocouples which were operating at that time, some on the roof, some down in the control room. Any one of these – the first that gets to 250. There is a blank period between about 4.05 in the afternoon until about 8 o'clock, because this is the time taken up in running up the control rods before one actually gets the pile critical. Take up time while the rods are slowly run out.

The Pile diverged at 7.30 in the evening of Monday, 7th.

The power level was gradually raised after 1.8 MW up to about 0100 on the Tuesday evening, one hour after midnight. By that time we had got our maximum uranium temperature

250° in two places, 2557 and 2757. The graphite temperatures generally were somewhere between 50 and 80° with one particular exception which was 2148 at 10 feet depth into the pile which had got up to 210 and was showing the characteristic signs of a release at that period.

CHAIRMAN The two thermocouples which went to 250°, where were they?

GAUSDEN The 2557 would be about here (indicating the place on a chart) and 2757 here, which is exactly where we would hope to get first heat.

CHAIRMAN Just below the middle; about a third of the way down from the middle?

GAUSDEN Roughly three-quarters of the way down from the top.

CHAIRMAN We've got it on that plane now - as far as through the pile. Where would that be horizontally?

GAUSDEN That would be about 15 feet from the charge face. The thermocouple is in the very beginning of the 14th cartridge, so, such cartridge is a foot long which makes 13 feet plus an odd inch, then there is a graphite space and hook boat at the beginning, making a total of 15 feet or just over.

CHAIRMAN What fraction is that? What is the total?

GAUSDEN The total through is 25 feet.

CHAIRMAN 15 feet from the face and the total distance through is 25 feet.

GAUSDEN From 1 o'clock to 4 o'clock on Tuesday morning we were running the rods back in and closing the reactor down because we had sufficient heating to start off the release and during this period the first good releases occurred in 2655 and 2661. 2655 was at 110° when the release started and finished up at 250° in about 3 hours, and 2661 was about 20° and finished up at 260°, again in about 3 hours.

CHAIRMAN Is this perfectly normal?

GAUSDEN I don't really think one can say what is a normal Wigner Release. Some go very sharply, some crawl slowly. This was virtually an intermediate one.

CHAIRMAN There was nothing in it to make you worry at all?

GAUSDEN Nothing at all. This was quite standard.

The Pile was shut down by 4 o'clock. We continued then with thermal heating with the graphite; the Wigner Releases spreading heat to further points. At 1105 on Tuesday morning the general tendency was for the graphite temperature to be dropping rather than rising, and it was obvious at that stage that unless we put in more nuclear heating the release would stop. The Pile was again run up with the bottom rods and more nuclear heating put in to raise the maximum uranium temperature which was 300° at that time to 330°.

KAY What is the normal limit when you're bringing it back? You mentioned earlier a figure of 250° to initiate the W release; now after that what are your temperature limitations?

GAUSDEN The criterion is that the maximum operating temperature under normal conditions is 395°, but we endeavour to keep as far below that as we possibly can with the W release still going on.

KAY These are central metal temperatures you are quoting?

GAUSDEN The thermocouple is inserted in the end of the cartridge so it is really the central metal temperature.

CHAIRMAN It is true the temperature of the U would be pretty well the same over the whole diameter at this rate of heat?

GAUSDEN Yes.

CHAIRMAN So you then made the Pile diverge at 11 o'clock on Tuesday morning to get a maximum temperature of 330° in the uranium elements in the middle of the pile, wherever the maximum was.

GAUSDEN Yes. The graphite temperature rose in sympathy from a maximum of 310 to 330 again in 2148.

At 2055 that evening the graphite temperature in 2053 had been creeping up slowly. It has shown signs (as you can see from these graphs) of a perfectly normal release but it had then, instead of staying static or falling away had slowly risen, showing that energy was still being released.

(The members referred to graphs in detail) If we start at 1600 you will see that going back down the graph there had been quite a marked release which steadied off or began to steady off up until about 11 o'clock on Tuesday and then began to rise again but this again is normal because at this time other points were going and putting up more heat, so one would expect a rise of this sort, but between 1400 and 1600 on the Wednesday, this took a slight turn upwards, the rate of temperature increase was rising and by 2055 it was decided that we would have to exercise some control on this temperature and the first step was to replace the covers over the inspection holes behind the scanner gear on the top of the reactor. This was to stop any air coming in that particular point and any air that was getting into the reactor would have to go through and up the chimney. This would provide the first measure of cooling.

KAY Is this a normal trimming operation?

GAUSDEN Yes, this is the first step one would take. This did nothing at all which could be seen on the temperature charts so the next step was to close the hatch at the base of the chimney which had been open so that any draught there was must now go straight through and up, and you also had the additional chimney lift to help you. At 2215 four shut down fan dampers were opened which gave a positive air flow through the reactor. This allowed air to come from the Blower Houses right through and up the stack. You will see that opening the 4 shut down dampers arrested the temperature rise and flattened it right off. This was for a

space of about two hours up to roughly midnight and then it took a turn up again roughly at the same rate as before. Again at 1 minute past Midnight the dampers were opened for ten minutes, but did not materially influence the temperature, so at 2.15 they were again opened for a longer period of 13 minutes and this did affect the temperature and it dropped, but within a matter of an hour the temperature again turned round and was beginning to rise again. At 5.10 the dampers were opened for 30 minutes and this reduced the temperature again and at this point it was noticed later that there had been a small increase in the stack activity. I think I ought to point out that the stack activity is not one of the instruments, during this operation, that we normally look at. We log the readings hourly but with the Pile closed down one would not normally expect to get stack activity.

KAY When was that actually noticed?

GAUSDEN It was pointed out to the operator on the roof approximately one hour later (at the next normal logging), but it was also pointed out that when one does start a flow of air going through a reactor, (this has been noticed particularly say starting up a blower which has been out of commission which has accumulated a fair amount of dust) that one gets this increase in the stack activity, and at that time no further notice was taken. It was not reported to me.

CHAIRMAN Where is the instrument that records this stack activity? Is it in the stack itself?

GAUSDEN There are three Ion Chambers looking at the underside of the filters and they are connected to this electronic equipment which transmits a signal back to the control room and it is recorded on a tape recorder in the control room.

DIAMOND Was this increase sufficiently different from normal fluctuation of activity to cause comment?

GAUSDEN It was barely different. Looking back one might say it was possibly rather more than one might have expected, but at the time it was not thought so. At 12.10 the dampers were again opened for 15 minutes and shortly after this there was a marked increase in stack activity. **I was informed of this immediately**, and I gave instructions to open the dampers, put on shut down fans and blow the Pile cold.

CHAIRMAN In the log book or whatever I have here, it says that at 5.40 it was noticed that the Pile stack activity had increased and at 12 o'clock noon on the Thursday it introduces Pile No. 2 as being suspect.

GAUSDEN I would like to correct that time. The time should be 1350 instead of 1200.

CHAIRMAN Perhaps you could explain why you thought it was Pile No. 2 if your instruments were actually in the chimney of Pile No. 1.

GAUSDEN You will see that the note says that a high activity reading on the Met Station roof was reported.

Well, the night before (Wednesday) two points of the stack activity in Pile 2 had gone sufficiently high to begin to wonder whether one should not close the reactor down suspecting

that one had a cartridge that was burst in the back portion of the Pile somewhere and putting out oxide. The first step was to check the instrumentation because there was still one point which was reading low and the normal characteristic is for all three to go up, but in this case two went up and one stayed down. I asked for the instrumentation to be checked on Pile 2 stack activity and it was in fact found that two of the leads were not in good condition as they might have been. They were repaired and the two high readings then came down to the level of the third reading which I considered to be normal. That was the story of Pile 2 for Wednesday night. If I can jump a couple of steps here and go to 1350, the time when Health Physics reported they had a high reading at the Met Station, I was slightly in a quandary then as to whether Pile 2 might not be the culprit after all, because Pile 2 was running and Pile 1 was shut down, but coupled with that, of course, we had the high stack activity on Stack 1 by now and it was quite obvious that it was not Stack 2.

KAY What readings were taken between 0540 when I assume the first hourly measurement of the No. 1 pile stack activity was recorded, how the readings went from then up till 1350 when you had this temporary diversion of the suspicion that it was Pile 2 and the realisation that it was Pile 1 had you continued to take these Pile stack activities on Pile 1 during the period.

GAUSDEN Yes they were logged at hourly intervals.

KAY Are these readings available?

GAUSDEN They are shown on the graph.

CHAIRMAN It jumped up a little bit at 540 and the following hourly records stayed about constant until it jumped up badly.

GAUSDEN That is so. As soon as we opened up the dampers and put on the shut down fans. At the second time, midday, the stack activity went very high as you can see from the graph and it was quite obvious that whatever was happening was happening in Pile 1.

KAY May I ask when was your attention first drawn to the Pile stack activity rise? It seems to have gone off the scale shortly after 12 o'clock.

GAUSDEN My attention was drawn to it at about 1.30 midday.

CHAIRMAN How long was that after the observation was noticed?

GAUSDEN I would point out at this stage that the operator is operating from the roof and he was watching very closely this 2053 graphite temperature and trying to ensure that he kept that under control; and at the same time he was watching 2153 uranium temperature which was adjacent to this and noted that that reading was still round about the 350 mark. However, when we put on the shut down fans, we expected two things to happen. We expected first of all the uranium temperature in 2153 to rise (because you are extracting heat from the graphite and passing it to the uranium) which it did and it steadied off at about the 400 mark. We would then have

expected it to fall away as the graphite temperatures went down, which they all did. They began to fall and one would have expected the uranium temperatures to do the same. 2153 steadied off and then began to rise quite sharply as you can see from the graph (no. 80), and in fact in the space of one hour rose from 340 to 400°. Then the rate of rise decreased somewhat over the next few hours up to 420 and then began to rise again quite rapidly at about the rate it had done just before the previous two hour period. By this time it was quite obvious that there was something very wrong in that reading.

KAY Would it be correct to say that the operator working on the basis of temperatures would not be concerned at the rise of activity. This was not a normal consideration at that time but only a secondary one?

GAUSDEN That is so. At 2.30 in the afternoon I asked for the turbo exhausters to be switched on, because although (2153 thermocouple) was the position we thought was suspect we were still not absolutely sure because there was quite a large area not covered by couples at that particular point. My intention was to do a scan with the scanner gear to locate this. The alternative was to take out anything up to 800 odd plugs to see if we could see anything. This was out of the question, so the quickest way was to do a quick scan with the scanner gear and try to locate the channel that way. We found that the scanner gear was in fact solid and could not be moved. This probably was due to the temperature generated by the release and in fact we have found this before, that at a normal Wigner Release, we cannot move the scanner gear at the end of the release. However, I had a report that the previous day the Maintenance Section had worked on the scanner gear and had moved it and I was hopeful at that time that we would be able to do a scan but it was proved impossible. I notified Mr. Hughes at 1440 down in Admin that we had some trouble and I think that he in turn informed the W.G.M. at 3.15. Entry was made on to the charge hoist at about 1600. There are certain preliminaries one has to go through: for instance an air count has to be taken to make sure conditions are alright on the charge hoist and the men themselves have to have a full change. I did not do so at that time. I went straight on. When we got on to the charge hoist there was a slight delay in locating the channel we wanted because of wrong labelling of the thermocouple, but this is only a matter of a few minutes and I don't really think has any bearing although I think I ought to mention it. When I disconnected 2153 thermocouple I was told by the roof that

this was the right channel and immediately had the plug pulled out, and looking up the channel the metal itself was glowing: not quite bright red at that period. The graphite was either black or a very very dull red which may in fact have been a reflection from the metal.

CHAIRMAN What did you actually see - was it a circle with a halo round it?

GAUSDEN Yes.

CHAIRMAN Like a sun? The rod is not red hot at the outside?

GAUSDEN At the charge face you have got your hook boat, the metal hook which stops the cartridges blowing through the reactor under normal operating conditions and then you have a graphite spacer before you come to any metal. My impression is that it was about the 2nd or 3rd cartridge, although vision is very limited up the channel. The first cartridge appeared to be reasonable. It seemed to be the one behind it, or possibly the one behind that.

CHAIRMAN But somewhere in the channel there was a fuel element glowing.

GAUSDEN Well in all 4 channels.

CHAIRMAN Nothing like 15 feet back?

GAUSDEN You cannot see 15 feet back because the charge plug is about 9" diameter which in fact feeds 4 channels diagonally up and down so you are looking at an angle and you can only positively see the first four or five positions. After that you are guided by reflection. The isotope channel in this one was quite black, understandably so probably because it was active.

DIAMOND Was this the one that had been discharged or had never had anything in it?

GAUSDEN Nothing in it for some time.

In spite of the fact that these elements were glowing we attempted to discharge them and failed. They would not go out. We then took out further plugs along the same level to see whether it was confined to that particular group of channels. It did in fact spread further and I took out a total of 6 plugs altogether looking down a total of 24 channels, that is from 2153 to 2158. These were all glowing.

KAY Can I ask about the attempt to push out the channels? This was using a normal technique?

GAUSDEN Yes.

KAY Were you able to shift them at all?

GAUSDEN We were unable to move them at all.

KAY In a normal discharge you put the fresh fuel elements in at the same time? Do you push the?

GAUSDEN We discharge first and then reload.

KAY You were using the normal discharging method?

GAUSDEN Yes. With a tube going up to the particular channel and then feeding up the discharge gear.

CHAIRMAN You pulled out 6 plugs and looked at the 4 channels each time. Were those 6 horizontal?

GAUSDEN Yes, on a horizontal line. Row 21 from 53 to 58.

CHAIRMAN When the 6th one came out and it was still glowing, did you pull out any more?

GAUSDEN Yes, one more and found that it wasn't. So at least we confirmed the horizontal spread on that particular channel. We defined the beginning and end.

KAY You were confined to a horizontal exploration?

GAUSDEN At that stage we then moved the charge hoist one level up to level 20 and confirmed that the identical channels above were the same and then we moved two levels below to 21, 22 and 23 and confirmed the same thing. At various intervals we did attempt to discharge channels and failed. I was in contact with Mr. Hughes at that time and the decision was taken that as we could not discharge this metal we would make a fire-break around the area; and a complete ring of uranium was then discharged right round the whole area, apart from level 26 which we could not discharge because it was immediately opposite the scanner gear and the scanner gear could not be moved.

CHAIRMAN You say to make a fire break. Could you explain this?

GAUSDEN At that stage it was the metal that was glowing; it was not the graphite, so we assumed at that stage that if we took out metal further out around, at least it would not spread further.

CHAIRMAN You had no thought in your mind why it was red hot? You were dealing with a Wigner release and would have thought the energy was coming from the graphite.

GAUSDEN Exactly.

CHAIRMAN And yet here was a situation in which it was the metal that was glowing.

GAUSDEN That is perfectly true, although I must admit that at the time that we had found this and were trying to deal with it was hardly the time to think what was happening.

SCHONLAND This area that you protected by a fire break, is actually the area that was affected by the incident – the flames, and everything else, refer to this area?

GAUSDEN There were no flames at that time.

SCHONLAND But there were later on.

GAUSDEN Yes.

SCHONLAND Did the fire break confine the area?

GAUSDEN Undoubtedly it did confine it, there is no doubt that initially it confined it but there was a tendency afterwards when the heat became more intense for it to jump across the gap upwards. One would probably expect this.

DIAMOND When you tried to discharge these rows which were not opposite Row 25, the force at your disposal was not sufficient to shift it.

GAUSDEN We had two sets of gear there: we had our normal discharge gear and we also had very heavy equipment which we use from time to time to jam the channel. This is solid 2" piping with a very heavy bob on the end and there was an 8 man team on, really thumping away.

DIAMOND This is the brute force method referred to in the report?

GAUSDEN That is the method which was used later on to in fact get rid of some, but we felt at that stage that it was not the right thing to do to spend too much time on an individual channel when we knew there was a fairly large area concerned.

DAVEY For the last 20 minutes responsibility has passed from Mr. Gausden and he was just carrying out instructions.

GAUSDEN I had reached the point where Mr. Tuohy took over.

CHAIRMAN Would you make it quite clear what was the last thing you did whilst you were in charge.

GAUSDEN I was busy taking out the first ring from around the affected area at the time Mr. Tuohy actually came in.

CHAIRMAN Then Mr. Tuohy came in and he picked it up from you?

GAUSDEN Actually from Mr. Hughes, because I was still on the charge hoist. Mr. Hughes had gone to see Mr. Tuohy.

PEIRSON What channels constituted the fire break?

GAUSDEN No. 1, shown on the plan.

DIAMOND If we could go back a little. On the stack activity graph there is a rise in activity up to 6 o'clock then a fall down to about a minimum at about 8 o'clock and then a rise again. This is the activity which had been deposited on the filter and has been recorded by chambers up there? Is this fall away the natural decay of the fission products?

GAUSDEN I find that difficult to answer. This is a characteristic decay of fresh fission products. It is not necessarily a characteristic decay of products after the reactor has been closed down for 3 days or so.

DIAMOND The fluctuation in air supply is unlikely to affect this activity, I take it?

GAUSDEN I would not have thought so. Once it is in the stack filters you would not expect to see any change from air supply.

SCHONLAND Will we have an opportunity of asking Mr. Gausden questions later?

CHAIRMAN I think we must and I think we should proceed by going through the officers that were in charge at the moment and then go back as we think necessary.

? Mr. Chairman. I am completely at sea over isotope channels and what was in them and why one was discharged and so on, but that can be taken later if you wish.

GAUSDEN I can answer the particular one that was discharged: this had some steel specimens in which did not require to go up to a temperature that would normally be obtained in a Wigner Release.

CHAIRMAN Was it anywhere in this zone or not?

GAUSDEN At 2360 - Just outside the zone, in the fire break.

KAY May I ask whether in this zone in which the fire occurred there were any other isotope channels loaded?

GAUSDEN Yes. There was a reasonable quantity of A.M. and one or two odd channels such as thulium and thallium.

? Are there documents here giving details of which channels had isotopes?

CHAIRMAN I think it would be better if we took the principal events as the officer in charge saw them then come back to specific points and focus attention on them. Isotope channels and A.M. may be important but should be dealt with later, so let us first begin by running through the operational events which occurred.

PEIRSON Does each group of 4 channels have an isotope channel as well?

GAUSDEN Yes. There are 4 metal channels with an isotope channel in the centre of the group of four.

KAY We would like to have a full account of all the isotopes later on.

CHAIRMAN Certainly.

SCHONLAND If we are keeping to the operations I would like to turn back to a remark at 0200 on 8th - there is the necessity for boosting the release. Is this usual or unusual?

GAUSDEN You cannot lay down a set pattern for Wigner Release. It is really one big experiment each time we do it. We have found on a number of occasions in the past that graphite temperatures tend to fall and that the release can be started up again by putting in some nuclear heating.

SCHONLAND Has there not been a change in your procedure as the years have gone on and you have found that you have had to boost the release process more and more?

GAUSDEN If we refer to the release which occurred in March 55 you will see that it was a perfectly good release. It left two very minor pockets right on the extreme edges. In November 55 which is after another cumulative 300 M.W. days another release was done and you will see that there was a very small release in the bottom half of the pile where one would normally expect to see it start and spread and in fact the top half went and left the majority of the lower half unannealed. Within three months of doing that it was decided that a further attempt should be made to release the energy in the bottom half. You will see that the bottom quarter went still leaving the bottom right hand quarter. It was felt that yet another attempt should be made to get this out and it was again attempted in April with no success at all, no anneal at all.

In November 56, which was again 30,000 after April, we had a good release and if one takes the pockets left in February, the right-hand bottom quarter had got up to 40,000 and in fact as it did not go in November 55 it had got up to about 50,000. It is becoming more and more difficult to release energy from these piles.

CHAIRMAN In the same amount of M.W. days?

GAUSDEN Yes, in fact at 30,000 now, it is almost certain that you will get no release or only a partial release.

SCHONLAND But the picture for November 56 looks nice.

GAUSDEN Yes, well of course it had a very good start because it had the bottom right-hand corner to give it a real boost.

KAY But this was the first occasion when you had left it for the new period of 40,000 M.W. days.

GAUSDEN Yes, over the whole pile.

KAY You had a pretty good one in November?

GAUSDEN It was a very good one, and temperatures went quite high.

KAY The graphite should have been in a fairly uniform state and this was a question of leaving it for 40,000 M.W. days.

GAUSDEN Undoubtedly.

CHAIRMAN November 56 was the last occasion on Pile 1 before the present.

SCHONLAND It will probably save further going back on this point with regard to November 56, the South Eastern portion was 50,000 M.W. days.

GAUSDEN Yes, because it went right back to March 55.

SCHONLAND And the South Western round about 40,000 was it not.

GAUSDEN The release in February would be 40,000, yes.

SCHONLAND And the upper portion would you say 30 to 40,000.

GAUSDEN It varied a bit, yes.

SCHONLAND Three at the bottom here - at the 50,000 mark.

GAUSDEN Three spots you mean? Well they did not go at all.

SCHONLAND So that by the time we come to the present annealing they would have had how much?

GAUSDEN It is all a question, you see, although one says there was no release if the graphite temperature is up above normal temperature there is **some** release, one doesn't know how much.

SCHONLAND I am not asking that. But between November 56 and October 57 how many M.W. days were there?

GAUSDEN Another 40,000.

SCHONLAND So this might have been quite high if it hadn't been released?

GAUSDEN Yes, although, of course, the channels are right on the outside, so

DAVEY To question Gausden on this - he was only carrying out a ? and Bell should talk on this.

CHAIRMAN Unless the Committee have any other questions, Mr. Gausden can be released.

KAY Looking at a chart here, am I correct in saying that in November 56 the graphite temperature had come up for the first time to over 400° centigrade; to that extent there was a precedent and at that point it did not cause undue concern.

GAUSDEN Yes.

KAY But previously it was below on all occasions?

GAUSDEN Yes.

DIAMOND Can you confirm that with Wigner releases previously you have experience of diverging more than once, and this is not the first time this has happened.

GAUSDEN Not by any means.

CHAIRMAN Thank you for the moment, I am sure we will want you again.

Break for Coffee

CHAIRMAN Would you please begin by saying what your job is?

HUGHES At the moment I am Assistant Works Manager in charge of the Chemical Plant, but also at the moment acting as Mr. Tuohy's Deputy in charge of the whole of the processing area at Windscale.

CHAIRMAN We have heard from Mr. Gausden what was done to the pile from the Monday until this incident began to assume its dangerous appearance and he has told us how he removed the metal fuel elements around the area which seemed to contain the glowing region and we would like to hear from you the operational events that you were concerned with from about that period. If you wish to have an overlap period with Mr. Gausden please do so, but tell us in your own words.

HUGHES Some time just after 2 o'clock on Thursday afternoon was the first intimation I had that anything was wrong at the pile. A phone call from Mr. Gausden: he thought there was a burst and he was preparing to cool down the pile in order to scan for this burst. This was about 2 o'clock. Shortly after that, Mr. Howells, Manager, Health Physics, came in to report to me a high particulate picked up at the Met. Station: this was a routine test that was carried on. We discussed this and agreed it was advisable to take a sample from the cyclone below the filter on pile 1. I contacted Chemical Services Department and asked them to set about taking a sample. Probably a quarter of an hour later Mr. Howells returned saying an air count had been taken in this area. The count had lasted from about 11 o'clock to 2 o'clock and this showed a reading of 3,000 d.p.m. total, somewhere in the region of $\frac{1}{2}$ m.p.l. Shortly after Mr. Gausden rang again to say that things were now quite abnormal and he was worried about the condition in the pile. I immediately took Mr. Howells up to the pile and we went first to the control room, had a look at the stack activity recorder, which at this time was probably between 200 to 300 curies. We then went to the pile roof and I asked that the staff take the temperatures manually of all the thermocouples on a routine basis. I then reported back to the pile control room and contacted Mr. Davey.

SCHONLAND These thermocouple temperatures had been taken for days: what was the change that you made?

HUGHES This was to do it on a more rapid turn round basis. These are manual points.

At this particular time there was evidence that one particular metal temperature was increasing gradually. It was something just over 400 degrees at this point.

CHAIRMAN Which one?

HUGHES This particular one is 21.53.

In order to get some idea of the rate of increase of activity on the stack, we asked that the range of the instrument be changed as it had just gone off scale at 300 curies at this point. It was then decided that the best procedure was to go to the charge hoist to locate channel 21.53 and if possible discharge it, and preparations were made to do this. About quarter to four, I re-contacted Mr. Davey and gave him the position as it

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was then. As far as detailed times are concerned for the next hour I am not very certain, but we did attempt to discharge the glowing channel at the pile face, but this was unsuccessful so we then decided that the best course of action would be to remove the fire break zone around the whole area.

This was after the extent of the zone had been defined to some extent by taking out plugs down and across the face.

Having seen the glowing channels it struck me that a possible approach might be to put argon on to the channels and at something in the region of 5 o'clock I contacted the Works Engineer and I asked him to get all available supplies of argon up to the reactor. We continued recording the temperature at the pile roof and continued the discharge of channels in the fire break. At this time I again contacted Mr. Davey and returned back to Administration for more detailed discussion on this.

Broadly speaking, that is the position as long as I was the senior person in charge. Mr. Tuohy arrived at this time and at round about 6.30 both of us returned back to the reactor to generally supervise the discharge of metal in the fire break zone.

CHAIRMAN Are there any questions?

DIAMOND You mentioned high particulate activity. Does the 'high' refer to the particles or just the activity?

HUGHES This particular quantity of activity was taken over a measured area and it was high by reference to what one normally expects in this area. I think the total count was in the region of 100,000. This one was slightly peculiar in that the particulate was somewhat smaller than had sometimes been obtained at other periods.

DIAMOND This activity was smaller than had sometimes previously been obtained.

HUGHES An analysis had been done of this and it has been proved to be older material, probably over a year old. As far as the high air count was concerned a gamma measurement has been done on this and from what information you can get from a gamma spectrograph it appeared to be something like 40% iodine.

CHAIRMAN The idea of making this fire break - what was in the minds of people? You were confronted with a dangerous situation and it must have been worrying to know what to do.

DAVEY I was in charge when it was pushed out. I really think I ought to give evidence at this point and allow Mr. Hughes to go.

CHAIRMAN I did not realise that you were there, in that case we will ask Mr. Davey.

Are there any more questions?

DIAMOND Did the argon arrive alright?

HUGHES Yes, the argon arrived later on, yes. We did not put it in because the quantity available was not large and on more detailed consideration it was thought it was too small to cope with the

CHAIRMAN So that we can be quite clear, what was the time of the first attempt to push out any of the cartridges?

HUGHES Between four and half past, in that region.

CHAIRMAN Well, Mr. Hughes, I think that is all for the moment, we may want you again, but not immediately. Thank you, Mr. Hughes.

DIAMOND It would be helpful to work in 24 hours of the clock.

CHAIRMAN Days of the week and 24 hours of the clock.

CHAIRMAN Well, we'll receive your evidence Mr. Davey. No need to ask who you are, or whether you know us.

DAVEY I shall have to apologise first of all. I have not had the opportunity to make personal notes, but just a few headings. I won't always be clear about the times and I shall have difficulty now in translating the 24 hour clock.

After 15.45 Hughes phoned me and the exact message was that there was a bad burst on pile 1 and I said right then you get it out immediately.

At 16.30 he phoned again and said he was quite concerned because the stack activity meter was showing of the order of 300 curies. This made it sound to me by far and away the worst burst we have ever had and I think I should say that Mr. Ross and I went up to the pile at this point and having got to the roof we had a look at the temperature recordings and certainly the rate of temperature rise on 21.53 was quite abnormal and instructions were given that this channel must be got out.

I then came back down here having asked him to let me know how he got on and I would think within the hour he phoned to say that they had taken a plug out and there were at least four channels glowing.

From this point onwards it was not easy, in fact it was extremely difficult, to make any clear-cut decisions, but having been told that at least four channels were glowing, bearing in mind the activity already recorded on the stack activity meter, I was convinced immediately that we could not bring the main blowers on because my immediate concern was particulate activity on site and in the surrounding area, and from our past experience if the filters are presented with a substantial mass flow of air then particulate does get through, and trying to visualise four channels in this condition there was not the slightest doubt that we were talking about exposed material and I did feel and I made the decision that we could not bring the big blowers on in order to cool the pile.

The next point was to try to contain this area and at the same time discharge. I should say at this time all effort was concentrated on containing the area by pushing out enough metal to establish an annulus and to consider means of bringing to bear additional mechanical effort in order to push out the channels in the affected zone.

I should also say that at this time with a picture of four channels glowing the engineers were asked immediately to manufacture graphite plugs to put into the entrance to these channels because some of the conflicting things were, I recognised immediately, that the air from the shut-down fans

were supplying air to the glowing channels, but there was the question of keeping the rest of the pile cooled. At that point I felt that if we could blank off these four channels with graphite plugs the situation could be held.

Instruction to make 4 plugs hadn't been given beyond 15 minutes when the next information was that something like 100 channels were involved and the manufacturing of plugs in a reasonable time was no longer a practical proposition. The emphasis was still on getting this metal out at all costs. We had another discussion about shut-down fans, but you will realise that not only did the shut-down fans supply the air to the pile, but via a booster they also maintain a pressure differential between the charge hoist and the pile itself and the feeling was the air was necessary to make the position tenable on the charge hoist in terms of personnel.

So first of all we had this most unsatisfactory decision to make that air would be kept on from the shut-down fans, partly in the name of cooling the remainder of the pile, and partly in the name of making or keeping the position tenable on the charge hoist.

While that was going on I mentioned the use of water as an extreme measure and some men from R. & D. were asked to investigate two points. One was the possibility of explosion when water met the hot metal, because my concern was that an explosion might blow out the filters. They were also asked to discuss the criticality aspect, although I must say that our feeling was that, as the graphite lattice sits up on a concrete slab with adequate drainage both to the water duct and to the air duct in front, bearing in mind that the lattice is not a solid structure but is well and truly perforated with spaces, the feeling was that there would be no hold up of water, but we asked that a check be made that should we have to use it we would not be involved in a criticality hazard.

Going back one step, to prove again that all emphasis was on discharge, although I did not know this zone in detail I knew it contained A.M. in addition to uranium. Therefore again it was emphasised that we must devise mechanical means of discharge. Throughout this time the emphasis was on discharging this metal if we could conceive of any means of doing it and very substantial effort went into that.

Then about that time someone mentioned that there was a road tanker of CO₂ sitting at Calder and I think rather forlornly we discussed the possibility of CO₂ quenching this fire. As a precautionary measure we did ask that it be checked that from a nuclear point of view it would be alright to displace some of the nitrogen by CO₂, although I had in the back of my mind that this calculation had been done some time ago, but that answer came pretty smartly that there was no hazard as a result of displacing nitrogen by CO₂. So, while the effort of discharging continued, pipes were rigged up for feeding CO₂ to the charge face and through a header presenting CO₂ to the zone which was on fire.

In the meantime we had had another discussion and the general feeling was that if we could bring enough water to bear we would quench this, although there was still the risk that there might be a bang and I do not have to tell you that my mind was all the time on the filters because I had known for hours that the filters stood between us and disaster.

Now I come to the sad part of my story because by 12.15 a.m. I could not sit up any longer.

CHAIRMAN That is 00.15 on Friday morning.

DAVEY And Mr. Ross took over, but I must make it quite clear that it was my decision that if we failed to discharge metal at a reasonable rate we would take the risk of putting water on.

CHAIRMAN Although you were not going to be there because you were sick, Mr. Ross would do that.

DAVEY Yes, it was my decision.

CHAIRMAN Your decision, yes we understand. So, if I could perhaps go back to 15.45 Thursday, that was when you first heard about it. At that time it seemed just like a bad burst slug. At that moment you had no suspicion that the temperature was going to heat up.

At 16.30 you had the evidence that the activity in the stack had gone up and that the temperature of one thermocouple was showing an alarming rise. You went over to the roof and you said we have just got to get that channel out.

DAVEY I knew then it was an abnormally bad burst.

CHAIRMAN At that time you had already conflicting wishes. One was that because of the temperature going up you would like to put on the main fans, but you were afraid to do that because you thought there was a burst and you could then possibly pump up a lot of stuff up and through the filters. You made the decision not to use the fans, but you had had the shut-down fans going in order to let men work on the pile face.

DAVEY In fairness to myself, I must say that was the consensus of opinion.

CHAIRMAN And then matters gradually got worse and you were thinking what was the last thing you could do if it really got to the limit and that was water. And the R. & D. people were thinking about explosive hazards and criticality. Criticality was not your main headache. The explosion risk of the hydrogen in the aluminium was your main risk because it could have blown up, and although you could not continue because of a bad back, you made the decision that water should be used copiously and left Mr. Ross to carry that out if it should arise.

DAVEY If it should arise.

CHAIRMAN And you yourself had to go home at 00.15 on Friday morning.

DAVEY I was back at 08.30.

CHAIRMAN Do the members of the Committee wish to ask questions on this statement.

SCHONLAND You have never had an incident of this kind before?

DAVEY No.

SCHONLAND Otherwise, you would have had graphite plugs available?

DAVEY Yes.

A fair answer is that presented with a situation of about four channels this was the first obvious thought: to blank off these channels and, as I said, the engineers were asked immediately to start turning graphite into plugs. We gave it up because, I think within an hour, I was told more than 100 channels were involved and it is of course not a simple circle. It has a stepped bottom to take the graphite boat and to be effective this had to be a reasonably neat job. An alternative was to turn graphite with a substantial flange which would blank off the face. That was feasible for four channels; I think it was not feasible for over 100.

SCHONLAND You spoke of 100. This shows about 36?

PEIRSON It is 36×4 .

SCHONLAND Four in each group.

DAVEY I am quite sure I received two messages. One concerning four channels and the second a calculated figure of more than a hundred.

I think I know what Dr. Schonland has in mind and certainly we have never had anything like this before but we have had experience of uranium oxidising at a fairly rapid rate. This is not in the pile itself, but cartridges lying in the back end, and a combination of a substantial source of uranium oxide and full flow of air through this pile does give us particulate through the filters; but this was my first thought and when you are told that four channels are glowing, meaning that beyond any doubt that you have exposed metal oxidising, I think one's first thought must be the surrounding area, and I am still convinced that if these main blowers had gone on we would have created a very substantial hazard in the surrounding area.

SCHONLAND The log talks about the flames, yellow flames and blue flames. You were there at the time; did you have the impression that the graphite was burning at that stage.

DAVEY No, that was quite some time later. I think reference to the graphite itself burning would have been perhaps about 23 hours.

SCHONLAND These flames you would have attributed to what? - to the uranium or some material other than uranium, some metal?

DAVEY I would certainly have attributed the flames to material burning in the channel as distinct from the graphite. I did not see the flames therefore I cannot draw any conclusion from the colour of these flames.

KAY The flames were seen at the rear face of the pile?

SCHONLAND Someone will be able to tell us about the flames.

CHAIRMAN Prof. Diamond, do you wish to ask any questions?

DIAMOND Just one, sir. The facilities for determining critical size, this is a service that is routine and laid on? It is the responsibility of R. & D.?

DAVEY Yes. May I say that quite early on we were not concerned with the critical size of the assembly because although it was not possible to discharge the hot zones, substantial quantities of metal were going out in the of the annulus and in the discussion a critical condition was a possibility with water and they were asked to look into this.

Another thing I suppose I should say: I also knew, although I did not know the details, that there was enriched material in this zone, but all this is only saying again that the emphasis was on discharging it. I think it is fair to say that we were not concerned with criticality in the reactor apart from a possible hold up of water should we decide to use it.

CHAIRMAN Can you give us any idea of what thoughts you had which led you to attach first priority to pushing out to get a fire break. You had these red hot metal elements and you decided that the thing to do was to push them out and if not to ring them round. Why did you decided to do that?

DAVEY I think that probably the cardinal point at the back of my mind was that we must not get particulate out into the area. Providing there was not a mass flow of air through the system I believed that the filters would work to a very considerable extent.

This is based on work which had been done previously and at that stage I felt we had time in which to contain the fire and to push this stuff out, but I must admit I had no conception of the extent to which fusion or something had taken place and that it would prove to be impossible to get a good deal of it out. I can only repeat that, at that stage there was a feeling that the filters would hold the position. Temperatures were running so high that I was afraid there would be transmission from the affected zone to the surrounding zone. So the fire break seemed a reasonable step to take because I knew that metal would go out quickly. In fact that was true. There was no difficulty at all in discharging the surrounding zones and they went out without much consumption of time.

CHAIRMAN Are there any more questions?

SCHONLAND There was a fire in two channels in the Marcoule G.1 fire. Were you acquainted with the history of that fire?

DAVEY I was not acquainted with the history, but I had got round to reading notes submitted by Fry about three weeks previously, but speaking from memory what I read was that

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they took no positive action in effect and these channels were allowed to burn out and I did not get very much joy from that.

CHAIRMAN Thank you Mr. Davey. Perhaps you might stand down now and we might call you back again later, and we might now ask Mr. Ross to come and give us his evidence.

ROSS I would like to say first of all that I cannot dissociate myself from the decision to put water on this pile. When I took over from Mr. Davey at 12.15, the temperature on 21.53 was creeping up all the time, and about 01.30 I actually established a post in Mr. Davey's office with two R. & D. blokes, Bell and Moore, to take notes, and these are actually the notes they took at the time. This I did rather than going up to the pile because Health Physics people were down in this building too and we were keeping track of the district survey all the time as well as the pile.

Mr. Tuohy and Mr. Hughes were up in the pile together. Mr. Hughes we sent home early in the morning because I felt I had to have men for the following day if it was going to be necessary.

Temperatures kept on going up. Meanwhile we were fixing the water. About one o'clock I felt that the position had got so serious that I must do something about a possible emergency. I rang up the Chief Constable. I also felt that, in spite of Mr. Davey being extremely tired, I must inform him that this step of warning the Chief Constable that there might be an emergency in Mr. Davey's area and I rang up and told him I was taking this action and asked if he was happy about this.

PEIRSON This log is wrong; it says 11.55, it would be 01.55.

ROSS It was after Mr. Davey went.

Howells was keeping a continuous check on the district survey and at 01.53 it was decided to tell everyone to stay indoors and don face masks.

On 21.53 the temperature had got up to 1300. We were then measuring with an optical pyrometer and the graphite in 20.53 was 1000° at 1.25. While we all felt we would have to put on water eventually, the main scheme was to push out as much red hot metal as we could and Howells was engaged in getting more and more pushers. We got to one stage where we thought that we might have to pinch scaffolding from Calder to keep up the supply of pushers.

At 02.30 (I am giving these figures because I think they are quite important from the point of view of the measures we took) the district survey showed that there was no need to wear face masks any longer and we gave instructions for them to be taken off. As I said, Mr. Tuohy was up in the pile but owing to the methods of communication we had to go through a second telephone. There was a telephone from the Pile Administration Building and the charge hoist where he was and there was a telephone from here to the Admin. building, so it was all a matter of passing on double information all the time through a third person.

At 02.35 I told Gausden to tell Tuohy that we still considered that we would have to put on water, but he must ask for special permission before he did so. Then we had records of temperatures which still kept on going on. Tuohy was meanwhile still pushing stuff down.

At 03.45 Tuohy came to the office here to tell me how things were going and we then knew that the water would be ready in 15 minutes whenever we wanted it and the CO₂ in three-quarters of an hour. At 04.19 they were running out of steel poles to push out channels — get scaffolding from Calder. At this time we found that the skip train was full and we had to take a decision to push the stuff into the duct. Two R. & D. people did quick criticality calculations and we felt that this was a safe thing to do. Tuohy was still at 06.30 pushing out as many channels as possible. This was with the idea that the more red hot metal that we could get into the pond the better, and the less red hot metal we would have to quench with water. At 07.43 Tuohy had to take out another row before using water. Then just at that time we warned everyone to keep inside as I did not know what would happen when we put water on. With Tuohy pushing out and one thing and another it came to the time when day people were coming in, so we had a lot of people about and we decided to delay putting water on until the day people were in.

This brought us to 08.45 when Mr. Davey came in and naturally I handed over to him again.

At all times our district survey was within m.p.l. except for when we asked people to put on gas masks and that was only in particular buildings and at one time on site. These figures Howells will have.

In addition to ringing up the Chief Constable, at quarter to three our Security Man, Mr. Barnard, turned up at the Works and informed me that the Chief Constable of Whitehaven was at the gate and would like to have a talk to me or at least a Chief Superintendent, and he told me that he had been alerted by the Chief Constable and he had all his policemen awake and dressed and was ready for instructions. I told him at present there was no need for panic, but I hoped I could give him good notice before anything happened. He also told me that part of our drill, which he had agreed with Mr. Davey, was that we should supply him with a certain number of gas masks. I checked with Howells that these were ready and available and I see by the Minutes that he set up a post in the Police Lodge. I did not actually know that at the time. We had the district fully alerted in case anything happened, and I think this completes my story until Mr. Davey came in and we actually opened up the water.

CHAIRMAN You started, Mr. Ross, by telling us when you took over at 00.15, but of course you had known things were going wrong well before then. Could you please tell us when you first knew about it and where you were?

ROSS I had been in Windscale for a meeting that day and I was actually sitting talking to Mr. Davey when the message came from Mr. Hughes to say there was trouble on the pile and I naturally wanted to see what the trouble was, and when I got there and realised what it was I decided that I could not leave the area. Although Mr. Davey was in charge I could not possibly leave the area until we were happier about things. I think perhaps I have not given you the picture: this temperature going up to 1300 and eventually to 1500°; with the R. & D. people there, the main concern was the possibility of a secondary high temperature Wigner release. The picture they painted of what might happen was quite terrifying. The feeling was that the whole lot of graphite

might rise in temperature by 1000° all in one, and this was a thing we dared not let happen from the point of view of district containment. I felt the same as Mr. Davey. We just did not dare to let the whole lot go up and scatter the full contents of the reactor over the area, and this made me feel quite certain that our decision to put on water was right. We could not do anything else; the alternative to putting on water seemed such a terrifying alternative – if it came off as our best information suggested it would.

CHAIRMAN You said that the R. & D. people told you about this risk of the sudden release of high temperature. Was that at the time, or was that over the last years?

ROSS One always knew about it but Moore and Bell whom I had with me as advisers and writing down logs, and they were very insistent. At first they said if it went over 1200, "God Help Us", and at 1200 nothing happened and one felt that you did not know what would happen at 1500 and when it kept creeping up it was quite frightening, just not to know what might happen.

CHAIRMAN When was the water actually poured on?

ROSS At 08.45 immediately after Davey came in.

CHAIRMAN We will go on to that in a moment then.

Are there any questions for Mr. Ross? Perhaps you would now sit over there Mr. Ross and Mr. Davey can now continue his story from when he came in in the morning.

DAVEY The decision to put on water had been made; the method of introducing the water had been discussed before I went home and after I went home, and at about 08.45 all personnel were cleared from the charge hoist well. The shut-down fans were kept on at that stage and fire hoses were introduced several channels up from the affected zone. These channels had directly been discharged and the hoses went in there and having got all personnel out, an initial flow, (this is only an estimate), of about 300 gallons a minute was introduced; within 10 to 15 minutes nothing untoward had happened and quite quickly this was stepped up to about 700 gallons a minute, and within a very short time after that I would think that the flow was 1000 gallons a minute. The shut-down fans were shut off and the reactor was left in that condition. There was quite a perceptible drop in temperature immediately after this action was taken and the zone generally started to cool off.

I shall have to digress a moment because of course the health physics results were of considerable interest at this time, and dealing with the surrounding area first, all the results that were coming in showed that we were still far short of the emergency level. To illustrate that, we were working to an agreed danger figure of 10^{-4} curies m^2 and the results coming in were 10^{-6} and 10^{-7} ; there was the odd 10^{-5} , but generally, both in terms of the surface surveys and air counts in the district, we seemed to be of the order of 100 to 1000 times down on the danger level.

CHAIRMAN Can you tell me where these measurements were coming from geographically?

DAVEY Calderbridge, Drigg, etc.; but all that has been plotted and is available.

CHAIRMAN We should like to look at that in detail at a future time.

DAVEY I would say that at 9 o'clock that morning in my opinion, and it is supported by the figures, we were not in a state of emergency as far as the district was concerned.

It was a morning of very little wind and, although there will be conflicting evidence on this, as far as I am concerned there is no doubt that at ground level and for several hundred feet above ground level the wind was blowing out to sea. Every stack seen here was going seawards and that was the general impression. I have said that there was little wind and after the shut-down fans had gone off the small plume which had existed on the stack declined and we started to get air counts on certain portions of the site. For short times these were high, but never generally on the site. I can illustrate by two examples. There was a period, I think, about 10.30 a.m. when air counts in the region of the Primary Separation Plant got pretty high, the figure is available, but the position for example here and in the main canteen was perfectly satisfactory. So what we did was withdraw all men from the Chemical Group and send them to sit in the canteen.

At about 11.15 the little wind there was had swung and counts were rising on the Calder Site. The general instruction there at this time was that people should stay indoors where they would be quite safe, but there was of course the problem of the construction force, inevitably working out in the open. At about 11.15 or it could be 11 o'clock (equal 23 hours) I asked the Resident Engineer at Calder to send his construction people home, and in the region of 23 hours to 23.30 I told the Works Secretary to ring the Chief Constable of Cumberland and Westmorland to tell him that an emergency would not arise because we were having fluctuating air counts on the site. They in themselves were not particularly serious and there was no general count over the site; also the information coming in from the district was that they were still of the order of 100 to 1000 times down on the danger level. Well then by the early afternoon of Friday.

PEIRSON I am sorry, you talk about 23 hours.

DAVEY I have got the clock wrong again. I beg your pardon, I meant 11 o'clock in the morning. That was when we sent the men to the canteen.

PEIRSON The air count was high at the Primary Separation Plant, that would be at 10.30 in the morning?

DAVEY I am sorry about that.

CHAIRMAN That's alright.

DAVEY Then by early afternoon only one zone was causing concern and if I may look at that diagram of the face of the pile – sorry there were two zones causing some concern in the early afternoon of Friday; the top right-hand corner and the bottom left-hand corner, and there was a cartridge thermocouple in the channel in the top right-hand corner.

SCHONLAND Something like 20.58.

DAVEY Something like 17.60 and you can get this accurately later, something like 28.51. And if I may use those for the purpose of illustration the 28.50 would be graphite temperature and 17.61 would be a cartridge temperature, and the cartridge temperature in 17.61 was still of the order of 390° and in fact showed a tendency to rise, not rapidly but steadily, and in the late afternoon of Friday we brought two hoses to bear not on that channel, but on an empty channel above it and another one I think to the left of it; and that temperature started to fall away. The water was kept on the whole of Friday evening and then through the night in diminishing quantities and we turned the water off about mid-day on Saturday.

CHAIRMAN One of the points I wasn't quite clear about. You told us how you started at 300 gallons a minute, worked up to 700, and then to 1000. How long did you keep it going?

DAVEY If you allow for varying quantity then water was going continuously from 8.45 until about noon on Saturday, starting with about 300 gallons rising quite quickly to 1000. I would say 1000 was maintained until late Friday night and then gradually diminished throughout the night.

CHAIRMAN Yes, I see, thank you.

DAVEY I think Mr. Chairman that that concludes the incident.

CHAIRMAN Any questions?

? Is this 17.61 – you said this was probably the number, but I think it is actually the number because on Sheet 8 there is a
(Interruption)

There is a fuel measuring point on this chart labelled 17.61, but the nearest graphite point on this chart appears to be 25.50, in the opposite corner.

DAVEY That could be right. Fortuitously I seem to have got one right and the other wrong.

CHAIRMAN And 25.50 is plotted somewhere? – Yes it is – on Sheet 5.

(Few words lost) verbatim in one of the papers, as having described how the pile was attacked from the top in order to get water into the system – that is just nonsense?

DAVEY Yes, there was a good bit of verbatim which I didn't even say.

Perhaps I ought to explain that if you have this picture of the lattice being roughly a cylinder 50' diameter, 25' long then all the holes in the top are vertical holes going straight down through the lattice and the only way to achieve any sort of distribution is to introduce the water through the horizontal channels. That is why no water was put in from the top. It is as simple as that. It would run straight through and out again.

CHAIRMAN It would not have done anything good.

DAVEY One did have a picture of this lattice as an enormous distribution if we could put water into it horizontally, then by its very structure, you have graphite bricks and interspacers and really this is the same reason why we did not worry unduly about this high temperature down in the bottom left corner because water was bound to percolate in that direction.

CHAIRMAN This ? now into the water duct and some into the air duct.

KAY Was there any noticeable reaction on supplying water on ?

DIAMOND No. No sign of any hydrogen. We took all the men off.

DAVEY Everybody was taken out.

At that time the only open point in the reactor was the top of the chimney that is 450' without any exaggeration allowing for air temperature but I think the affected point of discharge is another 100' on that and from there we went to sea. It would be a 600' discharge point at least.

CHAIRMAN Thank you Mr. Davey.

TUOHY I am Deputy Works General Manager. My specific responsibilities before the incident (they have altered a bit since) were to be responsible for the operation of Calder Hall and Chapelcross and to act as Deputy to the Works General Manager in his absence, but in view of the incident I was called in to help out on the Windscale Works.

CHAIRMAN Yes thank you. But start by telling us when you were first informed of something going wrong and what you did, and pay particular attention to whether you were in charge of something.

TUOHY I was at home on this particular day. My wife and two children – all three of them had Asian 'Flu – but I got a 'phone message from Mr. Davey at about 1700 hrs, asking me if I could possibly manage to go into the Works and I got into the Works in the Pile Group by about 5.30 in the afternoon.

CHAIRMAN We are trying to work on the 24 hour clock.

TUOHY At 17.00 hours on the Thursday afternoon I got a phone call and I got in at 17.30 hours Thursday. I went straight to the Pile Group, had a few words with the Pile Manager and went on to the charge hoist to have a look for myself, and I saw what everyone else had seen: that part of the pile below which we were discharging was a mass of flames. I then came down to Headquarters here for consultation with Mr. Davey and Mr. Ross. The decision at this time had already been taken to put a fire break right around the affected area of some 120 channels and after a consultation at this end where we discussed the possibility of shutting off the air, the possibility of using CO₂, Argon etc. I went back up again to the Pile Group. Having got up there I did an inspection from the Pile roof and looking down the discharge face through the top inspection holes. At this stage I would like to refer to these notes to get the times right.

About 19.00 hours there was an obvious red glow looking down one inspection hole, the east inner inspection hole. There are four of these distributed along the top of the reactor giving a view of the discharge faces. At this particular period it was no more than a glow. There was no evidence of flames and looking down the other three inspection holes there was no sign of fire at all at this stage.

CHAIRMAN Looking down this inspection hole you are only looking at graphite.

TUOHY But you can see the graphite right at the bottom, you cannot see it from the top because the inspection holes are set back from the graphite face itself, but as far as the affected part was concerned you could not see any glowing graphite because that was out of view, but you could see the glow from the fire.

KAY This is clear from Fig. 2.

TUOHY At about 20.00 hours (8 p.m.) for the first time I personally saw flames at the back. At this time it was yellow flames. Shortly after that I was asked to come down again to Control for a further consultation, and I was down at Control then until, I would say roughly, about 22.00 hours, when I asked permission to pop home for half an hour to see that the invalids were alright. I got back around 22.30 or 22.45 hours. I did another inspection up in the Pile Group to have a look at the discharge face (a little vague about time here - possibly about 11.30) and by this time there was evidence of blue flames at the back and also you could see signs of the fire not only on the east inner but now on the west inner inspection hole and also there was a glow on the east outer. But at this inspection there was, as yet, no signs of fire on the west outer, but on the other three there were definite signs, showing that activity was increasing inside the reactor. I came back down to the Control point again after that and this would be about midnight and in consultation with Mr. Ross we decided that we would get the fire brigade to stand by. So I called out the fire brigade officer and asked him if he would get up to the Pile Group with all available pumps he had, to stand by for action. Now about this time we were also in some difficulty about the amount of material that we had got in the skip train at the back of the reactor. It will only take a limited load, and the load is particularly limited when we are discharging 1.28 Co material and we felt that we were already up to the stipulated amount and perhaps a little bit over, so to give us more space in the trucks we decided to partially pull the skip train. We were discharging off-centre a bit in the reactor and therefore the skips which were to the west end of the reactor would still not have many cartridges in them and the one right at the west end probably none at all. We did this partial pulling of the skip train round about 24.00 hours to give us this extra space and then we carried on discharging.

I asked Mr. Ross if I could go up and take charge on the spot and spend the rest of the night up there, to which he agreed. I went on to the charge hoist at about 00.30 hours and I arranged for a top and bottom scan of the situation. In other words, we went up to the top and down to bottom of the fire and to both sides to do a scan to see if we were still containing it within the initial region. At this stage it looked as though we were certainly alright at the bottom, as there seemed to be no indications at all of any spread downwards, but there were

indications that it might be spreading upwards. It was difficult to be certain about this because flames would tend to shoot upwards anyway with the chimney draught and looking down the channels from the charge face you could not be just too certain whether the channels were alight above the fire or not, but you could see down the channels and you could see flickering flames at the back. But because the bottom seemed to be not extending at this stage, I decided not to complete the second isolating ring which we were now on with. We had got the whole of the top down on the second ring and both sides, but in view of this top and bottom scan I had decided that the right thing to do was to take out an additional row at the top. Meantime we had made decisions down at Headquarters that we would also utilise a tank wagon of carbon dioxide which had recently come in to Calder. That was brought over here, and the Works Engineer was asked to fit up equipment to be able to pipe the CO₂ on to the charge face. From about midnight onwards this work on the CO₂ was going on. We were also, between midnight and 01.00 hours, actively engaged in devising a method for getting water on to the fire should we make this decision. This was not very easy, because there were no water connections available and we had to devise something on the spur of the moment. We arrived at the method for getting water to the seat of the fire, or into the reactor, but one thing that was worrying us was how to hold the hoses in position when the water came on, because they might kick back when the pressure came on. But we eventually devised a simple method of using equipment which we have on the charge hoist called charge trolleys and we pushed a pipe, a standard isotope loading tube, through an isotope loading plug. This is a hollow plug which will take a 2" tube. We would attach the hoses to those and retain them in position by pushing these charge trolleys against the plug. So we had a plan, not only for getting the water there, but for holding the hoses in position. Now, having completed the second isolation ring apart from the bottom, but with an additional row on the top and continuing to do these top and bottom scans to ensure that there was no spread of the fire taking place, we decided at about 01.00 hours that we would try to push out some of the burning channels. An attempt had been made on the previous afternoon to do this and they had just jammed solid. We got a team of about six men as opposed to our normal team of about four, and using 2" steel tubes screwing additional pieces into the end to get the length, we tried then by brute force to move the burning channels. For the next few hours we kept on at this carrying out these top and bottom scans all the time to satisfy ourselves that, in fact, the fire was not spreading, and we did manage to get out the top burning row completely and then to keep as big a distance as possible between the fire and the parts not affected. I decided then to go on to the bottom row and try to work along the bottom row. This was going on well into the morning, but by about 3.30 hours I came down to Mr. Ross to report progress, and reported at this time that we had got water available if we wanted to use it at about 15 minutes' notice, and we could get CO₂ on at about 45 minutes' notice. At this stage we decided to try the CO₂ though no-one was optimistic of the effect of CO₂. We were quite honestly frightened of the water because we didn't know whether there would be an explosion or not if we started to pour water into the reactor at this stage, so we tried the CO₂. That was on for about an hour I suppose and it seemed to be completely ineffective. The

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only way I felt that one could assess the effectiveness of anything we were doing was by carrying out repeated inspections from the roof. This is apart from doing our top and bottom scans which were going on regularly into the night but to know whether anything was dying out, or getting worse, the only real way you could do it was by this viewing down through the roof. By this time in the morning, about 04.00 hours, looking down from the roof you could see flames, blue flames now, from both of the inner inspection holes shooting right across and impinging upon the thermal shield at the back. At both outer inspection holes there was a blue glow, not a red glow, at this stage. We thought now that some of the graphite must be burning and this was probably carbon monoxide burning in the fire. We did attempt to see what was going on at the front by using a television camera but this was completely abortive. If we put it sufficiently near the fire we felt that we might wreck the camera without getting any useful result and trying to put it some way from the fire, and looking sideways we could not see anything that was worth looking at. During this time we were pushing out the fire itself and using up steel rods at quite an alarming rate. Some of the channels which were difficult to push out meant that with six men heaving on the end you might move about 6" at each push. The rods were in the fire for quite a long time and occasionally when we got them out they were bent and absolutely red hot. Molten uranium was dripping from some of them on to the charge hoist and all we could do was to stamp out these small fires. Occasionally we pulled back a red hot graphite boat with our rods and these were just kicked to one side, picked up with a gloved hand and dropped down over the side of the charge hoist and down into the well. On two occasions we pulled back a cartridge which didn't in fact fall on to the floor - we spotted it in time, and using an ordinary bamboo push rod these were pushed back and the charge tube withdrawn, so that this thing would drop down into the charge space, the incoming air space into the reactor, but the Engineers organised quite a big effort and, in fact, during the whole of the night we were never without steel push rods for attempting to push out the burning channels.

We went on in this way attempting to discharge the burning channels with some measure of success, till I suppose about 07.00 hrs. in the morning. I then went up to the roof at about that time and it was obviously getting worse. There were also indications in looking through the charge face that the fire seemed to be spreading upwards, so I decided to abandon the pushing out of the burning channels and to go on to make an additional fire break above the fire. About the same time I had a consultation on the telephone with Mr. Ross and I asked permission to apply water. He said yes, but make sure that everyone is out of the area. Now unfortunately this decision coincided with the shift change and there were a lot of people coming into the Works and we felt that it was not safe because we weren't sure about explosions and what might happen. We decided not to apply water until we had everyone under cover, so the water, in fact, did not come on until 07.55 hours. We had had during the night men up on the roof who were taking graphite temperature measurements, all the recording equipment for graphite temperatures was up on the pile roof. I brought these men down from the roof - I posted a man at each stairway so that people could not, in fact, go on to the roof. I left only one man in the Pile Control room, and myself and the Works Engineer stayed at the bottom of the charge hoist sheltering behind the bulkhead

door and I gave the signal for the water to be turned on at minimum pressure initially which was about 50 p.s.i. I left it like that for five minutes and then asked them to go up to 75 p.s.i., left that for 10 minutes and then went to 100 p.s.i. for another 10 minutes, and then up to maximum. There was no violence at all in terms of putting the water on. I chose not to put the water on to the heart of the fire but chose four channels above the maximum height where I thought the fire had reached so it could trickle down and get to the fire through the cracks. I then went up on to the roof with the Works Engineer, who insisted on coming and was a great comfort, to have a look down inspection holes to see whether the water was making any difference as an extinguisher. It was not apparent that it was at this stage although we were producing a lot of steam which was now coming out of the chimney and therefore was obviously having some cooling effect. But in making this inspection it was apparent that a lot of the water was ineffective because it was shooting straight through the pile, so I instructed the Fire Brigade Officer to turn back to the minimum pressure to give a lift up to the height of where the hoses going into the reactor so that we could get the maximum volume flowing in at the minimum pressure. Immediately after this period I went up to the roof several times to see whether the water was being effective and I would assess that, on its own, it wasn't. I could not see any diminution at this stage in the sea of flame at the back of the reactor. We had only one recourse left to us - which was to see what the effect of putting the air off would be. We were worried about turning the air off on two counts. One was that if we turned the air off it was likely that we would make the charge hoist completely uninhabitable and therefore if it were necessary to do something from the charge hoist we would not be able to get on it. The other point about putting the air off that worried us was that at least it was having some cooling effect on the graphite which was outside the region of the fire. Mr. Bell, who knows most about graphite locally, was a bit worried as to what might happen in terms of the high energy Wigner release in which case the whole of the graphite might have gone up to something like 1500°C. However, we had got to such a stage that I felt that I ought to ask permission to turn the air off for at least a limited period to see what effect it had. We got the authority of the Works General Manager and we got the shutdown fans off at shortly after 10.00 in the morning. The effect was quite dramatic and within 15 minutes there was a very considerable diminution in the amount of flame which was observable from the roof of the reactor. I must have gone up there about four times between 10.00 and 12.00, and by 12.00 the whole of the back of the reactor was black: there was no sign of flame or glow or anything else. I then went on to the charge hoist to do an inspection from the front of the reactor and similarly all was black. There was no further sign of fire. Up to this time we had only had two hoses playing into the reactor itself and I decided that we would attempt to give the knockout blow by putting on all the water we had got, and went on to the charge hoist to fix up two additional hoses which we had available and also I had these hoses brought four channel rows nearer to the heart of the fire, feeling that it was safe to do so under these circumstances. We then put in all of the water we had got into the reactor and this was about 13.00 hours and I just decontaminated myself and came down to lunch and then went home to bed.

CHAIRMAN When was the water put on?

TUOHY The water was on at 08.55. To complete the water story: we kept the water on all night. I went to bed about 15.00 and I came in about 19.30 hrs. the same evening and the water was still on and we decided that we would leave the water on the whole night. This became a considerable embarrassment during the night and during the following morning (Saturday morning) because extremely active water was coming into the blower house and into the pile forecourt. The booster air ducts were filling up so we cut back just one, and then two, hoses and we were watching such uranium and graphite temperatures as we still had left to measure, and kept on reducing the water until finally at about 15.00 hours on the afternoon of Saturday all the water was cut off.

CHAIRMAN Where is all the water now, that has not been converted into steam?

TUOHY Well, the bulk of the water (I was not here at this time: this was during the night) as soon as it was realised that this water was going to well up and possibly going into the surface drain which would deliver into the river Calder, a pump was supplied to pump as much of the water into the cooling pond as possible, but when I came in next morning this pump was in operation but its pickup well that we had got it in was not sufficiently deep and the pump would suck down so far and then start drawing air.

On the Saturday morning I decided we had to get into the booster duct itself and about mid-day on Saturday we got an opening into the booster duct, and a pipe down into the duct itself so that we were not picking up from this very shallow trench on the surface of the forecourt. Now after that time we virtually stopped all active water going into the river Calder. There was still a small amount going because some of these pumps have a coolant system where they use the water which is being picked up, to cool, and that cooling water flows on to the ground, but this was relatively speaking a trickle compared with the amount that had been flowing away to surface drainage before that.

CHAIRMAN Any questions?

DIAMOND One of the reasons for keeping the shut down fans on was to make the charge hoist tenable. They were off when you went in the last time.

TUOHY Yes they were. We were in respirators of course during this period but the charge hoist space had already, by any normal standards, become untenable during the night. You cannot have a situation where burning uranium plus fission products are dropping on the floor, and then say that by shutting off the air you are going to make it very much worse, and this was one of the considerations I had in mind when I finally phoned the Works General Manager and said I think we should shut off the air, as a sort of last resort.

CHAIRMAN Do you know what exposure you have had Mr. Tuohy?

TUOHY I have got to make a confession here. I had several films during the night and several personal ionisation chambers but latterly we were changing the men around and

there were queues of men waiting for films and personal ionisation chambers and I was going to the charge hoist and the roof etc. and for the latter part of the night I thought other people's needs were much greater than my own. I had not had any radiation for a long time and from about 03.00 I did not have any film at all.

PEIRSON What did you have before?

TUOHY I have not checked back.

CHAIRMAN That is one of the things we shall have to enquire about later as to what exposure various people had.

SCHONLAND Mr. Tuohy, what actually stopped the fire?

TUOHY In my own view it was a combination of the water and shutting down the fans.

SCHONLAND You do not think that shutting down the fans alone could have stopped it?

TUOHY I do not wish to prejudge your consideration of the evidence, but in my own assessment, having gone over all the graphs and everything else, this fire started when all the air was off. If one accepts that then you must have a combination

SCHONLAND (interrupt) I don't want you to go into that - what you've told us is that the water by itself had no obvious effect.

TUOHY No obvious effect: that is so, except that there must have been some cooling because we were generating a lot of steam.

KAY The fire was raging at such a level by then that it might have taken a long time for any effect.

TUOHY Yes, I would say so.

CHAIRMAN Thank you Mr. Tuohy, but where will you be if you are wanted again?

TUOHY I will be available in my office.

BELL (Mr. Bell was very indistinct)

CHAIRMAN Mr. Bell, we have heard this morning from some of the Operations Staff the reasons why this Pile was shut down was on the release of the Wigner energy and we know that you have studied this type of matter particularly from the operational point of view and we'd like to get from you the ideas you've had, or reasons why things were done and I think probably it would be easiest if you would be good enough to give us the story in your own words, and answer questions when you have finished.

BELL From the beginning, Sir.

CHAIRMAN We want to know why the pile was (annealed?) - we've heard a figure of 40K MW. We want to know why that was chosen. We know that the Pile had been de-energised several times and parts perhaps had not gone off and we want to know from you your angle on these problems.

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BELL To deal with the figure of 41,000 first, 40,000 mW was the figure actually intended was based on some experimental evidence which I had accumulated over a period of four to five years. The original anneals were done at 30,000. This was chosen as a convenient time based on the amount of energy we had measured in a dowel or a series of dowels in a pile, and the recommended figure then was 30,000 mW days. In about 1953 I think, I started an experiment to see whether the low temperature stored energy, the energy which released at low temperatures in the Windscale piles, would saturate, because if it saturated then we would not have this problem of annealing at such frequent intervals and the results have been – well, we've known for quite a long time now that we could go further than 30K. But I have taken these results much further than their terminal point, to make quite sure that we were not making any mistakes at this point. I have actually here a curve which gives us the experimental results. These have not been reported yet. Along the bottom here are cumulative MW days times 1,000 and this is the Wigner rise as we call it. We measure this in terms of temperature rise as this is the easiest to apply to a pile being released. We would expect the graphite after it reaches its triggering temperature which is somewhere in the region of 1000 would go this Wigner rise up in temperature. The position we have been running at – you see the general form of the curve here is not saturating but has certainly changed form very significantly. At 30,000 mW days here, this is what we have been running at, and the figure we have on this curve is 210°C rise. At 41,000 mW days, the plot shows the actual release here: it is 237°C, there is a difference of only 27°C in this, which still brings us within the safe temperatures when we take a metal-graphite pile and therefore it was recommended that the period between anneals could be increased and this action was taken on this particular anneal. In other words we found that by measuring from the start, measuring the buildup to 30,000, taking this as a position that we could, very well – on our present knowledge have gone over this knee on the curve without much trouble.

CHAIRMAN One of the things I do not quite understand is, when you talk about 30,000 mW days: it could be of course the graphite in different parts of the pile had quite different amounts of

BELL This, I should explain, is the maximum in the reactor.

CHAIRMAN That is where you get the maximum temperature rise?

BELL Yes, this is about 5' from the charge face.

CHAIRMAN This 40,000 figure to which you were going to work was mW days allowing for the maximum in the Pile?

BELL The figure 40,000 mW days is actually total pile mW days.

CHAIRMAN I see.

DIAMOND If you divide by the number of tons in the reactor and multiply by the ratio of maximum to average you will get the figure in the middle.

CHAIRMAN How much more than 40,000 is that figure?

BELL It is 1.4 or 5.

KAY It depends on how much flattening you've got and how much enriched material and so on.

DIAMOND 1.4 or 5.

CHAIRMAN So that the decision was that when the power of the Pile had developed 40,000 mW days you were going to do this release, but that at the point where the biggest effect had happened from this neutron flux it would be about 56, which is 1.4×4 .

DIAMOND This depends of course on how this curve is presented. It may already have been taken into account.

BELL This curve is presented as total pile mW. We use the same terms here.

KAY But the temperature you have recorded here, is it that the temperature to which a sample of graphite would rise if it was in the centre of the Pile, where locally it had been irradiated a period of 30,000 mW days, or is it the maximum temperature occurring in a core which has had a cumulative average irradiation of 30K mW days?

BELL The figures quoted here are taken from a series of measurements made on graphite dowels placed next to the channels in the flattened region of the pile and the figures quoted here are of total pile mW days. That is the actual figure logged as the MW days the pile has operated. These are operational figures. They are not corrected at all for position in the pile.

CHAIRMAN Therefore, you would get more than this temperature that we would read off that curve at 40K mW days somewhere in the Pile.

BELL I am afraid I don't quite follow you.

DIAMOND It depends entirely on where these samples are taken from. If they are taken from the middle of the length, then they represent the maximum you would get.

BELL No. May I sketch on the board.

This is the Wigner rise, W, and this is the charge face of that pile. This is the discharge face and the air blowing in this direction. The temperature variation along the channel is of that form and the flux is this form and is about and the stored energy is dependent directly on flux and inversely on temperature, and the effect one actually gets is that the energy is stored in that flux along the channel. The points I have plotted are the maximum points observed in any axial position and and the in the flat region here, these measurements were made on region here, so we have the maximum position of stored energy in this case.

BELL This experiment consisted of irradiating, I think, three channels in each pile containing specimens which we had prepared in 1953. These are removed for all anneals; so that the graphite is never annealed in pile, and there are, I think, 4 or 5 specimens per container. One from each container is measured and the remainder put back in pile, and at a later date another from each container is measured, and we get the buildup on these, so they have had the same irradiation history. So on our irradiation position, the history is different as

SCHONLAND You will be acquainted with a paper by Simmons about a year ago which dealt with two samples. I think, from Windscale. This suggested that there might be a danger in these kind of piles. Would you like to comment?

BELL Well our evidence so far - I have no direct measurements myself on this - from the literature, from Simmons' work and also from Ack who did some measurements in Canada for me on this same basis, is that the rate of release curve (the one shown here) of energy with increasing temperature as stated linearly on the same curve as we plot the specific heat, does change. The form of this curve changes with dose. The first curve here - this is the important point here - is this form. This has had quite low irradiation, I think the figure is 19,000 MW days, but I can't remember exactly, this here, where the material has in effect a negative specific heat, is the position we were annealing Windscale piles. One takes it to this temperature, at which stage the action is self-sustaining and it will remain self-sustaining until it has gone past this curve and you will get the energy in this area coming out.

SCHONLAND Two areas between that curve and the specific heat curve are the same. Now Simmons took another example. Would you go on with that?

BELL Simmons' sample had a higher irradiation which also had some anneals and he found that this curve changes. The peak drops and this part of the curve, the intermediate stored energy, greatly increases. Now the worry which Dr. Schonland is referring to is whether this curve gets above the specific heat while this part here is still about the same heat.

If this is so, then of course the pile will run away to temperatures which are only limited by the heat losses.

Now, A's work gives exactly the same sort of curve for this, but these figures here are about at the moment and is supported by A, and we are at the moment working on this problem to produce these results as a routine now in Windscale for this reason.

SCHONLAND You are using Simmons' techniques now?

BELL We are now using it yet. We have built our own apparatus which is in the final stages of testing now, and we have borrowed Simmons' too to double up on the work here. These are for a 7-year irradiation period. There are other curves of this type from American and Russian sources which give the same sort of picture and we felt, and I think we still do feel, that the energy here is quite safe at the moment, but it is time we started to measure it in this fashion, and this as I say, we are doing now.

SCHONLAND You feel it is safe at the moment. Simmons of course, on the basis of a very limited look at the problem, has expressed some uneasiness.

BELL In this report you mean? Yes, his uneasiness is expressed in the sense that we should now make routine monitoring of this, but there is nothing to fear for a few years on this one. I think that is the correct position on this one.

SCHONLAND Could we take a minute just to check that report?

You see, it says that if the rate of energy release approaches the specific heat, while the peak at 200 is still large, the Windscale piles will gradually approach a dangerous state and of course if the 200° peak disappears before the energy release high temperature builds up, it will approach a safe condition. It is only if the trend suggested by this result persists, that it is dangerous to carry out an annealing operation. You haven't any notion how far this trend has gone in the Windscale piles, have you?

ROSS No Sir, this is all the information we have.

SCHONLAND And you are working now to get much more information?

BELL Yes, we were hoping to have the apparatus working at the end of last week, but it developed a fault and we are working on it now, with a deadline of Friday.

SCHONLAND Your own measurements really obtain the starting temperature for Wigner release and the end temperature at which the two areas became equal above and below the specific heat so that you get those two temperatures. Theta-W is the more important one, the temperature at which it all stops.

BELL Our measurements are made by what we call a dipping technique where the graphite is put into a constant temperature enclosure and the temperature/time ratio is measured and this is correct for heat losses and the figures we quote are the adiabatic rise in effect.

SCHONLAND But it does seem to me that you might get to reach dangerous conclusions because you might give a low theta-W when all the time, these curves are changing shape in a manner you don't know about.

BELL I have this week done a very rough experiment myself (there wasn't time to go into it thoroughly). I have taken some graphite and, instead of using a constant temperature enclosure of 200, I have used a constant temperature enclosure of 300 and 400. Now, if what you suggest is true, then the temperature will increase over these temperatures. Now, the 400 one for example, which I think is what is relevant in these particular circumstances, the energy came out rapidly, as one always gets it, with the curve following the general form of these you have in front of you. It came out rather rapidly but it did not reach 400. It then crept up to 400 as it took up the temperature of the enclosure and we left it at this for about 16 hours and it did not go over the 400 mark at all. I think this is the answer to that query.

DIAMOND This has been determined by experiment, by taking samples at different lengths in the Pile?

BELL This curve here is determined by taking samples throughout the full length of the pile, including the reflectors, not only the core itself and the flux measurements across the pile would be in the normal routine measurement for stored energy and we are quite sure that the maximum position is in the central portion here. There is not much variation in this direction because your temperature conditions change throughout the flux, and therefore you take this - I am quite convinced this is the maximum figure, according to our measurements.

KAY What you are saying is that you are in the flattened zone regularly and you have put your samples at different points along the channels to make sure that you have got the maximum axial position.

BELL We have measured the distribution axially and taken the peak to plot on this curve.

KAY And so you would claim that that curve represents the maximum temperature rise that you would get with the Wigner release after a total average cumulative pile irradiation of whatever is given on the

DIAMOND A further thing, I take it, that follows from this, that this method of stating the results is peculiar to this pile.

BELL Yes, these measurements were done purely and simply for Windscale Piles. The curve is actually made up from points from both Piles.

CHAIRMAN Thank you, I think we all understand that now Mr. Bell.

BELL I'm sorry I don't quite understand where you want me to go now.

CHAIRMAN Perhaps if we ask some questions we might get some more information. One of the questions that interests me is that when you do a Wigner discharge you do not always clear the whole thing. You have pockets left which have not been released. Now in this particular case of the discharge that led to the accident, were there any pockets where there was a lot of Wigner energy left from previous occasions?

BELL Not that I know of. One must realise that the thermocouple distribution on which we base this evidence is such that there is only one thermocouple for every, say, 40-50 tons of graphite. On the other hand, I have observed in the past on this one that during an anneal one often gets temperatures rising to the order of 85-90° and holding these for quite a long time in areas which show no positive indication of release. We judge the release of course primarily by the shape of the temperature buildup and secondly by measuring it afterwards. We have, I do know from my own experiments on this one, that if one holds irradiated graphite at this temperature for a matter of hours, then the great majority of this low temperature peak, this energy which would normally release in a Wigner anneal does come out and, therefore, without actual monitoring specimens to show

us whether the energy is actually there or not I would assume, on my evidence and experience, that any area of the Pile that had been held at, say, 85-90 for a matter of hours will have released the greater part of this energy. The other point on pockets too of course is that this curve now applies again and that, even if it has missed two anneals shall we say, the buildup is not a severe one.

CHAIRMAN Yes.

BELL The other point on these pockets, if I may push this one again, is that the pockets normally occur in the back of the Pile where the energy storage is low and it is for this reason that we don't get them out.

CHAIRMAN Yes.

Have you seen any evidence Mr. Bell in the measurements that you have been making that the quantity you call theta-W, the Wigner rise, is not always the same for the same amount of MWs. The point in my mind is that if you use this over and over again, in due course, after a number of years, this annealing does not seem to be producing as much or perhaps it may be producing more energy than it used to.

BELL Our evidence Sir, I have actually brought a curve in a report (IGR/TN-W586) for you to see, is that new graphite, virgin graphite, which has not been irradiated here before, stores energy at a higher rate than that which has been irradiated and annealed. The top curve is the new graphite and the bottom curve is for graphite which has gone through Pile anneals and therefore one sees that the amount of energy storage with time decreases. This is not directly comparable to this but they are coupled together.

CHAIRMAN What is the difference between them - one is virgin, what is the other?

BELL It is Pile graphite which was loaded in the first (charge?) the isotope holes in the graphite mass were, at the building of the Pile loaded with dowels to prevent or reduce the air flow through these holes. These, when we had the unplanned release, the first one, the accidental release - on Pile 1, some of these were sucked out to take measurements on, and it is some of this graphite which has been in and out of the Pile it might have been out for a month or so at a time to do measurements on it, but it has gone back into the Pile, which is compared with virgin graphite which we have had fitted in the machine. Also on this particular one, to check whether this difference could possibly be due to strain energy or something in the graphite due to its machining and working: this is not true. There are two sets of points on the top which you will not be able to see, and which show that some graphite was thermally annealed before it went to the Pile and some was not. There is no difference between these two. The difference is purely and simply between the virgin and the old graphite.

CHAIRMAN Dr. Schonland. This is a subject which you are very interested in, would you like to question?

SCHONLAND A general point: your theta-W curve, is that obtained by taking samples periodically from the pile and examining them in the laboratory?

SCHONLAND So you would like to go on record that your measurements entitle you to say that the Wigner Balance was safe at the time this accident took place.

BELL No Sir. I would like to go on record as saying that in my opinion, from the results which I have seen, it was safe - which is somewhat different.

SCHONLAND From the results that you had obtained there is no reason to suspect any runaway effect until of course you got into the high temperature peak where you gave very valuable advice.

BELL Well that is very true. The high temperature peak was our main worry in this event. As it happens I do not know whether the Court is interested in some measurements we have had this week on this high temperature peak. It may be of value in this enquiry.

We have not yet got a method of measuring the rate of release of energy with temperature up to temperatures of this order. We have tried and so far we have been unsuccessful, so I took some graphite which has had 250,000 mW days irradiation with all the anneals in Pile 1. No, at the present it is 290,000. I took some out some months ago and sent it to N.P.L. after treatment by us, to get the stored energy in this material.

SCHONLAND At 290,000?

ROSS No, at 250,000. The pile is at 290 now.

The method of doing this - we actually sent specimens but they couldn't handle the (high?) activity. We only had 4 results which arrived in Windscale on Tuesday morning of this week.

We annealed a piece of graphite: we cut the graphite down to dust form for bomb calorimetry. We annealed one piece at 550, another at 1,000 (maybe 1,050) and another piece at 1,500 and we had the total energy measured. I got the results of these this week and by a subtracting technique one could find out how the energy is stored in various positions and, of course, there are only four results: this is not very good. The interesting thing here is that there is no indication whatsoever of a high temperature peak. The evidence we have based on a high temperature peak has been American evidence so far, we have not been able to measure it. This is the first measurement we have had. It is only one measurement and I must stress that.

SCHONLAND I think you must explain to us what you've got there.

BELL I have plotted here the rate of release of energy with temperature. The curve here is specific heat curve, the temperature is along the bottom and the area underneath this: this is just spread out. We have made no attempt to make distribution - we have made uniform distribution between the temperatures measured. The area under these curves is a measure of the amount of energy which would come out in these various temperature ranges. Above the 1500, that is 30.6 cal/gm left, but one can't draw that here of course.

SCHONLAND You ran up there to 1300, was it?

BELL This temperature? 1500.

CHAIRMAN So, as far as those measurements go, it isn't there at all.

BELL As far as those measurements go. But it is only one measurement and I wouldn't like to place too much reliance on it.

SCHONLAND Just one more question Mr. Chairman.

Would Mr. Bell agree that if you had a combination of stored energy in the latter part of the curve, the higher temperature part of the curve and a fire that you might have a difficult situation arising: external exothermic reaction taking place, you help? it by stored energy.

BELL The stored energy in effect changes specific heat. The answer is Yes to your question.

DIAMOND You mentioned on that curve, Mr. Bell, that there is a further 13.6 cal/gm stored above the line drawn. How do you know? This means you know of the total amount of energy stored, does it?

BELL Referring to my figures here the total amount of energy stored is 170 cal/gm.

DIAMOND How do you know how much is stored?

BELL This is done by bomb calorimetry and the combustion changes with the amount of energy in the material.

DIAMOND The heat of combustion changes, I see.

BELL It measures the difference between heat of combustion for unirradiated and irradiated material.

SCHONLAND What are the total calories per gramme, say between 1,200 and 2,000 for these samples?

BELL It will be an estimate. We know definitely that there are 13.6 cal/gm, and between 1,200 and 1,500 there will be some 3.5 of 19 (12).

SCHONLAND These are of course exceptionally small compared with some figures we have been frightened with.

BELL These are remarkably small.

SCHONLAND Figures of the order of 1000s have been quoted by Cottrell

BELL This is for a different set of conditions. This is for Calder.

SCHONLAND Could you put the same argument you have led to here do you think in the case of Windscale?

BELL I would not like to guess.

KAY I am not quite sure whether this is the right point to put this question - but what were the normal rules observed when carrying out the Wigner release with regard to limiting temperatures? It appears to me that some limit has been

observed on the metal temperature and also on the graphite temperature. Some of these figures have come out in the historical records this morning. But I wondered if you could summarise the situation. It seems to me, for instance, from the record of the different occasions when Wigner release has been carried out, that prior to November 1956 the maximum uranium temperature in any case was 395°C and the maximum graphite on Pile I about 350 something; on Pile II in one case about 375 but in November 1956 we have 420° on the graphite for the first time. Could you summarise the procedure or the rules.

BELL The rules as I understand them are as follows, that when any temperature, uranium or graphite, of 360 is reached the Pile Manager or some senior member of the Pile Group must be informed and preparation must be made to replace the inspection hole plugs, shut the chimney base door and be prepared to open the dampers in the air ducts. At a temperature of 400, I think, the plugs are replaced, the doors shut, and some dampers opened, and it used to be I think I do not know whether they have changed, the ? but it used to be that after this the shut down fans were put on, but experiments on the pile, actual operating experience showed that the dampers were enough to reduce the pile temperature very rapidly and, therefore, I do not think that shut down fans are any longer in the instructions, but I am quoting from memory of instructions I have seen in the past.

KAY Can I just be clear that the normal procedure is that when a temperature of 360° is reached, either with the metal or the graphite, that some action is taken, using the dampers or whatever it may be, to limit further rise, and that rather more determined action is taken at 400°C.

BELL There is no actual action taken so far as I know at 360°.

KAY The Pile Manager is informed.

BELL The matter in effect goes out of the hands of the processing staff, the junior staff, but I would stress that this is an operational instruction that I don't really know about at the moment. I am quoting from my memory here on previous occasions. I am not too clear on this.

KAY Can I also ask whether there was a particular reason for the temperature of 420°C in November 1956.

BELL I do not know of any Sir.

CHAIRMAN Your memory of course may be unreliable, but I would like to make it quite clear whether you really meant something you said, you indicated that as you remembered it in the past, if any temperature had reached 400°C, that previously when dampers were opened and these other things were shut they had quite a good effect.

Now, did that happen on this occasion, because I know we have heard already about some of the actions that were taken was precisely this.

BELL I can only speak of course from these records which are in front of you now and it did have an effect, but when the dampers were shut the temperature rose again.

CHAIRMAN The effect was there, but the unexpected thing was that when the dampers were closed again the temperature shot up.

BELL I would not say it shot, Sir.

CHAIRMAN Well, it went up. Whereas, what would have happened on previous occasions do you think? Would it have stayed down or would it have done that also?

BELL I haven't seen the records. It could have done either because

DAVEY (I think) (interrupt) May I deal with this? The detail doesn't matter, but there is a sequence of events for dealing with an exceptional temperature rise and it does consist of replacing the plugs or redistributing them, and following from there to the opening of dampers, and the final move is of course bringing on shut down fans. But as far as dealing with a hot spot is concerned, having brought on air to restore temperature rise, being satisfied with that condition, and taking the air off again, if the energy release is not complete it is not unusual to have another rise.

(Short period too indistinct) This is a complete pre-eruptive release.

CHAIRMAN There is no reason at all then to be suspicious because when the dampers were closed the temperature started to go up again.

BELL No, I should say not.

DIAMOND From these records of previous attempts to anneal, in the laboratory, you are able to raise the whole of the specimen graphite to the right temperature and know what the temperature is, it seems that in these attempts to anneal here, and that one in particular, somehow or other, what do you suppose this was due to? Not getting the graphite up to the right temperature or what?

BELL I think that the only reason could be was that the graphite did not come up to this temperature unless it went up very slowly. There are two effects one can get here. If one raises the temperature fairly rapidly then the energy comes out with a bump, and you get a very marked rise in temperature. If you take it up very slowly, then of course the energy comes out very slowly itself. One can anneal without getting this bump by keeping the or something like that. I cannot say why this particular one did not go because I have not got the records to check with.

DIAMOND The rate of rise of temperature doesn't affect the quantity of stored energy coming out, does it?

BELL It doesn't affect the quantity; it just affects the rate at which it comes out.

DIAMOND This is the rate of rise of temperature to the activation temperature, is it?

BELL Yes. If I can refer back to the linear rise technique, the shape of curve one gets is dependent on the rate at which one heats the specimen. One always quotes a rate of linear rise of temperature when one quotes figures for this, because of the change of shape.

SCHONLAND This was the rise of temperature up to the point when the graphite takes over?

BELL (*Could not hear the witness*) Yes, to take it to the limits, if one takes a piece of graphite which has stored energy in it and one leaves it at room temperature for long enough, the greater part of the energy would come out. If one (raises?) the temperature this effect is increased and as I said before if one holds the temperature quite a short period, maybe for a matter of hours at a temperature of 85° or so which is below the actual point at which the energy comes out rapidly, which is the point where we go over the specific heat, then the energy would come out. We have actually done measurements on this under laboratory conditions which prove this point.

DIAMOND And in your adiabatic tests which you have described the amount of energy coming out is independent of the rate of rise initially. The total quantity of energy coming out is independent of $d\theta$ by dt . What I am trying to get at is whether the total amount of energy that you get out is affected by the rate at which you originally raised the temperature.

BELL Without certain limits, Sir. If the rate of rise is very slow then it is certainly true that it is affected by the rate at which we bring it up to that temperature but in this type of rise one gets in a Pile or in our experiments during? the same range of time scale then there are no figures which we have which will show this difference. We have actually made a study of our measurements in the laboratory as against temperatures reached in the Pile and there is no significant difference in these. There is a variation on either side, but the actual take? as a whole is not significantly different.

DIAMOND Have you got experimental evidence of that? Have you tried the different rates of heating in the laboratory?

BELL No we have not.

CHAIRMAN Mr. Bell, the various charts which surround me seem to show that some, if not all, of the temperature records are going up to 200 or 250 on Tuesday morning and they stay up there all through Tuesday. There are some going up and some going down right the way through Wednesday noon and then they shoot up – there is in fact a period of a whole day when by and large the temperature inside that Pile was over 200°. Now what sort of fraction of the total low energy, Wigner energy, has come out in that period? From what you are saying, I should have thought there wasn't much left in the pile?

BELL There is some left, Sir.

If I may refer to one of these drawings, No. 5.

If one takes the curve for 25.50 which I think demonstrates the point we are making, I am quite sure that no further energy can come out of this graphite until this temperature has been raised to whatever is this figure – 260. The only energy one would get after that temperature, provided we are correct in our previous statements, that the rate of rise is not going to lose us? the heat will be that which comes out in the temperature region through which you raise the graphite by

some other means. On here the graphite has risen to 280 and has then dropped and in that portion of the graphite which is monitored by this thermocouple there is certainly no further low temperature energy. The graphite has then risen in temperature from some other source of heat and in this region here energy will be coming out of the graphite but it is not a spontaneous release. If you stop the graphite rising in temperature, if you remove any other source of heat, an independent source of heat, then the graphite would fall in temperature.

CHAIRMAN Does that apply fairly generally through the pile, or is this at only a few of the places?

BELL This applies to any piece of graphite.

CHAIRMAN What I really mean is that all of those are covered by your statement. They are all alike. Some of these are not. Are there dozens of others that are still down here, or is the whole pile

BELL We can only speak on what temperatures are measured in the pile. This is the only way of telling whether this has happened or not. For example, these two curves here, 25.64 and 28.68, are showing definite signs of release at a later date. The bottom one looks very much like a release and we have another curve here which shows some in the same region peripherally which are releasing about this time: that is by the time it gets from the normal positions to the outside edge.

CHAIRMAN The picture you have at any rate, Mr. Bell, thinking about Wigner Release is that quite a lot of the Wigner energy had gone in this early stage, in some had not yet gone.

I will try to formulate a question which I think will explain what you are saying. You are suggesting a figure 5 here, which shows some thermocouples going up to 60, but in those parts of the pile where those thermocouples are registering, that the Wigner energy has come out already and that the thermocouples are falling because the Wigner energy is out and the energy is being conducted away, whereas for some of these other thermocouples which have not shown much of a rise there is still Wigner energy in those regions but that these rises show where it comes out.

If we could, therefore, think of the pile at about noon on Wednesday, all of the thermocouples now have pretty well shown that the Wigner energy has gone, but shortly afterwards the temperatures begin to rise. You are saying that the rise cannot possibly be caused by a release of further Wigner energy. Something has started to make the temperature go up and in going up of course a little Wigner energy comes out with that, but it is not the Wigner energy that has driven the temperature up. It helps the temperature to go up because more Wigner energy comes out after you raise the temperature, but it isn't the driving force.

BELL I'd like to amplify this if I may. At this time, 12 noon on the 9th, I agree with your postulate here that the temperature has been dropping in general; the energy has come out. After this time there is another rise. This is not in my opinion, based on my own measurements and the evidence which is available in the literature, a continuous release of Wigner energy from that part of the graphite. There are two reasons for this. One

reason is the heat coming from somewhere else. That heat is possibly coming from Wigner energy being released in another part of the pile and being conducted down, or it comes from some unknown source of heat.

CHAIRMAN You mean from another part of the pile that hasn't got a thermocouple so that we don't even know that it happened.

BELL There is no indication on the thermocouples elsewhere in the pile that there has been a release of this magnitude at all. There have been releases in this time for example 28.68 at the bottom of the dotted curve which is right at the periphery, but of course that obviously could not have caused this rise in temperature but I feel myself that this would indicate another source of heat other than Wigner energy.

CHAIRMAN Would you place that in time around Noon on Wednesday?

BELL Noon till 4 o'clock on Wednesday.

KAY I would like to see a quantitative analysis of this. What sort of source of heat does one look for here in actual magnitude to account for this ratio of rise in the graphite? Can we work back from these figures, and when you get above the previous peak temperature, presumably there is Wigner energy coming in as well. Now, if we can allow for that surely we can estimate how much heat is getting into the pile over that period?

CHAIRMAN Well yes it is of the order of the energy required to raise the whole pile by 50°. It is that sort of order; a very large amount of energy.

DIAMOND Is it possible for this rise which took place in 2868 and 2564 which both seem to be in the bottom right hand corner of the pile, is it possible that the heat released from there as shown by these late rises would then conduct it through and cause the rises in 2550 and 2655?

BELL I do not think so. The temperature here is very low compared to the others. The other material is already at a higher temperature.

CHAIRMAN You cannot conduct heat in any other way?

DIAMOND Provided these temperatures indicate the maximum temperature of the region and not the trend only.

BELL This is true, but to get this sort of magnitude one must have a temperature which is well above 280 here to get through. The surge to cause this type of thing is a large amount of heat. I doubt very much whether this would have been a release of energy in another part of the pile that caused this.

I think that the significant point here on these curves is that the whole mass of the graphite went up at the same time.

SCHONLAND Thermocouple 1255 which is anomalous and rises continuously from the very start (Sheet 7). This seems anomalous Mr. Bell. I do not understand why we have not got

figures for some neighbouring holes like 1011 w m 1160 for example. Would Mr. Bell be prepared to comment on 1255? Does he agree it is a peculiar one?

BELL At first sight it looks peculiar but if you observe that in the early part of noon on Tuesday, well 10 o'clock on Tuesday, the temperature has been for quite a lot of hours between 70 and 100 and in this region one would expect quite a large amount of energy coming out without a sudden increase in temperature. Above that one won't explain this by saying that heat was coming from somewhere else. We have seen this type of curve before I think (I am speaking now from just judging a recorder as it's going past of course) but one expects an increase in temperature at some point due to the energy coming in from elsewhere. It had been low at least to start with.

SCHONLAND So it is not necessarily anomalous, but it might be.

BELL A drift of that magnitude when observed on a recorded chart would not be considered anomalous.

CHAIRMAN If we could refer back a little bit to the log book, it was at 05.40 on the morning of Thursday that the pile stack activity increased, whereas on the temperature chart the sudden rise of temperature occurred at noon on Wednesday. Did anything happen at noon on Wednesday?

KAY You have got a Wigner release in another part of the pile, 1972 and 3256. One goes up smartly at noon and the other at 4 o'clock. They certainly do not go as high as the temperatures we are getting on 2655 and 2570.

CHAIRMAN On the log book there is nothing at all on Wednesday except at 2215 when it says the temperatures are slowly rising. The pile was not put on to release energy. You do not know what caused that rise in temperature?

BELL No. It is completely inexplicable.

CHAIRMAN There was no more nuclear heating after 11 o'clock on Tuesday, so that we have got an unexplained cause of increasing temperature around noon on Wednesday but we did not get any activity on the stack reported until 05.40 on Thursday.

KAY May I ask a question on Chart 7 referring to these measurements. On Wednesday at Noon and 16.00 we get 1972 and 3256 showing sharp rises in temperature. Are these normal Wigner releases for those parts of the core? Is there anything peculiar about these? They are very sharp so far as I can see from the number you have looked at on previous occasions. Would you say these are quite normal?

BELL These are very normal. On an expanded scale, they are running 3"/hour and we quite often get enormous (?).

KAY I appreciate this is a very condensed horizontal curve, but there is nothing that arouses any suspicion about those at all?

BELL I would assume these to be normal Wigners. They have gone up to a peak and then dropped in temperature.

KAY Exactly as you were.

BELL Yes.

DAVEY I would like to ask one point. I want to make sure that we are quite clear about this other source of heat. Could we have records of previous Wigner releases to compare the shapes of those curves with these, because in postulating another source of heat I am rather concerned that we shall not miss the transfer of heat from one part of the lattice to another part on ?

BELL My information is second-hand, but I believe this is being done at the moment.

DAVEY Speaking from memory, I have seen similar curves in past releases.

BELL Well, I went through this yesterday with two releases. That is all I had time for. Skimming through the curves - we had no time to plot these sorts of graphs of course - one does get this type of thing occasionally but not of this magnitude.

DAVEY I would like to be sure of the magnitude too, because if one looks at Chart 5, I would still like to be careful about this threshold of initiation because in the first Wigner release the maximum temperature shown is about 260, then there is a falling away but the next rise is initiated at perhaps 220-230 (the curve is rising there) and there is a different in threshold there of perhaps 30 or 40°. It is not as big as we are making it out to be at first sight and we just want sufficient heat to raise us back to the threshold of 260 for a further release to occur.

CHAIRMAN I understand that the essential difference was, that on the first occasion where you started at 60 or 70° there was Wigner energy and we had only got to release it and it came and that raised the temperature. Having done that you can't get any more out of that region. If you then go to 250 you don't get any spontaneous release. You get some, but you've got to drive the temperature up by some other means in order to tap that energy. It doesn't come out on its own, spontaneously. Is that not right?

BELL Yes, that is correct.

KAY That is, we are accepting a curve of that kind, which is based on information from British, American and Russian sources.

CHAIRMAN Yes.

KAY So that second rise cannot then be a spontaneous Wigner. It could have been an assisted Wigner.

CHAIRMAN An 'assisted Wigner' is a good description of it.

BELL That would do me. I see this myself better by looking at the growth of specific heat effects. In the normal spontaneous release, specific heat is negative; if you can

imagine such heat, and in the other one specific heat is less than the unirradiated material.

CHAIRMAN Yes, we all agree.

KAY May I come back to a point about the start of this assistance, we have said it is between noon and 1600 (or is it earlier than that?). If you take 2550 at the top of that bunch of 3 where it begins to go up it is in a sense a stranger feature: it does, in fact, start turning up about 0400 and gets up to the original peak of 260 about 1500. From there on you can presume some Wigner heating so that there is an apparently anomalous source of heat from 0400 to 1500 in that region.

CHAIRMAN Yes that is correct, although that heat may be heat which has spread from other Wigner regions which meets Mr. Davey's point because some of these other records are rising. But Mr. Bell tells us that these other regions are not big enough to produce enough heat to do that to all these other curves.

BELL While Prof. Kay was talking I was going to mention a point which is in effect answering your query. The type of rise which one sees from 0400 onwards I ignored before because this sort of thing is not really significant on pile records. From my experience, one does get this type of thing happening with heat coming from another source. But it is when one gets a very large one which is the period here, the dampers are put on. That region is marked by the lines at top from 2 o'clock to 12 o'clock on the 10th. This I find very difficult to believe, comes from other sources of released Wigner energy anywhere in the pile.

DIAMOND Is it true that in the bottom right hand corner of the pile the records indicate that there was a very late release of Wigner energy all over that region. It happened about 24 hours or more after the rest of the reactor.

KAY Can I come back to this temperature rise? If I understand Mr. Bell correctly, he suggests if the following curve 2550, from 0400 on Wednesday until say 1500, that that rise is not so large as not to be capable of explanation by transferred heat from another part of the core. Would you confirm that point that from 0400 to 1500 on Wednesday - the rise of the temperature on 2550 from about 210° at the bottom, back to the level 260, you have seen that sort of thing before on other records?

BELL I think you are taking my point too far. It was only the region of between about 400 to 800 which I was saying.

KAY Well can we pick it up at 800 then? We are at 220 at 800.

From 800 to 1500 is that temperature rise normal or explicable?

BELL Well it is very difficult to say, normal. We do not know what normal really means here. If I had seen that on a recorder I at that stage in the annealing, would not have been surprised at all. I would have thought no more about it. I would have expected this sort of thing to happen.

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KAY From 1500 downwards there is a possibility of Wigner energy contributing to the temperature rise. Can we say now, from our known Wigner energy data, how much of the rise beyond 1500 can be accounted for?

BELL I would have to make a very rough guess. We are at a temperature of 260 at 1500 and we are rising.

KAY By 5 hours we are nearly up to 320 so we have gone up about 40° in 5 hours.

BELL That is only about 8 cal/gm in the graphite in that region. Specific heat is about .3 in that region. That is a rise one might expect of total 25°. 25° could be explained by the energy coming out.

KAY 25° total, and in fact we have got nearly 50°.

BELL This is a very rough calculation?

As I understand Mr. Davey, he was suggesting that if one could explain the lower part of the curve from 0800 to 1500 as the transfer of heat from one part of the core to another we are then back to the minimum temperature at which further Wigner releases can take place. Is this other part explicable or not. Can it explain the whole thing away or not?

CHAIRMAN I do not think it can. You can only explain that rise in temperature by heat coming in from some other region. What puzzles me about it is, it is going to go up 50° and it is already starting at 250 so it goes to 300 and, therefore, the other region must be a good deal hotter than 300. You have told us that you only expected a rise due to Wigner energy release of 200 or so. It does seem to me that that other region which is by definition supplying this heat must be very hot.

BELL Unless of course it is the same region and the heat is being supplied directly. This is a possibility.

CHAIRMAN You mean from some other source? I am talking about Wigner energy.

It does seem that that rise is too big to be explained by Wigner energy tripped off nearby and coming in by thermal conduction.

BELL I am quite convinced in my own mind that that is not Wigner energy at that point of the curve.

KAY Would you say 50% of it could be Wigner energy? You did a quick calculation a few moments ago and said you could account for a rise of 25°. Is that

BELL That was a very crude calculation.

KAY Could that be verified?

BELL We could certainly verify this, back in the laboratory. It is certainly true that if you are driving graphite above the temperature it has reached before, then you will get energy coming out. There is no doubt of that.

CHAIRMAN You are saying there is a 25° assist if the temperature goes up quickly.

BELL As soon as you take off any other source of heat that would stop.

CHAIRMAN And as soon as the temperature rises above that of the region supplying it, the thermal gradient is reversed.

KAY What about the top curve 2053 which rises steadily from the first heat, it came up a little more slowly to the first point and then continue to rise continuously, at least until Thursday midday.

BELL There are two things here. I must stress that this is my own thought on this subject only - I have no proof for this. The first rise up to 11 o'clock on Tuesday is quite normal. Some more nuclear heating was put in there because the pile was cooling down instead of heat spreading. You get this bump up to 800 due to nuclear heating. From this point it keeps creeping up. In this period one would assume immediately that this is energy coming from some graphite released elsewhere. The only time it gets out of this is at the same time as everything else, where there is change of slope at that point again. It is throughout these curves.

KAY Can you put your finger on a particular point on curve 2053 which is difficult to explain under normal circumstances?

BELL I don't think I can until it gets to the stage to where it keeps heating up after the dampers have been opened.

KAY And yet they are up over 400 there?

BELL Yes.

KAY I am a little surprised that you are saying that 2550 is so unusual when the temperatures are actually lower, but going up perhaps a little bit faster there, but 2053 has been going up all this time.

BELL It is the form of the curve rather than the actual temperature.

KAY You mean that 2053 is continuing to release Wigner energy?

BELL Not necessarily continuing to release; continuing to receive energy from somewhere that is raising its temperature. This could very well be energy from another Wigner release in another part of the pile.

KAY And yet it is the highest of all the curves here, in temperature.

BELL Yes.

KAY So we are looking for a still higher temperature source of heat.

DIAMOND These curves we have been talking about for the last 10 minutes or so are grouped around this thickened area on the chart, and yet the two fuel temperatures, 2153 and 2557 which are in that area do not show any anomaly at all; they stay nearly flat.

BELL I think there is a point here that these graphite thermo couples are at 4', 6' and 10' from the charge face. The fuel is 16' from the charge face. The thermocouples in the uranium are put in at pile operating conditions where that is the maximum temperature in the channel. During Wigner energy release the maximum temperature is 400° throughout the channel generally.

DIAMOND You have got two entirely different papers. Are you quite satisfied that the bottom right hand corner of this reactor went off late?

BELL From these curves, yes Sir, it is obvious.

DIAMOND They are all consistent?

BELL Yes.

DIAMOND And that was a Wigner release?

BELL It looks very much like it. That is all I can say.

CHAIRMAN I do not know whether you have seen these drawings?

BELL No, they were done this morning.

CHAIRMAN They do show temperature records with time for uranium and in graphite. Who knows these? I am looking at the previous anneal and what catches my eye, and you will see better than me, is that in the first one, which is a uranium temperature, there are these rather big rises. Can we take these straight away, or does someone need to look at them first? We do seem to be getting to a point where we are getting evidence that something was producing heat at a certain time, but the evidence to me isn't convincing because I'm not sure that similar things haven't been observed before.

Now if we could show that there is something different about the recent records as against the earlier ones, then we really have got a significant point, but if it's happened before, it is not so significant. They are the previous anneal.

(At this point it was suggested that Mr. Gausden should be brought in later.)

DIAMOND I'd like about 5 minutes just to go through all these curves and make sure we are consistent. That is to say, to recognise the position of each of these curves with similar behaviour to the ones we are talking about on here, to make sure the first one that is not behaving the same as the others.

(Five minutes were then spent in looking at the charts.)

DIAMOND I was trying to plot the positions on charts of the thermocouples which all exhibit the same characteristic rise late as though there were an external source of heat, and we have been looking lately for some reading in that area which did not exhibit this source of heat, but have not found one. The 6th or 11th readings are self consistent – all being the same shape and the same characteristic or roughly so in that area. I don't know that this proves anything. I was just making sure there wasn't a reading which disproved it.

CHAIRMAN Mr. Bell, have you had a chance to look at those?

BELL I will do the graphite first if I may. These are the graphite temperatures at the highest point – in the rest of the curve except one same – exactly. This is quite normal. We would expect this drift here to be due to energy coming from other parts of the Pile. The only one which shows any difference here, and which is roughly similar in form to the present is this one here. You can see the temperature rising again in the middle here. But these thermocouples are all at the top of the pile and we always get the heat rising in the pile when we are annealing we actually heat the bottom up first to make use of it. That is probably just hot air rising. That is all there is on the graphite ones that I can see at a quick glance.

CHAIRMAN The scale is the same as the scale we've been looking at?

BELL Yes.

DIAMOND The temperatures are the same.

DAVEY One metal temperature was not very high here but I am afraid I have no explanation for that at all. One metal temperature here rises high but I do not know why. It could very well be heat from a Wigner source. Without drawing these out on a pile map and seeing where the various thermocouples show an increase of temperature at various times, and ? for the metal, I wouldn't like to say what they say.

?KAY These dips are of fans on and fans off, are they?

BELL These very rapid dips? Yes these are shutdown fans going on.

The release in fact is completed at this point. The pile has grown cold and the shutdown ? till the pile starts up again.

CHAIRMAN In fact it is from that period from 1200 to 1600 on Wednesday that the behaviour is beginning to depart from anything that you have seen before.

BELL That is true Sir.

CHAIRMAN Any more questions for Mr. Bell.

KAY Can I once again just go back to 20.53 which is the top curve on sheet number 5 and ask him again whether he sees anything peculiar about that curve.

BELL Well, looking at it now with the whole curve before me showing the whole curve after the incident, and being wise after the event, it is peculiar that it keeps on rising and then increases its rates towards the end here. I think I should put this point that on the recorder chart, this change of slope was not noticed. This is a 3" per hour and one cannot see it on the recorder chart.

KAY But just as we've said for that group of three, 2550, 2661 and 2655. Looking at it now in the light of experience, things really started looking odd from 12.00 hours onwards on Wednesday. Is there any point on the curve 20.53 when we can say that things looked equally really odd?

BELL Well the slope is changing about the same time.

DIAMOND It is more the slope than the absolute level?

BELL It is more the slope than the absolute level that I am considering here.

SCHONLAND Mr. Chairman, are we talking now of chart 5 where you were taking graphite temperatures 6' from the face?

If you turn to chart 4, you will find ? four feet from the face base. Now this is the same channel, isn't it? But you see the rise is much more marked, but not really differing from the others in suddenness.

KAY You mean from 12 o'clock on Wednesday?

SCHONLAND Yes. So 20.53 is not as anomalous as it appears from the 6' chart. What bothers me a little is that, as Diamond says, we have shown that these are anomalous, but we have not got the data for the distant ones and it would be very interesting if they could be plotted. The people who prepared the information concentrated on the danger areas.

KAY I would say that any graphite temperatures anywhere in the pile that are available should be plotted.

DIAMOND Would you get that cracking?

BELL You want all thermocouples in the pile plotted. Is there any preferred order in which you would like these done? This is a very long job and if you want them quickly then it means taking some selected points rather than going through the whole lot. It took 30 people all morning to produce this set of curves.

CHAIRMAN Where are the curves themselves, or are they just numbers?

BELL Most of them are on long recorder charts.

CHAIRMAN Is it out of the question to have the originals put before us?

BELL Yes, I think we could.

CHAIRMAN In a way there is almost too much detail here to look at everything. Do you think that having the originals would be all right?

SCHONLAND Perhaps Mr. Bell could look at them and sketch some trends.

BELL It is very difficult to see from the recorder charts to see what actually happened, but we could get some curves plotted.

CHAIRMAN I think the Committee's point is that we are very interested to see whether Prof. Diamond's suggestion that the rise in emperature which occurred at 1200 on

Wednesday did occur in that region and not elsewhere. Therefore what we are interested in, if you could, with your expert eye, look at the original record of placed thermocouples not in the affected region to see whether there were rises occurring at that time or not, and if there were rises, not at that time, when were those rises?

DIAMOND To be quite frank, the evidence presented to us all inclines us to the view that there is a source of heat in this area causing an increase in temperature over and above the

.....
We have not got a chance of checking this in other parts of the pile and we must do that. An the other thing on the same point, as this happened on Wednesday, we ought to see a detailed log and we have only one entry for Wednesday here.

CHAIRMAN Do you think you could do that for us Mr. Bell?

BELL Certainly, I could probably give you a ? at the moment.

CHAIRMAN We would rather you went away and when you are ready, as soon as convenient, we could call you again, but I do not think we can expect you to do that today, but if you work on that this evening we can call you at some convenient time tomorrow. Is that all right?

BELL Yes Sir.

DIAMOND Just one final point, labouring the point about the bottom right-hand corner of the pile again, where there was a late release apparently of Wigner energy, these two went off extremely suddenly. Have these been seen before?

BELL Yes.

KAY I did raise that point.

DIAMOND Sorry, I was too concerned with the theory.

CHAIRMAN Are there any more questions for Mr. Bell or shall we ask him to do the job we have given him. I think if we have any more questions, when we call Mr. Bell again we shall have the opportunity. Thank you Mr. Bell and press on with the original records.

Mr. Gausden was then recalled.

CHAIRMAN There are two things Mr. Gausden that we want to ask you about again. We have studied the temperature charts and Mr. Bell has been taking us through the Wigner effects and a matter of great importance to us is whether these records, obtained up and until the incident seemed to be occurring, were typical or not.

GAUSDEN Yes.

CHAIRMAN We have been looking at some of the details and we have some charts which are actually in front of you which show the records for the release of November 1956.

GAUSDEN Yes.

GAUSDEN Yes.

CHAIRMAN I suppose you haven't even seen them before in that form.

CHAIRMAN These are isotope channels, has everyone got one of these? There is one on the board, perhaps you could explain this without us all having a copy.

GAUSDEN I have not, no.

GAUSDEN There are two copies of this down here somewhere.

ROSS I got these out as a result of this morning's discussions.

CHAIRMAN Let's have a good look for them. Please proceed.

CHAIRMAN We were trying. You have some charts before you. Can you find chart number five. If you look at chart number 5 right at the top you will see a temperature record of 20.53. Now Prof. Kay was asking some questions about this because it shows a long and steady rise until some dampers were pushed on and then it wobbled about. The thing that interested us was that at these temperatures of nearly 400 the Wigner energy is not enough to take it to 400. It goes far less than 400 and therefore it did seem that either there is some extra heat coming in to make it keep on going up or that that particular region was getting heat from a nearby region which was releasing Wigner energy, and as that was the top one we could not see which other one was doing this heating. Perhaps I have not explained our dilemma very well, but we cannot see quite why that one keeps going up to well over 400. The question is have you ever seen anything like that on record accelerating upwards in temperature and is at the rather high value of over 400.

GAUSDEN Do you want me to confine myself to isotopes or metal as well?

CHAIRMAN I would like you to start with isotopes.

GAUSDEN Could I have the corresponding sheet for the 4' Sheet 4.

GAUSDEN The details as shown here are the isotopes actually in position in pile one. A.M. which is in the odd odd channels or odd even. If we go along the top row 18^c, 18.50 has an A.M., 18.51 an aluminium nitride. When I say aluminium nitride, there are a number of aluminium nitrides in the channel, I can quote the actual numbers if you wish to have them.

In general I agree with your statement that there seems to be nothing from the release which was contributing to this unless it was possibly 29.60 at the four foot mark.

CHAIRMAN Let's leave the numbers out for the moment.

GAUSDEN A.M. 18.52. L.M. 18.53.

CHAIRMAN Yes. On the six foot chart of course 20.55 is very hot. It is the hottest of all and it is hotter at six foot than it is at four foot.

CHAIRMAN Do members of the Committee know what the symbols mean?

GAUSDEN Quite true, yes.

KAY I think so, but I would like to be sure.

CHAIRMAN Perhaps we should not spend any more time on that. At least you can say you have not any picture of what this means. You have not any suggestion to offer that this is a fairly normal thing, you have not actually seen it before.

CHAIRMAN Will you explain what they are?

GAUSDEN Not in quite the same way.

GAUSDEN AM is magnalith. A.M. is magnesium lithium. Aluminium nitride, I am not at all sure what that's used for. Someone: for Harwell.

CHAIRMAN The shape of this curve, especially for a top curve, is different from anything you have seen before?

L.M. is Bismuth ? : Yes.

GAUSDEN Yes.

G. are normal graphite stringers which are in for monitoring purposes.

CHAIRMAN Does anyone wish to amplify that?

T.C. are graphite thermocouples loaded for the Wigner.

KAY Finished for the time being anyway.

CHAIRMAN There is one here, Tm.

CHAIRMAN Now to proceed to another matter which is a rather more extensive one. We should like you to tell us about the loading of this pile. There were in various channels different things and we would like to know something about them. We would like you to tell us what there was and where those things were.

GAUSDEN T with small m is Thulium. Another Harwell product for gamma ray sources.

Co is cobalt.

KCl is potassium chloride.

Steel was in fact in the channel discharged prior to the anneal.

CHAIRMAN Could you perhaps describe to us what the thing physically looks like. You have a channel which could have been, it is really in the middle of four.

GAUSDEN It is in the middle of four uranium channels and it is 1 1/2 inches in diameter and goes straight down the whole length of the pile.

KAY Are all isotopes in the isotope channel, or are some in the uranium channels?

GAUSDEN All in isotope channels, none in uranium.

CHAIRMAN Do they fill the thing from front to back?

GAUSDEN Very nearly, we usually leave about two feet at the back end of the channel not filled. The remainder is filled either with the isotope or with a lead cartridge to make up the weight of the channel.

ROSS Mr. Gausden should explain what he means by making up the weight of the channel.

GAUSDEN The point about that is that it has been found that with normal pile operation there is a minimum weight of isotopes, total length that you can put in a channel otherwise they tend to blow out, so the weight is made up by lead cartridges one foot long, one inch diameter, spaced at intervals along the channel.

CHAIRMAN Otherwise the air pressure pushes them out.

KAY And the isotope cartridges are wired together so that they remain as a group?

GAUSDEN No, they are usually interspersed. The normal pattern is to start off say, with three leads, then isotope, lead - isotope, lead, at equal intervals along the channel and then finish up with 2 or 3 leads again.

KAY They are just held in place by the weight of the lead?

CHAIRMAN All these different materials, are they all in the same can, or does each of them have a special wrapping so to speak?

GAUSDEN They are all in virtually the same can and same can specifications and within limits of the same diameter, by that I mean from one inch to 1.07 inches. The Mark III A.M.s for instance are in cans which are 1.07 inches outside diameter.

CHAIRMAN What is the can material?

GAUSDEN It is aluminium.

CHAIRMAN It is the same material as the fuel element.

GAUSDEN Yes, with no finning - a straight tube.

CHAIRMAN It is just a plain cylinder.

KAY And to make up for all this use of neutrons in this part of the pile we have a locally enriched zone.

GAUSDEN Yes, we have. If I can show you this chart here the brown represents a complete ring of what we call Mark 10 cartridges 1.28 Co metal and the zones inside that, that is the brown cross, the red and the yellow, are largely 1.28 Co metal, but they have in fact a small quantity of natural metal at each end of the channel. The make up of a channel in the inner three zones is two natural, 17 enriched, and a further two natural, and round the outside of that a complete enriched ring.

CHAIRMAN Are these enriched fuel elements also in the same cans?

GAUSDEN Same specifications of material.

CHAIRMAN I see, but actually they are not identical?

GAUSDEN No, if only for the reason they are in the main, some are machined, some extruded, some with fins, of course it depends.

ROSS Mr. Hardy will be able to give us information on cartridges.

CHAIRMAN We will ask Mr. Hardy in due course.

Have you had any trouble with any of these types of mixtures, fuel elements and isotope cartridges?

GAUSDEN The only thing I can say on this is that the A.M. cartridges we are using at the moment are lighter than the original ones insofar as the original ones had a lead sleeve on the outside and the A.M. in a separate cannister in the centre. The present ones are completely full with A.M. and have no lead sleeve and so these were lighter. In order to keep them in the channel and to keep loading them in sufficient quantity, we had to resort to some rather different methods of loading to normal. One method was to put a special lead can at the far end of the channel, that is the discharge end of the channel, with a special aluminium make up base on it and then wire from there with aluminium wire to the front of the channel and then at the front of the channel a special graphite locating dowel was placed in position which had a small aluminium sort of Maltese cross on the end which was just sufficiently big not to go down the channel, and the wires were brought back to this point holding them in this position. That was one method of loading new type A.M. Mark III. The other way was by having special -

PEIRSON That 1.28 is in the fuel element channels not in the isotope channels.

GAUSDEN All uranium in this case.

CHAIRMAN You were telling us -

GAUSDEN The second method of loading A.M. was to have a lead cartridge with a special spring retainer attached to it. It was roughly this sort of shape, sprung just sufficiently to be a good fit into the channel and give some friction.

CHAIRMAN Were any of these cartridges, not all of them because there were so many, were they such as to fill up this channel so that air could not get past to cool them?

GAUSDEN None at all, except insofar as where we used the graphite dowels for locating on the charge face as the anchor. There they were not completely filling the channels by any means, but they were a larger diameter than the isotopes themselves, but the Research and Development Group here informed us that, although we had restricted the air flow down the channel at the front slightly, there was so much leakage in from the Wigner gaps that this was of no consequence.

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CHAIRMAN I do not know whether you are the right man to ask - some of these materials as they stay in the pile become highly radioactive and therefore are a source of heat. Are any of them, such as the radioactive K or whatever it is you are making, causes of exceptionally high heating in the element itself. Cobalt for example? polonium? - the decay of polonium gives a great deal of heat.

GAUSDEN I am afraid I cannot answer this question.

CHAIRMAN Perhaps somebody could.

SCHONLAND There is another point analogous to that. Cobalt is a strong absorber of gamma rays and it does get hot.

CHAIRMAN Yes. Many of these materials are natural sources of heat.

SCHONLAND Yes in two ways, as absorbers of pile radiation. In fact, in Dido we have to provide water coolant sleeves for cobalt.

CHAIRMAN We will follow this point up with one of the nuclear physics experts later on.

KAY We would like him to look into the design of each of these isotope cartridges from the point of view of?

CHAIRMAN Really then, you state that the latest form of A.M. cartridge is rather light and therefore has to be held in either by restraining wire or by friction but that has not given any trouble?

GAUSDEN None at all.

CHAIRMAN None of these things have given trouble, you have not had bursts or anything?

GAUSDEN Not one.

CHAIRMAN Perfectly well behaved.

GAUSDEN Yes, as far as we are aware.

CHAIRMAN Do any members of the Committee wish to ask any questions?

KAY May I ask how long has pile one been used for the relatively large production of isotopes in this way?

GAUSDEN Many years.

KAY On this scale? Have you been gradually working up to this scale or have you been working essentially in this pattern for some considerable time?

DAVEY May I come in, Mr. Chairman? L.M. has been going in for possibly even 5 or 6 years. A.M. is a comparative newcomer, but it does not alter the picture. Cobalt is a fairly old one.

KAY Can you say you have had roughly this total absorption of neutrons for some considerable period?

GAUSDEN That has been building up.

SCHONLAND What was the picture at the last Wigner release as compared with this?

GAUSDEN Not very different to what we have now.

KAY How long have you had A.M. cartridges in the pile?

GAUSDEN They are continually changing, the oldest ones in at the moment went in in April 56.

DIAMOND The old design, or the new?

GAUSDEN These are the old ones. Of the new ones, the oldest are December 56.

DIAMOND Nothing had been put in freshly before the Wigner release?

GAUSDEN Nothing at all.

DIAMOND These are all as irradiated.

KAY Had any of these been in for an abnormal length of time or had they been irradiated for what has become the standard period?

GAUSDEN There are some aluminium nitrides which have been in two years, but this is not abnormal. Cobalt going back to September 54.

DAVEY I think a fair statement on isotope irradiation is that they all go in specified periods subject to the normal discharge. To illustrate that, if a particular isotope is due to stay in for two years, it might be there one year ten months, or two years three months for the appropriate discharge, but those are the limits.

GAUSDEN Yes.

SCHONLAND May I ask a very silly question, are these all the isotopes that are in the pile, all the isotope channels involved or are they just shown as surrounding the area?

GAUSDEN Oh no, they have just been shown in the area. Every one of these channels with a mark has an isotope of some sort in it.

SCHONLAND It is all over the place.

GAUSDEN Yes.

DAVEY A fair description would be packed to capacity.

KAY This has been going on for years?

DAVEY It has been building up gradually.

KAY Using higher enrichment (**GAUSDEN** That is comparatively recently) fuels to take part of this greater absorption?

KAY To allow you to get the greater absorption later?

DAVEY I cannot put a figure to it, but until comparatively recently isotope manufacture in the Windscale piles was a of spare neutron capacity.

As the demand for isotopes increased so, we have gone beyond that and had to introduce enriched material to cope with the demand.

KAY How long have you been using enriched 1.28?

DAVEY Some time in 56.

KAY Previously you had 1.045 or natural and now you have three enrichments.

GAUSDEN No, only two, 1.28 and natural.

KAY Presumably the can design for the 1.28 fuel is different from the 1.045? Taking account of the greater?

GAUSDEN It is in fact a smaller diameter.

CHAIRMAN Who is it who specifies a particular material to go in, A.M. for example, who says that this particular cartridge must be in, say, for six months or nine months? Is that your R. & D. Department?

GAUSDEN This is very largely Operations Branch at Risley who lay down an overall programme on how much is required over a certain period and that defines the length of time that we have to leave these in.

CHAIRMAN Yes, what I had in mind is not only are these isotope units, but also fuel elements and are themselves sources of heat. The fission products in the fuel elements and isotopes being made in these others produce heat, but the pile was shut down for a long time before anything was happening and during that time at least fission activity has a chance to cool off somewhat, whereas these other things won't cool off at all. Throughout the pile you have sources of energy, fuel elements and isotopes, and if we are looking for a source of heat everything is under suspicion and I was wondering how if you leave cobalt in for years and years it gets a bigger and bigger source of heat as time goes on and more cobalt is made.

GAUSDEN Any special isotopes – apart from A.M. and L.M. – these are the subject of special irradiation demands raised by the user which are passed through Operations Branch, Risley for approval.

CHAIRMAN You yourself do not see how hot a thing is getting, you just put it in and it is in there until you take it out again.

GAUSDEN That is so.

KAY Do you actually accept any cartridge that goes in?

GAUSDEN We are talking about the irradiation programme now.

KAY But the design of an isotope cartridge would be the responsibility of the Risley Design Office concerned, or of Harwell presumably if it is a Harwell isotope. Is that correct?

GAUSDEN There is a laid down specification as to how it should be canned.

KAY Checking the thermal aspect of it and what the heat generation will be, and whether you will get that away right, who is responsible for that? There used to be a Fuel Element Working Party. Do they deal with isotopes and cartridges as well?

ROSS There is a Cartridge Working Party now, but I am not at all sure that they deal with all the isotopes. The specification broadly was that a particular material should be canned in such a way as not to jeopardise the action of the pile and that is our requirement, but the requirement is met by the other organisations.

CHAIRMAN In other words you are saying that more than one section in the Authority can send to you cartridges for insertion in isotope channels and tell you how long to leave them there and you do that and it is the group that has said this that is carrying the responsibility for that being all right. Is that correct?

GAUSDEN Yes, that's how it works.

DIAMOND Does this include the thermal aspect? Does this include a calculation about the temperature of the outside of the can when the pile is shut down?

KAY Is that a fair question to put to Mr. Gausden?

DAVEY The only answer we could give, and I suppose it is better again to choose an example. Let's choose something simple first. Potassium chloride irradiation thereof is a Harwell requirement and as far as we are concerned in Windscale, Harwell is required to produce a cartridge which is satisfactory in terms of pile operation, but having said that, they send the cartridge to us to be irradiated for the time specified by them and the only limit was put on is that, approximately to that time, it will come out on a scheduled discharge, but we are assuming in all that, that they have studied the behaviour of this cartridge and declared it to be satisfactory in our circumstances.

CHAIRMAN As far as you here are concerned you do not take what they have told you to do, and say well now, this particular cartridge has got to stay in for six months: when it's been in for 6 months, if we shut the pile that gets so hot, it is a possible cause of trouble. You don't do that sort of calculation?

ROSS I think we must get this quite straight. Irradiation facilities generally are controlled by a Committee known as the Irradiation Committee which is Rotherham and people ask him for space in the pile. Space in the pile is dictated by the amount of plutonium you are prepared to lose and therefore that controls the number of spaces except for A.M. which you state what you want. By you, I mean Aldermaston.

DAVEY Again we must be clear about that because the Irradiation Committee look after capacity.

CHAIRMAN I understand. I wanted to check the specific point that you here do not check that a particular isotope element is a source of heat after it has been in the pile a

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certain time and therefore is it something that has to be checked? You yourselves do not do that?

DAVEY No.

SCHONLAND This surprises me. I thought the responsibility for the loading of the pile is under the direct control of the Works Manager. If some customer says it is all right and it is not all right, is it the customer's responsibility?

DAVEY In the present circumstances, yes. I am quite clear that the responsibility for the safety of the pile is ours, but we can only cover that by saying to anybody who wants to introduce an isotope that he must can it in such a way that it does not jeopardise the safety of the reactor.

SCHONLAND And you take his word for it?

DAVEY I don't see that we have any alternative.

SCHONLAND The thing is discussed at your technical committee.

CHAIRMAN It's not the canning problem that I'm on. I can see that that's all right, but the point arising is that we are looking for a source of heat and inside all of these cans is a source of heat and you have said that you yourselves do not check that source of heat.

DAVEY That is correct.

KAY It is the complete design of cartridge, both from a metallurgical and a thermal point of view. To be satisfactory you must have compatibility of material and also satisfactory thermal properties. You would be able to get rid of maximum heat generation without exceeding a dangerous temperature. It is like fuel element design. I was under the impression that the special isotope cartridges, such as L.M. and A.M. originating from the Risley Design Office, were passed through the same machinery as for checking the fuel element design.

DIAMOND It is quite clear that this calculation will have been done somewhere, the temperature to which these cans may rise after a certain period of irradiation under static conditions. We must then add that temperature to the static conditions by the temperature they had got to.

CHAIRMAN To be fair I must only ask people at Windscale this question: who has done this calculation?

ROSS There is a Fuel Element Design Office at Risley. Hardy could speak on this.

CHAIRMAN We will ask Mr. Hardy. Are there any more questions for Mr. Gausden.

DIAMOND Are any thermocouples put on the isotope cans?

GAUSDEN We have never done so.

SCHONLAND Is there any cooling in the isotope channels?

GAUSDEN The cooling is done by the normal air flow.

SCHONLAND There is room in those channels?

GAUSDEN Yes.

DAVEY In fact Mr. Chairman, I know you do not want to pursue it, but it is significant in the terms of the question you have asked that these are not even finned.

CHAIRMAN No.

DIAMOND May I ask whether isotope channels ever mixed or are they all one type of isotope?

GAUSDEN We have very occasionally put in a few odd isotopes together containing such things as one or two samples of in separate cans, but we have never mixed A.M. with L.M. or that type of thing.

DIAMOND In this case before us there are no mixed channels?

GAUSDEN None whatsoever.

CHAIRMAN Well, I think we can now let you go Mr. Gausden, thank you very much. We may still want you on another occasion. Can we now have Dr. Hardy?

(Chair introduces Dr. Hardy)

CHAIRMAN We'd like you to say for the record what your job is in the Authority.

HARDY I am Deputy Head of Laboratories at Springfields.

CHAIRMAN The last evidence we have heard, Dr. Hardy, has been about these isotope channels. We have got a diagram which shows us some of them, what they are, and we have been interested in seeing whether any of these can be regarded as a source of heat (optimistically), and having that in mind of course, we are very interested in the design of the cartridges for the isotopes and also the fuel elements, but I do not know whether you do the fuel elements. You do. Well then, we would like to hear something about the design of the cartridges, about the isotopes and the fuel elements and a little bit about the background on it so that we can lead up to more significant questions.

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HARDY The AM Mark 3 cartridge is given in drawing SK35179 of which I have a copy on the table and the specification of the cartridge is given by Specification OB51. The cartridge consists of a hollow aluminium tube of just over 1" bore and approximately 30 thou. wall thickness. The aluminium cans had a pep tube end being governed by Specification OB50. It is essentially pure aluminium of not less than 99.7% purity and minor impurities and it is closed at one end, open at the other end.

The AM Mark 3 bar which was sent to Windscale from Springfields and it was opened up. That is why the surface of the AM is bright - where it's been cut. But the bar is just less than 1" diameter and fits into the can. The composition of the AM is covered by Specification OB49 and this consists of magnesium alloy containing lithium, the composition being within the limits 11.5 wt.% and 13 wt.%. There are certain impurity clauses but I don't think they are really important, and the lithium is naturally occurring material with the natural isotopic content. The can cartridge is manufactured by taking the can. There is a manufacturing specification, Springfields Works, Specification PS21 and the manufacture is extremely simple. The AM bar and can are checked for dimensions and weight. The AM is put inside the can and the pep tube end is welded in place, that is the pep tube end as the cartridge is assembled, then the cartridge is evacuated through that tube after the end has been welded and argon is sprayed into it, then the pep tube is sealed by crimping and welding the end and the cartridge is leak tested before sending off to Windscale. Is this the sort of detail you want?

CHAIRMAN I think so Dr. Hardy. Are you going to take us through all the cartridges?

HARDY There is only one other cartridge I am proposing to take you through Sir and that is the uranium Mk. 10.

CHAIRMAN Please proceed.

HARDY The only point about this one to notice is that it consists solely of AM and aluminium but there have been earlier marks of AM cartridge which have contained lead, either lead at the end or lead as an annulus around the AM.

KAY Lead being required for weight?

HARDY Yes, to stop the cartridge being blown out of the channels.

KAY What feature enabled a plain AM to be used in the Mark 3?

HARDY A standard channel consists of 4 cartridges containing lead only, that are loaded in at the exit side of the channel and the other cartridges are loaded in afterwards and these first 4 cartridges stop the remainder from being blown out.

KAY You are merely putting the lead in a different place.

HARDY For the Mk 3 the lead was put in a different place. That is correct.

DIAMOND What has led to this change of design?

HARDY That lies outside my personal experience.

DIAMOND Do you know the volume of the trapped argon inside it?

HARDY The volume of trapped argon? No, that I don't know. The argon pressure is 200-300 mm mercury.

DIAMOND Could I ask after the AM is put into the aluminium can and the closure is made whether this is swaged or pressurised to push the aluminium on to the AM or is it just a sliding fit?

HARDY No, it is a sliding fit, just pressed home manually and then trimmed to length and welded.

DIAMOND Have we got the specification for the internal diameter of the can and the external diameter of the AM?

HARDY They are in the documents I quoted. The specification of the can: Internal diameter 1.006" min.-1.08" max. Wall thickness 0.029" min.-0.034" max. Outside diameter AM 0.99" min.-1.00" max. The uranium Mark 10 cartridge is given in the Authority drawing SK23081(C) and this is the drawing I am putting on the table. The aluminium can is of the same composition as the aluminium can for the AM cartridge. The diameter of the uranium is close to 0.88". I believe in fact it is given on that drawing and the cartridge is again close to 11" long. The uranium is cast, heat treated, machined, degreased, graphited. The can: first of all the first end seal is made after it has been inspected and this is done after welding in a cap rather like the cap for the AM cartridge, and then the weld is folded over and sealed again by brazing metal. The can is cleaned and the inside is graphited, then the bar is inserted in the can. An end cap is put in the open end, that is welded, the can is evacuated and helium filled and then the pep tube is crimped, closed, and again the weld is rolled over as shown in the drawing and sealed by brazed metal.

The cartridge is steam autoclaved at 150°C and it is also given a hot air test at 350°C for 48 hours, and it is then visually examined afterwards for pimples, blisters or any defect that has been caused by these tests. It is also leak tested and any unsatisfactory cartridges are not passed forward. If there are no other questions, would you like me to turn to the metallurgical characteristics of the cartridges?

CHAIRMAN I have a question or two first. You described the AM and fuel elements but of course there are a lot of other cartridges for which I suppose the can specification is very similar.

HARDY I believe the can specification is similar to the AM for the other cartridges that may be found in the isotope channels but I have no direct evidence to offer on that.

CHAIRMAN Let me be specific: there are some isotope channels used to make radio cobalt and therefore there must be a can on that? Who does all this business for these?

HARDY We could get the Works Manager, Springfields on that. He would be the best man on them.

CHAIRMAN Similarly for all the others?

DAVEY What we have to do is distinguish between cartridges of all sorts manufactured at Springfields and those which may be manufactured at Harwell. I think that is fair isn't it? Anything that isn't made at Springfields is made at Harwell.

HARDY That is correct.

GAUSDEN I don't know where the cobalt one is made.

HARDY At Harwell.

KAY The only ones which definitely came from Springfields are AM and LM. Generally speaking, the others come from Harwell.

GAUSDEN The AM is a very similar cartridge to the LM.

CHAIRMAN First I had better make sure there aren't any more questions on this part.

KAY One question on the Mark 10 uranium element. Would it be fair to say that apart from the smaller dimensions of rod, and the slightly larger size of pin, the technology of the cartridge is essentially that of the previous uranium cartridges used in the pile.

HARDY The technology and manufacture is slightly more complex but it is not essentially different.

KAY The fitting of the bar into the can, the sealing and testing?

HARDY The differences arise from this fact that aluminium and uranium interact when they are heated together and the differences in manufacture between the AM cartridge and the Mk. 10 arise chiefly from that.

KAY No, you misunderstand. I am talking about the difference between the Mark 10 and the previous mark uranium cartridge.

HARDY They are essentially the same.

KAY I fully appreciate the difference in technique between the uranium and the AM, but between these Mk 10 uranium at 1.28Co and the earlier marks at 1.05Co or natural uranium, but the dimensions might be different because of the enrichment and so on. The actual materials and technique of manufacture has not changed.

HARDY There may have been minor changes in the end designs that have influenced the manufacture but they are marginal.

CHAIRMAN Any more questions on that? Please proceed.

HARDY I would like to draw your attention to the melting points of these metals separately and in combination.

Pure aluminium melts at 660°C. Pure magnesium melts at 650°C. There is a Mg-Al eutectic melting at 436°C. The AM with a composition of 12½% Lithium melts in between 584 and 590°C as shown by the Mg-Li phase diagram I am putting on the table. The ternary system Mg-Al-Li shows no evidence of low melting point eutectics in the compositional range in which we may be interested. At 400°C, the ternary cross section shows liquid present only close to the lithium phase end of the phase diagram and we have examined this particular facet experimentally by taking in an alloy consisting of AM to which 30 wt.% aluminium was added and determining the lowest solidification point by thermal analysis. This thermal analysis curve I will pass round. The temperatures on it are accurate to about ± 3°C and they show an arrest point of a eutectic at 435°C which is very close indeed to that of the temperature of the binary Al-Mg eutectic which makes it appear that the presence of lithium has little influence on that temperature. There is some evidence that a liquid phase persists slightly below that but it is present in only a very small amount. I think the only other temperature we need to note at this stage is the melting point of lead of 327°C since that is present in the earlier marks of cartridges.

CHAIRMAN Was the lead that was acting as the weight to hold it in, was that just ordinary lead, or has it got something in it to make the melting point higher?

HARDY It was ordinary lead within my knowledge. There may have been a chemical specification for nuclear reasons but apart from that it was ordinary lead.

CHAIRMAN So we have the pile and we have got evidence already that parts already were up to 400°C with lumps of lead in it which ought to have melted.

KAY This is canned lead.

HARDY This lead is separated in the Mk 2 cartridge from the AM alloy by an aluminium can and then there is another aluminium can totally enclosing it.

KAY And even with a Mark 3 where you use lead

HARDY There is no lead in Mark 3.

KAY You are using lead, not in the Mk 3 can, but as a separate piece at the end, that is canned in aluminium and looks to all external purposes like one of these.

HARDY Yes.

DIAMOND Can you tell us the volume change on fusion of AM?

HARDY I am sorry I don't have that information.

DIAMOND Bismuth of course is known and goes the same way as water?

HARDY I am not sure that bismuth is present as bismuth metal or bismuth oxide.

DIAMOND I used it purely for illustration.

HARDY I beg your pardon.

These are all the opposite way round to bismuth. I can say definitely of all the alloys except the AM, for which I have no measurement, but all the others shrink on solidifying.

DIAMOND Can you tell us what the structure of AM is immediately on solidification? Is it hard like bismuth?

HARDY Brittle? No, it is not brittle: it is fairly plastic.

DIAMOND More like sodium?

HARDY More like pure aluminium perhaps.

CHAIRMAN What you have said really is that the AM material inside this cartridge ought not to have started melting until it gets up to 580 or 590°C. It is said it is in contact with aluminium on the outside and that may have made a little eutectic at 426°C.

HARDY I have evidence to offer on this. During the design of the cartridge the basic work on the AM cartridge for Windscale was done about 1954 but the question has been raised of an AM cartridge for Calder and work has been done on the design of that cartridge during this year.

For both these items of work, the question of solid solvent reactions between the aluminium and AM was studied and this was done essentially by taking clean pieces of AM and aluminium in close contact and heating them up. After certain periods of time, the couples were sectioned to look for inter-reaction and I can quote some figures for the degree of inter-reaction between these metals. From the 1954 work, at a temperature of 300°C after 14 days the layer of inter-reaction was given as one thou. At 350°C after 28 days the thickness of the inter-reaction was given as 21 thou. The work done this year on solid solvent reactions supports this. At 300°C after 32 days the total thickness of the inter-reaction was 3 thou, in one case and 4 thou, in another case, and at 350°C after 32 days the thickness of the inter-reaction was 11 thou, in one case and 10 thou, in another case; at 400°C in the 1957 work after 2 days the thickness of the inter-reaction was 23 thou. If these results are taken and extrapolated to 435°C they would lead you to expect penetration of the can wall in about 7 hours and we have studied that on actual cartridges. There are cartridges of two sorts involved here. We recovered standard Springfields cartridges from Windscale last Saturday and in the meantime we started work at Springfields on AM cartridges that differ slightly from the standard Mark 3.

Instead of having a single bar of AM inside them, they have three pieces of AM. They have one piece about $\frac{1}{4}$ " diameter and two pieces which were $\frac{1}{2}$ " diameter and were split diametrically longitudinally. These differences are not in my opinion significant but I draw them to your attention because the work that I am going to report does contain both these sorts of cartridges. Now the cartridges were heated in circulating air furnaces; the degree of air circulation was fairly slow. The normal amount that one has to permit even temperature distribution and we have carried out tests involving heating these cartridges at temperatures between 400°C and 550°C. The cartridges were examined at intervals, perhaps probably 15 minute intervals in the case of higher temperatures and considerably longer periods of time in the case of low temperatures.

CHAIRMAN Now these cartridges are exposed cartridges or virgin cartridges?

HARDY These are virgin ones, unirradiated cartridges. I have a graph here of the results which I will pass round for you to see. The red marks are cartridges which have burst, the blue circles are cartridges on which the tests were continuing yesterday, and you will see that the bursts have all occurred at temperatures from 475°C upwards. The bursts have all occurred very quickly. At a temperature of 400°C we had three bursts after about 56 hours and the bursts take the form of these specimens that I am passing round. That one can had one hour at 550°C; that one has flattened because it was one of the Springfields cartridges which has the three pieces of AM in it instead of the single piece, and during these tests we have had two fires. That is a cartridge that was heated between 450 and 470°C. It, again, is what I am terming as Springfields cartridges with 3 pieces of AM, but otherwise a standard cartridge, and it was on fire when it was removed from the furnace.

KAY It was on fire when it was taken out of the furnace?

HARDY Correct. And there was one other fire in a cartridge which was heated at 550°C for 1 hour.

SCHONLAND Was that in the furnace here?

HARDY Yes.

DIAMOND Could I ask you to go over again the internal arrangement in this three piece cartridge?

HARDY I will draw it for you. These cartridges were made to answer a request for cartridges containing a very closely defined weight of AM. There was an excess quantity made that was not supplied to Windscale and these were the cartridges on which we started work at Springfields and they contained 3 pieces of AM in the form of approximately $\frac{1}{4}$ " diameter rod and 2 half pieces of the same rod.

DIAMOND May I ask what there is in the gaps between the can and the rod?

HARDY I cannot answer that out of my own experience: it would either be air or argon. These cartridges were made several years ago.

DIAMOND Have you any idea of the amount of heat generated in these cans during and after irradiation?

HARDY During irradiation I can only quote the information we received when we were designing the cartridge for Calder: that the amount of heat generated in the irradiation region was quite small and that the temperature of the cartridge during irradiation would be very close indeed to the temperature of the gas.

DIAMOND When the AM has been irradiated for some time there is some internal residual heat in it. Have you any idea of the amount of that?

HARDY That lies outside my field.

CHAIRMAN That also applies to you see it is not only radioactive and it's producing heat, but in this case there is helium being formed so that you build up an internal gas pressure.

HARDY You will notice that in all the samples that have been passed round of the cartridges that had been heated, the end has domed due to the internal gas.

CHAIRMAN So you think there is gas inside? There must be. And is it the gas that is pushing out? And is the stuff swelling inside? It looks as if it is swollen. In that case it has gone the wrong way. So it was not full?

HARDY Exactly. It wasn't full. It was three pieces and because at these temperatures the aluminium is so soft that they have been unable to sink down towards the floor.

DIAMOND Do we know whether any of these 3 piece cartridges were in the pile at the time?

HARDY I don't know that.

DAVEY I think not.

KAY Has any examination ever been carried out on an irradiated AM cartridge discharged from the pile?

HARDY I am afraid I don't know. That information would come from Windscale.

KAY But R. & D. has facilities for examination at Windscale for irradiated cartridges. I wondered if they had handled

HARDY I'm sure they have but I haven't the information.

DAVEY They must have handled it because they get the AM out of it.

KAY R. & D. actually take the AM out.

ROSS Operations do it now.

HARDY I don't think too much should be made of the difference in appearance between the specimens that had 3 pieces of AM and the specimens with 1 piece of AM. If so happened we started the most severe tests earlier on.

CHAIRMAN What do you think caused it to break at all?

HARDY In my opinion which is supported by photomicrographs, there is an interaction between the Mg-Li alloy and the aluminium, leading to a low melting point eutectic and here is a photomicrograph at the top of this page of a specimen which was heated at 500 for 5½ hours. It has a 435°C melting point and you can see evidence that that region has been molten. There is a core structure there.

CHAIRMAN So in your view the presence of gas inside does not materially affect whether it bursts or not.

HARDY I don't think it does cause it to burst. I think the bursting is where you have the interaction. You have a line of liquid, or point of liquid and that oozes out or oxidises, and the gas is not the determining factor.

DIAMOND Do you know whether, on heating, this alloy gases, emits gas?

HARDY The AM alloy contains a considerable quantity of hydrogen when it is manufactured. During the manufacturing process at Springfields the bars are degassed by a vacuum treatment at a high temperature before canning. I do not believe that that takes all the hydrogen out and an irradiated bar will of course contain other gases produced during irradiation, so you would certainly expect heating an irradiated bar to release gas.

DIAMOND Is the argon filling, before sealing, about an atmosphere?

HARDY About ½ of an atmosphere.

DIAMOND So if it were due to the argon alone, at 500 or 450°C, the pressure inside is about one atmosphere?

HARDY Yes.

DIAMOND And any additional gas must be due to gassing of the alloy?

HARDY Yes. The gas would normally be in solution inside the metal unless it is released.

DIAMOND But this does not apply to any gas which is coming from the nuclear reaction?

CHAIRMAN The AM is making tritium and the tritium, or some of it, is decomposing and changing into helium and although helium atoms may be locked in this lattice at room temperatures, I am not sure how quickly they will come out at 3, 4 and 500°C. Presumably they will come out much quicker so that the difference between a virgin cartridge and an exposed cartridge is that the exposed one has some helium gas either in the lattice or in the space between the metals.

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KAY lithium^b giving you tritium and helium?

CHAIRMAN Giving tritium and helium, and the tritium itself, because it is radioactive, changing into helium.

KAY So you have got two sorts: you've got the helium^d from here and the change of tritium into helium^e.

CHAIRMAN This is quite a lot of cubic centimetres of gas but I don't know the quantities. It needs to be calculated.

ROSS If I might help in the elucidation of this, I think I would like to draw your attention to the fact that Mark 1 and Mark 2 cartridges were two aluminium cases, one inside the other with lead in between, and I would like Dr. Hardy to confirm that this was deliberately done in case of leakage through the first aluminium case.

HARDY That I am afraid is outside my experience because it was designed before I joined the Authority. I understand the lead was purely there to weigh the cartridge down and the aluminium casing between was to stop reaction between the Mg-Li alloy and the lead since these do react when the lead is molten. There is a sample which I have not passed round that shows the reaction in a heated specimen just starting and has all the appearance that it is curved due to a liquid phase and not due to a gas burst.

DIAMOND This temperature was 440°?

HARDY That is correct.

CHAIRMAN The question was asked whether there were any three piece cartridges in the pile.

DAVEY I am asking for this to be verified but the opinion of three of us is we have never We've had what we call the annulus type and this one.

DIAMOND Is the annulus type in contact with the aluminium all the way round?

HARDY There is a drawing of the cartridges in section. A lead annulus surrounds an aluminium can containing the AM. We have some evidence of attack between the compatibility tests at temperatures of 450 and 475°C and in these cases we had in one specimen a lead sheath surrounding the AM followed by aluminium and in the other case the lead sheath was absent. At a temperature of 475°C in the absence of the lead sheath, the aluminium was almost completely destroyed, and in the case of the presence of the lead sheath, the attack was slightly less but the attack has not been completely prevented.

CHAIRMAN Dr. Hardy, are you staying here tomorrow? You have given us a great deal of information and I think most of us would like to reflect upon what you have said and perhaps call you again if we think it is necessary to focus on something.

HARDY I'd have some further information.

CHAIRMAN How long would it take? We have to hear someone else who has to go off to Canada.

HARDY About half an hour.

CHAIRMAN Could we start with you first thing in the morning? But Prof. Diamond wishes to ask a question.

DIAMOND What is the temperature at which AM exposed to air will support combustion?

HARDY That was in fact my next piece of evidence.

CHAIRMAN And that is tomorrow. Is it a long answer or just a single figure?

HARDY Well, the general figure is 450-465 depending on local conditions and the rate of oxidation can become catastrophic at temperatures between 400 and 425.

CHAIRMAN I think we'll take that tomorrow.

(Chairman introduces Mr. Wright)

Thursday p.m. (17th)

WRIGHT I am concerned with the effects of pile irradiation on various chemical reactions and we have been concerned, particularly on the project side, with the reactions between graphite and gases under irradiation.

SCHONLAND This is in the Chemical Division at Harwell?

WRIGHT Yes.

CHAIRMAN We have been talking mostly this afternoon about the isotope channels, the cartridges, possible reactions with fuel element casing, and we are wondering just where you can help us in that field. We have heard, in the A.M. cartridges for example, that penetration of the cans seems to occur at 400-435° in seven hours, but this does not seem to me to be quite in your field.

WRIGHT I certainly have no experience in that particular field, Sir. I do not think I have anything to contribute which an ordinary chemist would not know.

ROSS Mr. Wright was asked to tell us something about the possible oxidation of graphite in the Piles.

SCHONLAND Mr. Wright could tell us about that and any other matters - he must have heard about events - on which he can shed some light.

WRIGHT I do not think the effects of radiation on the oxidation of graphite are very relevant because in this particular case, the reactor was shut down and the gamma radiation is a very small quantity relative to the doses we are concerned with in the main, and I do not think that the effect of that gamma radiation would be appreciable. One effect which I think might be appreciable is the effect of pre-irradiation in the graphite itself and its increase in chemical reactivity. Some measurements have been made both by Mr. Davidge in the early days and later by R. & D.

Branch at Windscale, indicating that the irradiated graphite is more reactive than normal graphite by a factor of about 10. I do not think it is clear yet whether this is entirely due to the radiation damage effect or whether it is due to salt deposition and the catalytic effects that result from that salt deposition, but it is undoubtedly an increase by a factor of something of the order of 10, over the whole period.

CHAIRMAN Does it matter how long the graphite has been irradiated?

WRIGHT Apparently not, because the factor of 10 was observed by Mr. Davidge as early as 1953 and there has been no substantial change. I learnt today from R. & D. people that some samples taken out just before the incident had been examined, and I believe it is true that the factor of 10 still stands.

KAY What are you quoting: is it activation energy or rate of reaction, or what?

WRIGHT Rate of reaction at 400°, with reaction with oxygen at 400°C. From our own experience we can say nothing of the activation energy changes that occur, though there are a number of American data indicating a reduction in activation energy as a result of radiation to a dose of something of the order of 10²⁰ ndt, so that one does know there is that effect and there are effects also on the surface properties, heats of absorption and such, at much the same sorts of doses, but in this particular case I do not think it is quite clear whether it is due to this sort of damage or whether it is due to the catalytic effects. The second thing that I think might be of interest is that, even allowing for this factor of 10, the rate of heat release and the rate of rise of temperature under adiabatic conditions would be too small to give you any appreciable effects at temperatures below about 400.

I have some figures here, calculated for 300, 350 and 400 and under adiabatic conditions they indicate about 0.1°C per hour rise in temperature at 300, 1.5°C per hour at 350 and 17.0°C per hour at 400. One can put these figures in another way and relate the heat output from the whole of the graphite, burning at these various temperatures to the normal heat output of the reactor, and ask whether the fans would be capable of taking the heat away and again one finds that at 400°C total heat release would correspond to about 3 mW. So that clearly at 400 one has a certain amount in hand. Certainly with full fans - I don't know what you'd have with the standby blowers on.

CHAIRMAN 3 mW is a lot with nothing on.

WRIGHT Yes, but this assumes the whole of the graphite oxidising at this rate. What one would say is that if you have a local region oxidising at this temperature, a corresponding amount of heat would be produced per channel and would presumably be removed per channel. Only, I think, if you got above 400 would you run into trouble and we feel that certainly at 550°C you would be in a region where you would be unable to remove the heat even by your full fans. The figure of 500 for the whole of the graphite oxidising is 140 mW.

DIAMOND When you say of the graphite, do you mean the whole of the graphite by mass, or the whole by surface area?

WRIGHT The whole by mass.

The oxidation of graphite at these temperatures is a mass phenomenon, not a surface phenomenon. It is only controlled as a surface phenomenon by the rate of diffusion of air to the surface and products away from the surface when you get to temperatures of 800 or so. At these low temperatures the rate of reaction is such that it is not controlled by diffusion processes and is therefore a mass phenomenon.

KAY How does the oxygen get to the graphite?

WRIGHT By diffusion through the pores of the graphite.

DIAMOND With cold graphite or at least graphite at 400° the reaction rate is so slow that the whole mass of the graphite can go on equally?

KAY The rate (controlling factor?) is the chemical rate, by reaction kinetics, and not by diffusion of the oxygen into the graphite and the products away?

WRIGHT Up to a temperature of 750° to 800°.

DIAMOND In the Windscale Pile as it stands, you can get 3 mW of heat at 400, was it?

WRIGHT Yes, if you were to oxidise the whole of your graphite, I think.

One has got to take this pro-rata for any individual channel. It has merely been more convenient to calculate on the basis of the whole reactor. I think these are the main points, Sir. I will amplify any you want me to.

DIAMOND This is the reaction of graphite with atmospheric air.

WRIGHT Reaction with air in all cases.

DIAMOND 140 mW for 500. That will still go on at an equal rate throughout the thickness of the graphite.

WRIGHT Yes, certainly at 500° and I would expect even at 600. At 600 I think in a bulk mass of this kind as distinct from small samples in the laboratory. I think one is beginning to ask whether you can get the air to the blocks themselves.

KAY You expect a falling off from the laboratory rate on small samples as the temperature goes up with the larger

WRIGHT Yes, on the small samples

KAY Diffusion becomes a limiting factor?

WRIGHT Well, rather access of air through the passages between the blocks rather than access of the air from the block to the inside.

CHAIRMAN So really the summary of your evidence is that graphite at 400° creates really a very slow rate of rise, but once you get up to 500 however, it runs away.

WRIGHT Between 550 and 551 we could be certain that it would be an embarrassment.

CHAIRMAN If we could look at these temperature records, we have to think pretty hard when we see temperatures well above 400 but at 400 nothing much.

WRIGHT I think that is true Sir.

DIAMOND Could I ask whether the experiments on which you determined these energy quantities were done with graphite 7" thick.

WRIGHT These are indeed the results of calculations on the known heat release per gram mole of carbon oxidised. The experiments on rate of oxidation were done on quite small samples, but we have found no effect of sample size?

DIAMOND This is in fact a deduction, on your part, that the air would get 4" into the graphite and that the CO₂ would get out again. It would be a continuous and uninterrupted process.

WRIGHT It is an extrapolation from some experiments made on specimens. I think, as thin as $\frac{1}{2}$ " thick and as thick as 3" or 4".

KAY Supported by any sort of analysis of the rate of diffusion in and out.

WRIGHT No, merely direct measurements of the rate of loss of weight.

KAY So you have carried out your measurements on a small range of graphite samples and then you have calculated from there on a mass basis to the complete size of the core?

WRIGHT This is early work, not of my own but of Mr. Davidge.

CHAIRMAN The reactor in which ? at all is when the graphite is bigger, the inner bits will not be getting oxidised. They can't go faster than they would have done if it was small.

DIAMOND The figure which surprises me is 140 mW at 500°C. I wonder whether we ought to qualify that?

WRIGHT It is derived from a figure of .14 watts/gram.

DIAMOND Observing the quite limited surface area of graphite in the pile, the velocities of the air into the pores in the one direction and the CO₂ out in the other direction must be extremely high.

KAY I agree with you.

CHAIRMAN Yes, but the mobility of the oxygen through these layers to the graphite is astonishingly high. It goes between these planes, does it?

WRIGHT Yes, I think certainly there would be no sign of a diffusional control under these conditions.

CHAIRMAN ?

DIAMOND CO₂ has got to get out.

WRIGHT Yes.

CHAIRMAN It slides along in between the layers, doesn't it?

WRIGHT Well, you don't need to get within the layers of the crystallite. No, one is concerned only with getting it into and out of the macro and micro pore structure. Between the crystallites themselves the oxidation occurs purely on the surface of the crystallites and not within the crystallites.

CHAIRMAN I was wrong in what I was thinking.

WRIGHT There is no need for that, but you see some 30% of the total volume of graphite is in fact (porosity?).

CHAIRMAN I think you have given us a very short but important contribution.

WRIGHT I don't think there's anything else I could contribute except that looking at these curves, it does not look as though, in the early stages, up to this period here, reaching 425 in some of the graphite, that oxidation could have had any effect. One has to explain those rises by some other source of heat.

Board of Enquiry

Friday 18.10.57

Evidence heard from:-

Hardy	pp 2.1 - 2.5
Saddington	pp 2.5 - 2.8
Hall	pp 2.8 - 2.11
Gausden	pp 2.11 - 2.14
Bell	pp 2.14 - 2.15
Davey	pp 2.15 - 2.16
Simmonds	pp 2.16 - 2.20
Mummery	pp 2.20 - 2.22
Rennie	pp 2.22 - 2.24
Bell	pp 2.24 - 2.32
Towler	pp 2.32 - 2.34
Turner	pp 2.34 - 2.36
[Discontinuity: reverts to	
Simmonds	pp 2.36 - 2.37]

Friday, 18th October - 10 a.m.

WITNESS: DR. HARDY

CHAIRMAN You have some further scientific facts to present to us, but before we actually start I would like you to confirm some important statements you told us yesterday. You said that Magnesium Aluminium had a eutectic which became liquid at 436, on or about that temperature. I think 435, that an A.M. cartridge kept at that temperature for seven hours, showed that the material inside was seeping or penetrating.

DR. HARDY No that is not quite correct. I said that compatibility tests on solid A.M. against aluminium which is essentially a diffusion couple when extrapolated to 435 would lead you to expect complete penetration in seven hours. That is in the presence of the magnesium, diffusion would have reached the surface in that time. But I also showed tests on these cartridges where the actual time was longer than 7 hours at a temperature of 440.

CHAIRMAN Suppose the temperature has been somewhat lower at 400 so that there was no suggestion of any liquid anywhere, then it would be a very long time indeed would it? Or can these solid things still penetrate?

DR. HARDY They can still penetrate. At 400, I have one result here, a couple was kept for two days and the thickness of the inter-reaction layer was 23 thou, so this means that it would have penetrated approximately one-third of the wall thickness.

CHAIRMAN What I had in mind was that these cartridges are in the pile in its normal operating state, and are presumably a good deal less than 400 although I haven't heard a figure given, but if something started in the solid diffusion or? and then the temperature got up to this, it is away. This 435 gives us very little margin in the temperature.

HARDY Correct.

DIAMOND One specimen you showed us yesterday had just penetrated. What was the temperature and time for that?

HARDY This was 34½ hours at 440. It had just penetrated. These of course were cartridges which had not previously been heated.

DIAMOND Do you know the normal running temperature of these cartridges in the pile?

HARDY I understand it is 160.

CHAIRMAN That sort of temperature is perfectly all right?

HARDY Yes. In my opinion the cartridges are safe for a running temperature of 200°C, shall we say for a year, and in the evidence available in 1954, I would say they were safe at 250 and safe also for short times, say three days, at 350.

KAY What is the maximum normal graphite temperature in the Windscale piles

HARDY I do not know.

DAVEY ...at about 380. I could verify that.

? Graphite?

DAVEY Yes.

KAY These are in the middle of the graphite, aren't they? If there is any heat in the cartridge, presumably it has a temperature equal to the graphite?

CHAIRMAN Well, there's air going over them.

HARDY There is air going over them and if there is no heat release in the cartridge, they would be quite cold.

(No definite figure was quoted here)

KAY But the maximum: suppose there was no effective air flow, what would be the equilibrium of the graphite?

HARDY If there were no effective air flow it would be in equilibrium with the graphite.

KAY Do you know what that is?

DAVEY I should say approximately 280-285 but I will verify this.

HARDY There is a possibility of in-leakage of air from other channels when the exit temperature of the air from the fuel channels is 160.

CHAIRMAN Does anyone wish to ask questions to follow up a point on the evidence so far?

DIAMOND When you say that in your opinion the A.M. cartridges are safe for one year at 200°C, is this based on a diffusion rate?

HARDY It is based on an examination of the diffusion at higher temperatures in extrapolating down to 200.

DIAMOND Does it mean that after one year at 200°C, penetration would be right through?

HARDY That I could not answer off hand, but looking at the figures and knowing that the rate of diffusion goes down, I would say was safe at 200, but to answer your question I would have to put some numbers into it.

DIAMOND You feel there would be some penetration at 200 in one year?

HARDY If you have a good clean interface between the two, there may well be.

CHAIRMAN If you have some more things to tell us will you please proceed.

HARDY Tests have been carried out on the oxidation and ignition of solid pieces of A.M., and at a temperature of 400-425°C I have results of thermal balance studies showing that the rate of oxidation at both these temperatures becomes

(these are weight gains plotted against time in minutes) very rapid after something like 100 or 200 minutes, then ignition can occur ...?... temperatures 450-465. If we had a liquid alloy such as you get when the cartridge penetrated ...?... have an alloy containing magnesium aluminium and lithium at a temperature as low as 440, then I would certainly expect that that could take fire, and we had one that did take fire at that temperature of between 450 and 470. In some cases the oxidation is stifled. These have not actually caught fire, but it looks as though the oxidation had stifled the egress of the metal, but in other cases fire has certainly occurred and this seems to be exactly what one could expect, particularly if you have a molten cone where a new surface can be continually exposed, perhaps by some disturbance. I was going to turn now to the data available at the time the A.M. cartridges were proposed for Windscale, and this is contained in two Minutes, the Thermal Reactor Cartridge Working Party minutes. This is the 17th meeting on 19th November 1954, and the meeting reported to the 18th meeting of the Thermal Reactor Design Committee on December 2nd 1954. At this time there were four schemes under consideration for the irradiation of A.M. These were an A.M. alloy in isotope channels with hollow 1.24 Co. uranium bars in normal channels, an A.M. alloy in isotope channels, and solid 1.24 Co. of smaller than normal diameter than standard in normal channels, and an A.M. alloy inside a hollow 1.24 Co. uranium bar in normal channels and the third one, an A.M. alloy inside the graphite boats of 1.24 Co. uranium cartridges in normal channels. I have mentioned these possibilities even though they were not all taken up, because it is necessary to consider what was believed to be the operating temperature of A.M. in these different cases. For the case which was accepted, an A.M. alloy in isotope channels, solid 1.24 Co. uranium of smaller diameter, than standard in normal channels. The estimated A.M. alloy temperature was given in these minutes as 130°C under normal operating conditions with an asterisk indicating 350°C during Wigner release. I think the only other temperature we need consider is that if A.M. alloy were inside a hollow 1.2 Co uranium bar, where the A.M. alloy temperature was estimated to be 420°C, and at that time it was considered that an A.M. temperature of 350°C for one day a year during Wigner release could be tolerated, but 420°C for the entire life would require special care and seemed doubtful. This was a statement made by Mr. Grainger in the Thermal Reactor Design Committee. Dr. MacIntosh, in confirming this view said that the compatibility of aluminium with magnesium was poor at 400 owing to the incidence of eutectics. The magnesium would probably combine with the alloy to form a homogenous mixture and the product would be lost. It would be equivalent to starting without a can. Mr. Wild also thought that the product would fuse out, and then the remainder of the discussion did not consider any further metallurgical implications, but the scheme in which the estimated temperature of the A.M. during normal running was 130 and 350 during a Wigner release, was accepted.

CHAIRMAN What was the date of all this?

HARDY During the last quarter of 1954. Meetings were held in November and December. This is report TRDC M18. At the end of 1954, the T.R.C.W.P. which was considering

A.M. cartridges ceased to report to the T.R.D.C. and on this subject reported to the Works General Management Committee, and about that time also, a special sub-committee was set up to consider the production and manufacture and the reactor facets of A.M., and the problems that were considered in particular were the reactivity changes if A.M. cartridges were blown out of the pile, and it was this sort of conception that undoubtedly led to the incorporation of the lead. The design which I believe was under consideration at that date was not of course the Mk.III design because that was designed and manufactured for the first time during the last quarter of 1956, and I believe the reason for the change in Mark was to obtain a larger production of A.M. Although lead was incorporated in the Mk.II design, which was probably the one under consideration in 1954, and was absent from Mk.III design, I would consider that the lead only gave a marginal improvement in the safe operating temperature of the cartridge. I was going to turn now to uranium bars, if there are no questions immediately on A.M., and I shall be coming back to total heat released on combustion later.

DIAMOND If this A.M. alloy became liquid in the middle of the bar but not at the outside, what effect would it have, would it crack the bar?

HARDY No I do not believe it would crack the bar, because the alloy would be quite plastic. It would lead to a release of gases. The gases normally held within the solid would almost certainly be released but these aluminium cans are very plastic at temperatures of 300° and above and I would expect them to expand but not necessarily to burst. You realise that in order for A.M. to be melted inside the bar it has to be at a temperature of 590 or thereabouts.

SCHONLAND You told us that this meeting was in the last quarter of 1954 at which it was considered safe for the A.M. cans to be kept at 350 for one day during Wigner release. Is this the last occasion when the matter was given committee consideration?

HARDY We have examined the minutes in the last few days of several committees and it is the last one we have been able to trace. Our examination has not been exhaustive, I cannot claim that. But it has been the last one we have been able to trace.

SCHONLAND We have information before us which must have been available to you that in November 1956 the maximum graphite temperature reached was 420. Did you know this?

HARDY I was not aware of that.

SCHONLAND Would you normally be made aware of it?

HARDY No, I would not be made aware of it officially.

SCHONLAND If you had known would you not have drawn someone's attention to it in view of the decision that 350 was the safe temperature?

HARDY I cannot honestly say I would have drawn attention to it in November of last year, but I am quite sure that I would have drawn attention to it during this year when we were

considering the design of A.M. cartridge for Calder because that was to have operated at a higher normal temperature than the one in the Windscale Piles and we were considering the upper temperature limit of that cartridge.

SCHONLAND Are you a member of the Technical Committee which is responsible for running the Windscale Pile?

HARDY No Sir. I am a member of the Technical Committee of the Calder piles.

CHAIRMAN Will you please proceed with the uranium cartridges.

HARDY There is a reaction between uranium and aluminium when the two metals are heated in contact. You get diffusion, inter-diffusion and the formation of an intermetallic compound, and it is in order to stop this reaction that a graphite interlayer is provided on the bar in the can during the manufacturing process at Springfields. This allows the cartridges to be operated in the Windscale Piles at the normal operating temperature up to 395° I believe. Failures do occur due to breakdown and flaws in the graphite interlayer and leading to uranium atoms being carried to the outside by diffusion. This burst cartridge is then picked up by the scanner gear and I think that each channel is examined every 90 minutes. A channel containing a burst cartridge is discharged and on examination of the burst cartridges one has uranium oxide present but the amounts of oxide are not large in proportion to the amount of uranium. In tests carried out at Springfields of uranium metal inside aluminium cans we have heated in the course of our experiments cartridges at 400, 450 and 500°C. The furnaces containing these cartridges have been examined periodically, the test are of course being carried out continuously 24 hours per day and the furnaces are opened up periodically either to examine the cartridges or to see that they are still satisfactory, and we have had cases where defective uranium cartridges have oxidised completely in between the periodical examinations. In the case of 500 this complete oxidation has occurred in less than 8 hours. In the case of cartridges heated continuously at 400, the complete oxidation has occurred in less than 12 hours. This has not occurred at temperatures of 350 and all production cartridges are given a 48 hour period at 350 during the course of their manufacture, and although in the many thousands which have been produced there must have been some defective cartridges going through this treatment, no fires have been reported at that temperature. The oxidation of uranium; I have results of thermal balance studies and the oxidation becomes rapid at some temperature about 350. Here again we have weight increases plotted against time in minutes for different temperatures, and at these temperatures, 300, 350, 370, and at higher temperatures it is increasing continuously, so you can in fact probably get self maintained oxidation at some temperature of 350 upwards. Oxidation of this nature would only occur of course if there were a burst cartridge in the pile, and although I have said we have fires or complete oxidation of experimental cartridges in periods of hours, such cartridges have run successfully for periods of 200 hours at 500°C, perhaps even longer – considerably longer than that at periods of 450. So we have the evidence from Windscale that although there are cartridges in the pile at 395, if they are pushed out within a period of one or two hours, then

oxidation is not severe, but at out-of-pile tests we know that the complete oxidation of the cartridge at a temperature of 400 can occur in a period of 12 hours.

CHAIRMAN When you measure these rates of increase of weight, and must get the oxidation rate, you supply air at 350.

HARDY Yes.

CHAIRMAN And you supply a good current of air?

HARDY No, it is not usual to supply a good current of air. It is normally done in fairly steady air.

CHAIRMAN If we have only to look for temperatures of 300 to 350, well, we have got them; if in the pile there was a burst of exposed uranium then we have got conditions which are not like your experiment. The air flow has been shut off, it is a thermally insulated place and has just a little air, it is still very slow. So if it was only at 300 it might just rise and gradually get a little warmer after a period of a day.

HARDY Yes. Of course the frequency of burst cartridges is not large and you would not expect probably more than one.

CHAIRMAN (interrupt) But one would be (*silence implies one would be enough*).

HARDY I have calculated heat releases of the complete oxidation of the uranium in a mark 10 cartridge. It is 2,300 K calories per element, per cartridge. This is the heat release on forming UO₂ from the uranium in a Mark 10 cartridge. That is the smaller one, the enriched one. It is 2,900 kilo calories for normal Mark 6 and for the aluminium can the formation of U₃O₈ it is 1100 kilo calories per can.

CHAIRMAN At what temperature does aluminium start to burn?

HARDY Aluminium does not normally burn. It would be well above the melting point. It is normal industrial practice to melt aluminium in air. It forms a tenacious oxide film, but it does not burn.

DAVEY Bearing on the uranium side of the story it might well be worthwhile to look over some of the records, because I think we would find that an incipient burst in a uranium cartridge has developed during the shutdown and has been spotted immediately by the scanner gear.

CHAIRMAN But the scanner gear wasn't working.

DAVEY Sorry, spotted by the scanner gear when the pile is being brought up. I think this could be significant because it may be possible that an incipient burst uranium cartridge, or one with a very small hole has been lying in the pile under shut down conditions, has given rise to nothing untoward, but has been found within a hour or so of starting the pile up.

CHAIRMAN Will you just check my memory, did we not hear about this from Mr. Gausden.

DIAMOND What I am trying to demonstrate is that in terms of the uranium cartridge it may not be dangerous under shut down conditions to have an incipient burst or a small burst in a uranium cartridge.

CHAIRMAN No, that is so but if by something we do not understand, there has been a fairly serious burst of a cartridge you would not have detected it until the stack.

We have seen this in the log book – they saw something at 05.30 on Thursday morning.

DAVEY I am bearing in mind your temperature rise on the the previous day.

CHAIRMAN That's right. That's exactly what I have in mind.

HARDY You will realise that of course I am reporting the more severe incidents. Numbers of uranium cartridges have been exposed at Springfields that have not caught fire in this way over these periods of time. There is a considerable variability of behaviour.

CHAIRMAN We must not make too much of this, but it's something we must think about.

HARDY In the heat of combustion of an A.M. rod, complete combustion gives 1250 kilo calories per cartridge, and the aluminium can – Mk. III – 370 k c/can.

MR. ROSS Could we just check, you said 1100 for the uranium cartridge.

HARDY Yes.

MR. ROSS So there is .8 kilo calories in the fins approximately.

HARDY Yes, I think the evidence I have presented is the conclusion (this is of course for your consideration) that a uranium cartridge could have oxidised completely during a shutdown, but that once you reach a temperature of 440 or thereabouts a number of A.M. cartridges would fail and you would expect quite a number of these to oxidise at a considerable rate.

DAVEY I could not agree with that as a statement.

HARDY This is my opinion, Mr. Davey.

CHAIRMAN Let us record Dr. Hardy's opinion. He is entitled to that. I think that what you would say then is if there were no burst uranium slugs, no cartridges of uranium burst, then they are all right at 400 or so, and if we are looking then for something which started the trouble, the A.M. cartridges are a possibility, but the temperature of that cartridge has got to be above 400 certainly, before anything untoward could happen. In fact you might go a little further and say 450 if only for a few hours. Is that somewhere near what your views are?

HARDY Yes, that is very close to my views.

SCHONLAND As I understand it, he is picturing the uranium cartridge burning or oxidising and simultaneously or subsequently, a Wigner release, is that right?

HARDY Well this would be during a Wigner release, because the evidence shows that such cartridges are picked up during normal operation.

SCHONLAND Have you made any calculations showing that if you had a complete channel or uranium fuel elements and they were all oxidised, that the neighbouring isotope channel would rise to a temperature that would cause A.M. trouble.

HARDY We have made very rough calculations of the temperature to which the graphite would rise in the surrounding channel and I do not think the graphite temperature has to rise very much before you reach self-sustaining oxidation of the graphite.

SCHONLAND I am asking whether you imagine complete oxidation of the fuel element channel, and you have calculated (at least roughly) that you would have oxidation of a neighbouring A.M. isotope channel. Would you now go one from there. You now introduce oxidation of graphite.

HARDY May I just tell you, we have been trying to calculate the temperature the graphite would reach if the neighbouring channels remained at a temperature of 400°C. Supposing we had a burst cartridge oxidising in one channel and we maintained the surrounding channels at 400°C by Wigner release and there was sufficient air flowing through them so that the temperature does not exceed 400°C, then the graphite loses heat to these channels by conduction but the graphite temperature can easily be envisaged going up to about 650°C at the surface.

SCHONLAND This is without taking into account the oxidation of the graphite.

HARDY This is without taking that into account.

?DIAMOND Could we be even clearer than that.

DAVEY You visualise the temperature of the graphite going up to 650.

HARDY May I make it clear that I am not an expert on heat transfer. I asked for these calculations to be done at Springfields and these are the results. I am sure that more accurate calculations have been made at Windscale by people better qualified.

CHAIRMAN I am not sure whether it is right to press Dr. Hardy on this. He has given us his opinion, but I do not think he is an expert on these things.

KAY We should put these questions in this field to Mr. Hall.

SCHONLAND Dr. Hardy gave us this evidence and I am entitled to ask how he reached his conclusions. I don't want the details, just how he reached the conclusions.

HARDY We had it calculated.

SCHONLAND Let me just finally ask... you spoke of oxidation... information before us yesterday, I think, was that it was unlikely until you reach considerable temperatures... it starts about 500.

DECLASSIFIED

CHAIRMAN This is from what Dr. Wright said? Yes.

CHAIRMAN These were all new cartridges.

SCHONLAND I think that is what I was after.

HARDY I just have one general proposal I would like to put for your consideration and that is that each fuel element design, when it goes forward for manufacture should be accompanied by a certificate defining the operating conditions for which the cartridge was designed, the expected mode of failure if these conditions are exceeded, and it should be incumbent on the Design Committee to report new information to the Operating Committee as it comes forward, and on the Operating Committee to go back to the Design Committee if the operating conditions differ from the design conditions which were used in the design of the cartridge.

CHAIRMAN Why do you say this Dr. Hardy?

HARDY This is to my mind my lesson from this incident, and we have at the moment a Thermal Reactor Cartridge W/P which is responsible for new fuel element designs. This covers the Calder cartridges and the Windscale cartridges. I am a member of this Committee and also a member of the Calder Technical Committee, and this cross linkage has proved absolutely essential. I am sure that the same considerations apply to other cartridges, that if it were written down in this form then it would be formalized.

11 a.m. 18th October

WITNESS: MR SADDINGTON

CHAIRMAN We have been hearing evidence this morning about the A.M. cartridges, eutectic temperatures, penetration of the aluminium cans, and I understand you may be able to help us in this field. Is that so?

SADDINGTON Yes, in the sense that I have been looking into the question of the temperatures at which the A.M. cartridges might well fail, and then turning my attention as to whether those conditions could have been reached in the present incident and have been a possible cause of it, and if so, why in fact it did not occur in the past.

CHAIRMAN The best way to proceed is to let you start by giving an account of this work.

(Mr. Saddington referred to a summary).

CHAIRMAN Before you start for the record would you just say what is your job.

SADDINGTON I am Deputy Head of Laboratories of R&D branch, located here at Windscale.

I started with the results that were obtained over the weekend at Springfields, on experiments put on to establish how easily A.M. cartridges might fail at various temperatures and for various times, and those results you will see are indicated broadly in the table and the general tenor of them is that over 440° you can expect the cartridges to fail by the mechanisms of which Dr. Hardy has probably told you. The time in which they will fail depends as you will see on the temperatures to which they are exposed. On the consequences of failure the metallurgists advised me that the magnesium is almost certainly bound to catch fire, whether immediately or after a period of time depends upon the thermal contact.

SADDINGTON These were unirradiated cartridges and so we have evidence therefore of failure of such cartridges at temperatures above 440° if they are exposed to the necessary length of time.

May I turn your attention to Pile 1 in this present incident and the examination of the thermocouple readings, and as you will see we have determined the thermocouple reading – the maximum graphite reading of 420° – this lasting for 3½ hours at a temperature above 400° for 17 hours. Now, if we are to take the thermocouple readings as being completely accurate then we have not quite reached the conditions of the Springfields tests. Thermocouples can vary – they are not completely accurate, and of course the number of thermocouples in the Pile does not give you a complete picture of what is happening throughout the Pile and so I considered that it was quite possible that there could be a hot spot of the region of 440° minimum which then gives us a temperature consideration linking up with the Springfields tests and the length of time again if we take 3½ hours above 420° as being 3½ hrs above some temperature around 450°C – we then bring that into the Springfields tests. There is a possibility that conditions during this present Pile incident approached those which Springfields tests have been shown to cause the failure of the cartridges.

CHAIRMAN You say that the temperature did reach 420° for 3½ hours. When did that actually happen?

PIERSON It is marked on Chart 5.

CHAIRMAN It is the graphite temperature and it occurred when the dampers were opened.

SADDINGTON That is not the right chart. It is Position 10.

KAY On Channel 21.48 – temperature reading 400°?

CHAIRMAN At what time was that?

SADDINGTON On Wednesday at 15.00 hours.

CHAIRMAN At 15.00 hours on Wednesday, Channel 21.48, the graphite channel reached 400°. The one you really call our attention to is 21.48, and 20.33 on the following day.

SADDINGTON Both as possible temperatures.

DIAMOND You have called attention to 21.48 – for what reason?

SADDINGTON That reached the 400 mark.

KAY I think we have to point out that 20.53 reached 400 on Wednesday evening at about 20.00 – in fact it stayed above 400 for nearly 20 hours.

SADDINGTON It was actually 17 hours from the thermocouple readings.

CHAIRMAN Yes, thank you. And you are suggesting that if two thermocouples reach 400

(a) that the thermocouples may not be all that accurate, and
(b) that there may be some other places which may be hotter than that which have not got thermocouples.

SADDINGTON Yes.

CHAIRMAN But to do any damage according to your figures. – of course these are statistical and there are not many tests – but you would seem to suggest that even at 425° these cartridges are alright for 60 hours?

SADDINGTON Yes. This is so. It should be pointed out that these tests cannot be claimed to reproduce conditions exactly as they are in the Pile. Other tests are being carried out to stimulate them more. Whether a cartridge fails and fires will depend very much on the degree of contact of the cartridge with the graphite boat, on the shielding, the mass of the graphite around the particular channel in order to save heat escaping, etc.

CHAIRMAN Have you considered the difference between a cartridge which has been exposed and therefore has inside some tritium and some helium and these new ones? Is there some difference to be expected do you think?

SADDINGTON The difference is that in the irradiated cartridge, if it has gone to full irradiation – 300 MW days per ton – the amount of gas can be between 200 and 600 m³l; the spread is dependent on the position in the pile – whereas in the unirradiated cartridge the amount of gas could be between 50 and 150 m³l per cartridge.

CHAIRMAN How is that compared with the spacers? Is this giving a pressure of atmospheres?

SADDINGTON We have an experiment on at the moment to establish this. The point is that you cannot argue at the moment that all this gas is in the free space and creating a pressure. A great proportion of the gas is held within the alloy for long periods and then in any case there will be an equilibrium pressure built up which will put a limiting on. In one test which we did at Springfields on an unirradiated cartridge heated to 450°C for something like 15 hours there was no failure of the cartridge but there was definite swelling of the cartridge. A similar cartridge which we had definitely pin-holed to see whether there was any difference – that did not fail either and there was no swelling. We are looking at this to see whether in fact a gas pressure can build up which having reached the point when the eutectic could be formed, could in fact, take one from that sort of effect to pressure behind it and split to something like this sort where the incidence of fire is possible.

CHAIRMAN Have you yourself given any thought to the source of heat within this cartridge itself – an exposed cartridge. It has got tritium in it which is a radioactive element? At a guess that source of heat is very small, but we are looking for something which will lift the temperature only 20° or so. Have you done this yourself or not?

SADDINGTON This has been calculated. Mr. Hall may be able to give you the calculated figures.

CHAIRMAN If you haven't done this yourself we will ask Mr. Hall.

SADDINGTON The rest is pointing to why perhaps if you take this as a real possibility in this case, why perhaps it has not occurred before, and it is indicated that only on one occasion have we had a temperature of over 400° on a previous anneal – in November 1956 on Pile 1.

CHAIRMAN When it went to 420?

SADDINGTON Just up to 420 and down again. I put in for a period of less than a quarter of an hour. It went up and straight down again.

CHAIRMAN How long did this stay at 400°?

SADDINGTON 7 hours at 400.

CHAIRMAN So it was only at 420 for less than a quarter of an hour, and 400 for 7 hours. You got that by looking at the temperature records of that occasion, and at that time, of course, it was not the present Mark III cartridge.

SADDINGTON No – Mark II.

CHAIRMAN There were none of the present design at all?

SADDINGTON They were actually loaded after that anneal in December. To go to a comparison with Mark III, the only occasion on which the Mark III cartridge has gone through a Pile anneal was in July of this year on Pile 2, and in that case the maximum recorded graphite temperature was 385°.

PIERSON This document must be wrong then because it talks of a maximum graphite temperature of 305°, and a maximum uranium temperature of 385°.

SADDINGTON I am not sure which figure will be correct, but I think it is immaterial (although we need to get the record straight) for this particular point – 305 or 385 – in the sense that it is nowhere near 440°.

KAY It would be nice to know which is the correct figure.

PIERSON 385 is nearer than 305.

SADDINGTON This can be checked very quickly.

CHAIRMAN Are we talking about Pile 1?

SADDINGTON Pile No. 2, July 1956. It may be that I have given the metal temperature: I asked for the graphite temperature. The point is applicable to my way of thinking that in any case, the Mark III cartridges which went through a previous Wigner annealing were at a temperature significantly below 400°.

CHAIRMAN Right. And equally the earlier mark of cartridge did have 7 hrs at 400 on the occasion of November 1956.

SADDINGTON This is why in my own mind I am linking three parameters: time, temperature and possibly can; the can being perhaps of lesser weight than the other two. In November 1956: temperature - we may have had the temperature but not the time. In July 1957, for this particular type of cartridge, we have not had the temperature, so I haven't even considered the time.

CHAIRMAN But you are still some 25° short of explaining why the cause was an AM cartridge. You have no suggestion to offer where that extra 25° came from other than it was something that escaped observation on a thermocouple not recording right, or the high temperature not being where a thermocouple was.

SADDINGTON Yes, where you have a pile with Wigner releases in different parts it is possible to have a vast difference.

CHAIRMAN Do the members of the committee wish to ask questions?

SCHONLAND Were you aware that the release in November 1956 reached 420 for a quarter of an hour and was 400 for 7 hours. When were you first aware of that?

SADDINGTON When we looked at the records over the weekend.

SCHONLAND Not at the time of the release?

CHAIRMAN Do you mean last weekend or a weekend in November '56?

SADDINGTON The information was there in the records, but in order to follow up my own thoughts as to where it might have happened before, or why it didn't happen before I called for it the weekend after the incident.

SCHONLAND But you will agree that this temperature of 400 is far above the 350° during Wigner release which appears to have been the limit set by the Thermal Reactor Design Committee at the end of '54. You may not know that.

SADDINGTON I do know this. I actually got out these Minutes to check this one. My understanding is not that the Thermal Reactor Design Committee set a limit of operation, but that according to the Minutes, it was indicated that a temperature of 350° was the maximum release temperature which would be achieved and that they took that figure and considered that figure safe. Can you see the difference I am making?

SCHONLAND Then no one fixed a maximum temperature?

SADDINGTON A maximum temperature has only been fixed in the sense that the instructions for Wigner release indicates what action should be taken at 360° and 400°. There is a record of this in November 1955, a written record. We have tried without success so far to find an earlier one, bearing in mind that Wigner releases have been going on for many years, this may be where it is a little bit vague because the earlier Wigner releases were carried out in actual fact by people from Harwell because it was considered to be something which had to be done under expert supervision.

Whether there was a record of how they did it at that time I do not know. I can only for myself, think that the November '55 operating instructions have been drawn up for when it is considered that a Wigner release can be carried out as an ordinary operating procedure.

KAY I would like to ask if we can see these instructions.

DIAMOND These are Mark III cartridges and these tests were done at Springfields. Have you done similar tests at Windscale?

SADDINGTON We have done tests on two cartridges at Windscale and in neither case at the temperature of 450 did we get failure.

DIAMOND You tested two.

SADDINGTON We have tested two. We actually fixed up a simulated channel without graphite blocks round it, with air passing over it.

DIAMOND You have done two cartridges at 450 and they have not failed after.....?

SADDINGTON They were on for something like 17 hours. One was pin-holed and the other was not.

DIAMOND One was deliberately pinholed. What diameter hole?

SADDINGTON .03" roughly.

DIAMOND And that was done recently?

SADDINGTON Yes, last week.

DIAMOND Have you any more in running?

SADDINGTON We have already set up and they may have got it running, in which we have built up graphite blocks. In order to more simulate the conditions we shall put fuel elements in appropriate holes and cartridges in actual isotope holes and continue again to try and get a more representative picture. This is in addition to the experiment which we shall be doing on an irradiated cartridge to see just what actually happens with that and to determine whether there is any pressure build up.

DIAMOND So these tests have taken place at a higher temperature than some of these.

SADDINGTON Yes.

CHAIRMAN Mr. Kay - have you any questions?

KAY I have no other ones to put.

CHAIRMAN Is there anything else you wish to call our attention to?

SADDINGTON You did mention the differences in the cans. For my part I consider it to be secondary in the sense that if this is considered to be a cause, I don't think one would lightly say if we went back to Mark II cans we should be perfectly alright having got away from III. It is true there is a double skin of aluminium in effect in the Mark II and in the

proposed Mark IV which is to take the place of this. It is true that there is the lead which has been shown to have a retarding effect on the attack on the aluminium of the alloy, but I think this is secondary.

CHAIRMAN Well, thank you. I think that is all.

WITNESS: Mr. Hall

CHAIRMAN Mr. Hall – do you know the Members of this committee?

HALL Yes, thank you. I am in charge of the Experimental Engineering Laboratory in R&D Windscale, particularly with regard to heat transfer and fluid mechanics.

CHAIRMAN We have been hearing evidence, Mr. Hall, yesterday and today which has several times brought up heat transfer problems particularly with regard to the AM cartridges and possibly also in respect of other isotope cartridges or uranium cartridges and it is on that sort of thing that we want to question you. Perhaps I should start by illustrating the sort of questions we are interested in and then you would be good enough to tell us in your own words something about those matters.

Now, in the isotope channels the cartridges are just cylindrical – there are no fins on them – and the air space is rather small. During the shut down of the Pile for long periods there was no pumping going on, so that the only air flow was due to convection due to the fact that there must be some air there. We have got to the position where we know that the AM cartridges are very dangerous at temperatures in excess of 400°. We don't know just quite where, but 400 is perhaps alright; 425 is perhaps getting dangerous, and we are therefore in the position of studying rather small differences of temperature because the graphite during the Wigner release was 350, in fact some of them went to 400. Therefore we are interested in seeing how heat transfer affects temperatures during this period of shut down when the temperatures anyhow were 350 to 400. Have I explained the sort of things we are after well enough?

HALL Yes, I think so. I have concentrated rather in calculations I have done on the consequences of combustion of for example an AM cartridge. The problems of their temperature whilst in the Pile in the shut down conditions – I have not the information on heat release under these conditions but by analogy with the fuel elements. I would have thought that this difference would have been very small indeed. For example the fuel elements under shut down conditions about 3 days after the shutdown might give you a temperature difference of 10 or 20° between them and the coolant.

CHAIRMAN 10 and 20°. Is that when there is pumping or not?

HALL That is with no pumping – just natural convection in the channel.

KAY With the shut down fans off as well?

HALL Yes, but with a certain amount of convection.

KAY Due to the stack and normal flow of air?

HALL Within the core itself. It is rather difficult to be specific about flows under this condition because of course the channel is gashed by Wigner growth slots every 8" along its length.

CHAIRMAN Are there any instruments in the Pile or in the stack which give us a clue as to what currents of air do exist in these circumstances?

HALL No – the only indication is out of gas temperature, but I think this might be misleading under those conditions, because under those conditions without the shut down fans working and the dampers closed, and with the chimney vented then the circulation within the core may well be more important than a net flow through it.

CHAIRMAN Coming back to the information you have given us. The fuel elements – three days after shut down, might be something like 10 to 20° hotter than their immediate surroundings. Does that mean that after one day it would be three times as much as that?

HALL It would be in proportion to the decay curve. I could not say from memory.

CHAIRMAN Over the first day or within the first day you would expect temperatures of 40° to 50°. Is that the sort of temperature?

HALL It is rather an awkward one – natural convection from such an awkward shape of surface with fins pointing down towards the bottom.

CHAIRMAN And does that apply equally to complete natural fuel elements and to the enriched fuel elements?

HALL Yes.

CHAIRMAN (It must depend?) on the fission products that are there plus the slightly different design of fins or size.

HALL Yes – there is little difference from the exterior.

CHAIRMAN And you say that you have not given much thought to the cartridges which are not the fuel elements?

HALL What thought I have given to them is in getting an idea of the amount of heat they can release and their effect on the associated graphite.

CHAIRMAN All of them are there for the purpose of making something which is radioactive and that of course gives out heat by natural decay. I am not sure whether you are the right man to do this but I think it is one of the things we would like worked out.

HALL The temperature under shut down conditions.

CHAIRMAN It is a temperature difference really. The local temperature is something and this is in stagnant air current, no coolant to speak of, with air space, and we would like to know how much hotter the cartridge is than its surroundings, due to the generation of heat by radioactive decay of the components that are in there. And there is also the point, I think you made Dr. Schonland, that some of these things capture gamma rays very strongly and get a little enhancement from that.

HALL I will get an estimate made of that.

CHAIRMAN When you have left us, will you be able to do some arithmetic on this and when you are ready to tell us, would you be good enough to let us know.

DIAMOND May I ask Mr. Hall to include in his calculations the maximum temperature of the A.M. material and the can contact, because we understand that the tolerance on the can ID is in the range of 1.006 to 1.008, whereas the O.D. of the A.M. is .990 to 1.000, so presumably there is a line of contact.

HALL You would like the temperature of the can at that line of contact.

KAY May we be quite specific whether we are asking simply for A.M. or are we asking Mr. Hall to consider the possible heating effect in other cartridges in L.M. or cobalt?

CHAIRMAN I think we should ask him, or get advice, which of these materials is the greatest producer of heat, and work it out.

KAY I think we would like A.M. in any case.

CHAIRMAN There are others, for example there is a cobalt and, of course, now whether that is the same street or not, I don't know.

KAY I think some arithmetic is called for, even if only to rule things out.

HALL I should have thought that here you could discount all these from the point of view of heating up their surroundings. I think the important thing is whether the A.M. can itself could raise itself above its surroundings.

KAY Even though of course it may be obvious to you, I feel that figures which establish that would be helpful.

HALL I have some information on the sources of heat if you would like to have that to look at Mr. Chairman.

CHAIRMAN Yes, thank you.

CHAIRMAN Any more questions?

I think the next point we might cover, Mr. Hall has given us some fundamental figures. They are all quite clear and I do not suppose there are any questions about them.

KAY I should like to digest these over the next few hours and come back perhaps.

CHAIRMAN I think we will just accept these without questioning Mr. Hall about it at the moment.

We will now turn to the other point: you said you had done some calculations about what would happen if an A.M. cartridge burned or started oxidising, heat release and so on.

HALL I think these figures can be used to give you some idea of what would happen. I am taking a situation where, for example, a whole line of A.M. cartridges gives to the associated graphite, and by that I mean either the eight inch lattice that goes with it or possibly the whole graphite block because these blocks are vertical and cover about four channels, and they are fairly well isolated from the surrounding graphite. If one takes the associated graphite with the channel and puts the heat from the combustion in the A.M. into that graphite, then, taking the figure for A.M. plus its can total of 1.62 K.W. hours per cartridge, and assuming that this is released in half an hour, then the associated graphite, that is the eight inch square block associated with that particular channel, would rise at the rate of 430° per hour.

Now, the other figure, one might decide that in effect one ought to take the whole vertical block which covers three or four channels, which should bring it down to something like 100° per hour.

CHAIRMAN Now may I ask you just what you mean. You have got a source of heat from the burning and you have a certain 'heat-sink?' on the graphite and it is released in half an hour, does that mean you are neglecting thermal gradients. You are assuming that the whole thing is at the same temperature.

HALL That is my assumption on the grounds that the block is sufficiently well insulated by the gap surrounding it.

CHAIRMAN But at some stage the graphite must be much hotter near the point of burning than further away.

HALL Yes, the graphite at the surface of the channel will certainly be very much hotter. I am working on an average temperature of the block.

SCHONLAND This is not taking into account any question of oxidation of graphite.

HALL It is not. I think in that context it is of interest to look at the figures I have put in in ink under oxidation of graphite in terms of K.W. or Watts released per channel and comparing those with the Wigner release from the air.

CHAIRMAN You have neglected the third dimension in this.

HALL Yes, I suggested that first one took a whole row of these.

CHAIRMAN It would not start by that, it would start with one and you think that even for one this gives you some idea that temperatures must be going up very high.

HALL Yes. I would regard this as giving you useful information about the surface of the block surrounding this element, not upon the surface of the channel in which the element is placed. It gives an upper limit of what is likely to be happening on the surface of the block.

CHAIRMAN You have said it is a good approximation to assume that that is insulated from the next block of graphite, but of course if this thing is to spread, it has got to go from one graphite block to the next. Have you estimated how a fire in the one channel could raise the temperature in the nearest A.M. channel?

HALL The only information I have to, ? I think the difficulty here is it is not a homogeneous block of material, there are these gaps. Therefore I think the simple conduction solution would be quite meaningless, for example if you take a certain temperature gradient through the block and make an estimate of what its conductivity is, allowing for those gaps, and if affected by the temperature gradient, this then gives you a heat flux through the stack. You can also work out what heat you could remove by convection with a similar temperature gradient and a certain flow. Well if you do that the flow you need in a fuel channel to counteract the effect of this conduction through the stack is only of the order of one or two centimetres sec. So I think quite small gas velocities could be very important in this context.

I feel that the simple conduction solution, even allowing for the reduction of conductivity due to the gaps, is rather meaningless. But there is some practical information here, that is the way in which a Wigner release normally spreads, and I think that Mr. Bell could probably give you a better opinion on this, but it seems to be of the order of tens of hours to spread through the whole stack. But here it is slightly different in that you are triggering off a release, you are not relying on conduction. You are effectively reducing the specific heat by having a heat source within the graphite.

CHAIRMAN I am sure this interests you Dr. Schonland, do you wish to ask any questions.

SCHONLAND No thank you.

KAY I should like to ask whether he would think it worthwhile to attempt possibly a rather more detailed analysis of a possible sequence of events. Taking this, I would tend to start with the situation of the graphite temperature being at 400 degrees and the first point that would be of interest was the point we mentioned earlier to consider the possible nuclear radiation decay heating of say, an A.M. cartridge, and to establish what temperature differences applied, other things being equal, in thermal equilibrium with the graphite. But there was this internal heating, how much additional temperature lift would that give, that is the first point.

The second point, supposing we get to a temperature at which failure, oxidation burning of the A.M. cartridges, occurs. Could you do the transient heat transfer examinations? I know you must make a whole lot of approximations in this, but to consider this cartridge burning and the heat flow from

that across to the surrounding graphite and then through the graphite to neighbouring channels. Even with a certain amount of guessing, if we could spend a few minutes on this, I think it might be valuable to have some idea of rise of temperature with times in the next channel at the surface of the graphite. I appreciate that this is a difficult calculation.

CHAIRMAN It is impossible.

KAY Knowing Mr. Hall's skill in heat transfer matters, I feel sure he would be able to make a better approximation than anyone else.

HALL I have some calculations going on at the moment on this. But it does very much depend on vertical direction.

KAY I realise that this is a rather unreasonable request. The second request I'd like you to consider is that of a uranium cartridge which has had a bad burst and has not been detected and has begun to oxidise rapidly and is acting as a source of heat; and a similar problem, but with a uranium cartridge burning in the channel as a source of heat instead of the isotope cartridge burning in it, with such reasonable allowances as you can make in the two cases with regard to the air gaps and the transfer of heat across the gap. I am only too well aware of the difficulty, but I think even an attempt would be helpful.

CHAIRMAN How long will that take, is it a week's job, or a day?

HALL No, I think to go as far as we can go with the assumptions we shall have to make it will be a matter of a day or two days.

DIAMOND I didn't realise when you asked me that you have been dealing with a specific point when I was out of the room. I have two general points.

CHAIRMAN Mr. Hall, you will do these calculations. We must not set a time, it is unreasonable. I think we will ask you to let us know when you have got some answers, even if this can be done in rather a rough way.

KAY Did I make the two points clear?

HALL Uranium oxidising and A.M. oxidising, plus the earlier one on decay.

DIAMOND One or two other things. The first is graphite temperatures under normal running conditions.

HALL They are almost identical with the coolant temperatures, the inner temperature being of the order of 20°C and the outer temperature ...?... hundred and 60. I am speaking of my own knowledge of this...

ROSS Mr. Gausden is bringing all this information in as the next witness.

DIAMOND What I am interested in is the temperature of graphite most remote from any cooling surfaces; right in the middle of the block.

HALL And remote from an isotope channel?

DIAMOND Anywhere, the hottest temperature in the graphite.

HALL I am speaking from memory of a report I have read, it is of the order of 10 or 20 degrees hotter than the gas temperature. This is due to heat transfer at the surface of the channel to the coolant ...?... gradients of the graphite, but this work was done when there was a certain amount of doubt on the conductivity of graphite. If you like I will ...?...

DIAMOND The next one is the maximum temperature to which the uranium may rise under normal running conditions with the most adverse setting of control rods.

HALL It is a question of variation. I think this is again a question of the operating conditions since the reactor is run on what is thought to be maximum temperature of the uranium.

DIAMOND I had it in mind that this might occur on the edge of the flattened region.

HALL I do not think I can comment on that, it is a matter of looking at the actual measured temperatures in the reactor.

DIAMOND Another question: again under normal running conditions, the heating of isotope cans by in leakage from the main cooling channels.

HALL Yes, I think the point here is that the coolant or the air going down the isotope channels, these temperatures will be determined as much by the graphite temperature as by heat release from the A.M. cartridges.

Here again I am having a calculation done on this point. This to my knowledge has not been done at the present day on rods.

DIAMOND What I had in mind was that we ought to have a plot of the temperatures in the A.M. cartridge or indeed any of them, stringers from start to finish. If they are the same temperatures of graphite, well, they won't be the same temperature as the graphite, presumably slightly less, but there will be some leakage of air getting on for 160° from the fuel channels because they both go, I think, between the same static pressures.

HALL This is true, I think in fact the air in the isotopes channels would be a little below the graphite temperatures and therefore the air in the fuel channels and the air in the A.M. cartridges will be hotter than that.

DIAMOND Hotter than graphite?

HALL Yes. That is my feeling at the moment. I can give you this information - it is most probably available now - if you would like me to send it in this afternoon.

DIAMOND The pressure rise in the uranium channels should cause air to flow into the isotope channels rather than the other way?

HALL I think you would also have to be careful of the fact that distribution along the fuel channel is not linear but in the presence of (?)mixing devices. That is one thing that would disturb it. But I should be greatly surprised if the gas in the isotope channels differs very much from the graphite temperatures, in which case leakage would not be very important.

DIAMOND We will leave it at that, we want the temperature of the isotope channels.

CHAIRMAN Any more questions?

I think that is all for the moment Mr. Hall. If you could go away and look at these points we have asked you to deal with.

At this moment Mr. Gausden returned

CHAIRMAN Mr. Gausden, various points have arisen while we have been talking to other people and we have a number of questions in our minds about the actual operating conditions of the pile.

GAUSDEN Yes.

CHAIRMAN Things like graphite temperatures and so on. I don't know whether you can answer these for us. Another question is when you actually shut down the pile, there is of course still a good deal of heat being released, it then cools off. I think we would like to be absolutely clear, tell us something about standard operating conditions on the pile and actually what happened when you shut-down with particular reference to the last occasion. When you *really* shut it down, when you really did put the blowers on, and you really did see all the temperatures ...?... just to check these points.

GAUSDEN I can describe the procedure for shutting down. We are operating at a normal maximum metal temperature of 395. The procedure for shutting down is to run the rods in unison in banks so that you keep a balance of control rods on either side and the temperature reduction is not greater than 2° a minute. This continues on down until your control rods are fairly well in and the power, as indicated on the installed instrumentation is down to background. We continue to run the rods in, still with blowers on, and after running the rods in for about another 100 centimetres the main blowers are shut off and shut-down fans put on. The control rods are then motored right in to their innermost positions. At this stage a proportional counter is dropped in from the roof, which gives us a very sensitive indication of the number of neutrons which are there at the time, and the usual count rate is about 200 counts a second from this counter, and it was so on this occasion.

CHAIRMAN So that at a certain stage the main fans have been put off, but the shut-down fans were still going and the temperatures were going down at what sort of?

GAUSDEN The metal temperatures at that stage would be – the maximum metal temperature would be around 40°.

CHAIRMAN What sort of graphite temperatures would you have when you started the operation of shutting down?

GAUSDEN Just on commencement of shut-down?

CHAIRMAN Yes, what sort of temperatures are there?

GAUSDEN I think this graph would indicate this best of all. If I may show you – this was taken in the isotope channel 11.60 and at the outer, the charge face, the temperature is 22° rising as you see in this shape to 115° at the discharge face.

CHAIRMAN And this 115° is about the maximum temperature anywhere in the graphite.

GAUSDEN Yes this is a typical graphite temperature curve.

DIAMOND That is taken from a thermocouple embedded in the graphite.

GAUSDEN A series of thermocouples running through.

DIAMOND Near the surface of the graphite. Have you noticed just how much higher this temperature is inside the graphite – about 4" in?

GAUSDEN I have not.

DIAMOND How far from the surface are the thermocouple heads in these samples?

GAUSDEN These are embedded in graphite dowels which are 1¼" diameter and they are in the centre of the dowels.

KAY These thermocouples are in isotope channels. They do give a fair representation of a central graphite temperature, don't they?

GAUSDEN I would have thought so.

KAY They are not being stopped by the main flow of cooling air, or anything like that?

GAUSDEN No.

KAY They are filling up the isotope channels.

GAUSDEN Very nearly anyway, yes.

CHAIRMAN What did we say the maximum temperature was?

GAUSDEN 165°.

DIAMOND Can you sketch for us the shape and position of these thermocouples? That is an isotope channel.

GAUSDEN Yes.

CHAIRMAN I suppose it seems a silly question but how do you know that the Pile was really shut off? What is the answer to that stupid question?

GAUSDEN Well I would say the evidence of the Geiger BF₃ counter.

CHAIRMAN That gave ~ 200 neutrons per second.

GAUSDEN In addition of course the installed instrumentation was on its lowest range and showing zero.

CHAIRMAN Yes and the control rods really were all in?

GAUSDEN They were checked and double checked.

CHAIRMAN Another silly question, when you start shutting this thing off, you have got graphite temperatures of 180.

GAUSDEN 165.

CHAIRMAN Graphite 165. And now you begin to put the control rods in and your fans are still going, how do you know that some Wigner release is not going to start, be done with, before the Pile has cooled off?

GAUSDEN You don't really know.

CHAIRMAN You do not know that.

DIAMOND May I ask, is this under running conditions?

GAUSDEN Yes.

DIAMOND Then this maximum graphite temperature here is less than the outlet air temperature.

GAUSDEN The maximum outlet on temperature is the temperature at the highest rated fuel channel.

DIAMOND The figure here is 180. Is that the maximum from the channel?

GAUSDEN That is the maximum from the channel running at 395°.

DIAMOND This graphite temperature is 168-170. The graphite temperature measured is less than the air outlet temperature?

GAUSDEN Yes. After all, the extraction of heat is from the uranium under running conditions.

DIAMOND But there is heat being generated in the graphite which has to be conducted into the cooling air for cooling? So shouldn't this maximum graphite temperature be above the cooling air temperature?

GAUSDEN We have not found that. It is true to say, of course, that that is an isotope channel which is showing graphite temperature and the maximum an outlet temperature is from a uranium channel, so the two there are not quite the same thing.

CHAIRMAN Do you mean that if you have got thermocouples in the graphite which was up against the uranium cartridges you would have a rather higher temperature?

GAUSDEN You may well have.

GAUSDEN I think that that may well be a true assumption.

DIAMOND That is not my point. The point is that the graphite is having heat generated in it right through its mass. This heat must be conducted away through the mass to the cooling channel surfaces. Then there is a temperature difference to the cooling air in order for the heat to get away.

CHAIRMAN There is no evidence about this. It is considered a reasonable figure but there is no evidence.

KAY There is no direct evidence. Is the maximum ...?... actually measured?

GAUSDEN All of the heat generated under running conditions is from the metal.

GAUSDEN That is measured. That is the maximum we have found.

DIAMOND Some is from the graphite?

DIAMOND This graphite I take it was inside the enriched ring?

CHAIRMAN Do you mean from oxidation?

GAUSDEN Yes it was.

DIAMOND No. gamma ray absorption - slowing down neutrons.

DIAMOND And therefore the outlet air temperature at that point may well have been below 165-170.

GAUSDEN I see your point.

GAUSDEN Yes.

DIAMOND Roughly between 7 and 10% of the total heat generated in the Pile is in the graphite.

DIAMOND And the 180 refers to the outlet temperature from the enriched channels.

GAUSDEN Yes.

GAUSDEN It is the approximate temperature at the outlet of the channel which is working at 395.

DIAMOND So I am a little surprised, though there may be a good explanation, that none of this graphite temperature, at least at the outlet, is not above the air cooling temperature.

DIAMOND It is in the enriched ring?

GAUSDEN It is in the enriched ring, yes.

GAUSDEN I think if one takes this specific example of the isotope channel at 1160 one may find the outlet air temperature at that point.

DIAMOND What is the mixed temperature before going up the stack, just going past the void?

DIAMOND That is the explanation: this is not near the middle of the pile in fact.

GAUSDEN It varies to some extent with inlet temperature, but between 130-140.

GAUSDEN Not near the hottest channel at that time.

DIAMOND There is not a graphite temperature plot. I am sorry - yes this is a graphite temperature plot in the enriched ring.

DIAMOND It is not far off.

GAUSDEN Inside the enriched ring.

GAUSDEN The hottest channel I have is 0965. There is a peak. There is one point which is running at 395, there are others running at 390-385, but it is one maximum temperature.

DIAMOND Inside the enriched ring but not adjacent to enriched uranium.

DIAMOND 0965 is the hottest channel?

GAUSDEN Well yes. You remember, that the whole of the centre of the reactor has an enriched ring of 1.28 Co and then the position of the reactor inside that is loaded with mainly 1.28 Co metal but it does have natural metal at the ends of the channels.

GAUSDEN That was the hottest one prior to shutting down.

DIAMOND Which is a long way from the middle of the pile.

DIAMOND Is there any reason for this particular region being the hottest part of the Pile?

GAUSDEN It is in the enriched ring in fact.

KAY It is rather a peculiar distribution. Can I put a question on the same point in slightly different terms, would Mr. Gausden agree that the maximum probable temperature in the graphite would be 10-20° above the maximum air outlet temperature?

GAUSDEN I think, perhaps, that the flux plot would demonstrate this. There is a flux plot which was taken. (I am sorry I have the wrong one after all), flux plot taken down the same hole prior to the release last year and prior to the release this year. The red one is last year and the blue one this year and you will see that the highest peak occurs about here, so you would expect the highest temperature to occur there.

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DIAMOND But the graphite one you have measured is somewhere about here, a bit further in.

GAUSDEN It would be somewhere.

DIAMOND The maximum temperature of the outlet air is not much different from the

GAUSDEN It is not greatly different.

DIAMOND What I am trying to get at is, why this temperature is below that air temperature.

GAUSDEN I would not like to say that the graphite temperature is below the air temperature at that point.

CHAIRMAN Is there something important here Professor Diamond?

KAY We are only arguing about 15°.

CHAIRMAN We have been asking questions about the normal operation of the Pile. Can you follow that on. If not, there is one other question I would like to ask Mr. Gausden. This refers to the actual Wigner annealing operation. Have you got instructions as to the maximum temperature that has been recorded anywhere in the Pile before you do something drastic. What are your operating instructions there?

GAUSDEN There is an original instruction here issued on November 14th 1955. This was issued by Mr. Fair when he was manager of the Pile. Would you care to read it?

CHAIRMAN I would like to see this. Can we keep this or can we have a copy made. I think I will read this out because it is important.

Issued by Mr. Fair, Wigner Energy Release
"Will you please issue the following operating instructions to the operator engaged in controlling the Wigner Energy Release. If the highest uranium or graphite temperature reaches 360 then Mr. Fair, Mr. Gausden and Mr. Robertson are to be informed at once and the Pile Control Engineer alerted so as to be ready to insert plugs and close the chimney base. When the maximum temperature reaches 380°, unless further instructions to the contrary have been received, the roof plugs will be inserted and the chimney base will be closed. At 400° all of the dampers in the blower houses are to be opened and at 450° four shut down fans are to be started up."

So the first operation is to put some senior officers on notice. Then there are some other steps to be taken. Now we have heard what was actually done from the witnesses. I am speaking from memory. I think at least the early part of this check list is what actually happened. But there were changes of course, because it was thought there was a burst slug. There is nothing in this about what to do if you think there is a burst slug.

GAUSDEN No.

CHAIRMAN May I keep this?

GAUSDEN I can get another copy for myself.

SCHONLAND Mr. Gausden, have you had any accidental Wigner releases?

GAUSDEN There have been but I cannot really give you the story...

ROSS Mr. Chairman, I think I would have to speak to that.

SCHONLAND May I ask Mr. Gausden about this figure on Wigner releases and history since April 1956. Why this ...?... release did not take place?

CHAIRMAN Who should answer this? Mr. Bell, Mr. Gausden, Davey?

ROSS I think in this particular case any one of the three, preferably Mr. Bell. But in the history of the Windscale Piles it is quite unusual to have a successful general release.

SCHONLAND Shall we call him?

CHAIRMAN We shall try to finish with Mr. Gausden and then call whoever you wish. Your other question, about April 1956 I think Mr. Bell is probably the best man.

ROSS May I just ask one question. In putting in your counter through the roof if you read more than what is your limit? When are you satisfied that the Pile is shut down, from the nuclear point of view?

GAUSDEN 200 is the sort of figure we accept. If that begins to rise at all then we take further action. The first thing is to put in the shut down rods which at that stage are still up.

ROSS In this case there was no need to do anything like that.

GAUSDEN None at all.

ROSS What other action would you take?

GAUSDEN If it continued to rise?

ROSS Yes.

GAUSDEN We have additional cadmium rods available on the roof to put in some of the vertical holes if necessary. They are always available during Wigner release.

ROSS These were all available, the drill was normal.

GAUSDEN In fact at one stage, I did put in a cadmium rod just to check if it had any influence on the counter. It had none.

CHAIRMAN Any more questions at this stage?

Mr. Bell

CHAIRMAN Well Mr. Bell, Dr. Schonland has a few questions he would like to ask you about the Wigner releases which are shown on this drawing. I do not know whether you have a copy.

BELL Yes I have a copy.

SCHONLAND I would like to ask Mr. Bell if he can give us the history, if he knows it, of these diagrams which show failure of the Wigner release to spread and in April 56 the failure to start.

BELL Without the records this will be a bit difficult. I have some very rough notes which may be of help. My notes do not actually agree with this. It says the bottom right in my notes but as far as I can remember presumably this is bottom left in 1956 I am talking of now which has gone off. This anneal, as far as I remember, was started in the normal fashion: the rods taken out of the bottom to stop the flux to ...?... on the top. The anneal started and did not progress. I don't think any further ...?... was used at this time to push it further on, and an attempt was made at a later date in April, a few months afterwards, to try and get this pocket relieved. My information on my charts is that the pocket is still there. In other words we could not get rid of this one. I could not remember offhand that this had not moved at all, there is no indication at all.

SCHONLAND Have you had responsibility in regard to advising for instance, that the Wigner energy was accumulating and no release had occurred? Throughout 1956 when certainly $\frac{2}{3}$ of the pile under-relieved. Is this something for which you feel responsible in initiating some action?

BELL Well it is a very difficult one to answer. I think that my position in this one is that on the anneals, I act as a consultant or adviser of these things and it was probably on my advice they tried another anneal to try to get it out. I have no record of this. This was done by discussing. I cannot say at what particular time I may have been consulted on this one but it is normal that we get the results from these measurements when they are made. That is all I have to say.

DAVEY I will have to become a witness on this.

CHAIRMAN Yes.

SCHONLAND Can I ask the witness if he can tell us about uncontrolled Wigner releases or ... I want to talk about these uncontrolled or accidental releases.

CHAIRMAN Can we finish with Mr. Bell on this one point and then proceed with Mr. Davey. We do have another job which we are doing but would rather clear this particular matter now. Have you got the information you want? Mr. Bell has given us all that he can. Well perhaps we can excuse you again Mr. Bell, but we shall want you on that other point.

BELL I have produced a curve.

CHAIRMAN You could leave that.

Mr. Davey perhaps we can call you now. You know that Dr. Schonland has two points in mind.

SCHONLAND My two points are the history of these attempts to release Wigner energy which have given a partial failure in February 1956 and total failure in April 1956. My second question is on accidental releases.

DAVEY Do you mind if I turn them the other way round?

Of course I shall speak from memory but if the members regard any of this as being of great significance then I would dig out the appropriate records. The first known accidental or spontaneous release of Wigner energy was in September 1952 and this one was quite appreciable. It took us by surprise and it is as a result of the investigation into that spontaneous release that future planned releases were devised. It is also true that in doing a thorough examination of the spontaneous release in 1952, there was some reason to suspect that there had been two smaller spontaneous releases at an earlier date. These were quite small. In fact they were no more than blips on the temperature chart. Since September 1952 there have been quite frequent discussions on Wigner release in the Windscale Technical Committee and in the early days we decided to do releases at 20,000 mW days. This if you like was a first guess. It soon became evident that at this total irradiation there was not enough energy to give a satisfactory release, and again through Committee we gradually climbed to 30,000 mW days. This figure was maintained I would say for something like 4 years. In fact it was only changed quite recently. But sticking to the figure of 30,000 mW days it is the rule rather than the exception to have difficulty in having a satisfactory spread and a reasonably complete release, quite often it is pocket wise, and we were sufficiently concerned about this partial release something like 2 years ago to make a special point of asking for calculations to be done and these were done. Harwell people were involved chiefly on that occasion and at the next Meeting a report was made that even if one left a reasonably substantial pocket in one part of the pile, that itself would not be dangerous chiefly in terms of the total thermal capacity of the lattice, and the feeling that some release was proceeding anyway, and as we stand, I think it is true to say that by distorting flux and injecting nuclear heat at various points, every effort is made to get a uniform spread and a reasonably complete release, but if an individual pocket is left that in itself is not regarded as dangerous.

CHAIRMAN If you knew that there was a pocket, do you actually try to get that a bit hotter over the next normal operating period?

DAVEY No. Not during the next operating period, but by first of all using the control rods to direct nuclear heat towards one source, we have also tried subsequently to get another release to spread in that direction, by the use of plugs on the roof and that sort of thing.

CHAIRMAN The answer to Dr. Schonland's question, what accidental Wigner releases you have had is the one occasion in September 1952 which was quite clear. There were possibly two earlier occasions when it was very small and may not have been real, and since then no accidental Wigner releases.

DAVEY No.

SCHONLAND What I was trying to establish is that accidental releases were not related to faulty fuel elements or anything like that.

DAVEY No. I would repeat the statement that there has been no accidental release since September 1952.

CHAIRMAN Well that is a clear answer Mr. Davey. Now the other question.

SCHONLAND It is hardly so important now but I would like Mr. Davey to take us through this chart. Can you recall what happened at the time you could not get a release?

DAVEY I do not think I am competent to do this, because the period March 1955 to April 1956 is a period where Wigner energy and release were on the Agenda of this Technical Committee month after month because it was a matter of concern, but the best evidence and the best information we could obtain was that this in itself was not a dangerous condition. I think probably sometime yesterday, Bell produced a curve which does not level off but the rate of rise falls away quite sharply and probably he dealt with this when he was indicating and why they proposed that we should go 50,000 mW days. Well this is part of the argument which was being fed in in 1956. I am pretty sure that Gausden could contribute some detail on these diagrams now. He could not contribute to 1952 because he was not here.

SCHONLAND ...?... Gausden established that there was nothing peculiar in Wigner release effects in this matter.

CHAIRMAN Yes indeed.

SCHONLAND And I would be happy to leave it at this stage and hear what Harwell theorists have to say and then perhaps go into this. I don't know what they are going to say, but they may.

DAVEY Neither do I.

I should point out perhaps that this isn't evidence. The man who was the spokesman for Harwell was Fenning but I do feel that Renny could help.

SCHONLAND Let us clear up the Wigner if we can.

DR. SIMMONDS

CHAIRMAN For the record would you please state what your job is.

SIMMONDS At Harwell I am in charge of a small section studying shielding.

CHAIRMAN We are interested Mr. Simmonds in the Wigner energy release and what happens when the pile is shut down and these things happen, when there are pockets and any hazards which may be associated with these, and I am told that this is a matter in which you have made some studies and we therefore wanted to give you the chance of telling us what you know. Now I am not quite sure how we start. If you look at the board you will probably recognise things in these drawings and what they mean.

SIMMONDS I really have not been working on it for some 16-18 months but starting from the early days when we had first releases, we decided that it was desirable to release the energy on the basis of experiments which I did at that time. It seemed perfectly safe to do so, and the idea was that this

should be checked by periodic measurements through the piles using the dipping technique and that was to be carried out at Windscale. But nevertheless, at Harwell we continued to study stored energy in a more general sort of way and this is what I have been doing up to a year ago. Apart from our interests in higher temperatures for other reactors, as far as Windscale is concerned, I concentrated my attention on this question mainly: is the method of monitoring reliable, are the conditions likely to change so that it ceases to give a reliable indication? This was the question which seemed to me one of paramount importance and what I have put my attention on. I have tackled this in two stages, first of all; you understand the dipping experiment cuts the energy release off by virtue of the heat lost to the furnace. You have had that described presumably?

SCHONLAND We have had it mentioned, Mr. Chairman.

CHAIRMAN Please describe it.

SIMMONDS We thought in the early days the best experiment to test graphite under conditions similar to those in the reactor was to do an adiabatic experiment, the ideal experiment being to raise the temperature in the graphite instantly to some specific temperature, and then let it go adiabatically and we did a number of tests of this type and always got an S-shaped curve with a very flat top. In fact, we could never ...?... If apply heat to a specimen and at this point remove the heat and isolate the specimens, we got curves of this type. In the monitoring experiment this is a rather slow and tedious thing to do. In order to monitor many specimens quickly, the experiment is this - take a furnace which is maintained at constant temperature 200. We dip a piece of graphite in with a thermocouple on it and watch its temperature. This would do this if it were unirradiated but if it has stored energy it does that, at that point. Now providing this is a curve which is going to flatten off, this is a good indication of that providing that you know that if you do do it properly you will get a flat top. This however, does not give any indication of any change which may take place gradually, and if for example, the shape of this is changing with time, there may come a time when this does not indicate correctly what would happen adiabatically. That is one point. Secondly, this does not give any guidance as to what happens in the presence of an additional heat source. My first step was to take specimens tests and in this way, and I found this to be by no means easy, the difficulty being to get an accurate measurement of this flow after a very large rise of temperature. It is technically a very difficult thing to do. You can imagine, raising a temperature 200 or 300° and then wishing to measure its rate of rise very accurately, so I fixed this within the order of 5°/min accuracy of measurement, 5-10°/min and within that accuracy always found flat-topped curves. From that I deduced that those measurements were no better than the standard dipping type of measurements and were not worth while pursuing. They didn't give any fresh information. Therefore, I turned my attention to the linear rise experiment. This work continued up to and during 1955. We stopped, I think, because it showed no new information about the simple method. And the accuracy of this: one can measure the stroke at the top was not good enough to look for any gradual change during the years. Looking at old graphite and new graphite it was always flat. Then went to linear rise

method – this has the disadvantage that it is a less direct indication of what should happen in a pile. In this method we take a specimen and raise the temperature at a steady rate. a constant rate is the ideal experiment and we measured the rate at which energy is released. And this is by no means an easy experiment to do because at times you have to add heat and at times subtract heat to maintain the steady rise. We have done experiments of this type in the very early days and we regarded it as not the best method of looking at the graphite because the temperature condition is so very different from that which actually occurs in the reactor but having gone through the process of adiabatic rise, we returned to this because it has the advantage of being a more sensitive indicator of changes in the stored energy characteristics, although less readily applicable to predictions of what would happen in the reactor, but it is a sort of insight into the future as to what changes are taking place, and also gives one some clues as to the effect of additional sources of heat during release. Well you have already had this described and as I pointed out if you have a change in the shape, it may show practically no result on adiabatic rise. If one changes the shape from this, to say, that, you may shift this very slightly and show no result here but that may appear quite clearly on this measurement and indicate that the sodium graphite is different from what it was when we started out and this is a warning to do more measurements of this type. At that point I was already well into other work and left it at that point. We did a few measurements on the total amount of stored energy. We had a bomb calorimeter for this purpose. We were able to devote the effort to get a really good one, these were quite rough measurements but we got amounts of the order of 200 cal per gr. total, but I didn't regard this as important. I always regarded the behaviour up to 500 as the really important thing to know. I don't think it matters very much ...?... it.

CHAIRMAN It is a fact Mr. Simmonds that the bomb calorimeter measurements indicate that there must be another peak at some higher temperature but we don't know where.

SIMMONDS We don't know where that is. There's a little bit of data in the American results to indicate that it is very high up. It is very difficult to do calorimetric measurements of direct heat releases about 500, we cut it at 500 to make it practicable. I would not say it is impossible.

CHAIRMAN And then you work by differences?

SIMMONDS Up to 500 you can do a direct measurement. Above 500 the only method is to anneal, certainly above 1 thousand the only method is to anneal at intervals and measure the total stored energy. This requires extremely high accuracy of measurement, something of the order of 1 to 10⁴. We set our bomb calorimeter with about actually 1 in a thousand, and did some rough measurements. But this was not suitable for studying differential ...?... We didn't attempt that. We had only a small Group on this and we had other

commitments on high temperature irradiations. This work ceased a year ago.

CHAIRMAN Do members of the Committee wish to question Dr. Simmonds about this work.

SCHONLAND You have not any practical experience of annealing piles yourself?

SIMMONDS I attended practically every event up to 18 months ago and either I or someone from Harwell, usually myself, and I watched what happened. Up to that time I was watching the Pile always behaved in a similar way to the very early ones.

SCHONLAND You attended the Windscale releases?

SIMMONDS Yes, an an observer of course and one in BEPO.

SCHONLAND Would it be worthwhile asking Mr. Simmonds to comment on the thermocouple results in the recent release?

CHAIRMAN From what point of view Dr. Schonland?

SCHONLAND From the Wigner angle.

CHAIRMAN Certainly if his laboratory work gives him the opportunity.

SCHONLAND Curve marked 2053. Would you regard that as possibly due entirely to a Wigner release?

SIMMONDS If that were due entirely to Wigner release, as represented by the adiabatic curve the graphite must be of quite different properties from what it had at the time I took these curves. The question that arises is, if this is to be due to a Wigner release, we must have a quite different type of curve from this, because at this point it must flatten off pretty flat. The question is – is it possible for there to be some edgy behaviour of this type which would lead to continual release. First of all, we do not know whether we have recent measurements of this pile where we have that shape, but if we try to extrapolate results we think we might just about have a shape with a small peak, cross specific heat being fairly near it and certainly even if it were like that we do not really know how that would transfer; what it would do under adiabatic conditions. This is not directly applicable, one can't, without detailed knowledge of the kinetics process, go from this to the adiabatic rise. I have always feared that this might happen, this is why I studied the adiabatic rise method but this is too slow for laboratory experiments to show up. One can't measure heat over these long periods.

SCHONLAND If you take the three flat curves that run together, 2550, 2661, 2655, these would be typical wouldn't they?

SIMMONDS If the curve is one which goes through the specific heat and drops below it, these curves require an additional source of heat.

SCHONLAND I am talking about the three that go together.

SIMMONDS 2550, 2661.

SCHONLAND Take them from the first day.

SIMMONDS From the initial rise?

SCHONLAND Say from the beginning, zero to 24 hours on Tuesday the 8th.

SIMMONDS Up to that point it is quite normal. It depends on the actual amounts that happen to be there.

SCHONLAND Now look at 1750, the next one above.

SIMMONDS That could be quite normal.

SCHONLAND 1960 and 2053 are beginning to show abnormalities from the simple picture.

SIMMONDS So are the others later.

SCHONLAND I am not talking about later. I'm keeping it to the first day.

SIMMONDS 1960? Rather a slow release really. Yes you would expect to see something a little steeper I think.

SCHONLAND The latter part of 2053, after the steep rise. The latter part of 2053 we cannot understand either in terms of the storage as it used to be.

Now the next question is, whether we can understand it or not is it possible for different parts of the same pile to show such differing Wigner release curves if there was nothing producing the effect than Wigner release. Take the first 24 hours: would you say there is something there other than Wigner release? [That graphite could be at the same degree of radiation].

SIMMONDS Up to there I would think there is no real evidence to say there's anything but energy release because there might have been releases higher than 300 at other points. We have got very sketchy coverage. Up to that point there is nothing to make one think it could be.. this one curve. 1960 is possibly a bit slow. We can postulate that at 2050, had completed its release and was receiving energy from another part of the reactor giving a slow rise. 1750 was just raised to the point where energy starts to release and then went up quickly. That's OK. And we just say that 2564 and 2868 had not received enough heat to start them off. 1960 is a little bit peculiar but I would not have thought there was enough evidence to worry about it up to that point.

SCHONLAND Now, having got the evidence and sticking again to the first day. We have to remember you see, that you wrote a paper in which you gave a warning that two samples (admittedly a limited survey of the matter) showed a possible future danger, but not an immediate one. My question now is the same one as before: it would be unlikely for the same pile to show a dangerous condition in one part of the graphite and the same condition in another?

SIMMONDS To indicate on this type of graph, you mean?

SCHONLAND If you were given two examples from Pile No. 2 at this moment would you expect them to show roughly the same ...?... curve?

SIMMONDS No I would not really, because of the uncertain thermal history of the Pile, and the uncertain past history of the graphite. You need to make measurements along the particular ... it needs some sort of survey.

SCHONLAND Do you consider that one part of the graphite might have had a thermal history which would have made its Wigner energy content approach more closely to the specific heat than another part?

SIMMONDS What is more likely is that in the past some parts may have missed or not been properly annealed and this one would expect to lead to a larger peak rather than in the general level after the peak. But this is a little bit speculative because we have not surveyed with the linear rise method, a range of conditions. We took ...?... anneals followed by irradiation. We don't know what would happen if we missed a previous anneal though undoubtedly the peak would be higher. We know that that would be increased. The amplitude of the peak saturates with dose. I think, at temperatures corresponding to the middle of the Pile but at lower temperatures, say 40°C, the amplitude of the peak would probably increase in dose and could be somewhat larger. I say this because I know that whereas in the ordinary release it is something like ...?... It has not saturated the peak at lower temperatures, but probably has at higher temperatures. Therefore, if you have missed anneals you might expect a larger peak in the cooler parts but I could not say what would happen to the curve higher up but I should think it would be less sensitive to temperature.

SCHONLAND Now let us look further along the curves. The possibility of a catastrophic accident by Wigner release has been raised by you but not as an immediate graphite danger. Would these curves suggest to you that that is what happened or do they suggest some other explanation, or would you like to take them away and look at them?

SIMMONDS Well I've been studying them all day. The real thing I have been wondering about is, 2550 to 2661, why do these rise so sharply at 16.00 hours on Wednesday? It seems to me unlikely that that is stored energy from the same portion of the pile unless we have a curve ds/dt, the curve I mentioned wavering near the specific heat. We would expect it to have cut off. How shall I put it? The difficulty really lies further back. These three curves at 4 hours on Tuesday seemed to have been stopped short. At that point there could not have been a tremendous heat loss at that point at least not a heat loss in any way comparable with heat an hour earlier where there is a very rapid rise. So we presume that at that point the ds/dt must have dropped a bit below the specific line so a little bit of cooling stopped the release. So that if therefore we were on this type of curve we would not expect at a later stage when it had come back to 250°C to have any catastrophic effect. It makes one wonder, either there might be another curve of this type representing ...?... graphite, in which case we are not in a position to predict the exact shape

of an adiabatic curve. But there is not enough heat cooling arrangement available to stop these curves at 04.00 hours on Tuesday. If they would have gone straight up to 400 in the normal way and if you think of them as truly adiabatic going up to 400°; you wouldn't have been able to stop them there. They would have gone up. At this point the energy release was... the sdt as it were was following below the specific heat ...?... make that clear from that statement. I cannot really say that it is not due to stored energy, that rise. If it is then it means I think the shape of this curve must be very different from what it used to be and this is what I have predicted as it were, perhaps not so soon, and we do not really know how this would appear on our adiabatic curve. This has never been checked. We need additional information to go from the linear rise curve to the adiabatic rise curve. I mentioned that at the beginning of my remarks on why I did these types of measurements; that the linear rise one was not adequate.

SCHONLAND So your answer is that taking these three from 2550 to 2655 all three you would be surprised if that was a Wigner release but you would not exclude it. That second peak.

If it is a Wigner release, it is something quite different from previous Wigner releases.

SIMMONDS But I was half expecting some change in characterisation, so being a little bit speculative, it could be energy release, it could just be.

CHAIRMAN What sort of curve would actually do that? I am not clear. Would you need a double maximum? If you had got that curve out of one of your lab. experiments...

SIMMONDS Well you would obviously have to take heat away and interrupt the experiment.

CHAIRMAN Once you have taken heat away, then suddenly it starts coming back again.

SIMMONDS Between the first peak and the second rise there must be heat coming in somewhere. But that is not difficult to explain because it is a very slow change up to say 12 o'clock on Wednesday.

CHAIRMAN You mean it may be coming in from Wigner release somewhere else?

SIMMONDS Oh that's not at all difficult. The difficulty is where it becomes so rapid that you can't believe that there is that... you have to postulate a very rapid heat... that was the other possibility, that for some reason there was temperature rise somewhere else which fed heat in, round about 12 or 14 hours on Wednesday, but you also have to postulate some heat being lost at first and then gained but in the valley part of it. (This is very plausible and not very likely. We know there must have been a very complex air pattern.)

CHAIRMAN But what makes it shoot up again like this? You say the ds/dt curve being practically on top of specific heat there, would that do it?

SIMMONDS I do not know whether it would do it or not, all I am saying is that if the curve is of that type we are not in a position to say how a truly adiabatic curve would look. We

have only got measurements of adiabatic curves corresponding to linear rise curves of the early type. It is somewhat speculative to say one might expect, but one might perhaps expect a delayed release of that type but that is certainly guessing there.

CHAIRMAN Yes.

SCHONLAND Going back to this point. If you look at any one of these three curves, these triplets there, it seems surprising to me that round about 12.00 on Wednesday the 9th, although the temperature of the lower rows reached in the first Wigner release, they show such a rapid rise which could not be due to contained Wigner energy because they have previously gone past that temperature, between 12.00 on Wednesday and 20.00 hours. We are saying they are giving off Wigner energy but they have already lost that energy.

SIMMONDS May I go back to the point about the shape of this curve. If the true adiabatic curve was that, ...?... it must be somewhere here to cause the temperature to drop and when we come back up to this temperature we'd expect this to finish this curve off but if the rise curve was something like that, quite different from what it was in the past, we have cut it off at this point. And when we come back to 250°C we finish the curve off with still? an appreciable slope.

SCHONLAND But we're not coming back to it; we have gone below it.

SIMMONDS We've gone below it, but it does rise appreciably... Oh! We have got to postulate some heat supply between 04.00 and 14.00 on Wednesday. You have got to postulate heat coming in from somewhere in that period, certainly.

KAY Going back to the previous peak temperature of 260°C. You won't get any appreciable change till you come back to the highest temperature. So until each of these curves gets back to the previous peak of 260°C, that, on this theory, must be accounted for by transfer of heat from other parts of the pile.

SIMMONDS I think that is almost certain: practically certain.

KAY Can we draw any conclusions from the shape of these curves, onward... beyond 1500 or 1600. If that is accompanied? by Wigner energy, can we say anything about the adiabatic curve.

SIMMONDS Assuming that the behaviour after there is partly due to energy release, then the curve cannot be one of the type which would flatten off, because I do not believe we could have got enough cooling in the early peak if it would have gone up higher. If it would have gone to 350°C in the beginning, we would have to have had a very sharp heat loss at 4.0 on Tuesday to stop it. The fact is that probably what was a minor convection current stopped it. How that if it was a flat top curve, we could not get this extra bit out later. If however it was a gentle curve, different from the previous ones, then we might attribute some of these to restored energy and we would associate that with a changed form in the linear rise curve but we are not in a position to predict

from one to the other without a very detailed knowledge of kinetics. I am sorry that this is a bit indecisive.

CHAIRMAN But even if you had that sort of odd shape which would enable the temperature to go up, it does seem to me that it has gone up too fast.

SIMMONDS It does to me.

CHAIRMAN It takes time for this heat to come out, especially when it is only a very little amount of heat.

SIMMONDS If you are above the specific heat it can rise extremely rapidly. There is a very big variation of temperature, the rate is a very rapid fraction of the potential rates of rise... These actual observed rates arise in the reactor are never as fast as observed on small specimens in a laboratory, which we have always attributed to redistribution of heat through a block, that the different parts of a block start at different times. If you do it in the laboratory you get a much more rapid rise. I mean something like 600°C a minute. I have observed.

CHAIRMAN You have given us some really mathematical feasibilities related to curves but of all the things you have told us perhaps the most important is that it went up fast here and then stopped, and then it cooled off and then some heat came in from somewhere else and it got back to where it was and then it really went up fast. Because it went up fast there it is difficult to see what stopped it there.

Q? Well the air dampers were put in weren't they. There was a change in conditions.

CHAIRMAN Right back here you mean. What stopped it there? What stopped it at 260°C at 4.0 on Tuesday?

SIMMONDS I think at that point ... we can't imagine any vast source cooling, in any way in which it could be cooled very rapidly. This rate of heating is steeper than the cooling when the fans were put on. So we must say that it stopped because the stored energy Wigner release was being reduced. But we may have cut it a little bit with cooling.

CHAIRMAN What I am trying to say, and I think it is terribly important, and I must see whether I am interpreting you rightly, it went up very fast and then it stopped. These things happened and then it got back to the temperature it had had where previously it had stopped rising, and for some reason it was then rising very rapidly. What I am asking is that if it did all this rising here by Wigner effect, why doesn't it go straight up, there? Why did it not do that here when it had its first opportunity, and it did not do that; there was no special cooling there at that time and therefore I would argue that something else was heating it here.

SIMMONDS I had thought that was the most likely explanation but I did say that I did wonder whether the new characteristic might explain it.

CHAIRMAN I am not quite with you I am afraid. Are you suggesting that it had a new characteristic at that time that it had not got at that time.

SIMMONDS A new point occurred to me whilst you were speaking and that is, we couldn't stop. If this curve is due entirely to stored energy after 16.00 hours on Wednesday then had we not taken heat away on Tuesday, it would have proceeded at least as rapidly as that, and therefore we would have to postulate a source of heat which can cool at this rate here, a sink of heat as it were, and that is still a bit implausible. You cannot explain that on Wigner energy unless you explain a sink of heat at least equivalent to that rate of rise and it is getting a little implausible now, as it is getting rather steep.

CHAIRMAN Right, just to be perfectly clear, it stopped at 4.00 on Tuesday. That might have been because it had done a full release, or it may have been that some strong cooling came into operation. If there really were some strong cooling, then once it got back to that temperature it could rise again rapidly as there's still more energy...

SIMMONDS The cooling though would have to change the temperature at a comparable rate in the absence of stored energy with the rate of rise observed there.

CHAIRMAN And therefore we have to look at that time here. This has now become the important time not that. We have got to establish that there was no real fast cooling at 4.00 on Tuesday.

PIERSON If there was no fast cooling then it was due to something other than Wigner.

SIMMONDS There still remains the possibility that Wigner release could contribute to it.

CHAIRMAN Yes, Wigner assist is a very different thing to Wigner driven. We agree with you there.

3.15 p.m. 18th October

WITNESS: MR MUMMERY

CHAIRMAN For the record would you please tell us what your job is.

MUMMERY I am in the Reactor Division at Harwell and primarily responsible for Reactor physical work in connection with the pond and stage 1 and advanced stage of the reactors. In addition I provide liaison between that Division and the Industrial Group for the operation of Calder and Windscale reactors.

CHAIRMAN We have been trying to study the temperature records that the thermocouples in the pile show and we are trying to decide whether certain of them can possibly be explained as Wigner energy, and Dr. Simmonds, who was the previous witness, has called our attention to an extremely important point, and we would like to start with you on that point, to see whether you can contribute some thoughts or criticisms. I do not know whether you have available Chart 5. Now the particular records we are going to look at are a group of 3: 2550, 2661, 2655 and the point which we worked over with Dr. Simmonds was that this initial sharp rise here, to the knee was certainly a Wigner release, but then the temperature stopped rising, turned over, and began to drop, which would be pretty standard: just as we could expect

before all the energy was out, dropped away and then for some reason it began to rise again: that might well be that Wigner release was taking place somewhere else and the energy was coming into here, but now we come to the point, that it is not until the temperature has risen to the top of that knee again, that any more Wigner release can take place, and this rise of temperature here was very sharp, very quick, and what we cannot see is, that if that sudden sharp rise here on the 1600 hours on the Wednesday, if that was Wigner release, why it did not go on when it first had the chance, on the first knee, and we reached the conclusions with Dr. Simmonds that that was not at all a reasonable thing to say; either there was a very strong source of cooling applied at that knee, which did cut it off short, or if indeed there was no source of cooling there, then there must have been a source of heating at the other end. That is the point we reached. Can you from your knowledge give any contribution here?

MUMMERY We argue that we do not think that any source of cooling has been applied early on Tuesday to stop that peak, (a) on the grounds that experiments have been done in which Wigner release has been started. I am quoting other people's work here and you can refer to Bell I think, on this one, where Wigner release has been started in a block and the block has been quenched but you cannot stop the Wigner release. I have no more detailed information but I think Mr. Bell could give more information on that.

The second point is that if that was stopped first of all one can get a rough idea of the amount of coolant necessary by considering the change in slope, when various other coolants were added, and finally none of the other temperatures are still going up so it must have been a purely local effect, and this seems most unlikely, to say the least, so we would then argue that the most likely explanation of that first peak is that you have got out of that all the low temperature stored energy that you can get: all that in fact is there.

Then we come to consider what happens later on when the rise comes up again. We have to look very carefully into this and try to do some sort of heat balance (we haven't yet succeeded in doing this) because we must remember that the effective specific heat of the graphite at that temperature was slightly above the previous temperature and could be substantially reduced due to the tail on that curve coming up towards the specific value. If it did approach that, then quite minor changes in coolant characteristics throughout the system would swing temperatures fairly rapidly backwards and forwards.

CHAIRMAN But that sort of effect does not come into play until the temperature has passed the first maximum which is 260°, so that quite a part of that early rise there is no Wigner effect at all. The specific heat is just normal specific heat.

MUMMERY The first part of the rise is, if I may use this diagram, simply extracting this heat from this graphite, returning to this temperature. What I am saying is, that there is an external source of heat from other Wigner releases in other parts of the pile, feeding heat back into this particular region, then the temperature rise you will get for a given amount of heat will depend not on this specific heat value, but on the difference between these... If that gets small, then...

CHAIRMAN Then, on the left of that side the specific heat will be normal and therefore you would expect pretty well a discontinuity in this rate of rise when you go through there and there is not much sign of change of slope or anything. It is a pretty continuous curve.

MUMMERY Not much sign of an appreciable slope at any rate. It is very difficult to see a large change.

KAY Only one, which is 2661, might be a border line case.

MUMMERY This is true. Although we are postulating, and I suppose we must, that the rate of flow of heat into this region has remained sensibly constant over this region. As far as I know from the "bush telegraph" the plot we have made here is sound, that is that having reached this temperature our specific heat now effectively does have a discontinuity there but I am not sure how far we can press this. Discontinuities aren't often found in nature. There is a certain amount of other data accumulating which will bear with this postulate.

CHAIRMAN You mean experimental laboratory measurements.

MUMMERY I expect you will have seen Simmond's curve. We have, I think, about three or four days ago, some more measurements came in on samples, which although had had a very large dose, had never been intentionally annealed, because they had been removed from the pile before anneals took place and then put back again, and these have been measured by this technique in Canada, and gave rather more pessimistic results.

CHAIRMAN What sort of MW days are we talking about for these later samples?

MUMMERY This one is 155000 cumulative. The new ones which were unannealed, were only 100,000 MWD and although they were 1½ times down, therefore, on this dose, they sat more or less in the same position, which means if you multiply by a factor of three assuming it is linear. We know this is a pessimistic assumption, but we don't know very much more, then it does get somewhere in this vicinity. That is one thing. I would suggest then, at some later time (I do not think the evidence is sufficiently good to put to you), but some evidence has been accumulated by Mr. Bell on samples of graphite from this reactor, in which he will carry out his normal experiments, the so-called dipping technique which I expect he has explained to you, in which the things will be dipped at different temperatures, and one will see then whether or not there is any gradual drift up in temperature when it is dipped in an environment of 300°C and one of 400° which simulates this pattern.

CHAIRMAN You are talking about a very high exposure, 100,000 MWD.

MUMMERY The ones on which these will be done will be about 150,000.

CHAIRMAN Although we know there are pockets that do not always get released, it would be most unlikely that any graphite in the pile had a 100,000 MWD? exposure without having been annealed on some occasion.

MUMMERY Yes, particularly towards the front end of the pile, and I think measurements are available through the whole length of the channel too. I am afraid that particular postulate, that the specific heat is much reduced and this rise here is due to feeding in heat from elsewhere, looks a bit shaky and there does seem to be evidence against it.

CHAIRMAN There is no certainty about a statement but that is an extremely unlikely explanation. Do you agree with that?

MUMMERY I would say it is unlikely. I would not say extremely.

DIAMOND Do you think it possible that there are regions in the pile which have never been annealed?

MUMMERY Well, I have only become familiar with the details of the annealing procedures and so on comparatively recently and I have, therefore, had to rely on reading for previous history and I quite frankly do not feel I could answer that question. I do not feel that any answer I made would be backed by sufficient evidence to justify it.

KAY May I ask you to make any comment on the temperature curve 2053 on the same Chart 5. We start off there with a fairly rapid temperature rise: not quite as rapid as the group of 3 we were considering a moment ago, but still quite rapid. We start levelling off, but instead of coming up over a peak and cooling off we then start a long steady rise in temperature which goes on for about 2 days. Can you draw any conclusions from that curve? Are there any features about it which strike you as being peculiar?

MUMMERY It has been observed previously, gradual rises in temperature, presumably from feeding in of heat from other parts of the pile. I do not think we have ever seen one go quite so long and with quite such a steep upward curvature. It is starting to creep up quite markedly around 1600 hrs on Wednesday. That is the unusual feature.

CHAIRMAN That is the highest record anywhere. It is hard to see where energy can be coming from.

KAY You would expect it to be turning up if it was being heated from somewhere else. As it got hotter, temperature-driven ...

CHAIRMAN It goes the wrong way, doesn't it?

MUMMERY You have to postulate that you have some heat coming from a region which is not being monitored, or that is coming from the sample itself.

KAY If you think it is coming from another region, would you not expect it to flatten off towards the end instead of to steepen up.

MUMMERY It depends what that other region is doing.

KAY The other region would have to be going steeper still.

MUMMERY I think the only other point worthy of note is that that particular region did not seem to have a particularly good anneal last time.

KAY Would you be prepared to mark a point on that curve up to where you think it was a perfectly ordinary Wigner release?

MUMMERY Having had a somewhat limited experience of seeing these things I would be reluctant to do this, but if I were pressed and looking at this thing I would have said that one probably would not feel anything unusual happening until the period towards 1600 and onwards on Wednesday and there you have other things happening, and if one loosely said, well this is some more energy being released, then the top curve is explained.

SCHONLAND I would like to ask whether you are aware a year or so ago, in November 1956, there was a maximum temperature in the graphite recorded for 15 minutes of 420.

MUMMERY I wasn't aware of that.

SCHONLAND So we needn't pursue that.

CHAIRMAN Are there any points you would like to call our attention to, that we have not asked you about.

MUMMERY No, I think not.

SCHONLAND We've heard that some new measurements have been made in Canada by the linear rise technique. Are these available to us?

MUMMERY Yes, I would stress these are on samples which have not been annealed at any time as far as one knows.

SCHONLAND I would like to see the specific heat curve. We can take that tomorrow can we?

MUMMERY You can put the specific heat curve on from that.

CHAIRMAN May we borrow this: then we can do that.

MUMMERY Do you wish to retain the records of the last anneal?

CHAIRMAN Yes we do.

(Mummery leaves)

CHAIRMAN We have Mr. Rennie out there: can he contribute anything in this?

? I am not sure.

SCHONLAND We are getting further away from reality as we go. But we might ask him...

KAY Since we've had Simmonds and Mummery, wouldn't it be proper to...

CHAIRMAN Oh, we'll call him, but I am asking if there are any other questions to ask Rennie, or do we concentrate on these two charts?

KAY This picture with the five circles on...

3.40 p.m.

WITNESS: Mr. Rennie

DAVEY In previous discussion, Mr. Chairman, the argument against that happening was the relatively low temperature at which release was initiated, and it was very difficult to conceive of a small region or a number of blocks which did not undergo the temperature rise to the threshold.

CHAIRMAN Mr. Rennie may we state for the record what is your job.

RENNIE I have just recently returned to the Reactor Division, I was in that for several years but for the last two years I have been doing work on Overseas relations, but I returned at the beginning of last week to the Reactor Division.

CHAIRMAN We have been discussing with Mr. Mummery and Dr. Simmonds the possible interpretations of the records of the thermocouples which are plotted on various sheets, and we should like to see whether you can help us. On chart No. 5 we would first like to start with the three sets of records, 2550, 2651, 2655. They are all the same. They rise fairly abruptly to a maximum, fall away slowly and then begin to rise with a steady acceleration until they go considerably higher than the first knee. Now to us the significant point is that if the second rise of temperature were caused by Wigner release, we cannot see why it did not occur when the graphite had the first opportunity when it was going up this curve in the first instance. Now maybe it is possible that some rapid quenching, for some reason we can't explain, occurred then and blocked it but there seems to be no reason for assuming that, and therefore, we are tempted to argue that because that rise did not continue, the second rise must have been due to something else. The first fall is standard and maybe it began to rise again from its minimum here because Wigner release was taking place somewhere also nearby and heat was flowing in but it does seem to be a very rapid rise in temperature to be explained in that way. It must have been a very hot region that is feeding energy in, and this curve shows no signs of discontinuity of any sort. We might have expected that when the temperature reached the previous maximum, the graphite could then top once again this Wigner strain energy. If we could turn to the top record of all, there is a fairly normal start. This time there is a discontinuity: this time the temperature rises. It is rising well above all the other records we have got. It is a very long slow rise. Perhaps that could be Wigner release, but it seems to go faster and faster, and we do not understand that. We do not think that rise can be due to Wigner energy from other regions coming in simply because we would expect the thing to level off: as it warmed up it wouldn't take in so much energy. I have tried to summarise the way our thoughts have been going and would appreciate your comments.

RENNIE Naturally I am thinking the same thing. The first point seems to be these 3 records, which start off in the normal way as Wigner release, at about 250°, whereas from the general evidence I think one would expect them to have gone up higher, since this region of the pile (this is 6 feet in

and fairly near the centre) should have received a fair dose of irradiation so one might have expected it to go to more than 250, say 350. I think this sort of thing happened on the last anneal. The general temperature level was 350, so why did it turn over at 250. Naturally one looks for a source of cooling. I think one could argue that from the very slope of the curve one would have to put on the 4 shut down fans to take away that amount of heat. The slope happens to be just the same as cooling the graphite with the shut down fans, so unless the fans were put on and I understand they weren't, I didn't see any way in which you could have quenched that by cooling. Ordinary convection cooling would not have done it. There seems to be two other possibilities as to why it turns over. There seem to be two ways in which one might attempt to explain why this stopped short of what one would have expected. One way is to say that there had been some annealing in between the last expected anneal. There had been an unexpected one in between which had taken away some of the energy, which is just what one would have expected. The other way is very much more speculative. One can imagine, (I expect this has been mentioned before) that in the shape of the DS by DT curve there would not be much of a peak above the specific heat, so one might expect it to stop at a lower temperature than where you have a large peak, and in that second case, if that was true, which is against the evidence we have at the moment, you might also speculate that the tail of the curve was nearer the specific heat, so that the effective specific heat at temperatures above the point where it cut off would be less. If one follows on that second possibility, then one has to look as to whether this second rise could be accounted for in that way. In other words whether the heat which is being fed in from the other Wigner releases which are taking place at the same time in other parts of pile, would be enough, but I do not see how one can follow this through. A very rough heat balance shows this. There seems to be barely enough heat to do it and then of course you have the trouble that it can only come from a higher temperature region. Well, there are some higher temperatures in the 10' region, but I could not get very far on that hypothesis. It ...?... entirely. The other thing one could say if you assume this kind of behaviour, your stored energy curve is rather different from what you expected, this might explain something like channel 2053, which might even have gone to a point where it was actually above the specific heat. In fact it was almost self-sustaining. It was just above. I have no evidence for this, and so I think that just comes from following that line of thought. On the first one if there had been some sort of annealing, and so this was just a natural limit to 250°, then of course the second rise has to be explained by some other source of heat. That is as far as I have got in my thinking: not very conclusive either way.

SCHONLAND I would like to ask Mr. Rennie to mark his curve from letter B at 1100 hours on Tuesday, where you see a slight bend and at 1600 hours on Tuesday this interval corresponding to nuclear heating. Now you see that every one of the curves, the first four or five curves, shows the effect of that nuclear heating. There is a distinct bend at B. Here we have got heat applied to these portions of the graphite. If there had been any more Wigner energy in them, would you expect them to shoot up at that point instead of merely warming.

RENNIE They have not actually increased in temperature, well within the accuracy of the plot. On this group of 3, they have just levelled off and then dropped. You would not really expect an increase. You would not expect to initiate the release of any more Wigner energy until you had come roughly to the temperature at which it stopped before, until it had got back to 250.

SCHONLAND Well, if that is the case, go further on to Wednesday at 12 where the same triplet shows a much more rapid rise, you must argue that there is a greater inflow of heat to these regions even than was provided by the nuclear heating.

RENNIE Yes.

SCHONLAND You have got to explain the rapid shoot up round about 12 hours on Wednesday and you can only explain it by... It is below the Wigner heat... That source of heat is going to be greater than the nuclear heating. This is a considerable source of heat, how considerable I don't know.

RENNIE The nuclear heating was about 400 kW. I have not seen all the records from the thermocouples that were there. I have not seen them for the whole pile, for the top region and the bottom region, and I quite agree that it does need some sort of heat balance made in that region and one could at least get an idea then whether it was possible. Doing some sort of heat balance like that, you may be able to show that this is just impossible.

SCHONLAND If you turn to 4, to the region called BC'He. Nuclear heat in the region on the top curve 2055 again shows that nuclear heating merely slows up the fall in temperature: it does not produce the first release of Wigner energy. Is that what 400 K.W. does? I do not know whether you think that this is significant. It seems so to me.

RENNIE I think it is significant.

SCHONLAND I am only asking you whether you would agree on the evidence of the nuclear heating effect which you found in a number of curves, that much more serious rise late on Wednesday afternoon indicates something quite substantial in the way of a source of heat.

RENNIE It is true that the energy comes through that could get out through Wigner releases is greater than the nuclear heat.

SCHONLAND You have given it a chance to come out on Tuesday midday. Did it come out? It started... Something came out.

RENNIE Something came out on one of the other channels. 1960.

SCHONLAND 1960 has not been up. It is not similar to the three curves to the triplets on Fig. 4 which are all below the top Wigner temperature.

RENNIE I do not think one would expect the nuclear heating to re-initiate something which had had its first stage release, and if one is accepting the possibility of this, there might be a chance of getting some more out later on. You

would have to raise it to above the temperature of the first stage.

SCHONLAND At 12 Wednesday you are below the first stage and shooting up.

RENNIE I agree you cannot get it out until it gets up to the same temperature, and the thing I haven't been able to get any sort of feel for is whether it is possible to get these rates of rise which are a few degrees under per hour in the graphite by transfer from hotter graphite, because there is hotter graphite in the reactor at that time, and I just have not got any feeling for this. I think one needs to know... one needs a picture of the temperature contours all over the reactor, from all the readings we have, to be able to try to do that sort of heat balance calculation. I think this would be worth doing, but I don't know whether we have enough data to do it.

DIAMOND You agree I take it that even although that temperature was falling after the knee on curve 5, the Wigner release was still going on if it wanted to. If there was any more nuclear heat even if there had been some quenching of heat at that point, Wigner release would have gone on. These after all are measurements of temperature.

RENNIE Not after once it had started dropping, I would have thought that once taking the initial rise, once it had started to go up, it was unlikely the temperatures would drop until the Wigner release had stopped, unless there was some form of quenching.

CHAIRMAN Would you like to call our attention to any other point.

RENNIE I do not think so, because I agree that it is to explain these temperatures which is the point. Once one has the temperatures, one can postulate some series of events which would cause an incident at that sort of temperature. One might expect other things to happen, but I cannot certainly give any other easy explanation of how you get these temperatures. After that it is possible to say what might have happened.

WITNESS: MR. BELL

(This witness was very indistinct and at times could not be heard at all. This results in the transcript being rather disjointed.)

CHAIRMAN Before we ask you to report on the question of ...?... there are two questions I would like to ask you. We have had quoted some of your work and I want to make sure that this is what it means - that it is right.

A statement has been made to us that your work has shown that once a Wigner release starts in a block of graphite it goes on even if you try to quench it.

BELL Yes Sir. Would you like me to describe the experiment very briefly. We were interested of course in whether one could stop the Wigner release in a block of graphite. One can always stop it in a pile because of the heat transfer between the blocks if one provides sufficient cooling. We took a piece of graphite with stored energy in it and if one can visualise a picture of the rate of release with temperature and the specific heat curve. We took it to the temperature

where it was just on the specific heat curve in other words the specific heat was now practically zero and we dropped it from that temperature to the solid CO₂ temperature, we got about 200°C shock and then we measured the specimen in the normal fashion and there was no energy in it at that stage, so we surmise from this that one could not produce a shock of that kind in a pile and therefore a block of graphite under these conditions would not, once it had started annealing would not stop. This of course depends on the size of the graphite - our specimens were 6" × 1½" diameter.

CHAIRMAN It was that experiment you think that was quoted to us?

BELL That is the only experiment I have done on that subject.

CHAIRMAN The second question, Mr. Bell, I think you may be able to comment on, is - I think you have there records on sheet no. 5 - and we are interested in the height of that first knee on the three records which are 25.50, 26.61 and 26.55. We are interested in that maximum which is about 260°. One of our witnesses said he was surprised that it did not go higher - perhaps to 350. I think what he had in mind was that 40,000 MW in this graphite possibly with some more which had not been fully annealed at such time. If you started off with Wigner release he did think it would go more than 250.

BELL I think that is so. I have, since this morning, got some information on some measurements we've been trying this week which I think are relevant to this. You remember I showed you yesterday a curve on which we based our estimate of 40,000 MW days. We have done some measurements on specimens taken from this reactor (?) immediately before this event and the point we measured is here. In other words there is a drop of energy here which we can only explain (this is quite hypothetical) by an accidental anneal in the pile at some stage in the past since the last anneal. As I say, this is only information received today and this is the actual position on the graphite as we have measured it.

CHAIRMAN I have not quite fully understood. Would you please go over it again.

KAY I think he was asking why was it as low as 250?

CHAIRMAN Oh, well I still haven't understood.

BELL This curve here is a plot of the Wigner rise against the cumulative MW days dose. Now the position we are at here - this vertical line to your right - is the actual temperature rise one would expect which is about 240. Therefore one would expect this type of rise above the triggering temperature, before you had reached the temperature of the order of 350. Triggering temperature is the term we use when we go over the specific heat. This curve here is the continuation of these measurements over the years, to larger doses, and my last measurement here which I have just had reported today is this point here which is about 165, where it should be in the region of 320 or 330.

CHAIRMAN Why has it dropped?

BELL I can only suggest (I would like to look at these actual records of course) that some energy has been removed that is the Pile had annealed at some point and then built up on this curve to this point.

DIAMOND When were these specimens taken from Pile No. 1?

BELL A week last Monday.

DIAMOND Before the event.

SCHONLAND Is there another explanation of this, which Simmonds gave in his paper?

BELL I don't think a sudden change of this magnitude is likely from Simmonds's results. He expects a decrease in the peak which is in fact what we are measuring here. There is a second reading here other than Simmonds's effect. Simmonds's results, linear rise results, in effect tell us that the greater the irradiation, the higher triggering temperature which occurs. The curve cuts the specific heat at a higher temperature, and also tells us, if we plot the ...?... of the curves, that and the actual peak in temperature will be less. At the peak reached, the amount of energy coming out will be less, and this is the evidence here and this is what is plotted on this curve.

SCHONLAND I should like to have an opportunity of seeing that.

BELL I have talked to Simmonds about this, Sir, this afternoon.

SCHONLAND He would agree with you.

BELL Yes. We can only explain this by accidental annealing.

SCHONLAND I have asked about accidental annealing and understand that there has been no accidental annealing since 1952. Now you raise it as an explanation of a result.

BELL I agree that as far as we know there has been no accidental anneal since 1952, but I can only explain that result at the moment on that basis. I am not saying that this is a definite reason but I can only explain it on that basis.

SCHONLAND In order to explain that low rise in the Wigner release maximum temperature, you have to assume that this portion of the Pile accidentally annealed at some time in the not very remote past.

BELL That is so.

SCHONLAND It seems to be a bit difficult.

BELL I agree, Sir, it is very difficult to believe this, and as I say it is my only explanation at the moment having had these three results an hour or so, is that is the reason. I can see no other mechanism for it at the moment.

SCHONLAND Is this observation checked from more than one sample?

BELL Yes. This is a check from several samples taken along a channel: the peak temperature...

SCHONLAND There can be nothing wrong with the measurement?

BELL I have not checked the measurements myself, but I am quite confident that the measurements reported to me are what we see from the instruments.

KAY Would you expect to find any record of a partial accidental anneal from the temperature measurements at a previous shut down. Have these been checked back?

BELL I don't believe that's been checked back. I think the only way one checks whether there have been accidental anneals is to measure the bulk growth. I am sorry... The roof clearance above the graphite measured the amount of growth of the graphite mass and these are recorded very frequently and these would show...

KAY You would get evidence of anneal there, would you?

BELL Yes. There is actually a report by Brim, I think, which had done this two years ago and you can actually see the increase in the growth as the temperature rises and a decrease when the Pile starts up due to the actual growth being recovered.

CHAIRMAN I would like to check one point. The curve which you have just shown to us, this knee shape - that is the same curve which you showed us yesterday - but you have added a new exposure? When you were discussing that before the horizontal X co-ordinate was the power that the Pile had actually developed and it was 30 to 40,000 MW - the vertical coordinate was the maximum Wigner rise which would occur anywhere in a Pile.

BELL Yes, according to our measurements.

CHAIRMAN Why should you assume that because the maximum for 40,000 would be 240, that that 240 should apply to those particular bits of graphite that were around here? Could it be that there were fluctuations of this through the Pile as big as 150 to 240, or is that out of the question?

BELL These particular points you are talking about are all fairly central and I do not think there would be any variation across that central region of that magnitude.

CHAIRMAN You would give away a little on the 240 for this reason but nothing like 100 which is what would have to have happened.

BELL From my experience there is very little variation across the Pile face. These points I should say are the maximum taken along a channel - the axial maxima - and that maximum is moving forward a little with irradiation.

CHAIRMAN Would you compare sheet 4 with sheet 5; those same channels. The maxima on sheet 4 in the early stages are barely 200 whereas on sheet 5 they are 260.

BELL I can explain a certain amount of that. Sir, by the irradiation temperature of the material: at 4ft. it will be about 20 to 30°; at 6ft. it will be possibly 10° or more higher - I cannot quote the exact figure, and one gets the degree of higher temperature with higher radiation, about 100° above the ...?... temperature roughly, but I don't think it would explain that large amount.

CHAIRMAN Mr. Davey has got something which he thinks would possibly explain this.

DAVEY I am sorry but I am not going to clear up any mystery, but when I asked your permission to go out, Mr. Bell had made the statement that he thought a possible explanation of this was an accidental release of some kind. The records show quite definitely that at no time while this Pile has been shut down has there been a temperature rise which necessitated bringing on cooling air. That is categoric. If there were a partial release during normal operation then it is possible that this would not be perceptible under the running conditions, so that my contribution at this stage is that you will not find records which will show a temperature rise accidentally while this Pile has been shut down.

SCHONLAND It might have occurred during running.

CHAIRMAN Thank you, Mr. Davey. Will you please resume Mr. Bell. I would like to spend one more minute on this 25.50 and compare the position on charts 4 and 5, one of which is 4ft. and another 6ft. The maxima in the one case is 200, in the other case is 260. Now maybe the thermocouples are not accurate to 20°. Maybe there is a 10 or 20° rise in the air temperature as you go in to 6ft. It does look a little surprising though, doesn't it?

BELL There is another point. May I sketch on the board. I drew a curve yesterday something like this. This is the charge face and this is the discharge face, and the difference between charge face and discharge face and one plots the energy storage in that area, one gets a curve of this form. Now there is quite a significant difference between the relative peak, and that peak lies between 5ft. and 7ft. therefore 6ft. 1 is actually on that peak. I don't know whether that answers your question, but 6ft. 1 is at 260 and the other one at 200 in this case. One would expect a temperature difference between these positions on a normal buildup of energy in this area. Now what I'm saying is that with these figures, that one might be 260 and that one 200.

CHAIRMAN The 4 ft. position and the 6 ft. position. You can't put that maximum still further in than the 6 ft. position so that in fact another one is going to rise up to 350? You could not possibly believe that?

BELL I would find it very difficult, Sir. This low temperature is near to the front; it is very sensitive to irradiation temperature. There is actually... Sorry, I was going to refer to Atri's figures ...?... but these do show the same sort of picture. I think summing up the many measurements we have made over the years, we have said quite rightly that the peak lies between 5 and 7ft. in from the charge face, and it was for this reason I think amongst others that the majority of thermocouples are placed at 6ft. in the Pile during an anneal, this being where we expect the maximum temperature rise.

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CHAIRMAN So that really apart from this rather false explanation which you have been suggesting, that there has been an annealing that has not been detected, you are at a loss to explain really why that has not gone up to 350°.

BELL I am certainly at a loss other than by this particular hypothesis. It is very significant that our measurements on specimens taken from the Pile immediately before the anneal confirm this low figure. As I say I still cannot explain why it should be down there. The only hypothesis I can put forward is this annealing one, which, as I say, is very difficult to believe at the moment.

CHAIRMAN Do any other members of the committee wish to follow this point?

DIAMOND How much energy is released during the Wigner release?

BELL About 75 calories per gram at the maximum point of storage, which is 6'.

DIAMOND If it had taken place over an appreciable part of the graph but it is possible that it might cause an increase in the coolant gas outlet temperature?

BELL During operation?

DIAMOND If it took place during operation.

BELL Yes, but I think that during operation one would have to have conditions which would blank this; if you are running the graphite temperature at 150 (?) in this area and all the blowers are on, then this will not release energy in this form unless the temperature of the graphite is raised, therefore the outlet air conditions must be different in that stage, due to other things.

CHAIRMAN Shall we proceed Mr. Bell then to the matter on which you were going to enquire, about the temperature records other than those on these charts. Professor Diamond raised an interesting point that some of them further away might give some evidence of why these rises in temperature were occurring due to thermal conductivity or Wigner release in these other regions.

BELL Shall I comment on these, Sir?

CHAIRMAN Yes, please.

BELL You asked me to run through the curves and make some rough sketches and give an opinion, but I decided to look at the recorder charts, but it was an impossible task to decide if there was any change and I got a team in last night to actually plot a representative selection of these, but I did run through every chart myself to see if I could detect anything. Everywhere I had any doubts in the chart, I had the curves plotted, and in a lot of cases there was nothing showing; it was just my eye looking at a very long chart, so I had what I thought was a fairly representative selection from the top of the Pile here to show the distance. I think the significant points here are that we are getting in most regions a very nice

anneal, the normal thing one would expect in an anneal, except for a 06.54 at the top here just off centre where we are getting something which is not explained on this type of curve but can be explained by heat drifting gently from other sources but this is purely, very, hypothetical.

CHAIRMAN What are the times on that. Give us a reference point of time.

BELL It actually starts at the same time as the curves you have in front of you and it is exactly the same scale.

PIERSON About, you say, on the Wednesday, midday.

BELL This one rises to peak about midday on the 9th. The only significant thing I can see from these curves looking at them in this form and it follows the other ones too, is that ignoring for the moment 0654, as one moves down here towards the centre, the curve goes from what we accept as the normal shape which is drawn in nice and heavily here, to the type of shape we are getting on the curves you have in front of you on curve 5 & 4 here where it goes up and jumps and comes down and drops away. It has that sort of plateau, mesa form in the centre. 0654 on this one does exactly the same as on that one, exactly the same form and the rest as a series of anneals. I am afraid this curve is confusing; we tried to get too many to one curve. But the form is quite nice in a lot of cases here. There are one or two, 0256 at the top, curves here and drifts off which is alright. This is the normal type of thing we get. The same at 6.64, causing difficulty again and 12.55 appears to go up. This is getting further down in the pile and then gives this jump again in the middle, about the right... about the time of all the rest of the curves.

DIAMOND In a curve like this one, 1044, does the Wigner start to be re-triggered at this point.

BELL Yes.

DIAMOND So the triggering temperature is about 50°?

BELL There are two things I should explain here. Our experimental work gives us always a temperature of about 100 or that region, 90 to 110. In the Pile records from these anneals we often find this apparently lower triggering temperature. It is probably due to energy coming quickly from some other part, from another piece of graphite which has gone off and has raised the temperature quickly at that point and shoots it up.

DIAMOND But surely that particular one there is a right angle? Could it be explained by conduction?

BELL On this scale it is a right angle. The curve actually as seen is of this form but we have compressed the scale terrifically and these points are not probably very accurately plotted. It should not be a right angle, that is true. There should be no sharp point there.

CHAIRMAN We spent time earlier saying that the Wigner release ought to produce about 250. None of these are as much as that.

BELL Some of them.

KAY Just one there. It seems rather the exception.

CHAIRMAN As a rule they are much less, more like 150. I don't know where it is fair to measure from, but they're all about that much.

BELL There is one further point which I should have mentioned before probably. This experiment we do here, on which we base this figure of 250, is done on material which has never been annealed. We know from experiment that if one takes a piece of graphite which has been through Pile anneals and anneal it so it has no low temperature peak in it, and take another piece of graphite which is virgin and has not seen a Pile, and put these together in a Pile then the new graphite stores more energy than the old graphite, this is probably 20 to 30°.

CHAIRMAN That goes a little way towards explaining this.

BELL Yes sir it goes a little way to explaining it, the odd few degrees off that one and a few degrees from this, put together. We only had two points at ten feet and I plotted these for that region. They are quite nice. I also plotted three fuel temperatures in this area for which I could get the records.

I should explain before anyone reads any points on these curves that we had a large team in last night and unfortunately the people who drew this particular curve got the scale slightly out. There is no more than a few degrees out anywhere, but we've had to tie it up with this peak here, which we've missed, to put this paper stuck on top. I'll have it redrawn if you wish.

CHAIRMAN We would like to keep these.

BELL We have taken negatives in case you want further copies.

CHAIRMAN Thank you. I think if the originals could stay here we will study them.

Now Professor Diamond, this was a thing of interest to you.

DIAMOND Several people have said, and I think Mr. Bell did earlier this afternoon, that there was some surprise that the knee on these three groups was so low, and yet these curves do not seem to have risen higher. You can confirm that this is a surprisingly low figure, this 250, if it is all Wigner.

BELL It is certainly quite a low figure if Wigner has come out. I should certainly have expected a temperature rise of 200 degrees or that order from these things, provided of course the main material has not been held at a temperature of less than triggering temperature, but above irradiation temperature for any length of time, which does have the same effect.

DIAMOND I think these require some study to see whether we can account for the inflow of heat to the area of interest, but I would like to return to the point of the difference in temperature on sheets four and five between positions four feet and six feet because we seem to have a temperature difference of 60° over two feet. I do not know yet whether that means that it is quite easy to maintain a temperature

differential of 60° over two feet due to gaps in the graphite. It does not seem that they stay together, these two temperatures, and its only over 2 feet.

BELL I have a very crude experiment which may have some bearing on this. It was not done by myself, but I remember seeing it done. Two large blocks of graphite were put together in actual contact and one had a heater put in the centre and thermocouples on either side. I can't remember the readings now but certainly one could get a block on one side of this so that it was uncomfortable to touch, and the other side was quite cool. There was probably 30 to 40 degrees at least difference.

DIAMOND This seems to me to indicate at the moment that it would be very difficult for a source of heat some feet away from a particular region to affect it.

BELL To affect it quickly, yes. I think it obviously does affect it in time. We spread the release by this. Of course, there is a conductive path from one block to the next via the tiles and ...? ... which are in contact, but I agree the only way one can get heat from one block to the next by the shortest possible distance is either by convection or by radiation and radiation at these temperatures is very low.

DIAMOND I think I would like to look at these other curves at the same time that these rises occurred here at 16.00 on Wednesday, and see if we can account for the heat flowing in here.

CHAIRMAN You will do that, not now, but as soon as you have the opportunity.

Any other questions for Mr. Bell?

SCHONLAND Something I may have got wrong in my mind. Earlier on Mr. Bell suggested accidental release at the 165° point.

Now release requires an initial temperature of 130 to 140°. Would it have been possible to have got such a release anywhere except at the far end, the hot end, of the channel?

BELL I do not think so sir. Not during operating conditions.

SCHONLAND Were these samples taken from the hot end of the channel or were they taken from the cold end.

BELL The cold end of the channel.

SCHONLAND Where the temperature was not 130 to 140, so how could it be triggered accidentally?

BELL Mr. Davey has assured us that this did not happen during a shut-down. Therefore I can't explain this at all. It certainly could not happen unless the temperature of the gas, or of the graphite mass itself rose. Now it could happen, if the temperature of the graphite rose, but not to the triggering point, for example if the triggering point is shall we say 100° and the graphite has been rated at 30, then if we take the graphite up to some temperature between these, say 60 to 70, and hold it there for some time, then the energy will come out slowly without a sudden increase. We have done this by experiment, again, to confirm this point, and we have

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removed (I am again quoting off the cuff very much) but holding a piece of graphite at 85 for 2½ hours gives a 25% reduction in the energy stored at low temperature peak. It was certainly of that sort of magnitude.

SCHONLAND So that in your view it would be possible to remove a good deal of stored energy at the cool end of the pile without reaching this triggering temperature of over 100.

BELL If the temperature was kept up for a lengthy period.

SCHONLAND It would be if it was running.

BELL During normal operational temperature conditions, the temperature is quite well defined to the channel. It would have to be abnormal conditions to cause this.

SCHONLAND Abnormal conditions during operation.

BELL Yes sir.

KAY I would like to search these temperature records with Professor Diamond later on, but may I just ask Mr. Bell at this point whether he has noticed any cases where the graphite temperature has been appreciably above 300 degrees centigrade during the earlier part of the proceedings. I am thinking now about curve 20.53 on sheet 5 which I think we have generally agreed is a bit peculiar, giving a long steady rise towards the end, and we have the apparently normal, perhaps slightly small rise of temperature of the Wigner effect at the start. At the end of the second period of nuclear heating we have a long steady rise gradually steepening up. Now if that heat is coming from some other part of the pile, it is coming from a region appreciably more than 300°C.

Have you measured any records with temperatures more than 300°C during Tuesday night to Wednesday morning?

BELL I do not think so but I would not like to swear to that without referring to the records again. I think some of the temperatures plotted on here are the maximum reached during the period.

KAY Would it be possible to have a look to see if there is anything. I am trying to think of something to explain that long rise of temperature on 20.53.

BELL Yes. The period you are interested in is at the end of the nuclear heating to...

KAY From Tuesday afternoon onwards.

BELL Yes sir.

DIAMOND And you feel that on this point here, this is the idealised thing almost, what one would like to see. That this record in fact will be due to a soaking of heat into this position from elsewhere in the pile and raising this up to the temperature at which it will trigger a Wigner release and that temperature is?

BELL About 100° sir, on our laboratory measurements, under laboratory conditions.

DIAMOND So that portion there is heat soaking in from the rest of the pile, that is 100 there you see. So that is heat soaking in from the rest of the pile.

BELL Well, one would assume it would be, but I find it very strange that... This has been the explanation, but drawn in this scale it looks very odd that it should be so. Looking at the recorder charts, watching it, it seems very reasonable that it should soak heat up.

DIAMOND On the recorder chart is there a change of slope there?

BELL On the recorder chart on this particular case, it jumps practically vertically, a very rapid rise.

DIAMOND So there is discontinuity on the recorder chart here?

BELL Yes. One has close-printed points and then there is about two points between that and the top.

DIAMOND We are not missing anything by looking at this compressed scale?

BELL I don't think so. I think the picture is much better here.

DIAMOND Well we missed that point.

BELL Well there is a slight bend here, which is on this here is perhaps discontinuity.

DIAMOND What I am looking for is evidence here that you could in fact trigger it off at a lower temperature, a temperature which might obtain at the inlet end of the pile, but you rule it out?

BELL I can only rule it out of my experience. I have not observed to my knowledge anything of this order in the laboratory.

KAY Coming back to this familiar group of three on chart 5, we tended to say that the actual Wigner release starts at about 100 and rises from there to about 260. This is really the same comment as Professor Diamond's, one about where does the Wigner release start on that curve there. May I suggest that the Wigner Release in fact starts at a temperature of about 40° and the actual Wigner temperature rise is about 220.

BELL From the curves this is a very reasonable interpretation, but again I must say my own feeling is that it would not trigger at that temperature.

KAY If it did then the curious feature is no longer why is the Wigner rise so small, but why does it start at such a low temperature?

CHAIRMAN There is another complication, these Wigner waves, if you have a large slab of graphite which is highly polarised (or whatever the word is for the Wigner) at say 20° and you heat up one end of it up to 200 rather suddenly, so that this end is hot and that end doesn't even know it's happened, and your thermocouple is here, this end starts Wignerising, releasing energy, and you get a wave here?

BELL (Here Mr. Bell referred to a report) I certainly tried this experiment you are suggesting, an experiment which copies this suggestion, and we stopped this because we had more important work and we could not get away from

trouble. It is not shielded by other blocks and we intended to take this experiment.

The two things which are causing difficulty are lower temperature triggering. Now this presupposes that we are measuring immediate transfer from the block on which the dowel lies and the thermocouple itself. I think it might well be that this temperature is out by quite a large number of degrees on the actual recorded temperature, unless you get a sudden increase, and this may well mean that the block is at 100 but the thermocouple is reading 60. If I may comment on Professor Kay's remarks which he repeated during the break: I think if you look at Simmonds report which shows a linear rise, measurements for highly irradiated which has had anneals, as compared with a piece of graphite which has had few anneals and less irradiation, the actual temperature tends to increase, the temperature release tends to increase and I think I should make this point in case you are wondering whether the temperature has decreased for some reason. I think it is more likely that the temperature read by the thermocouple and the temperature of the surrounding material, the difference between these may well be the significant point.

DIAMOND Due to the thermal resistance of the contact between the two pieces of graphite?

BELL Yes, it is after all only... contact, a round dowel in a round hole.

CHAIRMAN And you discard the possibility, at least you're not impressed by it, that a block of graphite which is strained and which somewhere contains a thermocouple of which one end is rather cool and the other hot, that it starts there and at 150, or whatever it is, is added on to this temperature, not to the temperature where it really started.

BELL I would only be impressed with that one if the word 'cool' were taken out and the block temperature was somewhere approaching triggering temperature. Then I'd think we'd get thermal waves, but ...?... due to thermal energy, the Wigner energy coming out but as I say, I think I'm quoting correctly that it was 345 here, that a thermal wave is not propagated in a Wigner damaged piece of graphite. Our own very crude experiment was a very brief one just to convince ourselves of this and it did support it, but we decided we didn't carry the experiment through to its conclusion.

SCHONLAND Mr. Chairman may I put a question. Bearing in mind that all the graphite in this lattice has been irradiated for some time, if Mr. Bell feels that a Wigner release can be initiated at a relatively low temperature, then how would he explain that we have to inject a considerable quantity of nuclear heat to initiate the release?

BELL I do not think that I implied that Wigner release would take place at a relatively low temperature.

?SCHONLAND That was your answer to Dr. Kay.

BELL No sir, it was exactly the other way round. What I said in reply to Dr. Kay was that the curves in Simmonds report, which are supported from curves from other sources, indicate that if one was irradiated and annealed material at

high dose, as compared to a low dose material, then the point at which the high dose, as compared to a low dose material, then the point at which the high dose curve crosses the specific heat, is at a higher temperatures than the low dose curve, which implies that with further irradiation one has to go to a higher temperature to release the energy. I am sorry if I did not make it clear.

This is quite a small interval as far as we know of course, it is not a huge one.

KAY I must admit I am rather struck by the fact that curve 10.44 in the new ones, which you have rather held up as a model of what a Wigner energy release should look like: if you accept my hypothesis though I'm quite prepared to be proved wrong, because I've probably started at 60 there, that temperature rise is exactly the same as the total temperature rise of this bunch of three which we were previously worried about. I put it that if you concede that the Wigner release can start at a lower temperature there is nothing further to explain about that group of three. They fit exactly in the picture along with 10.44 in this other series.

BELL I find it very difficult to concede that point. None of my experience leads me to believe it can come up to that temperature.

CHAIRMAN It stopped and it did not get as hot as it should have done, and you are stuck with your hypothesis that it did some annealing for which there is no evidence, and that's all you can say.

BELL That is all I can say from the curve.

CHAIRMAN Right then, we have one more headache. This is one of the puzzling features.

BELL I thought I had better mention that point, as it is a significant variation from the evidence I've given you before.

CHAIRMAN Yes it is. Are there any more questions for Mr. Bell?

DIAMOND I wonder whether Mr. Bell could think it at all conceivable that there are any regions, perhaps very small ones, in the pile which have never been annealed.

BELL I think it is very conceivable sir. I have a report by myself and Dr. Livesey which I brought down for this purpose. This is a plot, a very detailed plot taken along a channel, of the stored energy in that channel. Now the normal curve should be of that form. One can explain this curve by anneals in various parts of the graphite at various times. You get very severe discontinuities here, because that makes up the gap between the block.

This here is the control rod position dropping the ...?... and also this here, but you see you get that type of curve. The simpler one you get is this type of curve. It is the same sort of thing. One can get a build up, I think this then applies to this saturation point as well. This will build up to the saturation point but it is true there may be pockets in a pile which we have not monitored. In monitoring a block of that size is bound to be a sampling which have more energy than one would indicate from the normal curve.

DIAMOND But from this curve with the plateau on it, the temperature to which those unreleased blocks would rise when they are released is not very much greater than at the 'knee of the curve.

BELL This too is the part of the pile which normally anneals very well where the maximum is. These are the maxima taken from the curves in every case. Therefore in any other part of the pile towards the back where these packs are normally observed, if one does a complete scan, one might expect this to be lower than this.

DIAMOND Is it your opinion then that we cannot look to isolated unreleased blocks of graphite in the pile to be the cause of very high temperatures when they are released.

BELL If you are talking of 600 and above then I certainly think not.

DIAMOND Supposing they have never been released. When they are released for the first time, the temperatures are going to rise, you would expect, up to about 300.

BELL 300 I would say. If we extrapolate this to the pile gross now it is going to bring us up to perhaps 400.

DIAMOND So it is conceivable we might get 400 from a block being released which had never been released before.

BELL It is possible provided one stipulates the position of the block. That is the maximum position.

DIAMOND The six feet end.

BELL As the dose increases without annealing the curve tends to move forward and flattens out.

DIAMOND Where would you think these blocks would be?

BELL Quite near the front of the pile. The peak as given from the string we have just measured, which gave this point, is 33" in, which is just at the core.

DIAMOND Above the starting temperature, so you have got to add 100 to that.

SCHONLAND Add 100 to that, 100 + 400 equals 500.

BELL I should say one could conceivably reach 500.

CHAIRMAN The fact that you have got some evidence for unrelieved patches of graphite means that there is a pretty strong insulation between those particular graphite blocks, whether they are typical or not, are well insulated thermally from their neighbours.

BELL I think one also must remember in this context that the evidence we have for these patches shows that the patches are to the back of the pile 10 feet backwards in general, the main peaks. If this is true, then the energy storage at that point in the block next to them which didn't go off, is relatively low and therefore the temperature differential between the blocks is again low as compared with something at the hot front of the pile. I think this has got to be borne in

mind that there are these two things in question. It may be as good a thermal heat transfer medium there as it is at the front of the pile but due to the temperature differential, there will be less effect.

Friday 5.15 p.m.

CHAIRMAN I want to take Mr. Bell back to curve 5 and 2053 and ask the same question that I asked before because it worries me. My recollection is that you have this in front of you, 2053, that you thought this might well be a normal Wigner going a bit slow. Is that right?

BELL On this scale up to 4.00 on the 9th there was discontinuity. No it's not discontinuity, it is a change of slope.

SCHONLAND Oh, you said up to 4 o'clock? The rise after that is abnormal and it does not show on any of the new curves you have here, any 24 hour run up?

BELL I do not think so sir. I must explain that I have not had time to analyse these curves properly last night.

SCHONLAND Will you look through them very quickly and see if you see anything of that nature? This is what bothers me.

KAY I had not noticed anything following a step, following an apparent Wigner release. There are some which go up gradually all the way. ...?...

SCHONLAND I am not speaking of the step which is the nuclear heating step.

KAY Here we have 2253 which had one Wigner release which might, you end off as with the bunch of three below, but even ignoring nuclear heating which might account for that, then we start this long and steady rise.

BELL On the curves you have for 6' in, 1255 shows something of the same type on a smaller scale. It is not for such a long time but it does creep up in that way and is probably what Dr. Kay is referring to, and at 0564 which does somewhat the same but the change of slope at 1255 appears to be overtaken again. The slope changes twice. On this one here, I have seen this type of thing before where the temperature rises rapidly. You get drift up which is energy it is always assumed, coming from another part of the pile and then the whole thing starts cooling down so you do quite often get a sudden rise in temperature when it drifts upwards.

DIAMOND Is there anything significant about the fact that the curves which exhibit that characteristic seem to be the top ones. They seem to be the highest temperatures, yet we are postulating that the gradual rise is due to heat flowing in.

CHAIRMAN Yes.

BELL I agree with you. This is certainly very significant, looking at a single curve this is the obvious explanation one would give. But looking at a group of curves together, like this, then it appears certainly very strange that the heat from some other point is producing this. Some other point monitored by the thermocouple of course.

DIAMOND Some other points are not monitored.

BELL If one takes...: the thermocouples are distributed one, two or three per channel. If one considers a channel as being the monitoring point, which I think is a very fair thing, taken at the face(?) of the pile then it works out about 1 thermocouple point, one measuring point for about 60 tons of graphite. So it is very difficult to say if what is recorded on one thermocouple need necessarily have affected the other thermocouple.

SCHONLAND The last question would be if Mr. Bell should have another 24 hours of most intensive thought about this.

BELL I have had more than 24 hours, but I cannot explain this except on another source of heat. What that is, I have no idea at all. I just cannot explain the sequence of events except by some heat coming from some source and that source seems to be located in the area which was damaged in this period where all these temperatures go up.

DIAMOND And you do not rule out that source of heat being a Wigner release in a hitherto unreleased small section of the pile.

BELL I cannot rule that out sir because we don't monitor ...?... pile. I think the pile as a whole is monitored but as I said before it is purely a sampling monitoring. One could always be missed, because there are large areas of the pile in which one cannot have graphite samples.

CHAIRMAN Unfortunately Mr. Bell, the statement that you made, with which I agree, does not explain all the facts. It does not explain, it does not get over that sticking point that knee is not as high as we think it should be.

BELL I agree, I can only suggest, as I have done before, that my latest results may lead to an explanation of that. I am very hesitant to put these suggestions forward because these results amaze me, the ones I have had today. I must restudy this point to find out.

CHAIRMAN These were for very high mW. If that new pencil point had been along in the 40,000 mW we would have said you have got it or you might have got it, but it is in the wrong region.

BELL I agree it is in the wrong region as regards the thing in question. I brought this evidence forward because I thought you should have the latest...

CHAIRMAN It certainly offers a glimmer of light, it does come out that way, that the 40,000 mW also is low.

BELL I am trying to think of what material we have out which was... I do not think we have anything out which has precisely the conditions you are asking for at this point.

CHAIRMAN There was one little further job you were going to do for us. That was to see whether you could find which thermocouples other than those here and here, have a really hot reading to see whether we can find the sources of heat which might explain some of this.

KAY Yes I think so. I should be interested to know if there are any parts of the pile at 350°C or above at any time up to noon on Wednesday.

CHAIRMAN Please repeat that.

BELL To confirm "Are there any parts of the pile which were at temperatures of 350°C or above at any time before or up to noon on Wednesday the 9th?"

CHAIRMAN Well we would like that answer tomorrow Mr. Bell, by lunchtime if possible.

BELL Do you wish a note, or do you want me to come back again?

CHAIRMAN We should be alright if we have the information, but it might be better if you came. This is a very important matter.

BELL Do you wish me to leave these curves?

CHAIRMAN Yes please.

WITNESS: MR. TOWLER

TOWLER I am in charge of the technical section in the Operations Branch Headquarters, responsible to Mr. Ross for reactor operations.

CHAIRMAN I understand Mr. Towler, that you will be able to tell us something about the design characteristics of the isotope cartridges. Also the fuel element cartridge or only the isotope cartridges?

TOWLER I can give you the background to the programme which was laid on to produce the 100 g. per annum of AM for AWRE.

CHAIRMAN We would be very glad to have that.

TOWLER There are two stages in this programme. First of all we were concerned with producing 20 g. AM which ultimately became about 24 g. by about July 1956. This was achieved by means of the Mark 1 AM cartridge loaded into Windscale reactors at natural enrichment with a number of cartridges held at 1045. We found it necessary later on to slightly increase the reactivity of the reactor. When we were informed of the 100 g. programme it was necessary to increase the available rate of reactivity in the reactors and match this with equal absorption from an AM cartridge. To achieve this we designed what is called a Mark 10 cartridge. This is a cartridge enriched to 1.28 Co with uranium 235. At the same time the Mark 2 AM cartridge was designed to match this in absorption. The Mark 2 AM cartridge was manufactured in advance of the Mark 10 loading, and was used during the first programme, that is the 24 g. programme. It was loaded with a pile of natural and 1045 enrichment. In August of last year we became aware that the absorption cross section of the Mark 2 AM cartridge might not be equivalent to the proposed loading of Mark 10 which in order to give the additional reactivity had to be 2 natural cartridges, 17 Mk 10 cartridges, 2 natural cartridges in a stringer. We therefore carried out Gleep tests of the Mark 1 and 2 cartridges and various increased weights of AM in aluminium cans. The properties

in these tests were sufficiently close to the Mark 2 cartridge and Mark 1 cartridge to make it comparable. From these we determined that we would require the 195 g. of AM to match the additional reactivity.

The Mark 2 cartridge consisted of 75 g. AM in an aluminium can, which was fitted and loaded in a larger can which carried lead in order to give the required weight loading in the channel of 50 lb. per channel. We were thus faced with the problem of requiring to fill... either to get a larger can to get the lead in or use the existing can and add the weight at the beginning and end of the channel. These matters were discussed and it was proposed that the most reasonable arrangement which could be arrived at was to use 195g. channels in a 1" can, the weight to be added at the front and the end of the stringer. That reports factually the situation until we reached the situation of loading. In order to check the absorption of the proposed AM cartridge against the Mark 10 loading an exponential experiment was carried out and then subsequently about half of one zone. I think of pile 1, was loaded and this was checked a second time, and this gave us a loading of 11½ and 12 AM cartridges respectively to the 2172 loading.

CHAIRMAN Everything you have said so far Mr. Towler has been balancing the AM cartridge against the enriched fuel cartridge and putting it in the pile and balancing the neutron economy of the pile. Were you personally concerned in other aspects of this problem for example the AM cartridge should not go above a certain temperature in the pile. Were you concerned with that sort of thing?

TOWLER No. Perhaps I should have explained at the beginning that my function in this matter when I discovered that we should not be able to meet the production programme was to bring the people in R&D Branch, in our Fuel Element Design Office to discuss this matter and this I did. The sequence of events which I have described to you evolved in these discussions which were held and through which we fed back the requirement into the R&D Branch and into the DO who would design the cartridge, and the R&D Branch who would examine the feasibility of the proposition. My concerns were to obtain cartridges which would meet our programme. It is not in any way my responsibility either to design the cartridge of course or to develop it. These are the responsibilities of associated branches. My function was to bring the people together and to ensure that we were taking the steps to meet the requirements.

CHAIRMAN You merely have to say the requirement is so many grammes a year of AM and really from there on the temperature and so on were for the individual groups within a team.

TOWLER Yes this is how we worked. At the discussions, the Fuel Element DO, the R & D Branch and ourselves were represented.

We naturally on the Operational side were concerned with the problems of channel stability, neutron economy and this kind of thing. This is our proper concern in looking at the programme and we aim to ensure that the experimental work was carried out to satisfy us that we should have satisfactory methods of operation.

SCHONLAND If that last sentence is understood by me, and I am not as bright as I ought to be at this time of day, you said you arranged for experimental work to be carried out to ensure satisfactory conditions in operation.

TOWLER We asked for experimental work to be carried out in connection with the proposed loading for two natural 17 Mark 10, two natural uranium cartridges, against what we expected to be an AM loading of 12 and 13 respectively in the two piles and for this purpose the R & D Branch set up an exponential experiment.

SCHONLAND So it was purely neutron physics you were concerned with and not, say, the temperatures to which the cartridges might be subjected?

TOWLER No, the only specific answer I can give you in respect of temperatures, is that we anticipated the cartridges would normally be subjected to a temperature of about 200°C although of course during Wigner release when a temperature of 350°C was anticipated and this was the maximum temperature as far as I am aware at that time which was allowed in operation.

SCHONLAND I have information that a year ago the maximum temperature had reached 420°C for Wigner release. That would not be normal at all, would it? Would that be reported to you? Would you have any responsibility in this regard?

TOWLER I think the decision to raise the temperature to 420°C would be taken by Windscale Technical Committee. I would have no direct responsibility to make a decision on whether or not it was raised. I could make recommendations on technical grounds as to whether or not I agreed, but as a member of the Technical Committee.

SCHONLAND What bothers me is that a year ago it was reported that the Wigner release reached 420°C for 15 minutes and 400°C for several hours and that Mr. Towler is an expert on the satisfactory conditions for operating the cans, and these are stated to be 350°C.

CHAIRMAN Correct.

TOWLER You are incorrect in assuming that I regard myself as an expert on the manufacture of cartridges. I am responsible for technical functions within the Operations Branch covering a very wide field and I am not an expert on the subject. I have described to you the steps I took in order to get our respective expert fields to provide us with the cartridges we required.

KAY Would Mr. Towler confirm that at the time this Mark 3 cartridge was being designed that your group in the Operations Branch at Risley and the Design Office and the R & D Branch were working to the figures of a normal operating temperature of not more than 200°C and a maximum temperature on Wigner release of not more than 350°C.

TOWLER There is a specific reason. I believe to be correct, is that we were concerned with the Mark 1 cartridge with the melting of lead which I think is at 325 and this was one of the

reasons for changing from Mk. 1 cartridges: the lead melted and jammed the inner tube of AM and caused difficulty in the subsequent extraction procedures, and this melting was related to a proportion of cartridges, which I cannot remember, in which this phenomenon occurred during Wigner energy release.

KAY But there was no mention in these discussions at Risley of any temperatures above 350°C.

TOWLER I can't say that this was not mentioned at Risley but my personal recollection is that it was not mentioned but I would not regard that as proof that it was not mentioned elsewhere.

CHAIRMAN There was general understanding that the design was to be satisfactory for a short period at 350°C.

TOWLER That is my understanding of the situation.

DIAMOND Is it your recollection, Mr. Towler, the situation was that the lead was removed so that the cartridges could go above 325 or whatever the melting point of lead is?

TOWLER No, I explained earlier to you that this was solely to enable us to get more air into the can, in order to advance the reactivity of the proposed loading in the Mark 10.

DIAMOND But the lead having been removed, that particular impediment to going above the melting point of lead was removed.

TOWLER Yes but this is coincidental and is not related in any way. Our concern was to achieve the additional air and to balance the reactor up and to get the proof.

CHAIRMAN Is there anything you would wish to call to our attention, Mr. Towler?

TOWLER I do not think so. I think in the description of the events which I have given you, ...

CHAIRMAN Perhaps not so much on AM but on a fuel element cartridge, or anything?

KAY May I ask whether your group within the Operations Branch at Risley is concerned with the various other isotope cartridges which I think originated from Harwell, or is your concern confined to LM and AM?

TOWLER All demands for these cartridges are passed through us and we examine them before they are transmitted to the works.

KAY From an Operational point of view or from the point of view of safety and reliability as well?

TOWLER We would consider them from that point of view and then pass them to the works to have the final say as to their acceptability. If a cartridge was proposed that we did not regard as acceptable, we would take it up straight away. We would probably advise the Works and say, you may receive this demand, because demands often come in from more than one channel, and we would say that this proposal has come to us, and we do not think it is reasonable in such and such respects.

DIAMOND Do you have any new cartridge checked for heat release and the temperature to which it might rise and the position it is put in the pile?

TOWLER I can answer you in respect to AM. I don't think I can answer your question generally. We have calculations done on the heat release of the Mark 10 cartridges in order to determine the size that would be required to give the reactivity, the size to give the same heat release as the Mark 6 and at normal natural enrichments?

DIAMOND That is uranium?

TOWLER This is in the uranium. Calculations were done as to the heat release we would obtain with the various Marks of AM cartridge, but to the best of my own recollection this was not physically checked.

DIAMOND Have you these records? Have you the figures for them?

TOWLER No.

DIAMOND They do exist though?

TOWLER I presume so. I can recall calculations being done. Whether they came into any official documentation I couldn't be sure.

DIAMOND And how about the others, cobalt and so on. Are similar calculations done for them to your knowledge?

TOWLER I presume so, but this is well beyond my terms, so I couldn't answer authoritatively on this.

KAY May I ask you whether you did calculations on the AM cartridges?

TOWLER I'm afraid I'm not sure. It might have been one of my staff at Risley. I am almost certain it would be repeated at R & D Branch Windscale. Normally this would be their function to carry out this type of thing. We might, as in this case, and this is why I am aware of these calculations, do them for our own satisfaction, to check them.

KAY Would you have contact with R & D Branch in connection with that?

TOWLER Any steps which we take to co-ordinate this work, we brought in the R & D Branch at Windscale as the primary interested body in that Branch.

CHAIRMAN Well thank you Mr. Towler. I do not think we shall require you any more.

WITNESS: MR. TURNER

TURNER I am Deputy Director of Chemical Plants in the Engineering Branch, Risley. This in fact includes responsibility for design of the plants for manufacture of fuel elements and for processing them after irradiation.

CHAIRMAN Thank you. We have been asking questions from several people this afternoon about the design of the AM cartridges and also some questions about the design of the fuel element cartridges, and we have just heard from Mr.

Towler the story and the background of the Mark I and Mark III AM, and we thought you must have something also to relate about these design problems and specifications. What sort of specifications you made and who made them. There are two things in our minds, one is that this fuel element for AM has to meet certain neutron specifications. It must not upset the balance of the Pile power; the other specification is that it must be able to stand the maximum temperature it will actually experience in a Pile. We wondered whether you could give us your angle on these problems.

TURNER You will already know that there are three types of AM cartridge involved, the first one being the design including a small $\frac{1}{2}$ " diameter AM alloy rod, to meet the initial requirement of roughly 20 grams. All of these cartridges I imagine were pushed out of the pile long before this. The second design was one involving a larger diameter, $\frac{5}{8}$ " of AM alloy inside an inner aluminium container. These cartridges, the Mk. 2 design, were made in larger numbers to meet the larger annual requirement of 100 gms. The design of these was started something over three years ago now, and was considered at several meetings of the Thermal Reactor Cartridge Working Party which initially was asked to produce a design of cartridge for the Calder reactor for producing AM from those reactors. But in considering that problem the alternative proposal was made of getting the increased output of AM from the Windscale Piles by using some enriched uranium cartridges and putting a greater number of AM alloy cartridges in the isotope holes; and that Group of people, having started to consider the problem for Calder, carried on for a few meetings. I can't remember how many, and arrived at a design of cartridge for meeting the requirements and conditions as then stated, in the Windscale Piles. I think you are familiar with the discussion at the main meeting of that particular Working Party, which considered three alternative designs and recommended that the first alternative should be adopted: to work within a maximum temperature of 350°C during the Wigner release. Now the details of that cartridge were rounded off by two or three meetings and put out in the form of working drawings approved at the level of Assistant Chief Engineer in my office and subsequently submitted for final approval to the Works Manager.

CHAIRMAN What part in all that were you and your staff doing? You were given the specifications – what did you do, actually design it? There was so much thick aluminium, so much lead, so much diameter, that sort of thing, or did you have to consider much more intricate things such as thermal transfer or problems of that sort? I am not quite sure just what your staff were doing. What were they responsible for? They took information or they did something; what was it they did?

TURNER My own staff in this were concerned with ordering of the components for the cartridge and of the equipment for making the cartridge. I emphasize my own staff as distinct from my own position as Chairman of the Working Party which you realise included representatives from several other Sections of the Organisation.

CHAIRMAN The temperature of 350°f which is quoted as being the maximum temperature achieved in the Wigner release and therefore the cartridges must stand at least 350 – where did that come from?

TURNER That, if I remember rightly, goes back 3 years and was the temperature stated by Phillips who, I think at that time, was still in charge of the Windscale Piles. Gausden's predecessor. I think I am right in this.

CHAIRMAN Was it part of your job as Chairman of this thing to check that figure experimentally or in any other way, or was it also your responsibility that being given that figure you had to check that the fuel element would in fact stand that temperature. Are you responsible for that?

TURNER As Chairman of a group of people of that sort in which several experts are involved, I naturally call for information from the particular expert involved, on a particular matter, and expect the information given to be – well how shall I put it – if the information asked for is a question of fact, I would expect the answer given by the expert to be a statement of fact. If he is giving information on reactor operation, I would expect the information given to be a statement of fact, if he is giving information based on R or D work I expect that again to be factual information on the results of his tests.

CHAIRMAN Perhaps I should give members of the Committee the opportunity to ask questions.

SCHONLAND I do not think my questions will do more than clear up a point. As Chairman of this Working Party you ceased to have responsibility for the cans and the cartridges once the Working Party had decided on a specification. You do not have periodic meetings to review the specifications do you, to find out how the cans are behaving?

TURNER This depends on how long the particular problem carries on. You see the general way in which design of fuel elements is kept track of is this: I think you know already that for each main reactor project a Design Committee is set up to review the design of the reactor as a whole, and such a Design Committee has in each case, so far, appointed a Fuel Element Working Party which has been first chaired by a Reactor Design Office Chairman until the design of the fuel element has been established. After that date the Chairmanship has changed to my side, at the stage where the remaining work became predominantly that of filling in the details and of considering the actual methods manufacture of the fuel elements. Such a Working Party has normally reported to its parent Design Committee until that Design Committee has ceased to exist, and normally this has covered the period of commencement of production of the fuel element, until the main snags in manufacture have been overcome. After this date the Fuel Element Working Party has ceased to be. After this date there is normally quite a long period before any further information is available on the performance of such a cartridge and subsequent reviews are normally made by the Technical Committee of the factory.

SCHONLAND Well the point I am getting at, the information given you by Phillips which may have been correct at the time, was found to be wrong in November 1956, when in fact the maximum temperature was over 400. We want to know if that information would come back to you or not. The information was wrong.

TURNER I have no knowledge of that. I had no knowledge of an increase in Wigner temperature until the last few days.

SCHONLAND That is what I was trying to get at. If you had been continuing the session with your committee to follow up how your cans were behaving you would have been informed of this. In fact they were designed for 350° and the Wigner temperature was over 400. You would have been informed of that if that had been a function of your Committee to keep in touch with the product.

TURNER It is a point which ought to have come out if there were a continuous review.

PIERSON Is there a Thermal Reactor Cartridge Working Party?

TURNER There is a Thermal Reactor Cartridge Working Party which, following the disbanding of the parent body, the Thermal Reactor Design Committee for Calder about a year ago has reported to the Calder Works Technical Committee and this, strictly, has been concerned mainly with the Calder reactors.

PIERSON So there is no Risley regular review of Windscale cartridges.

TURNER There has not been a Windscale Fuel Element Committee for several years since the disbanding of the original Windscale Pile Design Committee.

KAY May I ask you about the Mark 10 Fuel Element, in the Windscale Piles. One appreciates the slight problem that obviously arose, when the original Design Committee and the original Design Office has disappeared. But there are two aspects to a fuel element design. There is the design from the point of view of the reactor – normally almost the starting point of the reactor. Secondly there is the aspect which I think Mr. Turner is mainly concerned with, with regard to the ordering of the materials and detailed specification of the manufacture. The bases of... The two aspects concern both the isotope cartridges but even more they concern the fuel cartridges. I was wondering who really initiated the design of the Mark 10 cartridge. Is that done entirely in the F.E.D.O. If it was a new ...?... altogether that would start in the Reactor Design Office. What happens now when there is no Risley Design Office.

TURNER In this particular case, going back about 3 years, the design of the Mark 10 enriched cartridge started off in the physics portion of the Technical Section of the Engineering Branch, about the time that the just before the Physics Section of it was transferred to R & D Branch side. The group of people who started on this in the Technical Section, soon after this date, moved over to the R & D Branch side into the Physics and Engineering Section there, and some of their work on this overlapped for a while.

KAY So they took the place of the D.O. in initiating the technical features of the fuel element.

TURNER That is so, and they were the people who put forward at this particular meeting of the Thermal Reactor Cartridge Working Party, the basic design of the Mark 10 cartridge.

KAY And you took it on from there as though it had originated from a Design Office and worked out its detailed design and considered its manufacturing problems.

KAY Originally the Physics Group of the Technical Section which was transferred to the R&D quarters and is now the ... of the R&D.

They would deal with what I would call the Nuclear and Thermal Design.

TURNER That is so. They in fact worked out their proposals in this case in conjunction with Phillips, the then Manager of the Reactor.

CHAIRMAN Any more questions? Mr. Turner, is there anything to which you would wish to call our attention, something significant might have occurred to you.

TURNER Well, I know very little of the actual evidence that is available. Am I right in thinking that it is established that the actual temperature during Wigner release went up to something in the order of 410 to 420.

CHAIRMAN The only evidence that we are quoting is evidence that had been given to us about a release last year which seems to have been in that region, is the evidence given, we are not talking about the recent event. We want every bit of information and help we can get.

(There is a discontinuity here and the succeeding section appears to refer to the earlier discussion with Simmonds).

CHAIRMAN Mr. Bell has shown us one more point on a curve. The curve was the Wigner release versus MWD in the pile and whereas the curve as we ...?... it was going was nicely bending over, just as you have said it ought to be, then at ...?... MWD it becomes very low. Now that is surprising and we wonder if you have any comments?

SIMMONDS Well the immediate suggestion is that it was an unscheduled release at some time between. That is the obvious explanation, but ... I think you'd want more results to be quite sure.

CHAIRMAN It is so ...?... that you think the whole curve is in question?

SIMMONDS Well, the whole curve.. it seems too sudden a change.

CHAIRMAN One point as against a dozen.

SCHONLAND There's also some difficulty in explaining an unscheduled release at that point in the pile. The temperature in the graphite ...?... 200.

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SIMMONDS Well I haven't looked into the conditions at that time.

SCHONLAND We don't know the time.

SIMMONDS If we don't know the time, we can't get much further with that.

DIAMOND But the temperature to the point would not have gone up above about 50°C.

SIMMONDS Why not? Sorry, I can't see why.

DIAMOND The position in the pile.

SIMMONDS Well, this may happen during shutdown.

SCHONLAND No we are assured that it didn't.

SIMMONDS Assured that it didn't? I see. You know it didn't happen during a shutdown in this case.

SCHONLAND We have evidence that there was no unscheduled shutdown.

SIMMONDS I see. I can't imagine any conditions in which it would do that in operating conditions ...?... observation. But then there are already several points I believe supporting that which haven't been fully worked out or haven't been drawn.

DIAMOND If that curve were to come down and that is really a point on it, doesn't this mean that as the radiation dosage goes on it becomes a sort of energy release mechanism instead of an energy storage mechanism?

SIMMONDS Well, this tendency I believe is there, and I am saying that the peak in the curve here does go through the maximum all the way. There is evidence that this does happen ... there is American data for example.

DIAMOND And this point was obtained with an adiabatic test?

SIMMONDS That is true.

DIAMOND And therefore it has less energy in at 40,000 or whatever the dose was - I forget now - less energy than it would have had, had it lain on the curve that had been drawn previously.

SIMMONDS Yes but it's not a true adiabatic test. It's only an adiabatic test if you know that the adiabatic curve is one which flattens off. If the adiabatic curve is going to be different from that, then you can't describe this as an adiabatic test. It is conceivable to imagine a curve.. for

example, if this peak goes only slightly above the specific heat, the dipping technique would probably give a low value, a lower value than a true adiabatic test would. In fact this does happen. The dipping technique seems sensitive for smaller amounts of energy.

DIAMOND How was this point obtained, with the dipping technique?

SIMMONDS Yes.

DIAMOND Not a true adiabatic test.

SIMMONDS No, that would be extremely difficult to do. The only evidence against that is the rather sudden change, the curve coming up and dying away down. It's rather a sudden change.

DIAMOND To put it in its simplest terms, if that point is right, it means that as irradiation goes on and is stored, there comes a point when it begins to release energy?, because it seems to have less stored energy than samples with lower irradiation.

SIMMONDS No, what is happening is that the stored energy is being redistributed, in the temperature spectrum. The tendency is exactly the same as the one shown in my report, but it is a little more sudden than one might expect.

SCHONLAND The stored energy is going up to/coming out at? a higher temperature than the release.

SIMMONDS You are saying that at a smaller irradiation you get a curve like that, and with a larger irradiation you may get a curve like that. And if the specific heat is ...?... in between, the true adiabatic rise loses its precise definition as soon as you depart from a curve which falls away. Only when you have a peak with a ...?... of energy in, can you talk of a specified rise. If you have a curve(?) of this type it becomes a little indefinite. We can only talk about a fixed rise when we have a peak which falls under/along(?) the line of specific heat, beyond it. It is conceivable that that curve would give a low value on the dipping tests and yet not be a low value of stored energy. But still that is rather a sudden change - it looks a bit too sudden to believe. My first interpretation was a unscheduled release, but

CHAIRMAN Well although it is a series of points; that point is an average of several, it is only the one channel, so it's a bit dangerous to put too much weight on that one observation, but it does give cause for thought.

SIMMONDS Indeed, yes. You do need the linear rise data on these things to understand them fully, but they are slow to obtain.

Board of Enquiry

Saturday 19.10.57

Evidence heard from:-

Bell	pp 3.1 - 3.4
Eastwood	pp 3.4 - 3.6
Charlton	pp 3.6 - 3.10
Moore	pp 3.10 - 3.12
Ross	pp 3.12 - 3.13
Tuohy	p 3.13
Hall	pp 3.13 - 3.14

This evidence was taken at 11 a.m. on Saturday, 19.10.57

This witness was very indistinct and at times could not be heard at all. This results in the transcript being rather disjointed.

CHAIRMAN Mr. Bell, we asked you to do a little investigation, but since then there are a number of other questions we would also like to ask you about. Perhaps we could run through them first.

The first one, I am not sure I am asking the right man and if not perhaps you would say so. The question is this – suppose that some time during Tuesday or Wednesday when this pile was in this warming up de-Wignerisation condition that one of the AM cartridges started smouldering. The question is would the meter on the pile, up somewhere in the stack, that is the Pile Stack Meter, would that have shown any activity do you think or not?

BELL Well, Sir, that is not my line at all.

CHAIRMAN Who would be the man we should ask?

BELL I am afraid I cannot think of anyone in R. & D. I think it would have to be one of the pile people. I cannot suggest a name.

CHAIRMAN Never mind, we will wash that question out.

One other question I would like to ask you. You have spent a lot of time looking at these temperature records in the graphite and so on. Have you personally ever had reason to suspect the accuracy of the thermocouples?

BELL No Sir. These thermocouples, as used now, were put in because of the inaccuracy of the previous thermocouples. We feel that except for the odd one that fails of course, that they give an indication of the temperature. I would not like to commit myself and say they are accurate as a whole, but I think the trends are certainly all right. But it is impossible to do an actual measurement in the graphite stack itself, to do a comparison which is really needed before you can answer this question.

CHAIRMAN Has that been done?

BELL I do not think there has ever been a measurement of the graphite stack as a whole with a thermocouple actually embedded into the graphite mass itself. All measurements have been made either by the original technique which was a sprung dumbbell (I think is the best way of describing it) which was a split graphite cylinder with springs in it, down a vertical hole and then we removed that type and put in dowels which lie in the channels as you have seen.

CHAIRMAN Would it be fair to say that, when you are given these records and find the thermocouples like all thermocouples have not gone haywire, you have trusted them within 10 degrees or so?

BELL Yes, that is about the order we would take. We normally quote our temperatures in the graphite specimens we have to plus or minus ten for this reason.

CHAIRMAN I think one more question, Mr. Bell and I want to refer to one of these charts. I want to look at chart number six.

BELL Can you tell me which one that is? Mine aren't numbered.

CHAIRMAN It is the graphite temperature 10 feet from the charge face. Now on previous occasions when we have been talking to you, we have looked at the knee on three curves, you remember the three we had, and we were puzzled because that knee only went up to 260° when all the people that know about these Wigner releases had thought perhaps 350 would have been the right amount. Now there is on this chart that we are now looking at, at 25.50 which does in fact go to 300 at any rate, quite quickly, and that one is ten feet back and it is in the same general region of the pile; ten feet back, which according to you is well past the optimum, so that one at any rate is pretty well what you would have expected?

BELL That is the sort of rise we would have expected. I would have to refer to my other graph if I may. You would expect there with a fair dose I think about 200-220° rise, which is what we are getting.

CHAIRMAN Yes that one is fairly well what you would have expected in the early period at any rate.

BELL It is if anything a bit high.

CHAIRMAN Well now, I believe some other members of the Committee have some questions to ask.

SCHONLAND I had a look through the Wigner releases for 56 and would you agree, Mr. Bell, that these are perfectly normal curves, or would you

BELL From memory, yes. May I see them before I

That one is perfectly normal certainly, Sir and there again I find nothing unusual, nor in these ones, except for the possibility of this 19.60 at 10 feet which has crept up to a high temperature in a fashion very reminiscent of the present curves we are considering.

SCHONLAND If you hadn't seen the present curves, you wouldn't have worried about 19.60?

BELL I might, having seen this curve plotted on this scale. I certainly should not have done so from the recorder charts, the actual creep here is what we expect from heat coming from another part of the pile.

SCHONLAND Rather a peculiar one?

BELL On present evidence, Sir, yes.

SCHONLAND Looking back with hindsight, (BELL Yes) The present curves have a good many of this 19.60 type haven't they?

BELL Yes.

SCHONLAND You would be able to tell us the reason for the appearance of this slow rise in our present curve.

BELL Well, in general, Sir, one expects a slow rise in some thermocouples recording because of heat coming from another part of the pile, but the present form of these curves indicates that it is not that type of slope at all. There is, I think, some quite large source of heat it appears to be, but I have no suggestions as to what that might be.

SCHONLAND Something anomalous?

BELL Something anomalous.

SCHONLAND Would you exclude some peculiarity in Wigner heating like pockets which have not been annealed, could you exclude that?

BELL I would not exclude them, Sir. I feel it is unlikely, but I could not exclude it. There is not any evidence from measurement on the reactor to provide or disprove it. There will certainly be pockets in the reactor, but that is as much as I can say.

SCHONLAND Have you any idea how we could get, not necessarily within a few days, but ultimately to the point where we could say that this slow rise type of curve is not due to Wigner energy releases in some form which has not occurred before. I will put this in another way. These papers are all very beautiful but they are not typical of the Windscale piles. They are almost academic exercises in the study of the subject.

BELL That is true, Sir.

SCHONLAND How would you get to the truth with regard to the reactor itself if you had all the time in the world and all the assistance?

BELL There is only one way to be perfectly sure and that is to core the graphite, and get an actual specimen from the bulk of the graphite which has been there since it was built. If this could be done I think we could measure it. The other possibility is that probably in the pile II at the moment, and possibly in pile one if we are very sure about temperature measurements there, there may be some of the dowels which were put into the pile originally which have not been touched. I know there are probably some on the periphery, but I doubt whether there are any near the centre, but these would probably be worth taking out to get the type of curve of which Dr. Schonland speaks.

SCHONLAND These dowels have not been taken out during annealing periods.

BELL There are some, I believe, still in the reactor which were put in when the reactor was built and are still there.

These were put in to reduce the airflow through the isotope holes. Of course in the centre of the reactor where the dose is high, all these have been removed to provide isotope

radiation facilities and experimental facilities. There is a ring of graphite at about half the radius, in that general position, which is used for monitoring, but this has been taken out and annealed in the laboratory at various times and is not truly representative of the pile. I have also one piece of graphite, one only I am afraid, which was taken from 1961 in pile isotope hole earlier this year from which my mpl measurements were made. The remains of this one dowel is left and I have reserved this for this measurement (mean rise measurement) to try and get some information, think can be to linearise measurements to get an estimate information it has had 25/29 of pile history.

Now this has had 25/29th (this is our estimate) of the total pile history.

It has had all the anneals in the pile up to the time it was taken out, which was after the November 56 anneal. Sorry, I'd better say I **think** it was taken out in November 56, it may have been out before the anneal, I would not like to swear to this.

SCHONLAND But really to get a true picture of the Wigner energy in pile one, if you had the same mix in the reactor as Pile 2, you would have to core it.

BELL There may be some graphite etched (?) of course, but I doubt it. If there is we'll use that, if not we could core it. I would almost certainly say the only way to be sure is to core the graphite, to be very sure that you are getting a representative sample, because we cannot be sure that the graphite in per isotope hole has exactly the same history as the graphite in the core itself.

CHAIRMAN Professor Diamond.

DIAMOND Mr. Bell, can we go back to your discussion on the thermocouples? Do you know when the thermocouples were changed?

BELL This is a very long time ago, Sir. It must be all of four years. I did an experiment to measure the distribution of temperature through the graphite mass and I think it was at this stage that the type of boat was redesigned and taken over as the standard type. I think it was about 52 to 53, I would not like to swear to the exact date.

SCHONLAND Is there anything peculiar about the thermocouples which were taken out that we should know? What are they made of?

BELL This reply could not be heard. Chromel alumel or iron constantan.

DIAMOND Do you consider that there is a difference in the characteristic of stored energy with distance from the fuel element. For example, on the surface of the fuel channel there is higher fast flux than there is further in the graphite. Would this make a difference?

BELL One would expect that as the damage is caused by fast flux to the best of our knowledge, that the point of highest damage would be at the point of highest fast flux (the remainder of this speech was too indistinct).

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DIAMOND But the dowels are the most remote.

BELL Yes.

DIAMOND So the dowels may not represent the conditions of the graphite at the face of the cooling channel.

BELL That is true, Sir.

DIAMOND There is one last question, Mr. Bell. These two curves of 25.50. These triplets we talk about – these three curves. At four feet from the pile face, no sorry six feet, that is curve five. They come up to just over 250 and there has been some mention it is perhaps not as high as you would have thought, and yet at ten feet, which is beyond the position of maximum storage and yet is the position of maximum flux, it has gone up to 300.

BELL That is true, Sir.

DIAMOND Is that significant?

BELL It is certainly unusual. My measurements taken along the channel have always indicated that it falls off at ten feet quite significantly.

DIAMOND Then this large temperature rise further along the channel is a new development? Is this the first time it has been noticed?

BELL This is the first time I have noticed it. It has not been mentioned to me. I would like to go back here, I think in November 56, are there not some higher there? I have a suspicion that one or two of these, I think it is November 56, they vary. Some parts of the pile are highest at 6 feet, some highest at 10 feet and some highest at 4 feet. In November 56 the same thing applies: some are higher at 10 feet than they are at 6 or 4 feet. These two curves.

DIAMOND So November 56 was the first time you observed that?

BELL To the best of my knowledge, Sir. I would not like to say without going through all the records. We have had so many anneals that I cannot remember the details of all.

DIAMOND Thank you very much.

Can I just return to the dose rate again with the distance from the fuel element? Have you any idea of the difference in dose rates, maximum – minimum?

BELL No Sir. Not dose rates, sorry: stored energy. The only information I think we have on this are some measurements on ? which Kinchin did some years ago in BEPO. I haven't got this information at the moment.

DIAMOND There might be quite a difference?

BELL There might be, I would not like to say.

KAY May I come back to thermocouples for a moment? You mentioned earlier that there had been a change because they were unsatisfactory or inaccurate. In what way were they unsatisfactory?

BELL I am speaking very much on memory of many years ago, but certainly when I first came to this factory the measurements of graphite, to the best of my knowledge, were made by spring dumbbell type of thing, and when we did our experiments to measure (these were just point measurements in the graphite mass through a vertical hole) these I think we slid up and down too to get to the position one wished.

When I did my experiment to actually get a measurement of this temperature, this was a request from Harwell for some oxidation measurements they were doing at the time, we put dowels actually in a channel and compared the figures obtained from our measurements to figures from the vertical holes and these were quite widely different. I think ours were higher, but I am speaking from memory, and from then onwards I do not know what action was taken at this point, but the thermocouples were then started to be used in the type of dowel we use now.

KAY The measurements in isotope dowels were reading higher than the previous one by what sort, say 30 to 40°?

BELL I think it was of that order.

KAY This was a question of the nature of the contact with the graphite. The actual thermocouples were the same in either case?

BELL I think the thermocouples were actually all right in this sense, it was just a matter of making contact.

KAY Has any check been made of consistency of accuracy of measurement? You did say earlier you believed these to be accurate to $\pm 10^\circ\text{C}$. Is that based on any check or calibration?

BELL As I say I did this experiment some years ago and the scatter along the curve showing the development of temperature towards the back of the pile was small, which gives 9 thermocouples (degrees?) along this length.

KAY Along a channel axially?

BELL Axially, yes.

KAY You could draw a smooth curve through this which made sense with only about 10 degrees scatter.

BELL The curve was much better than that. We have done this again quite recently and the same type of thing applies, the curve was better then, so the scatter is not between thermocouple and thermocouple, but we estimate that during operating conditions, the fluctuation during any period of time, due, for example, to ambient air temperature changing, will give us $\pm 10^\circ$.

KAY You have no reason at all to suppose some of these might be reading as much as 30 to 40° out?

BELL No reason at all, Sir.

CHAIRMAN We might now turn to the questions which we asked you yesterday. You were looking at. I think the question was by Prof. Kay. What temperatures in the first two or three days actually went above 350°? Could you tell us what you have found?

BELL I have examined all the charts except one which I have not been able to trace (I have had ? information on that point) and on none of the charts have any of the temperatures, other than those plotted on the curves you have in front of you, gone up to 350 before 12 noon on the 9th. Recorder G, which I cannot find the chart for at the moment, the following points were already plotted in the curve - 28.68 at six feet, 32.56 at six feet. These are already plotted on the curves you have. Now 28.46:- This recorder was initially transferred to another recorder before this time. The maximum reading on that was about 75 so that one can be ruled out. From the log which was kept; the system is that during an anneal there is a man on the roof all the time reading these figures. He records triggering time, triggering temperatures, the maximum temperature reached, and the information on the remaining thermocouple is in the log. 31.62 at six feet never got above ambient, and the suspect thermocouple was not connected; it was u.s. 30.50 at four feet triggered on the 9th October at 06.25 at a temperature of 75 and went up to 280. 30.50 at six feet triggered at 06.35 on the 9th and triggering temperature was 80 and the temperature after release 270. These releases were very definite half hour and two hours from the minimum to maximum.

There is no maximum temperature recorded on the log but I am assured by the first keeper of the log that the temperature went to 270 and dropped, so apart from the curves which you have in front of you, there are no other temperatures of 350 or above. Would you like this note leaving, Sir?

CHAIRMAN Yes please, I would. Does it say what you have said?

BELL Yes Sir, it answers all Professor Kay's original questions.

CHAIRMAN Any questions on this? In that case, Mr. Bell, I think we may let you go. We shall not want you again today.

DIAMOND I should be interested to have Kinchin's report. My recollection is that you can have a factor of about 10 or so difference between ? factor through a lattice.

BELL On this facility?

DIAMOND Yes Sir - I cannot remember exactly.

BELL I will get this report, Sir and make sure that a copy is brought down here. But again I want to comment, I must raise this point of saturation effects on this one, and the position is not 10 times, it is something less.

CHAIRMAN I believe Dr. Schonland has one more question.

SCHONLAND I want to ask whether Mr. Bell has detailed knowledge of incidents and deWignerising say, at Hanford. Is this available please?

BELL No Sir. The only American work I know of is the practical experience of the Brookhaven Pile which Mr. Sheard went to watch. I have talked to him and I have also read his report and the Brookhaven reports, but I have not seen any others.

SCHONLAND The Hanford people do not let us have results, other than samples (?), the dose.

BELL I have not seen any. I do not know whether the results are available to us; I doubt it, because I see most, if not all, of graphite work that is reported.

SCHONLAND The Brookhaven Pile - in what sense is it comparable with the Piles here?

BELL It is graphite moderated, air-cooled. It is cooled from the centre rather than the outside. The air goes in the centre and blows upwards. Again I am speaking from memory of some years back. I think we saw the results they got on various measurements made of the recovery of growth and that sort of thing tie up very well with ours.

SCHONLAND What is the relation for flux to ?

BELL I cannot tell you off hand, Sir. But the other Pile of which you have knowledge, BEPO is down by a factor of 10 in flux.

SCHONLAND And the Wigner releases there?

BELL There has been one during the Pile life - of BEPO which I watched - but I cannot remember. Everything went all right as far as I can remember.

SCHONLAND How many do you think there have been at Brookhaven?

BELL I am pretty sure there has only been one, there has been no indication otherwise. It was some time before they found it necessary to anneal. I think the temperatures were high here.

SCHONLAND Thank you.

CHAIRMAN Thank you then, Mr. Bell. We shan't want you today or tomorrow as far as I can see. Could you leave your telephone number just in case; you need not hang around though.

WITNESS: MR. EASTWOOD

CHAIRMAN For the record, Mr. Eastwood, would you begin by saying what your job is in the Authority.

EASTWOOD I am Deputy Head of the Isotope Division and apart from that I am in charge of all the irradiated isotopes that are made either in BEPO or in the Windscale piles with the exception of certain materials that are made by the Radio Chemical Centre. Dr. Charlton is here with me - I expect you will be seeing him after me.

CHAIRMAN We have spent a good deal of time looking at the isotope things, for which you are not concerned. AM and LM, but there were of course in Pile No. 1, a number of channels with isotope cartridges and we would like to have a little account of what they were and what they might have done. We are trying to look for causes. The sort of things we have in mind are, take for example a Cobalt one, there is this radio-active cartridge. Perhaps you could tell us a little bit about its design. A point on which we are very interested is whether that particular cartridge, when the Pile was shut

down and the Wigner release was proceeding, whether that cartridge could have got hotter than its surroundings due to the slight amount of radio-activity that is in it. These are the sort of questions. What I would like you to do is to begin by giving a short account of those things for which you are responsible which were in the Pile.

EASTWOOD Yes. In Pile 1 there are approximately 17 channels which are occupied by our materials: I say approximately because there are several other requests, for which we have not had the paperwork back from Windscale, there may be in fact three or four extra. The majority of the material, except for about three or four channels, is entirely Cobalt in a metallic form. All these pieces of Cobalt are individually encased and hermetically sealed in aluminium capsules. This is to prevent their oxide from getting off when we use them subsequently. They are loaded in the same type of slugs as we use in BEPO, that is 1" dia. and 12" long aluminium cans.

The Cobalt comes in various sizes – I have actual exhibits if you want to see them, Sir. There are some small cylinders which are approximately $\frac{1}{2}$ " dia. \times $\frac{1}{2}$ " long. There are some rods which are 4 m/m dia. \times 10 cms. long rather looking like a shortened pencil. There is also a type which is a disc 17 m/m dia. (that is about the same diameter as a shilling) and about the thickness of 2/-, $2\frac{1}{2}$ m/m thick. As I said, each type is individually capsuled in aluminium. These different types are all put in the same type of can, these 12" long cans. In some cases, they have spacers between which are made of pure aluminium to space them for our own convenience when we undo the can. The loading of these materials in the can is done by simply dropping these Cobalt bits into the can with spacers if necessary. The can is then flushed with Argon twice. This is in our own interest to prevent oxidation by not leaving any air in the can, and they are then braze-sealed with an aluminium braze on the end of the can. This is a process which conforms to a Windscale specification which was laid down in the very early days of this Pile. After doing this they are subjected to a temperature test of 100%, that is, every one of these cans is run up in a temperature oven to 300 and kept there for 12 hours and any that bulge or burst are rejected, but the majority of them are quite alright.

If it is of interest, Cobalt has come back from Windscale after being irradiated in the Pile for as long as two years it appears, after we have opened it, to be well preserved from any corrosion, that is to say, the Argon filling of the cans keeps the Cobalt quite bright and metallic. It looks like a nickel plated surface rather than a blackened corrosion which you would get otherwise.

You were asking, Sir, about the heat generated. I cannot answer this specifically, but I think the following calculation might be of interest. We have recently had quite a lot of heat computations done on the heat developed in similar Cobalt specimens in DIDO. Now this was calculated in a flux in DIDO of 10^{14} which, of course, is higher than in this pile which is about 6×10^{12} , it varies but of that order. The heating in Cobalt of this type is almost entirely due to the captured gamma when the neutrons bombard the Cobalt. Cobalt has quite a large cross-section and the calculations seem to show that in a flux of 10^{14} one might expect a steady generation of heat due to these captured gammas of about 5

Watts per gram of Cobalt. Now if you scale this effect down so as to assume it is in strict proportion, and say that there is a flux in this Pile of 5×10^{12} (I don't know if this is legitimate, but I think it is), this would give you $\frac{1}{4}$ Watt per gram of Cobalt. The cans which have contained the most Cobalt in them – I will take the worst case possible – is a can which has 120 grams of Cobalt – so that this might be expected to give you in this flux something like 30 Watts per can while they are being irradiated.

CHAIRMAN 300 or 30?

EASTWOOD I am sorry – 30. Thank you.

SCHONLAND 120 grams.

CHAIRMAN 30 Watts per can.

EASTWOOD The can surface is of the order of 250 sq.cms. so that as regards the wattage per sq.cm. of can surface, it is not very high if you assume that there is a good transfer of heat from the Cobalt to the can.

CHAIRMAN This is when the neutrons are there?

EASTWOOD This is while they are being bombarded. Perhaps this isn't the case you are interested in?

CHAIRMAN We are interested, but I don't think it's the one we are mainly interested in, but still it is interesting.

EASTWOOD Can I say that the only other material that we have besides these cobalt cans are small radiographic sources of Thulium. These are very small cylinders of approximately 2 or 3 m/m diameter which are compacted and sintered from Thulium Oxide – Tm_2O_3 . They are sintered at something like 1,000 to 2,000°C in order to make the compact hold together and become a ceramic type of material, so it is very unlikely that they would disintegrate or cause trouble. It is a fairly inert sort of material, thulium oxide. These are contained inside some quite small magnesium alloy capsules just to contain the material and these capsules are loaded in these same type of cans in the same way.

DIAMOND The aluminium cans.

EASTWOOD Yes. When I say magnesium alloy, it is mainly aluminium with a small magnesium content, not like Magnox which is almost entirely magnesium.

CHAIRMAN There is another – aluminium nitride – is that one of yours?

EASTWOOD That is Dr. Charlton's, Sir. Cobalt and Thulium – these are the only two things we have in the Pile.

CHAIRMAN What you are saying is on the Cobalt in a neutron flux the main thing is due to the capturing of gammas and that gives you 30 Watts/can and has 250 c.c. You do not know what sort of heat is generated. How many curies of Cobalt are there in a can? Do you know?

EASTWOOD The highest specific activity is something in the region of 5 curies per gram and I gave you 120 grams in the worst can, 600 curies – this is the worst case and most of these are probably a tenth of that.

CHAIRMAN So we can do a little calculation of 5 curies, 120 grams, and see what energy is being released – a straightforward piece of arithmetic.

EASTWOOD Which, I think, is very small compared with what the energy would be while they were being bombarded.

CHAIRMAN I expect you are right. We will check it. Do any members of the Committee wish to ask any questions?

KAY Can I ask you about this Thulium cartridge? What is the reason for the magnesium alloy?

EASTWOOD These small pellets of Thulium are used for radiography; they are very small dimensions, 1 mm – 2 mm, not much bigger than the size of a pinhead. In order to use them you have to

- (a) contain them in something
- (b) make them so you can handle them, pick them up and see them. The little capsule in which they are contained is made with a tail with a couple of little nicks in it so you can hold it in a spring clip.

KAY What is the overall size of this capsule?

EASTWOOD About 1" long and something like $\frac{1}{4}$ " diameter.

KAY And this is a just loose fit in the aluminium can?

EASTWOOD They are screwed inside a second small screw type aluminium can which goes inside the big one.

KAY Is it wedged by spacers in the big one? Is it argon filled?

EASTWOOD Can I start with the Thulium capsules? About four to five of these are put inside a small screw top aluminium can $1\frac{1}{2}$ " long and $\frac{1}{4}$ " diameter. That can is then put – 3 or 4 – inside the 12" long outside can with spacers between them, the whole being filled with Argon which of course runs throughout the whole because they are not gas tight.

KAY The outer can is a standard aluminium can just as would be used for the Cobalt.

EASTWOOD To the standard Windscale specification and the same oven testing.

KAY You mentioned these things were tested to 300°C and if they were alright they went on. Are some of them not alright?

EASTWOOD We have on occasion – 1% or less – found cans which have burst. This is of course the object of the test. The reason for this we have found now is usually that something was left on the materials that went inside the can, for instance, the aluminium spacers that we used at one time were not properly degreased and probably still had a little of the cutting oil left on. If you put spacers which are slightly greasy in this can and then heat them up, the cutting oil presumably volatilises and forms a pressure and the whole can bursts and splits down the side. There can be no question of whether it is going or not going. It definitely blows out – $\frac{1}{2}$ " diameter again of the normal can.

KAY And why 300°C?

EASTWOOD This was the specification laid down by the Windscale people. This was assumed to be the top temperature they would be required to meet.

KAY That was laid down by the Windscale people?

EASTWOOD It is written out in the early records here. I can find chapter and verse if you want.

CHAIRMAN What sort of number of curies do you get in the Thulium capsule?

EASTWOOD Very little, Sir. Each of these little capsules may only have 200 to 300 millicuries. The gamma radiation is very soft – it is only 80 K volts.

SCHONLAND Do you ever use Magnox in outside canning or spacing outside?

EASTWOOD No – we never have.

CHAIRMAN Thank you, Mr. Eastwood. We shan't want you any more. We will have Dr. Charlton.

WITNESS: DR. CHARLTON

CHARLTON I am in charge of a section which produces labelled compounds, isotopes other than C14, but previous to that I had responsibility for the production of Carbon 14 from an irradiated aluminium nitride.

CHAIRMAN We would like to hear a little bit about the cartridges in the isotope channels of the Pile. We have already heard from some of the Industrial Group people about AM and LM, and Mr. Eastwood has told us about Cobalt and Thulium. If I am right, that leaves aluminium nitride (and there was another steel that wasn't there but we won't have to worry about that) and potassium chloride. Are we addressing the right person?

CHARLTON There is probably also Thallium metal and there may be one channel of miscellaneous material sent up for loading, but we have not received notification that it was actually loaded.

CHAIRMAN For the record, what was the miscellaneous?

CHARLTON The reference is on Request G.19. It was to be loaded into one channel and consisted of seven cartridges – 3 containing calcium oxide (these are small amounts of material not a cartridge packed solid with material), 3 containing zinc metal, one containing nickel metal, and one containing calcium carbonate enriched in calcium 44.

CHAIRMAN May we start at any rate with the three that we have mentioned – Aluminium Nitride, Potassium Chloride and Thallium metal. Will you please tell us something about these?

CHARLTON May I deal with the small ones first – Aluminium Nitride is rather a long story.

Potassium Chloride consists of what we call standard isotope cans – Mr. Eastwood probably showed you a sample – just containing Analar Potassium Chloride. We have had

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considerable experience of irradiating this material for periods of up to two years in Windscale. When we get the material back it is highly coloured – black almost, but otherwise appears to be unchanged. It flows quite freely and there is no gas pressure in the cans – no corrosion of the aluminium which you would expect if there had been any dissociation.

CHAIRMAN What do you make it for?

CHARLTON Primarily for Sulphur 35. Also Chlorine 36. Do you wish to hear any more about potassium chloride?

CHAIRMAN Are these things that are being made – these radio-active things – do they produce any heat?

CHARLTON I don't think any great amount; the cross-section of these cartridges is reasonably small and I think 200 m²/W (?) a Potassium Chloride cartridge.

CHAIRMAN How many curies in a can?

CHARLTON I would say from 10 to 50 curies of Sulphur 35 in a can. There would be Potassium 42 in quite large amounts as well – I could not give an estimate of that.

DIAMOND And these cans are 1" diameter and 12" long.
(**CHARLTON** Yes)

CHAIRMAN Very well. Now proceed to whichever one you wish.

CHARLTON Thallium Metal next. Would you like the reference numbers of these. It is Request G.15. Two channels were loaded on the 23.7.56 – channel numbers in Pile 1 were 06.55 and 06.59. They were loaded at two cartridges per channel and each cartridge contained three lots of 20 grams of Thallium. that is the Thallium Metal was divided into three small aluminium cans and spaced out along the cartridge. I think possibly the only thing to say about this is that the Thallium might well reach its melting point.

CHAIRMAN What is its melting point?

CHARLTON It is around 400. There is some other Thallium in the Pile which was not put in by ourselves but was originally put in by Harwell. The reason for this is that the Thallium 204 is used for Beta sources for static elimination. These sources were originally made by Harwell and about two years ago we took over their manufacture. The Thallium was in the Pile during this time and it was transferred so that we could arrange for it to be unloaded.

CHAIRMAN What sort of numbers of curies have we got in the cartridge?

CHARLTON Perhaps I could refer to one of the earlier cartridges made by Harwell which contains a lot more Thallium. These were Request A.28 loaded at some date in 1953. These cartridges contained something like 700 grams of Thallium and after an irradiation of approximately three years, contained something like 300 curies each of Thallium

204. From examination of these cartridges when we received them, it was evident that Thallium had been molten at some stage, in that it was packed in discrete lumps and when it came back, it was a solid bar along the bottom of the cartridge. There is a point about this which should be checked but, to the best of our knowledge, Thallium will not alloy at all with aluminium. This is a point we have checked to some extent ourselves as we were interested in trying to form sources of supplies.

12 noon Saturday

CHAIRMAN Are there any more of these available for inspection which have not been opened?

CHARLTON No. There should be two cartridges remaining in the pile and they are channel numbers 2653 or 2651. We do not know which channel was discharged.

DIAMOND That is two cartridges, not two channels?

CHARLTON No. There would be two channels each containing 2 cartridges: one of these channels has been discharged.

KAY What was the second number?

CHARLTON Originally on this demand A.28 two channels each containing two cartridges.

KAY Just two channels?

CHARLTON One was 2653 and the other 2651. One was discharged about a year ago, but I do not know which one.

DIAMOND Could we get this right? Each of these contained 700 g. of thallium which, after 3 years, gave 300 curies per cartridge.

CHARLTON About 500 millicuries per gram of thallium.

KAY Can I be clear about the construction of these? This is just a plain lump or bar of thallium?

CHARLTON Yes. I believe they were made first by putting lumps or bars of thallium – you purchase aluminium in the form of rod $\sim \frac{3}{8}$ " diameter and pieces of this were cut up.

KAY And this is cut up and put in the standard can. Anything in the space – argon?

CHARLTON This was a Harwell thing. We did not load these.

KAY Can you come on to the kind you do now?

CHARLTON The kind we loaded contained a lump. Well, let us start from the beginning. Take a lump of 20 g. of thallium and put that in a small screwed cap aluminium tube, the lead held in by foil and three of these units were taken and mounted in quite stout aluminium tubes and used to fill a standard cartridge. This was because we had a very unpleasant job of cutting up We wanted small pieces.

KAY And the total weight in this new cartridge?

CHARLTON Was 60 g. 3×20 .

KAY Very much less than

CHARLTON I think I gave you the channel numbers of this new pattern. Yes I did.

CHAIRMAN Do you know when the 700 g. cartridges were taken out? The ones, one of which showed melting.

CHARLTON Approximately last December.

DIAMOND So it was only one of these cans which showed the melting.

CHARLTON Both.

DIAMOND They were loaded in which channels?

CHARLTON 2653 and 2651 and one has been discharged and we don't know which one.

DIAMOND These cans which had molten came out of either but we don't know which?

CHAIRMAN The two which are left are in the other one.

ROSS They will be out now. That zone is discharged.

CHAIRMAN That was in the pile when the accident happened?

ROSS No.

CHAIRMAN Well then the aluminium nitride.

CHARLTON There are miscellaneous isotopes which we can deal with quickly. Well, natural calcium oxide first of all.

Calcium oxide was prepared by heating up calcium carbonate and loaded into the silica ampoules as a protection against ingress of moisture and so on, and three of these ampoules, each containing 2.5 g. of calcium oxide, were, I think, loaded into stout aluminium tubes for protection and then into a standard cartridge. We have incidentally irradiated calcium oxide before and nothing untoward appeared to have happened to it. The zinc metal: 250 milligramme pieces of zinc were sealed into the silica tubes and three of these silica tubes loaded into a cartridge using blocks of Windscale pile graphite to space them out. We took cylinders of Windscale pile graphite drilled in the middle and used these to pack the silica tubes in. The nickel metal was spec. pure nickel sponge.

KAY Have you loaded zinc before? (**CHARLTON** Yes).

DIAMOND Zinc, what is its melting point?

CHARLTON I believe zinc had not melted. The nickel metal was spectroscopically pure nickel sponge. These were single cartridges which contained 3 aluminium capsules of nickel metal, each capsule containing 13 g. of nickel. The calcium carbonate C44: the cartridge contained 2 silica tubes, each containing 10 milligrammes of calcium carbonate enriched in Calcium 44, again both nickel and calcium carbonate had previously been irradiated. The point about the calcium

carbonate you will see, is that even if it were to decompose, the amount present is quite insignificant compared with the volume of the cartridge.

SCHONLAND Do you actually test these cartridges up to some temperature before you

CHARLTON All cartridges go through the standard testing procedure which I imagine Mr. Eastwood has described. There is a document describing it.

SCHONLAND He has described it. I just wanted to confirm the limiting temperature.

CHARLTON 300°C. On the other hand, when choosing materials, we do, if possible, choose materials which will melt a good deal above 300°C. Thus, when we have irradiated tin compounds, we have chosen stannic oxide rather than metallic tin. Aluminium nitride: A very great deal of work has been done on aluminium nitride because we had to satisfy the people in authority that this was a suitable material for loading into the piles.

CHAIRMAN When you say the Authority, who do you mean?

CHARLTON This is difficult. In the early days, I think all orders that we placed on Windscale were channelled through Dr. J.B. Willis at Harwell. Since then, I think there have been various other ways of channelling requests but in general we discuss our requirements with the Pile Manager at Windscale and with the Director of Production Office at Risley. Now aluminium nitride was originally chosen because of its extreme stability. Other materials previously used, such as potassium nitrate and potassium nitride, had been shown to be unsuitable under pile conditions.

KAY Chemical stability under irradiation?

CHARLTON Yes, if you like. I think it is both. I think it is fair, that is, under the conditions of temperature and irradiation. What we did first of all with aluminium nitride was to take small samples, to degas them and seal up in vacuo in silica break seal ampoules in quite small amounts, and these we irradiated at Windscale for a period of 3 months, took them out and examined them for gas pressure. The results of this are described in a Radio Chemical Centre Report RCCM53. Briefly, the result was that on a year's irradiation, the maximum amount of gas which could have been formed would not have given a pressure of greater than atmospheric in the pile. There is one point about this: when we seal aluminium nitride in cartridges, we seal them at about 300°C so that the pressure is about half an atmosphere, and results of our tests show that under the very worst conditions a pressure of one atmosphere would not be reached. On the basis of these tests, aluminium nitride was accepted as suitable for irradiation, but when we had cartridges, full scale cartridges of aluminium nitride, returned, we did test a number of these individually for gas pressure. These are cartridges irradiated for the order of a year and we found there was no detectable difference in gas pressure from that originally, both non-irradiated cartridges and irradiated cartridges showed a pressure of gas of half an atmosphere. There is another point, no aluminium nitride cartridges which

we have received back, and we must have had between 1000 and 2000 of these, has ever shown any sign of excess pressure or any distortion due to pressure. I think you might like a few words about the chemical nature of the aluminium nitride. It will react slowly with water giving you ammonia and hydroxide, and it will burn in air or oxygen if heated to a suitably high temperature. In fact our method of extracting carbon 14 from aluminium nitride is to burn it in a stream of oxygen and we find that in oxygen it will ignite at a temperature of about 700°C. We have of course a lot of experience of burning aluminium nitride in oxygen and we found that 650-750°C is the region in which it ignites.

We have in the pile two types of aluminium nitride cartridges, the first is of the variety Mr. Eastwood will have shown you, the thin walled 12" x 1" type of can. Aluminium nitride powder about 40 mesh is loaded into these cans and tamped down slightly to give a loading of about 100 g. per can. To bring up the weight to that required by specification, a lead weight wrapped in aluminium foil is put into the bottom of the can before filling. Having filled the can, the aluminium disc is put in the top and the ends are turned over. It is then heated at 300°C for 48 hours. This is to drive off moisture and ammonia which is always present in the nitride. The cartridge is then sealed by brazing as soon as it is taken from the oven. After sealing, it is again heated for 48 hours at 300°C and examined for any distortion. It is inspected and tested for leaks in the usual way and this is described in one of these reports. Now this is the earlier type of cartridge. About a year ago, we changed over to a different sort of cartridge which is essentially one of greater capacity, its outside diameter being 1.25". Its length is the same as before but the wall is heavier and 16 gauge. This type of cartridge is essentially similar to the other kind only we get these cartridges as lengths of tube, the ends of which have been turned out to about something like $\frac{1}{4}$ ", then we can drop the sealing disc so that it rests on the shoulders: so we fit in the disc, turn over the top and braze it and then fill it as though it is an ordinary cartridge, and do the same to the other end. This type of cartridge was chosen because of its greater capacity and also because we have had a great deal of trouble due to distortion of cartridges during unloading the pile. The amount of aluminium nitride loaded into pile one is quite considerable. I can give you the details of all these channels if you wish.

CHAIRMAN Have you got them written down?

CHARLTON I have.

CHAIRMAN Can you table them for us please? What is the number of curies per can?

CHARLTON After a year's irradiation it is of the order of 15 millicuries per can.

CHAIRMAN I see, it is very small.

C.450? And is there anything else in it.

CHARLTON There is no significant activity other than C14.

CHAIRMAN The question that strikes me rather forcibly is that you test these to 300° before you put them in and yet you had one that you knew had been through 400. Why do you accept the 300°C test then?

CHARLTON I suppose the question is: What does the 300°C testing for 48 hours imply? I think this test is probably to show that there is nothing grossly wrong with the cartridge. Because one can hardly extrapolate from 48 hours at 300°C for perhaps 2 years under pile conditions. I think the argument is that the material itself must either be beyond reproach or must be extensively tested and this heat testing of cartridges is then to spot anything grossly wrong with the cartridge.

CHAIRMAN Do you spot anything wrong?

CHARLTON No.

CHAIRMAN Who specified 300°C?

CHARLTON This is a paper by Bobin. It is A.E.R.E. I/M4 issued in January 1951.

CHAIRMAN And that specified that any of these isotope cans to go into the Windscale pile should be tested to 300°C?

CHARLTON Yes. I'll look at the pile graphs, shall I? Oven testing. The cans are placed in the electric oven and heated to 300°C and kept at this temperature for 48 hours: there's a note in brackets (maximum permissible graphite temperature 250°C, normal 180°C).

DIAMOND I wonder whether Dr. Charlton could tell us where we could get diagrams of all the cans he has described: their assembly as put in the pile. Are there in existence diagrams or pictures of the cans and their contents?

CHARLTON We can send you drawings and samples of cans.

DIAMOND They are in existence? They all exist?

CHARLTON Yes. The original isotope cartridge is described in this paper which I have just mentioned.

KAY Have you a drawing of the complete assembly? Do you in fact produce a drawing each time for the complete assembly or do you relate it up as it were to the current requirements?

CHARLTON In aluminium nitride. The empty cartridge is taken and aluminium nitride powder is put in, to a given rate.

DIAMOND There are others which you have described.

CHARLTON Yes. I have some sort of drawing of one can here.

DIAMOND But not with the contents shown?

CHARLTON Yes. I have one in my briefcase showing how the cans are loaded when graphite blocks are used to contain the material.

DIAMOND Is all this material you have described in powder form?

CHARLTON All the aluminium nitride is, but other materials were in the form of massive metals. Incidentally the contents of these cartridges are described when we ask for

them to be irradiated, and also their cross section is determined in GLEEP before irradiation.

DIAMOND When you empty out the aluminium nitride, do you find there is any evidence of the centre portion having been fused together?

CHARLTON No, aluminium nitride fuses at about 2,000°C. There is no evidence of any change whatever. It is packed more or less to the same degree of firmness as when it went.

DIAMOND But we can, if we want, get the sketches of the arrangement of the content of various cans from Harwell?

CHARLTON Yes.

SCHONLAND At what temperature does thallium oxidise rapidly?

CHARLTON I think it will oxidise at temperatures greater than its melting point quite rapidly. I believe thallium oxides are volatile or practically volatile at the temperature at which thallium melts. I have never ignited the material but if you just heat in air there is a very rapid oxidation with liquid oxide present.

KAY I just want to ask, if you melt a piece of thallium in air, does it burn?

CHARLTON I have melted thallium in air.

KAY Do you get a smoky flame?

CHARLTON I have not got it to burn. I think the point is here that you do not raise it to a high enough temperature.

If you just melt it, it obviously oxidises and liquid oxide forms. Probably it is difficult to supply enough heat to ignite a small sample.

KAY This is a temperature of about 400°C?

CHARLTON Yes.

CHAIRMAN Thank you, Dr. Charlton. We shan't want you again as far as we can see.

WITNESS: MR. MOORE

MOORE I am a Research Manager of the Reactor Technology Group at Windscale in the R. & D. Branch, responsible for experimental work that was done during the start of the Calder Hall reactor and that is being done on the nuclear side in the reactor. I am responsible for getting R. & D. experiments into the Calder Hall reactors and into the Windscale reactors, and responsible for engineering organisation in the R. & D. Branch, i.e. the workshops, the Design Office, and Liquid Metals Laboratory, and also the physics work done in the Laboratory.

CHAIRMAN We have not heard from anybody anything about loops that were in the reactor that has caused the trouble. We do not know whether there were any loops in them and we understand you are the best person to tell us what there was and something about them.

MOORE First of all 4" holes – there are 4 of these used for experimental purposes. These are vertical holes and they were all empty during the incident. By empty, I mean there was nothing in the holes within the Pile. Now, I'll run through the position on each hole.

First of all, C3. This had been used for an experiment for measuring bowing of an element under 1 rad. The whole rig was taken away before the Wigner release started. The liner tube was taken out and a new liner tube inserted prior to inserting a graphite annealing experiment.

C4 (next hole) had been used for a multi capsule experiment. The experiment, before the Wigner release, had been wound out of the hole and was located in the hole within the thickness of the concrete roof. Do you need the details of the actual experiment?

CHAIRMAN I don't think you need. We are really interested in the pile.

MOORE I should mention that on C3 there was no liner tube in the hole: we had removed the liner tube with the experiment we had taken out and the new liner tube hadn't been inserted.

Next hole, C6 – That hole was empty. The Harwell experiment had been removed before the Wigner release; the liner tube had been taken out and a new liner tube inserted. Next hole, C7 – The element in this hole was again located in the thickness of the concrete roof. It was not in the Pile. It was a uranium cartridge that was hollow.

Now the 2" holes: – there was only one of these that definitely contained something. This was hole C8. The experiment consisted of a Columax magnet inside an aluminium can: the specimen was not heated; the experiment was being done at higher temperatures.

CHAIRMAN What is an Columax magnet?

MOORE I can't remember the exact composition; it is a magnet with a high Co content. It is the type that is being used in flow meters and in a level trip in the fast reactor at Dounreay. We are checking the effect of irradiation on it.

CHAIRMAN That was inside the Pile.

MOORE Yes. It was inside the can, inside the aluminium liner tube, which has a closed end.

I am not completely clear yet whether there is anything in hole B7, that is the hole which is operated by Chemical Services Department. If there was anything in it, it would be Calcium Oxalate in a silica container 15mm diameter 30mm (?) long, which was packed in graphite inside an aluminium container. The al. container being $\frac{5}{8}$ " dia. x 4" long. This is again inside an al. liner tube with a closed end.

All the other 4 holes were empty. No experiments in at all. The only hole that any operations were done on before the incident was hole B6. The Harwell experiment had been removed on the Monday.

DIAMOND What was that experiment?

MOORE I am not sure of the details of this experiment. It was an experiment on pure metals. I think, to determine the effect of the radiation. There was no fissile material.

CHAIRMAN Any questions on this?

DIAMOND These experiments which had been taken up into the concrete before the Pile was released (they were C3) had no liner tube, it was an experiment to measure the bow of an element under compression?

MOORE There was a liner tube in while the experiment was in. C3 had been removed completely.

It was C4 in the concrete.

This consisted of a number of small specimens. These small specimens were inside. There were 12 of these. Each one inside a s.s. container in sodium. The s.s. containers were mounted above each other in what is effectively a long s.s. tube with individual heaters wound on outside of tube. This had been operating at temperatures up to 550°C.

DIAMOND The specimens were contained in sodium inside s.s. capsules. Fissile specimens

MOORE Yes.

DIAMOND The s.s. containers, how were they made?

MOORE These were machined out of the solid and the lid welded on. Shall I sketch one on the board?

DIAMOND Yes please.

Is that thimble made out of solid?

MOORE I am not too sure of this. I think this thimble is welded in here.

DIAMOND So there are two welds.

MOORE Yes. The specimen is vertical in the rig. Even if the welds had gone, sodium could not spill out.

DIAMOND Is it impossible to put them in upside down?

MOORE Yes, because you wouldn't get the thermocouple in. The thermocouple has to be threaded in Pyrotex thermocouples which must be machined as well.

DIAMOND And all thermocouples were used on this occasion.

CHAIRMAN Any more questions?

KAY You say this was electrically heated with a Pyrotex heating element, wound on the outside of the container.

MOORE Not this container.

KAY The other container, and the container was made of s.s. The overall dimension of that thing was about?

MOORE I can't be too precise on this. It was about 1 $\frac{3}{4}$ ".

KAY This was lowered in to join the experiment into the liner tube.

MOORE Yes. Supposing this is the experimental head, it is located on the end of an aluminium tube which has teeth cut into it by which it is wound up and down by a rack and pinion

mechanism. On the outside there is an insulated tube which contains magnesia insulation and then there is an aluminium liner tube sealing the hole. Coolant is passed down and up here, during the experiment.

KAY What sort of coolant?

MOORE Air.

KAY Does it blow air down.

MOORE ?

KAY Is any temperature measurement made of the liner tube?

MOORE Yes, we have a liner tube temperature. We set an upper limit of normally 350° on this, but we have had it occasionally at 400°, not during this experiment; in a previous experiment.

KAY And that liner tube is in contact with the graphite?

MOORE It is in contact probably at some parts. It is nominally a loose fit in the graphite but in fact not perfect.

KAY Have you a temperature record for this experiment?

MOORE Yes, but I have not got it with me.

CHAIRMAN I am a little puzzled. None of these experiments were in process during the de-Wignerisation of the Pile. They were all up in the concrete.

KAY When were they taken up there?

MOORE The one in C4, this would be taken up at the very latest on the Monday before the Wigner release.

KAY Some time on the Monday?

MOORE At the very latest. It might have been the day before, I'm not sure. The one in C7 had been in concrete for a week or two.

KAY But C4 was wound up some time on the Monday or before.

DIAMOND Have you removed those specimens?

MOORE No, I am afraid they are still there.

DIAMOND They are too hot.

MOORE The rig is contaminated and we feel we can't attempt to divert engineering effort to move them just yet.

DIAMOND So you have not been able to inspect the bottom of the liner tubes?

MOORE To inspect the bottom of the liner tube, you would have to take the experiment out, dismantle the rack and pinion at the top of the rig, take it away with the crane, take the capping away and run an inspection lamp down the hole. I haven't been pressed to do this at all yet. I believe you were asking if any lithium had been found in samples from the stack.

CHAIRMAN We would be glad to have any factual information that you have got on this.

MOORE We have a cyclone sample unit fitted below the filter and one above the filter. The one fitted above the filter was in operation from 16.00 on October 8th to 15.30 on October 10th. In other words, it was taken off something like an hour after the shutdown fans were put on for the first time in the middle of the incident. The analysis of this sample shows that the magnesium content is 1.4 mg, the lithium content is less than .025 mg. This particular sample I do not consider to be very significant, but it does show that some lithium had passed up the stack before 15.30 on October 10th. Making a rough estimate of the velocity in the stack we can calculate how much was going up the stack and this worked out to be 2.8 grams of Mg and less than 0.06 grams of lithium. In the sample taken above the filter we are more satisfied that the results from this are reasonably accurate. This was fitted on the 27th September and removed at 15.30 on October 10th so it did contain some material which passed up the stack before the incident started. The analysis shows that the total weight of lithium was 0.63 mg, and magnesium 1.5 mg. Estimate of the amount going up the stack, above the filter, is 3 grams magnesium and 1.3 grams of lithium. Associated with this, there are 2.4 grams of uranium going up the stack. From this we can only make a very rough estimate of the filter efficiency and the cyclone does not stop (all?) particles. Well, the cyclone efficiency starts to drop for particles lower than 15 μ . It is falling fairly rapidly below 10 μ . The cut off for particles of this density would probably be around 3-6 microns, so there could have been a lot more material going out, of very small particle size.

I think all we can say from these samples is that there was certainly some lithium and magnesium going up the stack before the time I quoted. The best estimate we can make is that there was between 3 and 10 grams plus that that went out as very small particles.

CHAIRMAN This is at 15.30 on the 10th, which is Thursday. There wasn't much air current there, was there?

MOORE We estimate that after the shutdown fans were put on, the flow velocity in the stack would be 1.2' per second. This is an estimate. We hope we can state this more accurately later on.

CHAIRMAN The shutdown fans were put on at 13.45. They give how much?

MOORE We estimate 1.2'/second.

CHAIRMAN You have got a filter here that is very inefficient at 12 or 15 microns.

MOORE The cyclone sample unit is very inefficient; the filter is also not very efficient.

CHAIRMAN You only have a speed of 1.2' per second which would not take very big particles up.

MOORE We estimate that for spherical particles the cut off would be about 100-200 μ for graphite particles, and for oxide particles the cut off we estimate is about 60 microns.

CHAIRMAN So you have enough draught to take things up which are less than 60?

MOORE We are picking up particles larger than this because we get them from the cyclone analysis. We have made a particle size distribution measurement of both these cyclone samples, and we had some particles up to 350 μ . I made a point that these cut off sizes were for spherical particles ? flat plates of this size.

CHAIRMAN Have you used Stokes Law for this?

MOORE Yes.

CHAIRMAN I'm afraid Stokes Law is not very good at 100 microns.

MOORE I realise that this is not highly accurate evidence. All that we can say is that it is an indication of what was going on at the time.

CHAIRMAN I accept that. Any questions?

DIAMOND I would like to get the time right, 15.30 on the 10th. Over what period had this collection taken place?

MOORE For the one fitted below the filter it had taken collection was from 16.00 on October 8th to 15.30 on October 10th. For the one fitted above the filter, this was installed on the 27th September and removed at 15.30 on October 10th. We are trying to make an estimate of the amount of activity in the uranium that has gone out of the stack. From estimates of filter efficiency these cyclone measurements and the activity remaining in the stack filter, we have very provisional estimates.

CHAIRMAN Perhaps in a day or two we can call you again on that point.

At this point someone (Dr. Charlton?) showed the Panel various pieces of hardware:

- outside can
- disc seal
- block of Al as spacer
- inner can filled with capsules
- 3 shapes of cobalt specimens

and how they were put together.

This evidence was taken on Saturday, 19th October at approx. 2.30 p.m.

WITNESS: MR. ROSS

CHAIRMAN Mr. Ross, for the historical record, we would like you to tell us what you actually saw coming out of the stack of the No. 1 Pile and the times.

ROSS Well, the time I was particularly interested in, first of all, was after we had put the CO₂ into the Pile which was at about 3 o'clock I think roughly, and I went out to see the stack then, and I felt that it was a dark grey, but certainly not black. Then again I watched it out from 7 o'clock to 9 o'clock (a.m.) until we got the water on, and at 7 a.m. there was a grey plume which changed to a white, typically steam, plume as soon as we got the water on.

CHAIRMAN When you said 3 o'clock for the CO₂, was that in the afternoon?

ROSS No – in the morning.

CHAIRMAN So that was in the hours of darkness.

ROSS You could see the plume coming out of the stack. It was what I would describe as a lazy plume. There was not very much air and it was just coming out, not falling straight down, but very slowly from the top of the stack.

PEIRSON According to the log it was 4.30 the CO₂.

CHAIRMAN And the water went on about 8?

ROSS At 5 to 9. Between about 7 and 9, I went out several times just to have a look to see because I knew Tuohy was pushing out cartridges then.

CHAIRMAN Did you notice it after 9 o'clock in the morning?

ROSS No – I did not actually.

CHAIRMAN Alright. Thank you.

WITNESS: MR. TUOHY

CHAIRMAN Mr. Tuohy, we should be glad if you would tell us what you saw coming out of the stack during this emergency work. During this period there was stuff coming out of the stack. Tell us what you yourself saw and when.

TUOHY When I came in about 5.30. I had a look up at the stack and I don't recollect seeing anything at that time, but then it was getting towards dusk. During the night I looked up several times, but it was too dark to distinguish anything at all at the top of the chimney, but at first light of dawn in the morning, there was definitely smoke coming out of the chimney. I am not talking of a dense black cloud of smoke or anything like that; it was dark in colour, but it was a thin hazy type of smoke. It was the sort of smoke that you see on a power station that has been well stoked and the smoke is dying down. There was no dense black cloud of smoke, just a thin wispy sort of smoke. That would be shortly after dawn. From then on, I was actually in the reactor making the final arrangements for the water to go on and I did not come out and look at the chimney again until the water was put on. By that time there was no sign of smoke as such – but there was quite a plume of steam. It was a white plume at that stage, and that gradually died away after the air went off. There was steam coming out the whole of the time the water was on, but after the air went off, that plume gradually died away until about midday there was no sign of anything coming out of the stack then – midday to 1 o'clock, that sort of order.

CHAIRMAN Yes. Any questions? Thank you, Mr. Tuohy. Sorry to bring you for such a little.

WITNESS: MR. HALL

CHAIRMAN Mr. Hall, we left a lot of problems with you and I do not know how far you have got. We know they are pretty difficult ones.

HALL I have about eight people just about in the middle of them at the moment. Some of them will be completed tonight and others some time tomorrow. Shall I just run through the things we are doing?

Firstly, you asked for the heat release and temperature difference between the can and its surroundings for all the cans in the reactor. We can give you this information this afternoon.

Secondly, the same thing for Uranium under shut down conditions and Wigner release conditions. That is with the small shut down flow and with no flow at all. We are doing that experimentally at the moment and we shall have an answer by tonight or early tomorrow.

The temperature of the AM rods under normal operation, and the maximum temperature of the graphite under normal operation. We are doing those two together because they influence each other rather considerably. We are in the middle of that at the moment – we should have an answer about midday tomorrow.

The biggest one – the effect of a fire either from Uranium or an AM rod on the surrounding graphite – the two models we are taking at the moment are the two dimensional case where we have a line of these things burning and also virtually a point source case where we have a single rod in a block on fire and we are looking at it in two steps. Firstly, treating a block on its own, this is a block which has four half channels in its sides and two isotope holes, a fire in one of the isotope holes and conduction in this block, the unsteady conduction in this block and independently an assessment of the effect of heat losses from the surface of the block, so we can get an idea of the time scale for these two processes and see how they are likely to interact and at the same time, an assessment of the rate of oxidation of the uranium rod in the channel. That one, I think, will take us certainly until tomorrow night.

DIAMOND You have some results now?

HALL No, nothing with me. I can let you have the first one, the heat release from all the cans, in a matter of an hour or two.

KAY May I ask you two factual questions? The thermal conductivity of the graphite – the actual thermal conductivity of a small piece of graphite.

HALL This is unirradiated?

KAY No, irradiated under Pile conditions.

HALL I have taken Bell's advice on this one. The position is that it depends on the irradiation history of course and in both the temperature and the dose. For example, a certain dose with radiation at a higher temperature will produce a smaller reduction in conductivity than if it had been irradiated at a low temperature. We have come to the conclusion that the best figure we can take for the graphite in its present state in the reactor after an anneal, is $\frac{1}{15}$ th of the normal conductivity. The normal conductivity varies – .25 to .35 – depending on the direction of the

KAY .25 to .35 calories – cal/cm.

HALL Calories per cm/second thickness.

CHAIRMAN Natural graphite?

HALL That is natural graphite.

CHAIRMAN Unirradiated Pile graphite.

HALL The spread here is associated with the different directions with or across the direction of extrusion.

DIAMOND And it is greatest in the direction of extrusion?

HALL I could not say that. I can find out.

KAY We are interested in the minimum conductivity, but $\frac{1}{15}$ th for irradiated after an anneal is your figure. The second point

DIAMOND What is it before an anneal?

HALL I think between $\frac{1}{20}$ th and $\frac{1}{30}$ th which is about as close as I have been able to tie up. I think it might be useful to have an independent opinion on this one: this is what I have gathered from Bell.

KAY The other point is this. If you consider a heat release in one part of the graphite, spreading through the rest of the graphite, as in a Wigner release, can one for various purposes, with a very rough calculation, simplify the problem to the extent of quoting a sort of equivalent of the thermal conductivity, taking account of the various gaps and passages, etc.; I realise this is a gross oversimplification but I wondered whether you had in fact, for the purpose of rough calculation, arrived at any equivalent of thermal conductivity as though it were a solid block of material.

HALL We are in fact using this technique to see what goes on outside one single block. We are dealing with the single block initially: we shall use this technique to see how the temperature wave spreads outside that.

KAY But you have not got this figure?

HALL The figure is probably available, but I haven't it in my mind.

KAY Perhaps you could let us have it when you quote some of these other results later on.

CHAIRMAN Well, you have given different times for when these things can be done. I don't want to interrupt your work. (Addressing the members of the Committee) How would you like to take them – as they come?

KAY Shall we leave it to Mr. Hall's discretion?

CHAIRMAN What I had in mind was we don't want to keep on taking evidence from him. He could give us these results, and then he could give them again as evidence later on.

DIAMOND What about last thing this evening?

CHAIRMAN Shall we let him go away and we will see how he is going on. We don't want to press him. Let him go away and do his work and we will take it as evidence some time in one go. But perhaps we could have some preliminary values.

HALL If I feel that I have a quantity which is worth passing through I shall send it in.

CHAIRMAN Yes, pass it to us informally and then later on as evidence. Thank you, Mr. Hall.

(Query here as to whether a Mr. Bensell should be called or not)

Board of Enquiry

Sunday 20.10.57

Evidence heard from:—

Moore	p 4.1
Gausden	pp 4.1 – 4.6
Moore	pp 4.6 – 4.7
Phillips	pp 4.7 – 4.8
Jenkinson	pp 4.8 – 4.10
Toole	pp 4.10 – 4.11

Sunday 20th October - 10.50 a.m.

WITNESS: MR. MOORE

CHAIRMAN Mr. Moore there is one question we wish to ask you. We would like to know if you can estimate or have any practical experience that would enable you to give an opinion on this point. Suppose an A.M. cartridge in the pile started to leak and smoulder, would any instrument in the chimney stack pick it up? The assumption is, of course, that the pile is shut down, there are no fans on but there is a small draught of convection going up the stack. Do you think that any instrument would show activity?

MOORE No. Not from the lithium.

CHAIRMAN What I had in mind, was there is tritium in this thing which would burn, make water vapour which would be in the air and that would go up.

MOORE This is mainly alpha activity isn't it?

CHAIRMAN No. beta.

MOORE Well, the ion chambers might pick this up but I would not have thought it would be significant against the background in the chimney. One would need to work this out to see how much tritium would be present.

CHAIRMAN I did a very rough calculation. I don't suppose it is right and it wants checking but one gramme of tritium has a tremendous number of curies, thousands of curies, and I suppose that any one A.M. cartridge might contain one hundredth of a gramme or so of tritium?

MOORE I am not sure of this. I seem to remember it can be as much as 200 c.c. of tritium in a single cartridge.

CHAIRMAN Well, this is an important point and we want someone to work it out. Could you do that Mr. Moore?

MOORE Yes.

(The Chairman then requested Mr. Moore to go and work this out and Mr. Moore said it would take him half an hour to do so. The Chairman also said they would have lunch at 1 and immediately after would visit the pile and start examining witnesses again at 2.45 p.m.)

WITNESS: MR. GAUSDEN

CHAIRMAN The first thing we would like you to do if you have it, is to table a loading diagram of the pile.

GAUSDEN This shows all the isotope loading and the various zones of enriched and natural metal.

CHAIRMAN We shall all be looking at that. There is one question we have been wondering about. We have a bit of a sketch of that, only a part of it, but the middle vertical column has nothing at all. That must be something to do with the way the pile was built. Is that right? Channel no. 57.

GAUSDEN Yes, there is a vertical hole right in the centre of the reactor which we use for flux plotting, and that cuts right down through the isotope channels so you cannot load those.

CHAIRMAN Do members of the Committee wish to ask any questions about this diagram?

GAUSDEN I might mention that it has only been completed fully in one quarter because it is completely symmetrical.

CHAIRMAN We shall be studying this. It is possible that we shall be asking you questions on another occasion if there are any points on which we want information.

(Later) We now wish to ask you some questions about the pile being made re-active in order to produce heat to release Wigner energy and the questions we want to ask are something about the two occasions when it was done during the present release, and when we have explored that more fully, because you have already told us something about this. We shall probably wish to ask about nuclear heating for the same purpose on previous Wigner releases which come within your own experience. Now the first nuclear heating which is shown on all the records and you have described to us before, appears to me at any rate to be absolutely standard and what you have done many times before. Could you perhaps take us through it again.

GAUSDEN From the shut down stage?

CHAIRMAN Yes please.

GAUSDEN Well at 11.45 a.m. on Monday we began to run out the bottom control rods. These were running out for one hour and ten minutes and then switched off to see exactly what effect this was having. At 1325, that is 1.25, the rods resumed running out again. At 1415 the shut down fans, the booster fans and the sealing air fans were all stopped. At 1535 the control rods were stopped again because we were not completely satisfied with some of the graphite thermocouples. They did not seem to be behaving as they should have done and we asked for a re-check of certain thermocouples on the charge hoist. During this time the BF3 counter was running. We had reached about this point here when we stopped running the rods for the second time while the thermocouple check went on.

CHAIRMAN That is absolutely alright?

GAUSDEN Yes, quite satisfactory. This check was completed by 1658 and control rods were again switched on and run out again. At 1925 the pile diverged with the coarse rods and the fine rods which were making up the bottom banks at 404 cm out of the pile. At 2120 all coarse rods and fine rods were in line at 403 cm and the two highest uranium thermocouples were reading 185°. At this stage numbers 2 and 4 banks of rods were switched off at cubicles. Those are the back rods and the front rods were run out a little further. This was to get the heat in towards the front. At 22.35 they were running these rods, groups 1 and 3 rods, out to get a maximum uranium temperature of 250° and this temperature was reached at 2325. The rods were then slowly motored in keeping this maximum temperature at 250° until by 0440 the following morning (Tuesday) they were again right in. That was the first run up and down of the pile.

CHAIRMAN Are there any questions on this particular occasion?

DIAMOND Did you go for 250° as on previous occasions?

GAUSDEN That has always been the first temperature. 250° maximum uranium temperature. anywhere?

KAY Initiating the general heating?

GAUSDEN Yes.

CHAIRMAN There is just a point here. Your uranium thermocouples are fairly well back in the pile - 15 ft. and you have been trying to make the heat come forward, towards the latter part, so we do not know what the maximum temperature would have been in the front.

GAUSDEN No. Not absolutely.

KAY I would like to ask about the trouble with the thermocouples earlier on. At one point you stopped your control rod movement to check thermocouples. What aroused your suspicions there?

GAUSDEN There were certain readings on the Kent recorders which rather indicated open circuit thermocouples. and yet only a few hours before they had been checked and were apparently correct, so we thought it most unlikely that it was actually the thermocouple going into the graphite, but was the connection on the charge face that was causing the trouble, and it proved to be that.

KAY It was a pure matter of instrumentation and not the measurement?

GAUSDEN It was the connection from the thermocouple in the graphite to the installed cabling running up to the roof.

CHAIRMAN Would you be good enough to take us through the second occasion in a similar way.

GAUSDEN At 9 o'clock the following morning (Tuesday) the general mass of the graphite was not heating up, but was either stationary or falling, so it was decided to again run up the pile to raise these temperatures again to keep the release going. The Pile diverged at 1105 with the coarse rods in banks 1 and 3 at 550 cm out of the pile and 2 and 4 at 460 cm. No. 1 fine rod was at 600 cm and No. 2 was right in. The maximum uranium temperature at that time indicated was 300°. Now the instructions at this time were for the temperature to be raised, this 300° or whichever was the maximum temperature, to be raised to 330°. The Foreman on duty at that time states that instead of using what we call group control, that is a master control which runs all the rods together, he was in fact using the push button control on the individual rods, inching one side out and the other side out. There was at this stage a temperature rise which is indicated on the Kent recorder charts as up to 380° on one point only, and the rods were immediately run in and this temperature stabilised at 330° as requested. There was no indication here of a gradual rise at

all. It went straight from one point to the next point and was up at this temperature.

CHAIRMAN I would like to check this on the record. Is it on sheet No. 8?

GAUSDEN It is the top point.

CHAIRMAN It is the top point of all and you say it was at 300 and it shot straight up to 380. It did that almost...?

GAUSDEN This is a 6 point recorder which is stamping 6 different points, so there is 3 minutes between stampings. between one stamp and the next.

CHAIRMAN Then you pushed the rods at once, and almost at once the temperature came down again?

GAUSDEN The next point is plotted on the graph.

CHAIRMAN 325.

GAUSDEN The rods were gradually run in from then onwards, until at 1650, Nos. 2 and 4 rods were right in and at 1820 numbers 1 and 3 were right in, and at 1925(?) the remaining fine rod was right in.

CHAIRMAN So that at 1650 you began to run in the rods. When this high temperature on a rod was seen 2557(?) jumped up, you say you ran the rods in then?

GAUSDEN The rods were run in a very short distance. Just a few centimetres. And that checked whatever was happening and came straight down to the figure we had originally expected.

CHAIRMAN It is rather odd, but I do not know that it means anything much.

GAUSDEN What I think to some extent the reason for this is that you are running up your metal temperatures which are already at 300, and in order to get the metal temperatures above that, your temperature increase is masked until your temperature begins to rise above that point.

DIAMOND They all are already at the graphite temperatures presumably, so any ...?... will raise them above 300 from the start.

GAUSDEN Yes.

KAY The graphite is already fairly warm now and the metal temperature will rise quite quickly as a result of nuclear heating.

CHAIRMAN Yes, but a jump of 80° in 3 minutes? Surely there is something wrong with the thermocouples.

KAY Either that or a higher rate of nuclear heating than one would expect.

DIAMOND You mentioned that the method of operation of the control rod was changed at one stage to an entering mechanism: is this on automatic or manual control?

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GAUSDEN This is manual control on the desk with an inching button.

DIAMOND These finer adjustments were done entirely by that way. Who is on the desk?

GAUSDEN The Pile Foreman. Whenever we raise or lower power it is the foreman who does it.

DIAMOND The reduction of power is a matter of someone sitting there and making quite certain that they are continually going in, following the temperatures.

GAUSDEN The rate of control rods coming out is ten times slower than you can put the control rods in. This is geared.

DIAMOND But both coming out and going in, it was at this time under manual control?

CHAIRMAN Will you look for one moment again at sheet A and look what the other thermocouples are doing, in the way this is drawn, and of course the scale has been enormously compressed, all the others seem to be going up just as fast.

GAUSDEN They all did the same thing.

KAY Have you known such a sudden jump in similar circumstances on previous occasions?

GAUSDEN No. I have looked back through the previous Wigner release charts where we did do re-heating, but I can find no such occurrence.

KAY Have you a measurement of the pile power at this moment?

GAUSDEN At the peak temperature you mean?

KAY During this three minute period.

GAUSDEN Indicated at three megawatts.

CHAIRMAN So that the moments these jumps were seen, the control rods were pushed in a little, and they all dropped again, all in unison and all equally sudden. They then went on and then further rods were pushed in, and you reckon that the last stage was at 1925 when the last rod was pushed in, and that in your view is when the nuclear heating ceased.

GAUSDEN In fact I think it was before that, because it was only one fine rod which was running in last.

CHAIRMAN What steps were taken at that time to check that the thing was off, is there a BF3 counter, or something?

GAUSDEN At that stage there is no BF3 counter in because the heat is such that it immediately destroys the counter as soon as you put it in.

CHAIRMAN You are relying on the fact that all the rods are in and it always is shut off when that happens. That's the way an engineer would look at it.

GAUSDEN That was the way of thinking, yes.

DIAMOND Is there a continuous record of the position of the control rods at all times?

GAUSDEN No there is not. It is logged; there is no recorder chart which records this.

DIAMOND Was the BF3 counter in at this peak?

GAUSDEN It was not.

DIAMOND The only indication we have of pile power is on thermocouples.

GAUSDEN And the installed BF3 ion chambers. They show the power, these show the temperature.

KAY Normal measurement of pile power is taken from the ion chamber? Is this what recorded 3 MW?

(**GAUSDEN** Yes)

KAY How often – is that a spot recording?

GAUSDEN The pile power is a continuous recording.

KAY Is that the chart you have there?

GAUSDEN No, this was the original BF3 counter as inserted when we shut down the pile and while we were running the rods out on the first occasion, showing how the counts gradually rose.

SCHONLAND How long did this go on, these 3 MW were on? 3 minutes or 6 minutes?

GAUSDEN I would say a matter of $\frac{1}{4}$ to $\frac{1}{2}$ minute.

CHAIRMAN Could we see or could we have copies made of the record of the pile power during this second nuclear heating.

GAUSDEN Yes, there is a Kent Recorder Chart which shows this quite clearly.

CHAIRMAN Could we see that?

GAUSDEN Mr. Bowen has it at the moment.

CHAIRMAN If we got it would you explain it to us?

GAUSDEN Certainly.

(*Chart brought in*)

GAUSDEN This is the normal shut down condition as indicated by the power level recorder. When the rods were running out at this time, the range at this time is 0 to 200 kW. There is a very slight turnover here and an immediate increase up here which was reduced by switching to the second range which is 0 to 2 MW. It was still found to be off scale; was reduced again by reducing the range to 0 to 20 MW and this is the resulting peak here. It rose up to this point here which is exactly 3 MW and then action was taken to run the rods in and it immediately fell straight down again. The explanation of the trace coming up again is switching the

range back down again to follow it. This is segregated into hourly readings from the time it actually turned up here to the peak point here, was of the order of 10 minutes according to this chart, and then it immediately dropped down again and about $\frac{1}{4}$ hour after that was down at the 400 kW level.

SCHONLAND It was up at 3 MW for longer than $\frac{1}{2}$ minute.

GAUSDEN That is the peak point which goes up and immediately turns over and comes down again.

SCHONLAND What was it doing on the earlier side, when you went off scale?

GAUSDEN That is only indicated by the lines across here.

SCHONLAND It could have been up at the peak, couldn't it? I don't understand whether the first rise to 3 MW from that last. Your ...?... scale is instrumental or operational.

GAUSDEN That is merely switching from range to range.

SCHONLAND Is it not possible it was getting to 3 MW at this point here.

GAUSDEN I do not think so, because this represents one hour, so this is probably two or three minutes from that point there on the line, to that point there (above) the line.

SCHONLAND Is the instrument's response instantaneous, or slow?

GAUSDEN It is a matter of two or three seconds.

KAY I think Dr. Schonland's point is that bit there is not a rise of power but a response of the pen in the recorder, going on to the new scale; that is was probably at 3 MW from the point where we...

GAUSDEN No. If you switch range from the top here, it overshoots straight down to the bottom there, and you can in fact see where the overshoot occurred. It then came back to that point there, and then built up there.

KAY You would say it was only at two MW from there to there.

GAUSDEN Yes, certainly.

KAY Did the 2 MW go on the second range? So that it certainly got to 2 MW? You are saying it is not at three, it is actually at two, so it does represent a growth of power and not an instrument response to a change of scale?

GAUSDEN Yes.

CHAIRMAN This is an important record, Mr. Gausden, and I should like to keep it here. Will you be hindered by not having it? We'd like to table it for the record.

GAUSDEN Not at this stage.

DIAMOND Is there a time scale on this, date and so on?

GAUSDEN The time is not in fact marked on the chart, but this was the first heating on Monday evening running through to Tuesday morning.

SCHONLAND I do not seem to have here the power on which you operated the first release.

GAUSDEN The maximum point reached was 1.8 MW.

SCHONLAND You said you had never on previous double releases, had you pushed up the power in this way. What is the sort of procedure you remember on previous occasions.

GAUSDEN On previous occasions we had a small indicated power of the order of hundreds of kilowatts, which stayed at that point and raised the temperature slowly to the point you wanted.

SCHONLAND How long did you keep them on?

GAUSDEN This is varied, but hours.

KAY Have you ever known the pile power respond so quickly to what appears to be a very small control rod movement?

GAUSDEN No, I questioned the foreman on this and he had never known it so touchy.

KAY Is it possible to move any other control rods other than these.

GAUSDEN No, because they are physically disconnected at the driving point. The drive shaft is disconnected.

KAY One just has the choice of speeds on the push button control?

GAUSDEN There is no choice of speed. The speed of withdrawal is confined to 1.2 cm a minute.

KAY You've got a button to start movement and another button to stop it?

GAUSDEN No, you keep your finger on the button all the time.

KAY You can't press more than one or two at once?

GAUSDEN No, it makes your finger ache to keep even one button going.

KAY Two at the most?

GAUSDEN Not even two, they are too far apart.

DIAMOND So it's possible to have two inching out, but not more?

GAUSDEN Yes.

KAY You could press two buttons, one with each hand.

GAUSDEN Yes, you could but that in fact was not being done. It was one rod and then the other rod.

SCHONLAND Would Mr. Gausden give a little more clarity as to the length of time that he thinks from the records, you had say 2 MW developed: was it as much as 5 minutes?

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GAUSDEN At the point where we were 2 MW or above?

GAUSDEN Yes.

SCHONLAND Yes. What would you say was the upper limit to it; I know it's very hard to say.

SCHONLAND On 1960 it is rising fast. I take your remarks to mean the general mass, not the particular record.

GAUSDEN Of the order of 5 minutes.

GAUSDEN I was referring to the general over all picture.

SCHONLAND Could it have been as much as 10?

SCHONLAND Could I ask that the 2053 records be plotted? I know it's a lot of work.

GAUSDEN No.

GAUSDEN I will get that done as soon as possible.

CHAIRMAN I should like to ask Mr. Ross whether we can later call the man who was actually pressing these buttons. Who would it be?

CHAIRMAN One other subject we would like to ask you about. Perhaps we could look at Chart No. 5 again. Now if you look at the time when the dampers were opened and closed, this was in the late hours of Wednesday night and the top of the record shows it. Look at that and look at record 2053 during that period; you will see that when the dampers were opened first, 2053 shows a temperature just over 400 and then it slightly rises and does a number of oscillations as the dampers were put on and put off. Now, at that particular period - it is in the early hours of Thursday morning. I am not sure whether you were at home as would be normal or whether you were on duty.

GAUSDEN It was the foreman, Mr. Toole.

CHAIRMAN We would call Mr. Toole. I am not quite sure when, and I would like to get from him his experience of what he did and what he saw.

GAUSDEN He is on rest at the moment.

CHAIRMAN Are there any questions on the second nuclear heating?

(None)

GAUSDEN I was not on duty.

CHAIRMAN Then I don't think it is fair to ask you the question. Who then was on duty?

We would like now to change to another subject completely. In studying these records of graphite temperatures I would like you to look at Chart No. 4 (*later changed to Chart 5*) which is the graphite temperatures 4' from the charge face and I would like you to look at the period on Monday when record 2655 (*later changed to 2053*) is the top one, the top knee of all. What we are interested in is the fact that that has gone up to very nearly 300° and what we are wondering is whether from the records that Windscale has, whether we could have on that particular channel, the record of what it did on the previous Wigner releases. What we had in mind was we want to see whether that channel, where that instrument is recorded, whether in fact it has had a previous Wigner release. We have got that there was a Wigner release last November and one before that and one before that and so on but because that particular thermocouple reading is so high, we would like to follow that one back through previous Wigner releases to see whether it went off or whether it did not.

GAUSDEN A Physicist III.

CHAIRMAN I am a little perturbed that because the temperature was over 400, that when the dampers were opened it caused diminutions, and then it went up again. It looked as though something funny was going on. The dampers did not seem to be doing what they...

GAUSDEN Some charts were drawn of the previous Wigner release.

GAUSDEN It was pulling it down.

(Correction of chart and thermocouple numbers).

SCHONLAND Chart No. 5 refers to the top period of 2053. This is obviously an abnormal sort of Wigner, if it is a Wigner. I have looked at 1956 release and this channel was not released in 1956. I want to know from you the information on all previous attempts to get a Wigner release on this channel, 2053, which has two thermocouples, on all previous occasions. All those before this one in 1956.

CHAIRMAN It was pulling it down, but recovering again, and there was no Pile stack activity noticed until 5.30. There was a period, according to these records, between midnight and 5.30 when one graphite temperature locked above 400 and although the dampers were opened and closed, no more serious attempt was made to get the temperatures down. According to standing instructions about the pile, they are written in such a way that if temperatures are over 415 (admittedly it only went up a couple of degrees over 415 so it was very marginal) but it was rising, I would like to know what the man in charge thought.

It looks to me as obviously it has not been released in 1956. And a supplementary question is that I would like to draw his attention to a point that somewhere in the records it says "the general mass of the graphite was either stationary or falling". Now on these two curves it was rising. On 2053 and 1960 it is true it is flattening off on 2053 at the time you decided to... but it is rising.

GAUSDEN He is on duty today.

CHAIRMAN What is his name?

GAUSDEN Mr. Jenkinson.

CHAIRMAN We will call Mr. Jenkinson later in the day.

KAY Could I ask about the air outlet temperature. If you look at the chart showing the air outlet temperature in the west air duct there are one or two slightly puzzling things about it. Perhaps you could start at the beginning, on Monday and follow it along. The first thing it shows is the period of nuclear heating when you are starting the whole procedure

and the air temperature rises quite understandably and then it starts to fall towards the end of the nuclear heating period, when the heat is perhaps falling off. And then it remains steady between 40 and 50 degrees, about 42/43° with no measurable variation at all and right through the second nuclear heating period it is absolutely steady even though we rise to a higher Pile level, during that second nuclear heating. Then we remain steady or if anything slightly falling, until 6 to 8 o'clock on Wednesday when we start a slow and gradual rise. You then get into the period of Wednesday night and the early hours of Thursday morning when a lot was going on, dampers are being opened and closed and there are very understandable fluctuations. Up to that point could you comment a) on the absence of any temperature rise during the second nuclear heating period, and secondly the long slow gradual rise starting at about 6 o'clock on Wednesday.

GAUSDEN Taking your first point where the second nuclear heating was done. I think the point here is probably that by that time you have very static conditions inside the reactor shell and the effect of putting nuclear heating into one particular portion is not necessarily going to affect the outlet air temperature.

KAY In the first one, you have only just closed off the fans and we still have a certain residual flow through the reactor and on the first nuclear heating, the heating was reflected very quickly by temperature in the air duct, and the second one was almost static?

GAUSDEN That would be my opinion. The rise taken from approximately 6 o'clock Wednesday, right through Wednesday until 21.00 - I am not quite so clear about. Certainly I would think that the general graphite temperatures had risen over that period although whether they would, in fact, have been reflected in quite this manner I find difficult to answer.

KAY There will be some delay because the air is shut off to the pile during the Wigner release, for the very reason that you've given me: that the absence of a peak on the air temperature during the nuclear heating period would apply with equal force to the delay in reflecting the gradual heating of the Pile in the measurement in the upper air duct. Would that strike you straight away as being peculiar compared with previous records or not?

GAUSDEN I would like to look at previous records before assuming that categorically, but my impression is that we have seen this sort of rise.

KAY May I ask Mr. Gausden to look into that.

CHAIRMAN Yes please.

DIAMOND Returning to the nuclear heating curve: where is the recording element for the air temperature?

GAUSDEN In the outer air ducts leading to the chimney.

DIAMOND Round the void.

GAUSDEN On either side.

DIAMOND Going back to Professor Kay's point about the difference between the kicks in the two cases. In the first one there is a kick up and in the second there is nothing. The dampers were in the same condition in both cases?

GAUSDEN Yes.

DIAMOND I find it difficult to understand why there should not be an indication on the second one if there is on the first, because the air flow must be the same in both cases.

CHAIRMAN Would it be worthwhile to go back to the original records?

DIAMOND I think so, and it seems to me it might be explained by a marked difference in the integrated energy put in during the two periods, if the first was much bigger than the second, it would account for it but I do not gain that impression.

GAUSDEN It certainly isn't my impression.

DIAMOND I gather that more was put in on the second occasion than on the first.

GAUSDEN Certainly, from these records.

DIAMOND That makes it all the more curious that there is no change in the outlet air temperature in the second case.

CHAIRMAN Perhaps we could ask Mr. Gausden to produce the original records.

(Recap of what Gausden has been asked to do.)

MR. MOORE

CHAIRMAN You know what the question is, Mr. Moore?

MOORE The point is the activity from tritium would be soft beta which will not be picked up on the ion chamber and there is nowhere else it could be picked up. The ion chamber operates at 10 atmospheres of argon. It has a steel sheet about $\frac{1}{16}$ " thick which will not be penetrated by beta particles, and I am quite sure there is no other instrumentation that would pick up soft beta particles. The last time I was in, I estimated the amount of tritium in a cartridge, and at 200 cc it would be between 50 and 150 cc of tritium H3 atoms. In addition to that there is between 50 and 150 of hydrogen and 100 to 300 of helium. The activity is estimated as being 2 curies per cc of tritium, beta activity. The energy would be less than 0.019 MeV.

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CHAIRMAN The real thing is, if it had been burning the ionisation chambers... the activity would not have been detected.

KAY The only activity which you could pick up would be gamma activity.

This evidence was taken at 12.30 p.m. on Sunday, 20th October 1957.

MR. PHILLIPS

CHAIRMAN Good morning Mr. Phillips, do you know the members of the Committee?

PHILLIPS I'm afraid not, I know Professor Kay.

Introduction of members.

CHAIRMAN For the purposes of the record, will you please tell us what your job is now with the Atomic Energy Authority.

PHILLIPS At the moment, I am the Works Manager of the Reactors at Dounreay.

CHAIRMAN We wish to question you mainly on one point. We have been looking at the maximum temperatures which have been reached in the pile during Wigner releases on the last few occasions and at the possible behaviour of the different cartridges, both fuel element and isotopes, and we find that one of the temperatures that had frequently been quoted and apparently used went back to the time when you were in charge of the pile, and the statement attributed to you is that the maximum temperature reached in the pile during a Wigner Release was 350°.

PHILLIPS That is during my experience of Wigner Releases?

CHAIRMAN We are referring to the time while you were running it, not since you have left.

PHILLIPS I would say it was of that order. I wouldn't like to guarantee without looking back at papers, but my recollection is that it is of that order, certainly not in excess of the cartridge temperature that we normally operated the reactor at.

CHAIRMAN What cartridge temperature did you?

PHILLIPS It had reached 395 before I left.

CHAIRMAN The 350 I presume is mainly graphite temperature during a Wigner release.

PHILLIPS Yes, the highest temperature released reached.

CHAIRMAN When did you leave pile work?

PHILLIPS September or October 55.

ROSS Mr. Gausden did not take over directly from Mr. Phillips. Mr. Fair has been in between.

CHAIRMAN Mr. Fair succeeded you?

PHILLIPS Yes.

CHAIRMAN Do the members wish to explore this question a bit more?

SCHONLAND Can I ask you Mr. Phillips, was A.M. loaded in the time you were in charge?

PHILLIPS Yes there was a small amount of A.M. loaded as part of Harwell isotopes. These are very small amounts and we did begin the production loading of the first type of A.M. cartridge.

KAY Can I ask Mr. Phillips on what date approximately were fuel element temperatures first taken up to 395? At an earlier date, I think I am right, they were running around 350 to 360.

PHILLIPS 350 and 375 for long periods. I would not like to trust my memory on the date.

KAY But there were three distinct steps - 350, 375 and later up to 395.

ROSS There were steps in between - 380, 385.

PHILLIPS 380, 385, for shorter periods.

KAY During Wigner energy releases on the piles during your period as Manager, did you work so that the graphite temperature was limited to 350. Did you prepare to take action to cool off the pile if it rose above or did it simply not happen?

PHILLIPS We were prepared of course. We would not allow any maintenance on the main blowers for instance, we always had the main blowers standing by until the Wigner release was over. This was to have them available if temperatures reach a level which in my opinion at that time; we would have needed to take that action.

KAY Would that temperature have been 350 or would it have been the fuel temperature?

PHILLIPS I would have judged them to be necessary when they reached the fuel temperature; the 395, primarily because this was the temperature to which we were prepared to take the fuel under any conditions.

KAY It is a little different when it is the graphite and not fuel, and you would have taken the graphite up to 395. You would not have stopped that at a lower temperature?

PHILLIPS I don't think so.

CHAIRMAN Do you know whether you ever did take it up to 395?

PHILLIPS I don't think so. I would say no.

CHAIRMAN How many Wigner releases took place while you were in charge of the pile?

PHILLIPS Of the order of five or six.

SCHONLAND During that time did you have any double attempts at release?

PHILLIPS Yes. We had great difficulty. I can remember one particular occasion when it took two attempts to get it out, separated by a period of 2 or 3 months. It was an energy release starting and then fizzling out.

SCHONLAND On this occasion the releases were 24 hours or less apart. Did you have any experience of double releases or attempts at release at such a short interval?

PHILLIPS No, not at such a short interval.

CHAIRMAN You never had the pile on for a bit to get it warm, and it started and when it was dying you put the pile on again - you never had that?

PHILLIPS No, once the release started we finished with the neutron heating and that was it as far as energy release...

SCHONLAND On occasions when you did not get an energy release why didn't you give it a second kick within 12 hours.

PHILLIPS I believe in order to get the graphite temperature up to the temperature at which you would start to get release to come out, we would, under these conditions have to have overheated the cartridges. The release always started in the most favourable position and to get say, the edge of the pile up to the temperature, you would have to overheat the centre.

CHAIRMAN Any further questions? (*None*)
 Thank you Mr. Phillips, it has been a long journey for so little. I do not think we need you again.

SCHONLAND We have been told that a double release has been tried on several occasions in the past. It was not tried during Mr. Phillip's period of office, in whose period of office was it tried? The next one is Fair, is he available?

ROSS He is at Chapelcross. We could get him down.

CHAIRMAN Do we want Mr. Fair, and if not him then it must be Mr. Gausden.

CHAIRMAN There are three possibilities - November 55, February 56 and April 56.

Sunday 3.40 p.m.

MR. JENKINSON

JENKINSON I am the Shop Manager (Technical) for the Pile Group Windscale.

CHAIRMAN Mr. Jenkinson we have been studying the records through this whole event of the Wigner release with as close as possible attention, and we understand from Mr. Gausden that you were the senior officer in charge at a certain time and I just want to check first that this is so and the time we are interested in is from about 12.00 midnight on Wednesday for a few hours of Thursday morning.

JENKINSON From midnight to 9.00 in the morning.

CHAIRMAN Now we want you to tell us in your own words just what you were doing, what you saw, what you did.

JENKINSON When I arrived in Mr. Goodwin was in charge and he was interested in the maximum graphite temperature which he noticed was not only increasing but seemed to be increasing at an increasing rate. He closed the dampers that reduced the temperature, and he was reducing the temperature again when I got in and for most of the rest of the time I was watching this. I was on the roof with the recorders watching this temperature. I opened the dampers on two occasions to reduce this temperature and that was the prime interest. I visited the Control Room on several occasions and then about one hour or so after the last damper opening I noticed a stack filter activity which was by this time decreasing.

CHAIRMAN When was it decreasing?

JENKINSON Looking back at the record it seemed to increase during the time when the dampers were open.

CHAIRMAN The record we have here shows that about 5.30 there was a rise and then it dropped to a minimum and then gradually rose to a very abrupt maximum at 12.00. When did you first notice it?

JENKINSON I don't know exactly but it was on the decrease. I think it was somewhere about 7.00 in the morning by that time and I noticed there had been an increase while the dampers were open and I tried to think what this was due to and came to the conclusion that something had blown through. I did not know what and then thought no more about it. Looking back at the record I notice that not only did it begin to increase when the dampers were opened but about one hour before then a very slight increase was noticeable on the original chart. It did not come out very well on the sketch.

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CHAIRMAN It had not come out very well on the sketch and so are the original records nearby? Could we have the original records Mr. Ross? You say the activity went up when you opened the dampers and dropped a bit when you...

JENKINSON I did not notice at the time. I noticed it afterwards. Had I seen it rising while I was blowing through I probably would have been very alarmed.

CHAIRMAN Yes. So that we are perfectly clear Mr. Jenkinson you say you were watching the thermocouple records and there was one or more of them causing you some anxiety. Do you know which one or ones?

JENKINSON Well it was point D10 on the recorder, that is what is in my mind as most and I see this is the thermocouple in 2053 at 6 ft. This is the chart. You have got it virtually steady until this time, at which all the 3 points show something of an increase, and here is. I see it is about 5.20 on the chart, but the chart might well be 10 minutes out, and here it stopped increasing at 6.00 on the chart that might well be... It was 5.40 when I closed the dampers last. So this seems... I do not think though I might have done, that we checked the timing of the chart at the time but I didn't do this. I just assumed that these two events did coincide. But I did perhaps think that might be significant, this increase at about 4.20.

CHAIRMAN Looking at the chart now you would say that that record showed some increase starting at 4.20. When you first went on duty which was at 12.00 midnight the temperature on that 2053 was already very nearly 400?

JENKINSON Yes. It was 405 I think. These are temperatures which have allowed for the fact that the recorder is 5°C higher, the temperatures plotted on the graphs here directly from the record chart. It was 405°C when I came on.

CHAIRMAN Then your first thought was that we must open the dampers and cool it off?

JENKINSON Mr. Goodwin was in charge. I observed it and then 2 hours later, once it had begun to increase after the cooling, (the cooling effect is delayed by a delay by something in the region of one hour before the graphite falls to the minimum after the cooling) when it started to rise again it had risen 5°C in an hour to 412°C so I opened the dampers. Then I watched it through a little longer and when it started to increase again after cooling, it rose 4°C in one hour and then 5°C in an hour, so I cooled again as much because it had increased at a slightly increasing rate.

CHAIRMAN You just opened the dampers for $\frac{1}{4}$ hour periods?

JENKINSON The first was for 30 minutes but the second, I deliberately left it open longer to give a bit more cooling.

CHAIRMAN Roughly how long?

JENKINSON It was 30 minutes the second time. Then I did no more until the next shift came on, during which time, once the graphite had begun to rise again after the cooling it rose

4°C per hour, then 2° per hour and it looked as if it was steadying out.

CHAIRMAN Was it within your discretion to keep the dampers open all the time or were you?

JENKINSON No. If it seemed necessary; for instance if opening the dampers had not had the cooling effect, I would have rung up Mr. Gausden.

CHAIRMAN But it was just not quite enough to do that?

JENKINSON Well on a previous occasion we did open the dampers to reduce the temperature and we did not consider it necessary to call anyone in on this occasion.

CHAIRMAN It was only that thermocouple record which caused you some anxiety? The others were going on all right?

JENKINSON Yes. The others were all right.

CHAIRMAN That one wanted careful watching and you thought the dampers opening and closing would give enough check on it to hold it at a reasonable temperature.

JENKINSON It was keeping the temperature down and the rate of increase down as well.

CHAIRMAN And then you noticed the pile stack activity and you have told us about that. Were there any more excitements before you left at 9.00?

JENKINSON Nothing I noticed.

CHAIRMAN Who did you hand over to?

JENKINSON It was Mr. Rutherford I believe. I must confess I am not sure. It was either Mr. Rutherford, or Mr. Hamer the Control Engineer.

CHAIRMAN What happened? You have to climb up and down by foot to the roof? You just told him about the pile activity and called his attention to that?

JENKINSON I don't think I told him about the pile activity. I'd forgotten about this at the time. I'd made a note of it and thought it over, and then thought no more about it.

CHAIRMAN Have you ever been on duty before when stack pile activity jumped like this?

JENKINSON I've never noticed it but I have never particularly looked for anything like this.

CHAIRMAN What did you think had happened?

JENKINSON I came to the conclusion that something had blown through. The significant thing is that I opened the dampers for 30 minutes this time which was twice as long as ever before.

CHAIRMAN And then you went off duty at 9.00. When did you come on duty again?

JENKINSON Not until 8.22 the following morning.

CHAIRMAN By that time things were happening?

JENKINSON They were beginning to put water in.

CHAIRMAN That concludes all I have to say. Any questions? Well thank you Mr. Jenkinson. I don't know whether we will want you again.

SCHONLAND Pile activity was entered in the log at that time.

CHAIRMAN We have got the log.

WITNESS: MR. TOOLE

TOOLE I am a Tech. 2 in the Pile Group. My job is at the moment on D shift, but normally I am known as Relief Tech. 2 and I stand in when anyone is off sick or on leave or for any other reason. I have been called in on D shift at the moment due to a promotion in the Group.

CHAIRMAN We have been looking for a long time now at all the records which the thermocouples have shown over the period of Wigner release of the pile and we have noticed various kicks and rises and we are trying to get some picture of what might have happened. and this morning, when Mr. Gausden was giving us his evidence he said that you were on duty during the period of the second nuclear heating. Now that would be around midday on Tuesday the 8th?

TOOLE It was the 8th but a little earlier than midday. 6.00 in the morning until 2.00 in the afternoon.

CHAIRMAN It is because you were on duty at that time that we want to ask you what you actually saw during that period. Now we know that when the second nuclear heating went on we know from the records that the temperatures of some of the uranium rods rose rather fast and then dropped and we would rather like an eye-witness account of what you saw.

TOOLE On the morning of the 8th I was not on duty in Pile 1, I was in Pile 2. I was called to Pile 1 to diverge the pile, to heat up certain portions of it which had not triggered off, because the Tech. 2 in charge of the Pile was too busy because he was taking graphite measurements, thermocouples etc. Pile 2 was quiet so naturally I was called over there. There was an instruction issued by Mr. Robertson as to just how the rods had to be moved on this pile. The pile had to be diverged with the coarse rods only; the fine rods had to remain at the fixed position and had not to be touched. Any rise in temperature had to be controlled by coarse rods 2 and 4. Any fall in temperature had to be controlled by coarse rods 1 and 3. I started to motor out these rods with this instruction written on a piece of paper in front of me on the desk. I motored out the coarse rods collectively. I mean that certain coarse rods were isolated so we were only motoring in particular rods. The temperature then, the highest uranium temperature, was 300°C. I think the thermocouple was 2353. I was told to hold the temperature at 330°C, highest uranium temperature. While I was motoring out these coarse rods the

temperature was rising very very slowly on this particular uranium thermocouple. I remarked to Mr. Robertson at the time that there would be a question of whether the particular temperature would reach 330° or the pile would diverge first, and his answer to that was that the temperature was the thing that counted; if the pile had not diverged, then to hold it at 330°C. Eventually the pile did diverge. Immediately I saw the chart turn the corner I started to bring in the coarse rods. This I had to do on the buttons because I only had to bring in two groups of rods. The thermocouple was only stamping once every 3 minutes. From one stamp to the next stamp, the temperature could rise quite considerably. I had a man posted at that recorder watching the temperature all the time and calling out the highest one. When I started to get a hold of the pile, to check it, the first temperature I had, this man called out, was 385°C. I realised this was high and kept hard on the buttons to bring this in, being careful not to kill it off and have to go through the whole business of diverging the pile again. I eventually got the temperature down to 334°C. I was then told to hold it at this, but during this increase in temperature I had been told by Mr. Robertson to notify the roof immediately the pile diverged, what the temperature was. When I got the control of it, to immediately phone the roof. This I did. Immediately the pile diverged I told them it had diverged. I told them it had risen to 385. I'd got hold of it and was bringing it back. I'd got it levelled off at 334°C. I was told to level it off at 330°C. I was immediately told that 334°C was quite all right, and to hold it at that.

CHAIRMAN So that I am quite clear, you had this high temperature and then you pressed on 2 buttons at once did you?

(TOOLE Yes.)

CHAIRMAN ...and these other rods started to come in and the temperature started to drop. How long did it take to get from 384 to 334°C?

TOOLE 10 minutes would be near, maybe 7 minutes.

CHAIRMAN When you got to 334°C you reported and they said that was near enough to 330°C. What did you do then?

TOOLE Well I'm sure everyone here knows just what it's like holding in pile at that temperature, at very low power on coarse control rods. To get this on 334-350°C and hold it on a straight line is virtually impossible, you get the thing meandering from 328 to 336°C. The people in the Pile Group were perfectly aware of this $\pm 5^\circ\text{C}$ was quite all right.

CHAIRMAN Did you keep pressing fine controls?

TOOLE Coarse controls. If the temperature increased I motored in Nos. 2 and 4. If the temperature fell off I motored out Nos. 1 and 3.

CHAIRMAN Have you done this before? Did you know whether the pile was responding just as it would normally do?

TOOLE It was responding but very slowly on the coarse rods. There were only 5 rods available. Normally, to get immediate response, one would use fine control rods but in this instance, that was out, so that the response was very, very slow indeed.

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Say in Confidence

CHAIRMAN How long did you go on trying to keep it at 330°C? This happened about noon and you were on this from noon until 2.00?

TOOLE I was on this until the end of the shift.

CHAIRMAN And you were still trying to do this the whole of the shift. Who did you hand over to?

TOOLE In that particular Pile I did not hand the shift over. As I mentioned earlier I was standing in. At 1.50-1.55 I went back to Pile 2 to write up the log and hand over Pile 2.

CHAIRMAN And then somebody came in at 2.00 and took the pile over?

TOOLE When I went off to Pile 2, there was the Pile 1 relief there. I never left the desk until I was relieved.

Board of Enquiry

Monday 21.10.57

Evidence heard from:—

Charlton	pp 5.1 - 5.2
Cutts	pp 5.2 - 5.5
Gausden	pp 5.5 - 5.11
Hall	pp 5.11 - 5.13
Cutts	pp 5.13 - 5.16
Howells	pp 5.16 - 5.25
Farmer	pp 5.25 - 5.30
McLean	pp 5.30 - 5.34
Cutts	pp 5.34 - 5.36
Farmer	pp 5.36 - 5.37

9.50 a.m. Monday, 21st

WITNESS: DR. CHARLTON

CHAIRMAN We are sorry to call you back, Mr. Charlton, but in looking over what you have said there was one point that did not seem to be right and we felt that we must, therefore, ask you to come back and check whether there really was a mistake. The point we could not check was a statement you made that Thallium metal melted at 400° and when we looked it up on the records it was 300°.

CHARLTON I regret this was a mistake on my part. I realised it later and checked it up. The text books give a value of 303°C and seem to agree on this.

CHAIRMAN That puts that straight. In view of that I would just like to go over briefly again the Thallium metal cartridges. We have the construction, you need not repeat that but because it melts at 300° and because undoubtedly temperatures of that amount are in the pile during the Wigner release at any rate, the thallium must have melted. Do we know anything about whether liquid thallium metal would attack the aluminium and corrode its way through?

CHARLTON There is evidence on that point from two sources. First of all, I have made a copy of the aluminium-thallium system from what I believe to be a reliable source and they appear to have limited mutual solubility only above the melting point of aluminium.

CHAIRMAN So that the implication of that diagram is that although the thallium was molten in its container, it will not corrode its way through at 300 or 400, in fact not until 600.

CHARLTON This is so and further there is the evidence of the examination of discharged cartridges.

CHAIRMAN Perhaps you would take us through this.

CHARLTON I think the important thing here is, first of all, that I have opened these cartridges personally. We received two cartridges. I can give you their numbers and when they were loaded. I must mention that the details of the numbers of the cartridges and where they were loaded should be checked because this was 'hearsay' from records sent to us. As our records go, we received two cartridges, Nos. P5205 and 5206, which we were told had occupied channel 2653 in Pile 1. These cartridges were withdrawn late last year, I don't know the exact date. When we received them at Amersham, one cartridge was found to be punctured. This was quite obvious from a visual inspection of it, but the cartridge, although somewhat flattened as is usual in isotope cartridges when you receive them, was not otherwise affected. The metal appeared to be perfectly good, but it had an obvious hole in it. The other cartridge appeared to be quite intact.

DIAMOND But it was flattened.

CHARLTON Yes. This is almost bound to be the case in the nature of these cartridges, which had a bar of thallium melted along the bottom and the space above it just unoccupied or full of air, so that on dropping into the pond, with the lead spacers and so on, it would get quite badly damaged.

The second cartridge appeared to be intact, but when we came to open it a few weeks later, we found water inside it, suggesting that it had punctured, perhaps a very small hole, and during its stay in the pond, water had entered. Now, although the aluminium of these cartridges appeared to be quite unaffected when we received them, during storage of about a month the aluminium had corroded very badly, so much so that quite large holes were apparent. Now the reason is this, both those cartridges had got water inside them and have been stored in contact with air. Under the action of water and oxygen, or water and air, thallium will give thallos hydroxide, this is a standard way for making thallos hydroxide. Now the thallos hydroxide is quite a strong base, comparable with say, sodium hydroxide and would be expected to attack aluminium in the same manner as a solution of sodium hydroxide. Furthermore, aluminium is known to react with thallos salts in solution; itself going into solution and depositing thallium metal, so therefore it is not surprising that the cartridges were attacked. But I think the important point is this: I opened these cartridges in a glove box using extra heavy gloves which shielded out the beta radiation. I was able to take strips of the cartridge and pull them off with tweezers, but where the thallium and aluminium were in intimate contact there appeared to be no attack there and no alloy. One could strip the aluminium can from the thallium like skinning a banana, and certainly there appeared to be no sign of any alloying there.

CHAIRMAN Which was the one you told us was very difficult to scrape the thallium off the can? Is that the one?

CHARLTON These were they.

CHAIRMAN When you say there was this first cartridge which had an obvious pinhole, had any thallium come out? Was there a blister there?

CHARLTON I am speaking from memory, but as I remember it, there was a hole as though something had hit it and pushed a small hole inwards in the space above the thallium. That is, you have the cartridge with the solid part at the bottom and an empty space above, and there was a hole in that empty space. This is not unusual with thallium received from Windscale.

CHAIRMAN You get an amount of mechanical damage in shoving it out.

CHARLTON Certainly.

KAY You mentioned that there were two cartridges containing a larger quantity of thallium, 700 grammes. I do not remember exactly how they were constructed. Can you tell me whether the thallium filled the entire cartridge or was there a space between the thallium and the aluminium?

CHARLTON These are the cans to which I am referring, the ones with 700 gms. They were filled by taking a can and dropping in pieces of thallium rod. I have some of the similar thallium here, and as I understand it (I did not fill these cartridges) pieces of thallium were just dropped into the can until it was filled and then it was sealed; so that having melted, only about half the volume of the can was occupied by thallium.

KAY Was the gas space filled with argon or was it just air?

CHARLTON I do not know.

KAY This was done at Harwell. These are not the ones for which you were directly responsible.

CHARLTON No.

SCHONLAND When you receive the irradiated thallium back, does it show only the activity from thallium or is there any impurity in it of shorter life?

CHARLTON We would not detect any impurity of very short half life because there is a delay of some weeks in getting the material, but apart from that there would be no other radioactive materials present whatever. It is an exceedingly pure radiochemical.

KAY The chemical properties of thallium I believe it bears some relation to the alkali metals. I was wondering how much is known about the reaction of thallium with air under different conditions, the way in which it would burn and so on. You did tell us something about this, but I wondered how far this was based on direct experience of your own.

CHARLTON Yesterday I tried some quick tests with thallium metal. I dropped it on to aluminium just below its melting point and it did not ignite but melted, and what appeared to be a skin of molten oxide, and thallic oxide, melted about 300 so it is said, and was brownish in colour and also brown fumes were given off and this was presumably a mixed thallic/thallic oxide. The literature states that thallic oxide which is what you partly get when thallium oxidises at high temperature in air, will start to sublime at about 500 with the composition of a mixture of thallic oxide and thallic oxide. I tried also dropping small pieces of thallium on to a silica crucible which was being maintained at dull red heat with a metal burner, and the same sort of thing happened. It did not actually ignite, it melted and spun around and gave off fumes. There is one thing which might be important. The fact that when thallium vapours get into a flame, it is coloured bright green, it is a very intense and very characteristic colour.

DIAMOND You have not tried this in the presence of graphite?

CHARLTON No, I have not.

DIAMOND There is one alkali metal, potassium, which in some association, which I forget, with carbon is used as an igniter in explosive.

CHARLTON I have seen references of this kind to the possibility of getting explosions when you heat potassium in the presence of graphite. I think we must qualify the statement that thallium behaves like the alkali metals. As a metal, I think it behaves nearer to ? perhaps, than anything else. It is only in its monovalent state that it tends to behave like the alkali metals and to some extent as ? . It forms an insoluble chloride but the hydroxide and carbonate have some resemblance to the alkali metals.

CHAIRMAN Well, thank you, I don't think we need retain those specimens.

CHARLTON Prof. Diamond asked me some questions at the last meeting on the construction of the various isotope cans. I have brought exhibits (which were tabled) showing the new pattern of aluminium magnesium cartridge with a tube bored at both ends. The aluminium disc is placed at one end and the end turned over. This end is now filled in with brazing which has an aluminium silicone detected and having been filled with aluminium nitrate, the same process is carried out at the other end and is a filled cartridge. This is the pattern of an isotope cartridge for thallium and for everything else except these latest aluminium nitrites. I have actually got the thallium cartridge of the kind we put in, and this was out of the same batch as we ourselves have loaded and was rejected for bad brazing. This contains three times 20 gms. of thallium, with potassium chloride on top as with aluminium nitrate. This is the job which we have been discussing this morning.

10.10 a.m. Monday, 21st

WITNESS: MR. CUTTS

CHAIRMAN Before we take any evidence, would you be good enough to say what your job is in the Authority?

CUTTS I am operating as a kind of Reactor Physicist although I do heat transfer work as well as Reactor Physics. I examine the performance of reactors when they are being designed, carrying out surveys with possible parameters and ultimately doing the final performance calculations of the reactors to be built. I work under Mr. Rotherham in R. & D. at Risley.

CHAIRMAN We have asked you, Mr. Cutts, to give us some advice on a question which I shall try to define as briefly as I can. In the Windscale piles the temperature of the fuel elements is measured quite a long way back in the pile. I think it is 16' back, and the temperatures are measured there because under normal operating circumstances that is where they are hottest but, of course, during a Wigner release, which was one of the things which was happening the temperatures are not necessarily hottest there. Now when the pile was heated up in order to start the Wigner release, and again later in the proceedings, the pile was turned on to give it a bit more heat to keep the Wigner release going satisfactorily, the fuel elements naturally got quite a boost in temperature, but the people operating the pile wanted to get the Wigner release going in the front of the pile, and therefore, they did various control rod operations that made the pile get warmer in the front. Our question is this, that if the pile is made divergent in such a way that it is getting hotter in the front regions and if the temperatures of the fuel elements are measured at the back and those at the back show quite a rise in temperature, what happened to those in the front?

CUTTS They must be getting a considerably greater rise in temperature.

CHAIRMAN This is the point on which we want some advice. Can you make this pile produce a lot more heat in one part than in another part and if we gave you where the control rods were, can you make any estimate of this?

CUTTS This would in fact be very difficult to do. You have in fact got very awkward geometry here with the control rods coming in chordally. I do not think you really have – are the shut off rods out of the reactor or were they in?

DIAMOND Out. I think, you must keep something in reserve.

KAY They are out. Some of the control rods are disconnected and the pile is being controlled on a limited number of coarse control rods – moving them out at the front and in at the back. We can get the exact movements.

CUTTS Even if one had the exact movement, I think it would be very difficult to calculate the exact flux distribution that you would get. Of course this would determine the heat output. I think one can only do this by measuring.

CHAIRMAN If you can only measure, and if you cannot calculate it, we cannot pin you down, but you did express a view, which we also assume to be the case, that they would get hotter in the front than the back.

CUTTS They will. The actual extent of this would be very difficult to determine, because what I know of the loading of the reactor, as planned a number of years in connection with the isotope production, the A.M. cartridges, the loading of absorbers would be to some extent uneven and in that the A.M. cartridges, I think, were planned to be rather centrally placed, and I do not quite remember, but I think the enriched cartridges were going to be the full length of the channel. Maybe they changed this idea eventually. This also would make it rather difficult to pin down things, and unless you can in fact pinpoint your flux distribution precisely, you cannot know much about the heat output, as far as estimation is concerned.

CHAIRMAN Would you be rash enough to make any sort of a guess? Could it be twice as much heat in the front as in the back or only 10%?

CUTTS I would think a factor of two would not be out of the question at all.

(Mr. Cutts then draws on the blackboard)

Here is the reactor sitting ? , and presumably the rods being inserted were more or less in this sort of position at the back of the pile with the gas coming out in that direction and we have the rods being withdrawn here. Presumably the flux distribution will tend to go of this kind. Now if the cartridge temperature is being measured here, obviously this peak can be considerably higher. It is quite easy – as a matter of fact we did do some calculations on the Windscale reactor for an entirely different purpose. We had thought of trying to get a very high flux in this reactor by making it a kind of two-region reactor, in which you had an outer region of natural uranium, a centre region of very highly enriched stuff, and get a very high peaky flux and our calculations showed that we could get factors of 2 or 3 quite easily in flux at peak, compared with the general level in the reactor. So that sort of factor would not be out of the way.

CHAIRMAN You cannot calculate you think with any accuracy whatever. The only way to give any accuracy would be an actual measurement.

CUTTS The only calculation you could hope to do would be if you had the reactor If in fact you could say the reactor consisted of a number of flat slabs of various kinds in this direction, and you could make some estimate then of this kind of flux distribution. But you could then be considerably adrift because, if you look in the other direction, you will have discontinuities there, and will have a kind of pool of reactor where no control rods are and this pool will tend to have a rather peaky flux distribution. You cannot hope to do this kind of thing without a digital computer. I am sure it would take several months to get anywhere with this problem. We have never actually carried out any calculations to work out the effectiveness of radially inserted control rods, by calculation. There have been some experiments done recently on an analogue scheme. This is a possibility now I'm just thinking of. There is in existence in the R. & D. at Windscale an analogue which essentially determines flux distribution in systems with control rods inserted chordally. This could perhaps be set up and an attempt made to find out flux distribution in this way. But there again you could not hope to do control rods that do not go right through the reactor. This is the difficulty. If the control rods do not go right through the reactor, you cannot make the analogue work, because it involves putting coolant through the control rods and obviously it has to come out somewhere.

10.20 a.m. Monday, 21st

CHAIRMAN Well, we have some engineers on the Committee. Perhaps they would like to ask you some questions.

DIAMOND It has always been the difficulty with these radial control rods to know what precisely was happening, but I think the problem has been put very clearly by the Chairman. What we want to know is, if we know the temperature to which the Pile has risen 16ft. back, how much more is it reasonable to assume it could have risen at the peak which is clearly going to be in front of that. You have confirmed that there is clearly going to be a peak in front of that and you said you think it reasonable that that might be as much as twice what it was at the back.

CUTTS Yes. That is the heat production which is twice.

DIAMOND Yes, we understand. I take it that it is assisted somewhat by the fact that under normal operating conditions we have not got as much flux at the front as we have in the middle and therefore we are not quite so poisoned as we are in the middle.

CUTTS That is true, Sir, for both Xenon poison and temperature poison. But going back to where we started, these poisons would have died away by the time we are talking about now presumably.

DIAMOND Temperature poison.

CUTTS What about Xenon? How long would the reactor have been shut down. At 24 hours it would be equal to its operating level.

SCHONLAND The interval is from 3 a.m. to 11 - 8 hours. Something like that.

CUTTS Eight hours after shut down on full power?

SCHONLAND Not full power but

KAY I was thinking about the second nuclear heating.

DIAMOND The onset of second nuclear heating after initial shut down.

KAY Which is surely more than 24 hours.

CUTTS I think the poison would mostly have decayed away. If it had not, there is always this possibility - if you do bring a reactor up to power and if there is Xenon present, and it has been shut down then you tend to burn the Xenon out very rapidly and of course you do add extra reactivity. This is very noticeable. You would notice it on the meters immediately.

DIAMOND It is not out of the question to do some rough calculations on this, is it Mr. Cutts?

CUTTS Not of the type I have mentioned where we presume slabs.

DIAMOND Certainly not of the accuracy to which you are accustomed, but enough to give an indication? For example, if we could establish a very strong probability that it could be a factor of 2, I think it would be helpful.

CUTTS I think it might be possible if one assumed that the reactor were infinite in all directions other than axially one might make some guess as to how, if we split up into two equal parts say, one part high reactivity and one is low reactivity. If we have some idea of the difference in reactivity between these, we might make some effort, but it is only a guide, after all. These sort of pools of reactive material that are present between where the control rods are pulling apart - these could peak quite a bit.

KAY It would be an indication of the order of magnitude.

DIAMOND We want rather more than an order of magnitude. Well, an order of magnitude on the scale of 2.

KAY Engineering magnitude.

CUTTS I think one can demonstrate that it is possible to have a flux peak and the sort of size one could have.

DIAMOND I think that is as much as we can expect. If we could do a bit of work to just establish this, I think it would be of great help.

CHAIRMAN I agree with Professor Diamond. I wonder if in fact we can define the problem with Mr. Cutts in agreement that it can be handled.

KAY All we can do there is take the approximate positions of the control rods. Whether from that Mr. Cutts can estimate the relative reactivity of the two parts of the Pile, I do not know.

DIAMOND A first model could be with a cold clean core to simplify the problem. Forget the isotopes and poisoning and start from there. He knows the position of the control rod holes. We must get a rather more close picture of exactly where the control rods were at the material time.

CHAIRMAN We would like you to do this, Mr. Cutts. We must be provided with information of where the control rods were and I think the situation we should take is where the control rods were at the maximum power during the second nuclear heating. That is the situation we would like studied.

KAY Is Mr. Cutts familiar with the sequence of events?

CHAIRMAN We have this in our records. It will take some time to dig them out.

CUTTS I know something of the sequence of events.

CHAIRMAN Shall we refer back to our own notes. Is that sufficiently clear?

Could Professor Diamond or Professor Kay, who are our Engineers, give Mr. Cutts a statement of the relevant facts.

DIAMOND We have not got the exact positions - we only know the rods which were altered.

KAY Groups 1 and 3 were moved - one group was moved out and one in and there is of course no record of the actual geometric position of the control rods. This is the real difficult. One is guessing what the reactivity changes amount to. Some are disconnected and presumably they are in definite positions and I am afraid there will be an element of guesswork as to how far you have to move the control rods to make the Pile diverge if you are moving one set in and one set out.

CUTTS In the work I can actually do, I do not think it will matter very much, because I will have to assume that the control rods are not there at the front of the Pile and are there at the back of the Pile and make reactivity balance accordingly.

KAY What more information does he need in order to carry out this calculation?

CUTTS If I start, as you suggest, with a cold clean reactor and ignore all the poisons, I think I would need no more than that. I am inclined - I would divide the Pile into two parts - one with cold clean reactivity and the other with some other reactivity necessary to balance. This would give the nearest point of reference. I could then see what the flux distribution looks like. Whether I can go any further than that and see what effect some other arrangement would give me, I don't know.

KAY Could you do the simplest possible to begin with and do that and report back on that calculation.

CHAIRMAN How long would it take? Can you do it here?

CUTTS Probably, if I could get hold of the other information I need - it would be a question of lattice parameter, which I can get from current reports - I could probably do it by lunch time.

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CHAIRMAN In that case, please go away and do it and when you have done it, perhaps you would pass word in that you have done it and we will hear what you have done as quickly as we can.

KAY Can I ask a question which may have some connection with this? Are you familiar with the arrangement for measuring the normal average power of the pile? This is done with an ion chamber at some point. Would it be possible to estimate what the relationship would be between the power level as portrayed in that flux distribution for the Pile, and the Pile level that one would measure from the neutron flux at the point where the ion chamber is located.

CUTTS Yes, I think so. If you have an ion chamber in a particular position, you measure the neutron flux at that position.

KAY Assuming that this is done and a certain power level is recorded on the normal meter in the pile working off an ion chamber, I am trying to get the axial level.

CUTTS I see what you mean. What you need is this compared with what the normal flux distribution is. I'll try.

CHAIRMAN I think that what Professor Kay has in mind is that the meter that says it is reading pile power if the pile distribution is not normal may not be reading pile power.

CUTTS This is quite possible. It is quite true, because it does depend on the point value. It could be reading nothing like the pile power.

KAY Perhaps you would look into that following your other calculation.

CUTTS This is a relatively small point.

CHAIRMAN Thank you, Mr. Cutts. If you will do your homework.

10.50 a.m. Monday, 21st

CHAIRMAN There are a number of jobs in front of us. Mr. Gausden. I would like to start with the records of the temperature in the air outlet to the stack during this period and we want to look at it rather closely because we are looking for small changes. Have you got the records?

GAUSDEN The actual chart, no.

CHAIRMAN What have you got then?

GAUSDEN I thought that you were going to refer again to the peak at the beginning of the chart which you already have.

CHAIRMAN There is only one thing. Where would the chart be?

GAUSDEN They are up in the Pile Group.

CHAIRMAN Have them sent down. We will proceed with another question. I am not sure whether you or Mr. Bell or somebody else was going to look at the previous releases on 2053.

GAUSDEN I was asked to do this. I have 4 graphs here with me.

CHAIRMAN You have got two copies?

GAUSDEN Only the original at the moment. This is the March, 1955 release plotted at both 4 and 6 feet and you can see that there were two releases which went quite normally and died away slowly.

CHAIRMAN What is the maximum temperature, roughly?

GAUSDEN 240°. The next one is November 1955 and there is no indication there of a release. The next one is February 1956 and there are indications of a release at this point here. One which went quite normally and one which came up, fluctuated, and went up slightly and dropped away normally again. It is the 6 feet one which is the lower one. The maximum temperature is 295°.

CHAIRMAN That is the top peak?

GAUSDEN The 6 foot one. The initial peak 220, final peak just over 240, 244. Then there was a further attempt in April 1956 and no release took place.

11.20 a.m. Monday, 21st

CHAIRMAN Is there anything in November 1956?

GAUSDEN November 1956 you have already got.

CHAIRMAN Looking back on this, it looks as if there was a pretty reasonable release in November 1956; do we know the pile power from then until now, February 1956 to April 1956, November

GAUSDEN Are you talking of the cumulative MW days?

KAY 20 months.

GAUSDEN 41,000 up to November 1956.

CHAIRMAN From when?

GAUSDEN From February 1956 to November 1956.

CHAIRMAN And from November 1956 until the last release another 41,000 MW days.

GAUSDEN Yes.

DIAMOND There is a total of 81,000, is there?

GAUSDEN 82,000 in fact.

CHAIRMAN 2 x 41s. Can we proceed to another question, please. This is just a factual point, Mr. Gausden. We are interested in knowing where the BF3 counters are, that are giving the power in the pile. There is an instrument in the control room which says the pile is now developing so much, and there must be cables back to some BF3 counters somewhere. Can you make us a sketch of the pile showing where these instruments are?

GAUSDEN I will draw first of all the top of the Pile with the scanning gear up in that position there, and the shut off rods in groups in that position there, with this being the centre line. There are then two rows of holes, B holes, and on the centre line, C holes. There are 9 B holes from the position approximating from there to there. The BF3 proportional counter is down this one which is called B1, and it is down to a depth of 15' from the top of the concrete roof.

CHAIRMAN Yes, is that the only one?

GAUSDEN That is the only one. That is the only BF3 counter; for BF3 ion chambers, installed instrumentation, you don't need

KAY How do you normally measure the average power on the pile? With an ion chamber? The BF3 counter is a special instrument for special measurements?

GAUSDEN Yes. BF3 ion chambers are ... there is a total of 6 on the roof, 4 of which supply the shut down amplifiers, one of which appears to the main power level recorder and one to a power drift recorder. That is the main power level recorder.

DIAMOND Which one?

GAUSDEN This one here.

DIAMOND Have these got numbers, Mr. Gausden?

GAUSDEN They are numbered just for convenience, 1,2,3,4,5,6.

CHAIRMAN Am I right therefore in saying that the instruments in the control room which produced that chart, those red lines which we discussed yesterday, is depending on whatever is happening in the thing you have now marked 1, and that is at the front, and above the actual pile?

GAUSDEN Just to the back - 2'6" back from the centre line.

DIAMOND Is this the one that is used in Wigner release?

GAUSDEN Yes.

DIAMOND Is that the only one used during a Wigner release?

KAY In fact you never connect any other ion chamber as a permanent connection to the main power meter.

DIAMOND In the other positions are there ion chambers installed?

GAUSDEN Yes.

DIAMOND When are they used?

GAUSDEN This one, plus these 3, go to shut down amplifiers and that one goes to the power level drift amplifier.

DIAMOND No. 3 is the power drift amplifier, and these are used only during running.

GAUSDEN Normal operating conditions.

DIAMOND How far down is the No. 1 ion chamber from the top of the graphite and the concrete roof?

GAUSDEN I think if I draw this pictorially - if you have a concrete roof there with your graphite starting at this position here you have a number of steps, approximating to a circle, the ion chambers sits very nearly on the second step, not quite touching. It comes up through this way.

CHAIRMAN How do you calibrate that?

GAUSDEN It is put into a position which approximates to the power and then a heat balance is done to confirm the actual reading from the ion chamber so there is a correction factor applied to that reading of the order of .95 or a few percent variation.

CHAIRMAN On the normal operating flux?

GAUSDEN Yes.

DIAMOND It is done by a comparison with the power output as measured by air flow and temperature recordings during normal operation.

GAUSDEN Yes.

DIAMOND This calibration, has it been found to vary with the history of the Pile?

GAUSDEN It varies slightly with changes of isotope loading and with the length of time that any particular zone of metal has been in the reactor, but again very marginally, say .95 to .96.

DIAMOND So it is about 1% variation.

GAUSDEN Yes, we do not get much more than that.

CHAIRMAN Could you turn back now please to the temperatures of the air which was recorded. Before we look at the records, we would like to know just where these instruments are that are measuring these temperatures. I think you said they were somewhere near the ?

GAUSDEN The recorders were in the control room but the measuring heads, the thermocouples themselves, are, in fact, on the static horns in the outlet air duct. I would explain there are two systems for detecting burst cartridges, one is the moving of the scanning gear, the second a series of 9 thermocouple holes.

CHAIRMAN On Fig. 1, would you point out exactly where they are?

GAUSDEN They are approximately in a position here and a position there.

CHAIRMAN How far off the ground?

GAUSDEN There are a series of these on each static horn and 9 levels of static horns and there are points on each of these, but only 1 on either side is fed through into the control room.

CHAIRMAN Do you know which one that is?

GAUSDEN On the west side that is what is called point 16 about half way up; on the other side, was point 3, which is fairly near the bottom.

CHAIRMAN We would like you to hold up these records.

DIAMOND Is it possible to get figures of dimensions for these. Very roughly in feet from the bottom.

GAUSDEN Point 3 would be approximately 5' up from the outlet air duct floor and point 16 would be of the order of 30'.

DIAMOND And the total height, Mr. Gausden, there?

GAUSDEN Somewhere between 50' and 60', without being too precise.

CHAIRMAN We ourselves have a chart which shows this thing and it is drawn very well, but we are interested in such small temperature rises and we would like to look at the original chart, so we know the general characteristics of the thing. The thing that puzzles or intrigues us is that during the period of the second nuclear heating the chart which we have got shows no rise at all; we are wondering about this.

GAUSDEN It is perfectly true: there's nothing on here either. Could I go back to one point just before that. I believe Prof. Kay was interested in this original rise here: I have ascertained what caused this. Up to that period, or in fact, up to this period here, the dampers were on automatic control. A request was made at that time to lock all dampers off and put on hand control. One damper was found to have crept slightly

open; and this accounts for the small increase here because there was a small air flow going through at that time.

KAY And the chimney then became affected in pulling through a small flow of air which reflected the heating during the first nuclear heating period.

GAUSDEN That is so.

KAY From then on, all the inlet dampers were cramped closed. Could I ask where these dampers are located and their nature?

GAUSDEN They are between. You have the blower motors and ducting which runs from them into the combined ducting. They are in their own section of ducting before they get into the ...

KAY On the shut down fans section of the ducting.

GAUSDEN Shut down and main blowers.

KAY ... and they are on the individual bits of ducting before you get into the combined. They are simply flat dampers?

GAUSDEN Yes, a shutter type.

KAY When it is closed, is the inlet side of the Pile effectively sealed off? We are looking for little cracks.

GAUSDEN There may be very minute cracks.

KAY They wouldn't be a square foot or so?

GAUSDEN Oh no, certainly not. A fraction of an inch, if anything, if it wasn't bedding down properly.

DIAMOND Is there any record at all of the amount of air flowing through the Pile and up the stack when everything is closed?

GAUSDEN I couldn't give an estimate of that.

KAY But it is sealed off as far as it is practically possible on the inlet side, so one would expect the flow to be very small indeed.

GAUSDEN Yes.

DIAMOND Is it possible to give us any idea of how much that damper on automatic control crept open?

GAUSDEN I have been trying all night, in fact, to contact the man who went round and did the closing, but I can only get second hand information. I am told that one of the main damper valves was open about $\frac{1}{4}$ ". This would represent possibly a gap of well a failure to seal of probably the same amount: $\frac{1}{4}$ " from full close.

DIAMOND How high is it?

GAUSDEN Total damper is about 4' square. It is in sections: there are several sections which open and close.

(Drawing on board) Taking a section of the duct in this way, you divide this up into four sections and these are opening in this fashion.

DIAMOND So that it is eight, 4' gaps $\frac{1}{2}$ " wide?

GAUSDEN Of that order – yes. You cannot see inside these gaps because there is a closed ducting, so one is guessing to a certain extent.

CHAIRMAN We would like you to leave that record, would you please do that?

GAUSDEN Certainly.

CHAIRMAN Did we ask Mrs. Gausden to look at previous re-heats?

GAUSDEN You did mention it and I have here a series of graphs showing the February '56 release when there was a very considerable re-heat.

CHAIRMAN This shows what?

GAUSDEN This is the same sort of chart which you have had for the release at the moment and the last release. This is a similar set of curves again.

CHAIRMAN If you will let me have those, we will look at them.

KAY Outlet air temperatures?

GAUSDEN No, graphite and uranium temperatures.

CHAIRMAN I forget whether I asked you to do anything else.

GAUSDEN This is all I had to do.

KAY Were we going to ask Mr. Gausden about the nature of the recording on the Pile stack?

CHAIRMAN There is a point; if you can't answer, perhaps you can refer us to someone. I would like you to look at this. Time is going that way and this shows the pile stack activity from counters which are near the filters, and you remember in the logs it said that the pile stack was showing activity at around 5.30 which is here. This is the event we are talking about. Now to me, but I have no experience of these things, that is a very odd record. There is activity being shown by these meters; there is no doubt about that, and it is tens of curies in that order. Now if that is particulate activity and it sticks on the filters, there is nothing in the pile that has been made by fission products for many hours, but there are various isotopes. What puzzles me is, why did it decay, why does the meter show a fall?

GAUSDEN It has puzzled me as well.

CHAIRMAN There are many conceivable explanations, none of which seem very likely. If it is particles lodging in the filters possibly they went through at that time.

GAUSDEN I do not think that is a very feasible explanation.

CHAIRMAN We do not think it could be gas, radioactive gas diffusing through the filter, because that would go through the filter in a pulse which started high and decreased

with time. It seems to be an awful lot of curies if it is that sort of decay.

GAUSDEN This shows not quite a typical decay curve for fresh fission products. It is not sharp enough in my own opinion.

CHAIRMAN There are no fresh fission products. The last nuclear event was during the second heating and that was mid-day on Tuesday; in other words, it was 16 hours or so before this.

DAVEY Could Mr. Gausden indicate the position of the instruments relative to the filters in round terms? Could you relate the air flow in the reactor to that curve? By air flow, I mean opening and closing the dampers plus etc. ...

GAUSDEN There were certainly dampers open at the time this rise took place.

(Drawing on board) That is roughly the sort of system. There are three of these ion chambers. The filters are in a mass above here, the ion chamber comes through the concrete wall right through to the "all-clear" sheeting (?) and not through the " ? " sheeting.

CHAIRMAN And there are three such?

GAUSDEN There are three of these. If I draw a plan, with the lift in this position here, there is one there, one there and one there.

CHAIRMAN Now that we have got the positions, Prof. Diamond was asking the question.

DIAMOND The air dampers opening and closing, I think the activity began to rise at 0420; there was a slight indication of a rise at 0420, and that was going on ...

GAUSDEN Yes.

DIAMOND ... at the time the dampers were shut. The dampers were opened at 0810 for 30 minutes and that is about $\frac{1}{2}$ an hour before the peak. It is about half an hour, I think.

GAUSDEN The actual peak is indicated at 6 o'clock.

DIAMOND So that the peak occurs one hour after the dampers had been opened for half an hour, and went on rising when the damper was shut. Have you any comment on that?

GAUSDEN No, I have no explanation for that.

DIAMOND These dampers were never on automatic?

GAUSDEN No.

DIAMOND Manual control all the time?

GAUSDEN Manual control or locked off.

DIAMOND And yet when the damper was shut, this thing was completed unaffected according to that record – this activity rise?

GAUSDEN It certainly indicates that here.

CHAIRMAN I find this part rather mysterious, but the drop in activity I find even more extraordinary. This really does mystify me. What about the meteorological conditions at the time? Have we a meteorological station here? Can we have from the meteorological station a statement about what was happening? It might be that heavy rain was washing the stuff out of the filters down back into the stack.

DAVEY If I may, I would like to pursue this question again because we are clear that these ion chambers are looking at the filters and what I wonder is, that if activity is carried to the filters on the air stream, could we know in a little more detail when there was a perceptible flow of air and when there wasn't, because if air for a period carried activity to the filters and the air then went off, one could expect this ion chamber to begin to show the decay on the filters.

KAY I would think that if there were many particles being carried in the air, remembering that when the dampers are closed the pile is effectively sealed, in which case there will be no flow (**DAVEY** There always is some) up the chimney. But if you open the dampers you start some increase of air flow. When you close them, that will slow down and you may get some reverse cold air in the chimney. If particles are carried up, as it were floating in the air, when you shut the dampers there would be some movement of particles from the top of the Only those which actually occurred at the surface of the filter would actually stay there. It is possible that you would get some falling off due to this partially reversed flow of air.

CHAIRMAN Is there such a thing as reverse flow of air?

KAY I think you will get partial flow of air reverse not uniform over the whole thing.

CHAIRMAN Well, those are the records; the question is whether we can understand them. I do not understand them.

May I go back to the point how can we get some meteorological information? What we would like is a brief statement about the weather for the whole of Monday, Tuesday, Wednesday and Thursday, but we are particularly interested, I think, in the period of Tuesday, Wednesday and Thursday. The sort of things with reference to rain.

KAY We require inversion as well.

CHAIRMAN Well, we will see what they give us. It is fair comment from your point of view that this activity record is puzzling you.

GAUSDEN I cannot give any explanation which would explain this, at the moment.

CHAIRMAN The log did show activity on the Met. Office roof.

KAY During the next day - 1 o'clock.

GAUSDEN 6 hours later, I think the samples taken and reported are after that.

SCHONLAND Is this peak unusual?

GAUSDEN This 6 o'clock one was undoubtedly.

We have seen under completely different conditions, we have seen an increase of this type, for instance, when we are running at full power, and you change a blower over - one which has been standing static for some time. The explanation for this is that this has collected a bit of dust. This blows through the Pile and gets up on to the filters and accounts for this increase.

It does decay, but in this sort of fashion, perhaps not as quickly as this, but it does come back down again.

KAY Have you ever had a bad burst with the pile shut down?

GAUSDEN I have never known it.

KAY We are not comparing like with like, are we? All our previous experience of the response of this instrument is with the pile running, and there was no record whatever of what happened if you have a burst while there is no air present in the pile.

DIAMOND Can I just expand on that? You have never seen a burst of this magnitude, a stack activity increase of the magnitude with the pile shut down irrespective of shape.

CHAIRMAN What's the half life of the xenon?

? Nine hours.

GAUSDEN We think it has all gone after about 60 hours.

12.10 Monday, 21st

DIAMOND Asked if there had ever been a bad burst with the Pile shut down.

GAUSDEN Replied that he had never known this.

CHAIRMAN Remarked that they were not therefore comparing like with like and Mr. Gausden said that our previous experience was that with the Pile running, there was no release whatever.

SCHONLAND Asked if they were to understand that there had never been a burst of this magnitude and Mr. Gausden said there had not.

CHAIRMAN Asked, what is the half life of xenon? Is the half life of xenon 135 just over nine hours?

GAUSDEN Said it was.

CHAIRMAN Mr. Davey had just produced wind speeds and rain records. There were very light winds during the whole period and only traces of rain. On the 8th there was nil and on the 9th there was a trace. We would like a copy of this tabled for the record.

DIAMOND So burst of this activity has never been seen of this magnitude before during shut down conditions, but has this magnitude been seen under running conditions?

GAUSDEN No, normal running conditions are of this order or slightly higher.

KAY You mean a bad burst would.

GAUSDEN No.

KAY Just normal running conditions with just removal of dust?

GAUSDEN Yes.

DIAMOND A bad burst is higher, Mr. Gausden?

GAUSDEN Normally not, because normally we picked it up on the scanner gear and are reducing power before it affects the stack filters.

KAY Is there any other measurement of activity, apart from the scanner gear which you are using under normal operation and the stack meter. I thought there was some measurement in the air duct itself?

GAUSDEN There's an argon 41 measurement taken.

DIAMOND Is this separate from these meters?

GAUSDEN Yes, this is taken at Pile roof level approximately. A proportion of the air going up the stack is drawn through into a settling tank arrangement with an ion chamber sitting in it, and that air then by-passed back to the stack again.

DIAMOND Does this show a reading in shut down conditions?

GAUSDEN Normally no, but under present conditions we are completely masked as far as I can see, because the filters become highly contaminated and it is just on full scale at the moment. The air you see is filtered on its way back and this was the air flow coming from the stack, through the stack filter and out again.

DIAMOND Does this show a reading on the two nuclear heating periods?

GAUSDEN I could not be categorical about this, but I think the answer is no.

DIAMOND Because there is no air flow?

GAUSDEN Yes.

SCHONLAND In the outlet temperature record there is a rise.

GAUSDEN This is the gradual rise you are referring to?

SCHONLAND On Wednesday, 9th at 08.00 hours you start the rise which culminated in trouble. Would it be worth looking at the argon meter to see whether that was showing anything on or about that period? There is movement of air now.

SCHONLAND Actually 07.00 hours on Wednesday, 9th the temperature first shows a rise. It is possible that the argon meter would show something around that period. I suppose that is not a very good question.

KAY There is too big a flow at that period; this could be a gradual warming up of the air due to

SCHONLAND Just a shot in the dark.

Chairman asked Mr. Gausden to explore this matter.

CHAIRMAN Any further questions?

SCHONLAND Is there any measuring instrument of pressure at the base of the stack?

GAUSDEN No.

KAY I wondered whether, from Pile 2, we could get any idea of the uplift of air in the condition when the dampers are shut.

GAUSDEN There is nothing installed. Really you want to get into the chimney itself, taking one duct is not representative.

DIAMOND There is some movement. I believe you said the other day that you are taping up the control rod holes on No. 1.

GAUSDEN Yes, there is a certain small flow through a number of cracks like that. We have not any ready made holes that we can do this.

DIAMOND There is no hole you can put a manometer in?

GAUSDEN The only one I can think of is by disconnecting the outlet from the argon activity and see if we can get through the stack that way.

DIAMOND This is some way up?

WITNESS: MR. HALL

GAUSDEN Sixty feet up. I do not think it would really tell us very much.

CHAIRMAN We asked you a number of questions and you have been working on them and you have been good enough to put down the answers on various pieces of paper which I have here, and I would like you to take us through them one at a time.

DIAMOND Well, perhaps we won't worry about that.

GAUSDEN Referring to one other thing. It did occur to me to look at a previous release and I found this quite interesting. This is a 1960 graphite thermocouple at 10' on Pile 1 during the November release, where you get this. You already have the graphs but I just plotted this out singly, where you get the release occurring here, and you do get a slow steady rise over quite a period with dampers opening and closing before it falls away. It went in fact to just under 420.

HALL The first sheet gives the heat release from the isotope cartridges under shut-down conditions. These figures correct the earlier ones. The one which is missing at the moment is Thulium, the difficulty here has been in getting information in quantity in the can. That is going ahead at the moment. I think it is likely to be the highest one, a guess is perhaps of 50 watts. Section (b) gives the temperature difference between the isotope can and the surrounding graphite as a function of the heat release.

KAY It was just under 420.

DIAMOND What was the peak temperature for the November 1956 release?

CHAIRMAN Just to make quite sure, does that mean that 1 watt gives $\frac{1}{2}$ a degree; if Thulium were 50W it would be 25 degrees hotter?

SCHONLAND There are several at 10 ft. showing that characteristic November 1956.

HALL I think it's likely to be fairly closely linear. The heat transfer coefficient may increase slightly, as the temperature difference increases, in which case that may be a slight overestimate.

GAUSDEN Yes, this was taken on the top one.

SCHONLAND Yes. I did analyse them. What is the number of that one?

DIAMOND Is this the average can temperature?

GAUSDEN 1960 at 10 ft., with a 418° peak.

HALL I haven't distinguished between the two. I think there will be very little difference in all these cases. It is such a small heat release that the Aluminium can effectively shorts the material inside, and there is virtually no variation.

DAVEY Mr. Chairman, I have not been here all the time. May I ask this question? Over a period of time, how have you received your guidance and advice on the maximum temperature to which to take a Wigner release?

DIAMOND But if we just go on a bit and come back to this one - if we take the A.M. can, the maximum temperature might be a matter of 3 over 2 times the average temperature.

GAUSDEN Over my rather short period. I inherited the instruction that you already have.

HALL This depends on the fin efficiency of a can which also depends on the heat transfer coefficient, which is, under these conditions, very much lower and the efficiency is virtually 1.

CHAIRMAN We have a copy of the instruction, which tells us what is to be done at various temperatures.

CHAIRMAN All right, Mr. Hall.

DAVEY Do you work to that?

HALL The second sheet gives the corresponding heat release in the fuel elements after various periods after shut-down, and the second section again gives the temperature difference between the fuel rod and the surrounding channel. We have measured this experimentally in the laboratory during the last two days, and the measurements quoted were done at a rather low temperature, but I do not believe this will be affected very much either by temperature or temperature difference in the range in which we are interested, but the experiments are continuing. This means that for 10 watts you would have a difference of something 12½ degrees.

GAUSDEN Yes.

DIAMOND Have we a copy of this instruction?

12.30, Monday, 21st

CHAIRMAN We have a copy, issued by a Mr. Fair, addressed to Mr. Gausden.

SCHONLAND Can I supplement that question, is that the only operating instruction - there is no handbook?

GAUSDEN No. Right back from Mr. Phillips' time, it was considered that a Wigner release was not a repeatable experiment. So general broad guiding principles were laid down and that was really considered as far as it could go, as I understand.

HALL Section III is the operating conditions of the A.M. rods. The stages in this calculation are firstly to determine the flow along the channel. This is complicated by the fact that the rod is not uniform. There are larger diameter rods in that end, in fact it turns out that this has virtually no effect on the flow in the rest of the channel, there is sufficient bypass of air

CHAIRMAN Well, if you would be good enough to look up that point on the argon, Mr. Gausden. There is no urgency about that, but no doubt you will do it quickly, and we will ask you to report again in due course.

through if the Wigner gaps into the isotope channels to maintain the same pressure gradient in it as in the fuel channels. The coolant temperature is then computed by taking the mass flow and adding heat from the A.M. rods, and also allowing for conduction through the graphite from the neighbouring fuel channels. The doubtful point here is whether the cross flow which exists in that end of the channel contributes very much by leading hot air from the fuel channels up into the isotope channels. I think the criterion here is the similarity of the temperature distributions in the isotope and fuel channels, and you will see on Fig. 1 that in the region where the flow is taking place from the fuel channel to the isotope channel, just after the first three graphite rods, there is little difference between the two coolant temperatures, so that even if the whole of the air was supplied via the fuel channels, this would be only a 5° difference. This then gives the coolant temperature along the isotope channels. We then computed the average can temperature, the average around the circumference of the can on a normal heat transfer basis, and then we have a separate calculation of the variation around the can. Fig. 1 shows firstly the coolant temperatures, and the coolant in the neighbouring fuel channels, then the average around the circumference, and this allowance for the fact that the rod is in contact with the can along the line.

Section IV. This is the main calculation we have been engaged on. This gives the effect of placing a line source of heat in the graphite core, and we have tackled it by taking a line source of heat, the strength of which has been determined by assuming the A.M. rods burn in 20 minutes. From their heat of combustion you then have a rate of heat input.

We have taken the solution for a uniform infinite line source embedded in a conducting medium, and in order to allow for the anisotropic conductivity, we have stretched the solution in one direction, so that the iso-therms will be elliptical cylinders with their axis running through the core, the major axis of the ellipse being in a vertical direction. In computing the transverse, i.e. the horizontal, conductivity we have allowed for the gaps between the blocks and we have also allowed for conduction through the dowels which connect the rods together at their ends. In the vertical direction we have not taken any account of the interface resistance between the blocks because we felt that even a gap of $\frac{1}{4}$ mm would not be a serious resistance and we felt that it was inconceivable that there could be a gap as big as that. We have allowed for the fact that the block is cut away at the sides where the fuel channels pass through it. Well, this model we have taken then gives a series of elliptical iso-therms, and on each ellipse the temperature versus time curve is as shown. The size of the ellipse is specified by parameter Y which is half the major axis of the ellipse, so that these curves would apply to all points on that particular ellipse, and we have a correction curve for giving the position of all the other points. The 4 curves give the temperature-time relationship for various distances from the source. I would rather like to go on to the next section before discussing these in detail.

This solution is not valid close to the source partly because we have assumed a line source and in fact it isn't a line, and partly because close to the hole the conductivity will be isotropic within the block so the other calculation we have done is to take a source of the same strength distributed over the surface

of the channel, and we have taken the two dimensional, no sorry, the cylindrically symmetrical case, with isotropic conductivity and we believe this solution to be valid within the time stated, that is roughly the time taken for the temperature wave to reach the surface block, so for that limitation the solution ... it does not know whether it is an intimate one or not ? block. If it did reach the ? of the block there would be a further resistance to heat flow and these temperatures would be a slight underestimate. I think this one is particularly interesting because of the quite rapid rise of the channel surface temperature. This is plotted in the curve against time and we feel that it is necessary then to consider the contribution of oxidation to the heat source. We have not done this calculation yet but just to give you an idea of its importance, if one takes an annulus around the channel of 1 cm thick, and assumes that a graphite is at a temperature of 650 in that annulus, then this would contribute a heat source equal to the burning of the A.M. cartridges, so after that point it becomes rather more fire (?) than the original one.

I have not had time to look through the previous curves in as much detail as I would like, but I feel that the spreading of the fire could be explained rather more easily on this basis than on the ? rediffusion of heat. I think there are two other calculations which we could perform which might be useful.

Firstly, to have a look at this combination of graphite oxidation triggered off by some other means. I have not formulated this very closely yet, but it might be possible to say how much graphite, at what temperature, one would need to make the system unstable so that it propagates itself. The other one which I think we might look at, is to take a channel of graphite, assume various airflows through it, and find a channel temperature above which the heating of the air by oxidation of graphite becomes cumulative and the thing becomes unstable.

DIAMOND This last one is a very good one. In this last curve, you mention that this gives an underestimate of temperatures. If any appreciable quantity of heat has reached the boundary of the block of graphite, because it's got to jump the Wigner gap, what sort of time was involved, does it depend on the time of burning of the cartridge?

HALL Yes. The times quoted here are for lower conductivity, 500 seconds, and for the higher conductivity, 250 seconds, which are both small compared with the time of burning we assumed. So I have quite a lot of faith in this calculation during that period.

DIAMOND So we can cut these curves off there.

HALL I think if there is no air flowing through the Wigner gaps, then these give pessimistic results. They will go up rather faster than that. If there is any considerable convection on the surface of the block, they may fall below that.

KAY You took 20 minutes as an hypothetical period for the complete combustion of the line of cartridges and that gave you a heat rating of 7/8 kW, and that appears on your curves. We go through a peak and then you die away. In addition you plot a continuous source and the two curves coincide to begin with. Does that mean that if you continued at that rate of heat release indefinitely, that in point of fact you would have

burned up all the So does this represent any particular ?

HALL No, we computed the continuous heat and cancelled it.

KAY That represents a part, a step, of your calculation.

HALL It might be useful if you were interested in a longer rate of combustion, one could then scale down.

KAY But your 20 minutes does imply the complete burning of everything there is to be burned.

HALL But this 20 minutes, the basis of this is not really very sound. We have burned such a rod end inside a silica tube channel and found it was necessary to blow air into this small furnace to keep it going, but it burnt fairly strongly.

KAY If you are taking a different period the upper curve would then be different as well ? it would scale down.

HALL Yes.

DIAMOND The Y's on this curve represent a point on the ellipse in a vertical direction.

HALL They define the ellipse. If you look at the first one, the Y is half the major axis and these curves apply to all points on the ellipse.

DIAMOND So this Y, the top one is 10 cm up and something less horizontally.

HALL On these two, 10 and 15, it is doubtful whether these curves can really be applied as close to the surface, but I think the other solution then takes over.

(This next part was too indistinct).

DIAMOND In calculating the horizontal conductivity, what assumptions did you make?

HALL Simply conduction across the gaps and conduction through still air across the gaps.

DIAMOND You added the resistances. The reciprocal conductivities and the lengths.

HALL We made an allowance for the resistance of the graphite block, the air gap in series and then in parallel out through ? .

DIAMOND You have just completely left out any question of natural convection in the gaps. If we did have natural convection in the gaps, it would presumably, at these low speeds, have the effect of diminishing thermal conductivity.

HALL I would have thought that the natural convection would be within a cell formed by the Wigner gaps in that they are cut off by the dowels at regular intervals and I would have thought you would have an enhanced conductive ? by circulation within the cell formed by the Wigner gaps and you would perhaps have increased the thermal conductivity.

Chairman congratulates Mr. Hall on his work - "Splendid job".

CHAIRMAN (after congratulating) We would like you to go to the two other problems you have suggested, especially the one about the propagation via graphite burning: the air along the channel that carries the fire further down.

HALL I think that is the easier of the two.

DIAMOND I gather you have tried burning an A.M. cartridge in a silica tube. What sort of products of combustion came off?

HALL It appeared to be just like a magnesium fire so long as we blew air into the mouth of the furnace. If we turned the air off, it just died down into a dull red glow inside the furnace and one could control the combustion.

DIAMOND Did the products of combustion come off as far as one could see?

HALL There was certainly smoke as one gets with a magnesium fire, magnesium oxide. That certainly took place but there were no dense clouds of smoke.

DIAMOND What temperature did it go off at?

HALL This particular one went off at about 600°C, it was canned but in a perforated can, about a 30 thou hole. I think this ? .

DIAMOND It went off without any difficulty at that temperature?

HALL The first thing we noticed was a slight bluish glow looking down the furnace. I'm not sure what this was. It was possibly lithium just - off and burning in a vapour phase but shortly after that the temperature went up to just over 600 and apparently a fairly small magnesium fire started and spread along.

DIAMOND You say the temperature went up to 600; you put it up to 600?

HALL We took it up to 600 when the blue glow appeared. This seemed to hold it at about 600 until the glow disappeared which made me wonder whether it was

DIAMOND You switched off the heat?

HALL No, we were still supplying heat to the furnace but the temperature of the rod remained at about 600 and then the blue glow disappeared, the thing went up rapidly then and soon caught fire.

DIAMOND The electrical heating was then switched off and you maintained the combustion by supplied air.

HALL Yes, this could be controlled.

CHAIRMAN Well, thank you very much. Would you please carry on with these two questions?

2.15 p.m., Monday, 21st

CHAIRMAN Mr. Cutts, before we start we should have pointed out, please don't pay any attention to all this apparatus. It is just to get our records straight. We do want as

correct a record as we can as we want to study it. You were going to do a little sum for us.

CUTTS In the short time all I could really do was to examine a reactor where the control rods assumed to exert a uniform influence over one half of it. This seems to be quite reasonable as a first try, and, assuming that the control rods are exerting an influence over the rear half of the reactor, that is, the discharge side, I compared two flux distributions that one would get and here they are. This is the charge face, this the discharge face, and this the axial direction. Now this faint pencil line curve is the cosine distribution one would get. It is a straightforward perfect cosine curve, and this inked curve is the kind of thing you would get if the reactors were infinite and of course in dealing with the flattened zone of the reactor ostensibly it is infinite in that there is no radial leakage presumed when the reactor is operating, and as you can see, the peak is pushed quite a distance further in this direction, about 1 ft., but the interesting point to note, I think, is that if the temperature measurements were taking place at 16 ft. from the charge face, then you would be measuring a rate of changes or change of temperature due to heat being produced at this rate, where if you have no correlation curve available and you assumed therefore that the distribution was of this type, then at a point here, you would be a factor of 2, not quite 2, 1.83, at that peak position below what you really get. A little further over this way I have not been able to calculate such ratios for every point, but at some 6' or 7' away from the charge face the ratios would be as high as 2.06. Going back to the assumptions: the rear portion of the reactor under the circumstances we were envisaging would not be uniform and the control would essentially be exercised roughly there. In fact that is roughly the line at which the rear bank of control rods would act, this would in fact tend to mean that this curve would be of a similar shape, would continue a bit further, and then you would get a greater hollowing out. It is not really possible to do much more with that: what I am trying to say is, that this type of curve would underestimate the effect we were looking for, rather than overestimate it. I have spoken to the man in R. and D. who carries out this analogue type of experiment and they could in fact do an experiment with that bank of control rods right through the pile, the rear bank. The experiment would take between a week and a fortnight to set up and do it, but they would get the flux distribution, in fact they would get the equivalent of this. They could in fact investigate the complete spatial effect, except that for one thing, they could not introduce the idea of a flattened zone into the reactor; they could only treat an essentially uniform core and investigate the flux distribution you would get with an asymmetric control system.

CHAIRMAN Any questions?

DIAMOND The ion chamber which measures the power level during Wigner release, do you know where it is located?

CUTTS The ion chambers are located in the concrete, not in the reactor proper, so I was told by the R. and D. Branch. Is this incorrect?

DIAMOND There is a sketch of it on the board. The second step is the ion chamber which measures power level during a Wigner release. This is in position 2'6" behind the centre line.

KAY We understood that it was on the second step of the graphite.

CUTTS That would be in this kind of position here, somewhere in that plane essentially, and the power that such an ion chamber would measure would be in some sense proportional to this flux, though that is only in the first instance. It would also be proportional to the gradient you would get in this plane in the radial direction. So that you have two things to consider; one is the magnitude of the shape of the curve and also whether this gradient bears the same relation to the peak in the two kinds of flux distribution we are considering. Because of the fact that the power is lower in fact by some 30% you would read low, as compared with the peak by some 30%.

DIAMOND Do I understand then, Mr. Cutts, these two effects are additive.

CUTTS The effect of the gradient being of this kind rather ...

DIAMOND No, I meant the connection between the maximum uranium temperature and the measurement of power level.

CUTTS They would only be additive if these were combined in any way in your estimation of what was going on in the reactor, but if they were combined, then any effect on them could be additive, I suppose. But I don't think you would necessarily combine these. You would rely on your temperature as the estimation of power, or the ion chamber.

KAY What Mr. Cutts just said is, if you raise the temperature 16' back from the place, the heat increase may easily be twice as much. The other thing he said, quite distinct, is that if the ion chamber is located 2'6" behind the centre line, it will give you a reading of pile power which will be low by about 20%.

CUTTS It looks as though it would be low.

SCHONLAND But that in fact has no relation to the previous statement.

CHAIRMAN Dr. Schonland, any questions? Prof. Kay?

KAY No, I think that it is quite clear.

DIAMOND May I confirm that this is a little of the back row of the control rods in, and front row out, and assuming that it is just in equilibrium at that position.

CUTTS Yes.

DIAMOND Mr. Cutts has asked us if we would like analogue system set up.

CHAIRMAN I would like to consider that later and pass a word to R. and D. Branch.

CUTTS I think through Dr. Mackie or through Dr. Mossop who was in fact in charge of the experiments.

CHAIRMAN Yes, we will not make our minds up about that at the moment. Thank you, Mr. Cutts. I do not think we shall want any more at the moment.

DIAMOND I wonder whether we might get an exact position of the control rods. Have you got it?

CUTTS Yes.

DIAMOND How much in and how much out?

CUTTS I have not got that.

KAY Would I be right in saying that to get further than your calculation means doing the thing in complete detail, taking a fortnight?

CUTTS I don't think so.

KAY There's not much more you could do quickly?

CUTTS No, to try to do the calculation would take a lot longer than the analogue.

DIAMOND The actual position, Mr. Cutts, is probably that some of the front row in fact are some way

DAVEY Mr. Toole told us yesterday that in order to bring the pile to the divergent point, he described how he was gradually withdrawing the coarse rods. You could have the degree of withdrawal from the plant records.

DIAMOND I asked a question as to whether there was a record of the position of the control rods against time, and I understood, possibly wrongly, that there wasn't.

ROSS Not a recorder graph growth, no, but there is a recorded figure.

DIAMOND Of the actual position against time?

ROSS I think Toole would have written this down.

DIAMOND Then I think we ought to have that, and I wonder if Mr. Cutts can tell us if we in fact looked at the model with the front rods partly in, how it would affect this model that you've drawn.

CUTTS I have thought about that and it seemed to me that if you look along the axis of the reactor where the front controls are withdrawn away from it, looking through the window left by, if you look along the centre line, looking through the space left by the withdrawal of these front control rods, we ought to see this picture of flux distribution and because it is a flattened reactor we ought to sensibly see this picture carried fairly constantly through that portion, but now because these control rods are, in addition to pulling down the flux in this region, because they are tending to pull it down radially too, you will tend to see a picture rather like this.

Here is the centre line (he demonstrated on the board) and we are at the moment looking along that line, and we ought to see a picture which is somewhat flattened across there and probably peaking up a little towards the edges then falling off like that. The normal flux distribution would, of course, be of that kind. This would tend to influence ion chambers in that they are controlled by the current of neutrons coming out, and the current is obviously proportional to the slope of this flux distribution when the reactor is operating normally, the current, for a given peak here, would be much greater than in

this shut down case where the control rods are depressing this flux distribution. I noticed from your saying that this ion chamber was a certain distance behind the centre line, that it appears to be pretty near the plane in which those rear control rods are operating, which would mean that the control rods ought to influence it quite a lot. They may shadow it. I was wondering if there was any experimental evidence of this ever having happened. The pile operating people should know.

KAY With reference to the ion chamber, do I understand correctly that the reading you get is dependent on the actual neutron flux and also on its gradient?

CUTTS Its gradient, yes. It is the neutron current that you measure.

KAY When you quoted the figure of 30%, were you referring to the flux or

CUTTS I was referring to the fact that this flux here would tend to be down below what you would expect it to be, by some 30%.

KAY So 30% is the absolute level of the flux, but on top of that you've got a variation because of the different gradient which is producing the vector flux.

CUTTS Even if the radial fall of the flux distribution were the same, you would expect to be down by 30%, but if the radial form is not the same, you may well be down by more. This sort of thing could lead to a factor of 2 again.

KAY So that would you not expect that measurements of power, It could be misleading by a factor of 2. It could be proved.

CUTTS I don't think you should put too much reliance on that sort of reading unless you'd actually calibrated the instrument with that flux distribution.

CHAIRMAN Well, the question was asked whether we should get Mr. Toole to see what records he had of the exact position of the control rods; but in the meantime, did you have anything for us, a diagram etc., if we had specifically stated that Mr. Cutts might make a better estimate? Well, I think Mr. Cutts is a better man to answer that.

KAY May we invite Mr. Cutts to question Mr. Tuohy and Mr. Gausden about the position of the control rods and gather any information from them?

CHAIRMAN Well, I think that I would rather have the evidence given to us, and then ask Mr. Cutts whether he can do anything with it. It might be beyond them. Could you hang on a little while, Mr. Cutts?

CUTTS Yes.

CHAIRMAN We will then proceed with other witnesses.

SCHONLAND I perhaps got a little lost, but I got the impression that whatever the control rods were doing, provided that they were making the pile go. This is my impression. I am not particularly clear here what we are chasing, and where the control rods were.

KAY We ought to confirm where the control rods were.

DIAMOND All the control rods were up at the front.

CHAIRMAN He has made this diversion. Now that may not be just what happened, so that this assumption fixes the reactivity of the front half.

DIAMOND It does in fact, the reactor heat changes. The portion of the pile is self-sustaining without the control rods in at all, and therefore we suppose the flux is in the region of the front control rods and that leads to a more complicated disposal.

KAY His main fact is on the radial distribution rather than the actual distribution.

DIAMOND Well, it is conceivable that they could move the front body up.

KAY I understand him to say that it would make little difference to the centre.

DIAMOND Well, it could do. You have measured the power output. Where is it coming from, as you are pushing the control rods? You limit the region to which it can come.

KAY Well, you are altering the first radial flux distribution.

DIAMOND I know you are.

CHAIRMAN May I interrupt for a moment, we have several of the Health Physics people available and I would like to call them. Mr. Gausden or whoever else can look up the records and have them available and get on with the work until we can call for him. We do not want Mr. Gausden just to sit there waiting to talk to him.

We will take Mr. Howells now. We will take Mr. Gausden before we leave tonight.

WITNESS: MR. HOWELLS

ROSS Mr. Howells is the Health Physicist here.

CHAIRMAN Mr. Howells, please sit down. Before we actually start, there are a few words I wish to say to you. Please smoke if you wish, and do you know the members of the Committee? (Introductions) Now for the record, will you begin by saying what is your job.

HOWELLS I am Manager of Health Physics and Safety.

CHAIRMAN Now, Mr. Howells, you are the first man we are beginning to question about Health Physics and Safety and, as you are in that particular field, we would like to ask you to give us a narrative account of what happened as far as you were concerned.

HOWELLS Shall I start Thursday morning?

CHAIRMAN Well, from the start, things looked a bit abnormal.

HOWELLS The first sign of abnormality I had was when we took an air sample down at this end. We normally run an air sample down there each day to get some idea of what the background level is and this was running on the Thursday.

10th, from 11 a.m. to 2 p.m. About 2.15 on that date. I was informed that the activity on this air sample was round about 3,000 disintegrations/min/m³. This is approximately about 10 times what we normally expected.

There was another aspect which later didn't prove to be very important, but we do carry out surveys on fixed areas for particulate. On that Thursday, we did a survey down there and found some deposition. This was at the Meteorological Station. As a result of these two pieces of information, I went to see Mr. Hughes, the Asst. Works Mgr., in the afternoon and reported these instances to him and asked him if he had any idea of the cause of this. He had heard from Gausden in the Pile Group at this time that they suspected a burst. Then about 2.30 p.m., I went with Hughes to the Pile Group. We went into the Control Room of No. 1 Pile and we looked at the stack activity meter there, and found that this was at full scale at that time. They were at the time making a change in the sensitivity of this. They reduced the sensitivity on the meter. It was about $\frac{3}{4}$ a scale reading when they made the change and within a few minutes, it had gone off full scale again.

Hughes and I then went up on to the Pile 1 roof and had a look at the temperatures as measured on the graphite there, as pointed out to us by Mr. Bell, and they then did a scan on the thermocouples across the piles and there was one metal temperature which seemed to be rising at this time.

2.40-3.10 p.m., Monday, 21st

HOWELLS We came back down to the control room and Mr. Hughes then rang through to the Works General Manager and reported the situation.

CHAIRMAN That is the events as you know them on the Thursday? We know from the pile stack meter record that the activity did jump up at 4.30 to five o'clock on the Thursday morning, that is a.m., but did you know anything about that?

HOWELLS We saw this on the chart at the time we went up.

CHAIRMAN That was after you had picked up these air samples in the early afternoon?

HOWELLS That is right. It was when I got up to the Control Room in the pile that I saw these records. Until I left here, I was not aware that there was any trouble at all in the Pile Group.

CHAIRMAN Having looked at that little ? on the curve from the pile stack meter as you saw it; if you had been shown that at the time, would you have said that there was any need to worry?

HOWELLS These aren't the sort of records that I normally see, but I would expect there to be some rise in activity if they switched on a blower or let any air into the pile.

CHAIRMAN This is not much worse than you would normally expect from a blower. You have taken us to the point where Mr. Hughes has told the Works General Manager. What happened as far as you are concerned after this?

HOWELLS Well, at the same time really, I informed my control station down at this end that we were suspecting trouble in the Pile Group. This would be about 2.30-3 o'clock. We started taking air samples right round the whole group at each of the buildings in the chemical plant (outside of building). This covers 10-15 points inside the works perimeter.

CHAIRMAN Yes.

HOWELLS We also sent out a survey van to start surveys in the district. One van was out at the time doing some work, so only one van was available.

CHAIRMAN Where did you send him?

HOWELLS We sent one van down towards Seascale along the track which leads along the coastline. This is where you can walk.

CHAIRMAN Why did you send him in that direction?

HOWELLS This was when we'd referred to the wind direction and it seemed to be blowing in that general direction.

CHAIRMAN You consulted the Met. people?

HOWELLS No. This was just a measurement of the wind taken outside.

CHAIRMAN Alright. Now you have got a lot of people taking air samples, one van out taking samples and another out somewhere else. Have you got radio contact?

HOWELLS No. The other van came back at 5 o'clock and was also sent out on survey.

CHAIRMAN Did you send him the same way?

HOWELLS No. He was sent up the road. We took an air sample taken near No. 1 Pile on the west ring road outside the fence.

CHAIRMAN Now having got your air samples you start measuring.

HOWELLS At the same time when the vans went out, we were primarily concerned with taking radiation measurements. They took radiation monitors with them and the highest reading was something like 4 millirads per hour. This was recorded at the Bailey bridge on Seascale cinder track road.

CHAIRMAN How many radiation monitoring readings were taken? Was this one of a dozen or one of hundreds or is it continuous?

HOWELLS He did a survey with a radiation monitoring along the track and reported back the highest reading taken. This was at 5.30 p.m. After that when the vans came back together, we organised then to do a run, one going to Calderbridge, across to Ponsonby, Gosforth and Seascale and the other one in the other direction - from Calderbridge to Beckermeth and back to the Works again. These vans are fitted with DPI ionisation chambers and they were told to take

readings on these. The readings we got when they came back were not particularly reliable due to contamination on the roofs of the vans. We did establish that the irradiation levels in the district were well below levels at which one might expect a district emergency. We were working on the levels specified by Marley and Fry, where one would expect an emergency level is round about 10^{-2} curies/m² for immediate evacuation within 12 hours, and 10^{-3} curies/m² for eventual evacuation. We were well below these figures.

CHAIRMAN That didn't mean to say that you didn't regard this as something to be taken most seriously. How many people were doing it? Anything like full staff?

HOWELLS Yes. We had everyone on this one. All monitors in group were taking air samples at the buildings; the two vans were in operation; and we set up control room at this end.

CHAIRMAN Where was control room? Was the control room in Health Physics?

HOWELLS With a problem of this kind there are three risks involved - radiation, inhalation and ingestion. We established quite quickly that there wasn't going to be a radiation risk in the district. You would not expect an instance of this nature to give rise to an inhalation risk. So eventually you come round to the main problem is one of ingestion and the only way to ascertain this risk is by biological monitoring that we started on the Friday morning.

CHAIRMAN Just so that I am clear. You have given us these levels for immediate evacuation and eventual evacuation. You have given them in curies/m². Can you give us the corresponding r/hours?

HOWELLS Urgent evacuation within 12 hours 10^{-1} curies/m² is equivalent to 2 r/hour at waist height. 10^{-2} c/m² where evacuation will be necessary, corresponds to 0.2 r/hour at waist height. 10^{-3} c/m² limit of temporary evacuation 0.02 r/hour. We knew that the levels we got were below the levels corresponding to the lower one there.

CHAIRMAN What levels did you find?

HOWELLS Highest level was from 4 milliR per hour.

CHAIRMAN Down in fact by a factor of five. Your highest reading was down by five on the figure given for temporary evacuation.

HOWELLS Yes. These levels refer to ground contamination. In all probability, the level we got at that time was associated with the plume passing overhead. At least it could be at that stage. I do not know if you want to go into the subsequent information we got on this.

CHAIRMAN I would like to take you through it if I may. You are obviously going to give us a great deal of information and many places and talk about surveys. I do not know how you want to give us that information. Could you put it in the form of a map or chart or something?

HOWELLS There is such a wealth of information available, if we try to put this on a map it just covers the map at the moment. You see radiation surveys: we've carried on taking

these readings all the time which are being logged. We could provide maps of what the position was last Monday and so on day by day.

CHAIRMAN I am really interested at the moment in the first day or so.

ROSS May I suggest that it might be useful to look at the control room. You have the original records there.

HOWELLS The survey sheets are there on the wall, but these are not presented in the form of maps for the period, say Friday and Saturday, because at that time, we were just taking spot readings and I was employing my vans primarily in the collection of biological samples. You see, on Friday morning it was more or less established that there was not going to be any radiation problem or inhalation problem, so we came back to the ingestion one, and we were relying largely there on collecting milk samples to ascertain the levels of contamination in milk. We collected a milk sample from the local farm on Friday morning. The analysis of that showed something like .003 mCuries/litre Iodine 131 in this – quite a low level. Then we collected samples from Seascale and the local milk on the Friday afternoon. These records came forward on Saturday morning and we ascertained that the Seascale sample for Friday afternoon was .38 mc per litre.

CHAIRMAN Could I just go back, please? Let me just try and explain. We want you to convince us you did all that was required so that we can pass on our view that it was done. Could we just look at these things? There are the three types of hazard – radiation, inhalation and ingestion. You have just told us that by Friday you had come to the conclusion that the ingestion was the thing to watch.

HOWELLS This is the thing you know before it started.

CHAIRMAN Let us therefore look at the evidence which proved to you that radiation was of no consequence. You have told us that you have got a maximum reading of .004 at the Bailey bridge. To be conclusive, to convince us that that's alright, you have to say 'We not only did it there but we covered a certain area'.

HOWELLS These are the places I mentioned earlier, that we went round taking readings all the time at these points. These readings were not all logged. The people in the van were told to report back immediately they found any high reading on the DPA chamber.

CHAIRMAN What I was asking was here is the pile, the Windscale Works. You covered a certain area. Now, could you not make us a little sketch of that area and say, by Friday 5.00 or whatever it was, 5.00 and something or whatever it was readings were taken inside that perimeter and that the maximum found anywhere was .004.

HOWELLS Yes. I could get this back from my log.

CHAIRMAN Then that is good evidence that you really did cover a big area.

HOWELLS We ran the vans continuously on this route during Thursday night. I make this point quite clear – in taking these readings we realised that they would be false for

they would be reading high, due to contamination on the vehicle, but we knew that the levels were below the levels that one would look for action. Now, in addition to that, since we were having difficulty in measuring radiation levels, I wanted to get something more positive, so they also took smear tests of one square foot areas at various points inside the district on this run. These were then brought back and counted in the laboratory here and from this we got some sort of estimate of what the depositing was on the ground.

CHAIRMAN What I am seeking is some way of summarising what you actually did on the radiation, just to show that you really did it. I have no doubt that you did it, I just want the evidence that you did it over a certain area. Could you make us a little sketch of that to submit to?

HOWELLS Yes, I can do that and show the area covered.

CHAIRMAN What we would like to do, if you would be willing, is to give you the opportunity to think it out, and draw it, and submit it again to us. That is as far as the radiation goes. Now, the next one is inhalation.

HOWELLS Well, there is a table of the air sampling we did carry out on that night which gives the levels that we measured inside the Works. These levels, while they are high, they are not startlingly high, and one would expect that the levels in the district would be considerably lower than these. So, having established that the air conditions inside the perimeter were reasonably good, one would expect the same thing in the district.

CHAIRMAN I would like to ask you a challenging question. The activity is coming out of a big chimney and the perimeter is round here. How do you know it's not coming to ground ...

HOWELLS You would again check this more sensitively by radiation measurement than air sampling, because air sampling is a particularly crude technique for measurement. So, while from a public relations point of view, one could say that air sampling is the thing that one should do, from the point of view of finding scientifically the situation, gamma measurement in the district is more sensitive than air monitoring.

CHAIRMAN What you are really saying is, if you got a high reading on the ground somewhere, that is the area where you might have done some more air sampling.

HOWELLS That's true. We did actually take an air sample on the cinder track region. It was considerably lower than the levels we got here.

CHAIRMAN You are going to table it for the record here?

HOWELLS It has been tabled.

ROSS I have not seen it.

CHAIRMAN Could we have that tabled now or the next time when you come back? And that gives the air samples within the Works?

HOWELLS It gives the highest ones at the various times from 2 o'clock on the 10th onwards.

CHAIRMAN We have disposed of two of the risks. Now we come to ingestion and I did interrupt, so would you start again.

HOWELLS This is the only one I consider was the real risk, the ingestion one. We collected a sample of grass from the Met. station about 6 o'clock in the morning. This was sent for analysis. I asked for an analysis for iodine 131, strontium 89 and strontium 90 as I knew that this was material which had probably been cooled only about 8 days or since the plant was shut down, so one would look for iodine 131, strontium 89, and subsequently, strontium 90. The grass gives you a lead, but what you are primarily concerned with in an incident of this nature, is the milk because this is directly into the human chain. We collected a sample, 12 pints, of the Sellafield milk, and this was tested for iodine 131, strontium 89 and strontium 90. Do you wish me to quote figures?

CHAIRMAN Just give us a sample.

HOWELLS If I quote times of milking, this is really the relevant feature. Sellafield, a farm, a.m. Friday 11.10.57, activity 0.003 iodine 131 m.curies/litre.

The afternoon milking, p.m. Friday 11.10.57 - iodine 131 0.47 mc/l.

a.m. Saturday 12.10.57, again local milk - iodine 131 0.48 mc/l.

p.m. Saturday 12.10.57, 1.32 mc/l. We also collected from Seascale. To go back to Thursday 10.10.57. The only report on that was little activity - they could not assess it. p.m. Friday 11.10.57 (no morning sample) was .4 mc/l.

a.m. Saturday 12.10.57 was 0.8 mc/l.

p.m. Saturday 0.6 mc/l. If I may explain my actions on this.

SCHONLAND I am puzzled that you have the times for milking, but not the times you got the information.

HOWELLS This is collection of the milk and this is taken directly for chemical analysis. **On the Friday ones, we would have got the information in the afternoon, of the local milk which was milked on the morning of that Friday, and the afternoon milking carried out at Seascale on the Thursday. Then, at some time on the Friday night or the early hours of Saturday morning, we would have had the afternoon results both for the local milk and the Seascale milk.** Because at this time we were doing a radiochemical assessment on these, we hadn't used a gamma spectrograph method which gives a much quicker result on the iodine. It is the morning milk at Seascale, the one that gave 0.8, which is in the one on which I decided that we have to take action about stopping the milk in the area. Because we had had a .4 mc/l on the previous afternoon, and at this time I was working to the figure quoted by Scott-Russell in his report ARC RBC5, where he quotes a figure of .39 mc/litre for iodine 131 in milk as a short term hazard level. When it got to twice that figure, which was about .8, I decided we must stop the issue of milk in the district.

CHAIRMAN Scott-Russell's figure was given as 0.39. What did you say this was?

HOWELLS A short term hazard to infants' drinking milk. This figure includes an exposure from iodine 133 as well, which we would not have got in this case at all. This particular level was certainly too high. This is based on giving an exposure of 200 rads to the thyroid of the infant, but at this time this was the only figure we had available. When I saw it going over this figure, I thought we must take immediate action.

CHAIRMAN So you took action.

HOWELLS If I might go back a little here. Immediate action: there is always a certain time allowable in these circumstances, because you must have this sort of exposure for a number of days before you get into serious trouble. Scott-Russell again quotes that this exposure of about 200 rads to the thyroid is based on something like a 12.3 microcurie intake to the thyroid, so even at the levels we were getting in milk at that time, one could afford to wait a day or two days before stopping the milk.

3.10, Monday, 21st

Mr. Howells giving evidence

CHAIRMAN On Saturday afternoon, you saw the results of the morning's samples.

HOWELLS At 3 p.m. on Saturday.

CHAIRMAN And you said, "This is rather too much, we must do something"? What did you do?

HOWELLS I notified the W.G.M. at about 3.30. He then asked me to get in touch with Dr. MacLean at Risley and inform him of the decision to stop the distribution of milk produced locally. At 4.30, I spoke to Dr. MacLean and he said he wished to discuss this with Dunster and Farmer and would let me know their decision later. Then at this stage there was no further action we could take, so at 7.45 p.m., I rang through to Dr. MacLean. He was not at home, he was at Mr. Farmer's house. I rang through to Farmer and spoke to Dr. MacLean there. He said they wished to discuss the situation with Prof. Mitchell and Dr. Marley and would take a decision by 9.0 p.m. that night. I was asked if I could make local arrangements for stopping of milk because earlier we had intended to do this through London by Dr. MacLean. I then rang Mr. Davey and informed him of the conversation with Dr. MacLean, and then rang Swift, the Clerk of the County Council, and informed him that there was a possibility that as a preventive measure, we would have to stop the issue of local milk, and what would he suggest as the method of carrying this out. He advised me to get in touch with Supt. Nixon at Whitehaven. I eventually got this system all arranged. I rang through at 8.55 p.m. to Mr. Farmer, and he informed me that they had taken a decision that local milk should be stopped and would I initiate action on this. By this time, Mr. Davey had come in to the Works and he got in touch with Supt. Nixon at Whitehaven. I provided them with a list of milk producers in this area, which had previously been provided by the Ministry of Agriculture, Fisheries and Food. There were something like 10 farms on this initial list.

we realised that there were another two in the district, which were added at that time and an additional five during the night, bringing the total to 17 farms involved.

CHAIRMAN How did you pick out the farms?

HOWELLS This was just a list of local farms. The information we got at that time was not precise enough to define the area. On the Sunday morning, Supt. Nixon asked if I could define the area. I said that at this time the best I could do was to say that the area was within a four-mile radius of the Works, and that as soon as I had further information from biological sampling, I would define this more precisely. On that morning, we had done additional surveys; grass sampling was the quickest method of getting a result, so it was by the activity on grass levels we were defining or delineating the area in which to stop the milk. They extended this then to about 7 miles long and two miles wide.

CHAIRMAN You said over a 4 mile radius and then you wanted more information, then you took samples of grass?

The sampling was going on all the time, because it was the quickest way of getting results. As a result of these samples you ...

HOWELLS I extended it down to the Ravenglass area. I defined to Supt. Nixon that the area involved was to draw a line from Calderbridge to Braystones and follow the main road from Calderbridge to the Memorial at Ravenglass and from that point down to Ravenglass itself. This forms a coastal strip 7 miles long by 2 miles wide. At this time, we did not have information in the region of the other side of the river at Ravenglass. Later in the day, on that Sunday, we had information which led us to extend the area down to Millom.

CHAIRMAN When did you ... you extended the measurements further down outside this first area, and that was going on on the Sunday afternoon?

HOWELLS It was really going on from Friday morning, that we were collecting this information. The delay of course, as was inevitable, was not collection of the samples but analysis. We could collect samples faster than they could be analysed. We kept pushing up the limits all the time, feeding this back into the Analytical Section and as soon as the information came forward, we were then closing the area off.

CHAIRMAN Were the samples done as they came in?

HOWELLS Yes. We had to leave some samples and put priority on others.

CHAIRMAN When did you conclude that the area was not big enough?

HOWELLS All through the day on Sunday, as the information came in we realised we would have to go further. It was a little difficult to be precise because we had to rely eventually on the milk levels, but we tried to do this in terms of the grass activity.

CHAIRMAN When did you pass the word to the Inspector to close the area, the last one?

HOWELLS This last one was done either Sunday night or Monday morning.

CHAIRMAN Have you got any way of checking this?

HOWELLS We would be able to check this with the Police, because all action was taken through them.

CHAIRMAN So you started off with just local farms from a list that you had and you added in 2 + 5 making 17, that was the first, then you extended the area of your sampling over a 4 mile radius from the Works.

HOWELLS Yes.

CHAIRMAN The next pronouncement to the people closing the area, was the area of 7 miles by 2 miles of the coastal strip, that is the second one, and the third one was the last big extension.

CHAIRMAN You did it in three stages. What evidence have you got that there was no need to go even further than you did?

HOWELLS We surveyed as far down as Blackburn, right down the Lancashire coast to Fleetwood into North Wales and as far as the South of Scotland. By the time we reached this stage, we had additional help from Harwell on the Monday and more survey vans and we sent out the first vans with gamma surveying equipment which would give us an idea of the levels above back-ground radiations level. This indicated the regions to go to look at the milk samples. We have found in practice that these correlate very well. Where we got the high gamma levels we also got the high milk samples. So milk sampling was done at the spots where we found these levels and then you filled in afterwards, as time was available.

CHAIRMAN What was it that guided you in the direction you took? You knew the wind was blowing - you had seen some smoke from the chimney down that way. This could be misleading. I suppose there must be different levels of wind.

HOWELLS It was misleading to some extent, the wind seemed to be blowing directly to sea. The wind velocity was particularly low on that night, Thursday night, only about 2 to 3 knots, but it is quite conceivable that the wind at say 500', which is the height one would be interested in, might be different.

CHAIRMAN You started off then thinking that you knew where it must be from, the direction of the wind, and you thought that went mainly out to sea, but, from the ground surveys you were making, you gradually realised that there was a strip down the coast.

HOWELLS This was largely delineated by grass sampling. You could do very little by gamma monitoring with the type of equipment we had at this time, in that the radiation levels were very little above the normal background level one would expect to get. The background level that one expects around here is about 0.1 MR/hour. The general levels we were

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getting might be 5, 6 or 10 times this background with the occasional high spot which might be 50 times background.

CHAIRMAN Clearly it was the iodine activity that was mainly in your mind, you were thinking of milk. What attention did you pay to other radioactive isotopes? For example, what have you done about strontium?

HOWELLS Right from the beginning, we measured this in the grass and in the milk and we found that these were low. This is still continuing. We are still watching the strontiums. To get the Sr 90 figure it is only about Well, we haven't got firm figures on Sr 90 coming through yet. It takes something like six days to get the yttrium flowing in order to be able to measure this. We measured total strontium and these figures are certainly not all that perturbing. This in one way was one of the things that was misleading. There was this bias on the iodine. You see, if the activity measurements, as we picked up by radiation in the district, had been the normal fission product ones, we might have found much less contamination in the milk. It is because of the specific iodine coming out which has given the high milk levels.

CHAIRMAN What I had in mind is that the whole world now is frightened of strontium, and you've found iodine. We must therefore prove that the reason you have been talking about iodine and measuring it so much was that that's what was there.

HOWELLS The first thing we looked for was strontium, the one we were afraid of finding was strontium. We would have liked to have got an answer on it right away. But one would obviously ask for I 131 because of the short cooling period on the material coming out. So I asked for the three: Iodine 131, Strontium 89 and Strontium 90 and these are the isotopes we have been looking for.

CHAIRMAN You said it takes time to measure Strontium 90 and yet you also said there isn't much there.

HOWELLS We measured total Strontium - 89 and 90.

CHAIRMAN You can measure the two together?

HOWELLS Yes, but you have to wait to get the 90 figure.

CHAIRMAN And the two together are not a disturbing figure? Have you any maximum values or anything that could be ...

HOWELLS We have a mass of data on this which is all being listed. I could bring these lists in.

CHAIRMAN Yes, you can bring them in in due course. You can think of the best way of presenting them.

HOWELLS It might be best if the court came and looked at them.

CHAIRMAN I think that's a good suggestion.

KAY ? It's not a court, as the witness said.

CHAIRMAN No, it's not a court, it's a board.

We have now got to the record of what you did through the Sunday night when you asked for a bigger area of milk distribution to be controlled. You've told us that the measurements went on all the time and have been going on since then. What about the Monday and further on - there is nothing outstanding since then?

HOWELLS No. We are watching the trends on the iodine figures on the meter. It went up to a peak on the second day and has since been falling.

CHAIRMAN What is the most you have found in any milk, anywhere?

HOWELLS This would be about 2 microcuries/litre. The reason I say 'about' is that there has been some calibration factor changes in the assessment, but it is certainly of the order of 2 mcuries/l.

CHAIRMAN As against the figure on which you took action of 0.39?

HOWELLS Actually, where I did take action, it was 0.8.

CHAIRMAN It was 0.8. But it was Scott-Russell's figure of 0.39 that you had in mind, and 0.8 was twice that.

PEIRSON You said that eventually it went to Millom - I thought it went to Barrow?

HOWELLS This might have been so at one stage on the Sunday night; we were looking at Millom. We did go eventually to Barrow and this included the Barrow peninsular. This is the area of 200 square miles.

PEIRSON The limit is, as far as Barrow. You raised your milk limit to 0.1.

HOWELLS Yes, this was agreed afterwards, after I had taken the decision to stop the milk in the area. I was then informed by Mr. Farmer at 9.0 p.m. on Friday night that they had decided to set a limit of 0.1 microcuries/litre. This has, of course, eventually been ratified by the M.R.C. meeting the other day.

CHAIRMAN When you say ratified ...

HOWELLS I mean they agreed that this is the official figure as far as M.R.C. is concerned.

ROSS ? Minutes are available to substantiate that.

CHAIRMAN So that is only a quarter of Scott-Russell's figure.

HOWELLS I think Scott-Russell's figure is too high. This other figure gives something like tens of rads to the thyroid as compared with Scott-Russell whose is the 200 rad figure.

CHAIRMAN How are the milk levels running at the moment?

HOWELLS If we take the Barrow peninsula, the majority of these are now below 0.1. There is a strip - for instance, at Askham we have got a reading of 0.3 at present, and at Pennington it is 0.19 microcuries/litre. There are still several in the immediate neighbourhood: Seascale is now .13, and the figure for the local milk escapes me at the moment.

CHAIRMAN So the figures are patchy, but you still have figures bigger than 0.1.

HOWELLS It is patchy, but it makes sense in the sense that, when we did a gamma survey, one can see the direction of the plume right down the coast through the Seascale area, over?.... which is the hill area, then it cuts across the middle of the Barrow peninsula, through the Askham region, then it swings round into a region between Morecambe and Fleetwood in N. Lancs. where we also measured some gamma levels. We can pick out the areas where milk levels are high and it's not patchy for no reason; it fits in with the whole pattern. This was the direction of the plume.

CHAIRMAN As these various records come in and they are getting better - what do you do - do you inform Mr. Davey?

HOWELLS The Ministry of Agriculture, Fisheries and Foods have a representative here, and they will eventually decide when an area can be de-restricted.

CHAIRMAN Your records are available to their representative who is here; he sees exactly what is happening and his Ministry decides when to take action.

HOWELLS The provisional figure is 0.05 microcuries/litre

CHAIRMAN Well, I suppose it is not our business to comment on limits of 0.1 by a further factor of 2. On 0.05 it is going to take many days, if not weeks.

HOWELLS This is provisional.

CHAIRMAN Of course.

HOWELLS One could argue some justification, in that people were drinking milk before the ban came in, and we have to make an allowance for the exposure involved.

SCHONLAND My question may sound rather as though something is wrong. That is not the object of it. These discussions on Saturday night on stopping the milk, first - there was no real argument, was there?

HOWELLS Not as far as I was concerned. I can only speak from my own point of view. At 4.30 on Tuesday, I decided that as far as I was concerned, the milk should be stopped in this area. I then passed it over to Dr. MacLean.

SCHONLAND The only reason for delay was that it had to go through these channels and MacLean and Farmer wanted to discuss it.

HOWELLS I thought at that time that they could initiate action more easily than I could because they could contact the Ministry of Agriculture & Fisheries in the London Office. I thought we might have had difficulty in doing this locally, but we had to do it locally eventually and we did this at 9 o'clock.

SCHONLAND Just for the record, where was the sampling done; in your laboratories?

HOWELLS Yes, to avoid any difficulty on this, we also sent samples to Harwell; this was done right from the beginning. On Sunday night, 13th, we sent something like 40 milk samples to Harwell by special car to get a quick analysis and to provide comparison with assessments that we done here. It could be done more quickly at Harwell by their gamma spectrometer set up in order to do specifically the Iodine 131 without going through the radiochemical approach. We eventually got on to this when we got one calibrated by the measurements we had made on the milk.

SCHONLAND Was there any shortage of equipment at any time or not enough of it for dealing with an emergency?

HOWELLS I don't think so. One might say that on Friday we should have called more people, but I feel that in circumstances where the situation is confused, if you have a team of people who know the area and know what type of work to do, it is much easier and quicker to get on than calling in people from outside who have to be worked into the organisation; the delay does not justify the extra effort. We did get this on the Monday when the position was clear and we'd closed off the areas. This was delineated in such a way that we were quite certain that if we made any change, we would bring areas out of the restricted group, rather than bringing them in. When we expanded them, we did get this organisational problem of building up.

SCHONLAND Was help from Harwell readily available, and possibly elsewhere?

HOWELLS Dr. Marley contacted me on Sunday morning, the 13th, offered any help.

SCHONLAND I am a little puzzled as to who sets these standards. Scott-Russell is an employee of the Agricultural Research Council; he set one standard. Then later on, M.R.C. presumably advised the Ministry of Agriculture on another standard. Why did you adopt Scott-Russell's standard in the first instance?

HOWELLS This was the only one available in the literature.

SCHONLAND But did you not have in your mind that you might have to have a standard somewhere?

HOWELLS Yes, this is something we have looked at. It was under active consideration, but hadn't reached the point where we had actual figures put down. You will find a figure in another of the Geneva papers by Chamberlain, which limits the intake, again these are for continuous exposure, to 0.1 mcr/day to infants of Iodine 131, and one has the difficulty of whether this constitutes a continuous level. One has to set an emergency level somewhere for this type of incident.

SCHONLAND The spread between Scott-Russell and Chamberlain is a factor of 40.

HOWELLS Scott-Russell is a single incident one. Mr. Chamberlain one is continuous exposure. One could possibly argue, the sort of argument I had with myself on the Thursday night, that go ten times the Chamberlain figure which would give you 0.1 mc/litre, which was the figure which was eventually adopted. But, at the time, in order to arrive at a reasonable figure, one really has to work this out in some detail, and to decide to what exposure one should submit the thyroid. But it did seem to me that the 200 figure that Scott-Russell uses is excessive.

SCHONLAND It seems to me that it would be right to ask Mr. Howells, in the light of all that happened, whether he feels that he did the right thing in making this decision at the time he did?

HOWELLS You mean in stopping the milk? I had no doubt at the time, that this was necessary.

DIAMOND What mechanism exists for discussion and settlement of these radiation levels in the Authority, and as between the Authority and the Ministry of Agriculture?

HOWELLS This can be done through a Health Panel organisation, an Authority organisation, to discuss what sort of proposals to put before any outside bodies. Dr. MacLean would be better able to answer this question.

CHAIRMAN We will take it up with Dr. MacLean.

KAY The reference back to Risley. Are you empowered to take local action here using your own discretion, or were you obliged to refer this matter to Risley to get a decision on whether to stop milk?

HOWELLS This would be a decision for the Works General Manager. I advised him that we should stop the milk and then he informed me to ring through to get Risley to organise it.

KAY But the reference to Risley was one of getting help in organisation rather than asking Dr. MacLean and Farmer whether it ought to be stopped. In point of fact, it didn't help you. I don't mean to be critical in saying this, but after some 4½ hours they rang back and said you had to arrange it yourself. Were you impatient about that? Did you feel that more urgent action was necessary, or not?

HOWELLS Well, I did ring through at 7.45 p.m. to find out if they had made a decision. I felt that delay would obviously make it much more complicated to stop the milk. It was Saturday night and I realised the difficulty in getting in touch with people.

KAY Looking back (and I know it's easy to be wise after the event), if you had known that you'd have to make all the arrangements yourself, you would have gone into operation at 4.30.

HOWELLS It is quite true, that. One mustn't imagine that precipitate action was completely necessary; if we'd wanted another 24 hours, it probably would still have been perfectly satisfactory. It wasn't a desperate situation, but I felt we would come to it sooner or later, and the sooner we made the announcement we were going to do this, the better. Obviously with the public involved, we knew we'd be having trouble, to announce that we were stopping the milk, say, on Tuesday, was far too late.

3.40 p.m. Monday, 21st

CHAIRMAN I'd like to ask about a slightly different subject. If you are not the right person - if so, please say.

I want to inquire a little bit into the employees of the Works during the incident where they were rushing about around the pile; a number of people must have had some exposure. They had film badges and?.... sorts of?.... Are you responsible for

HOWELLS Yes, we have complete records on this.

CHAIRMAN Are you able to make any short statement about

HOWELLS Having been tied up primarily with what was happening outside the fence, I could not quote offhand what the figures are. But certainly on the Friday, I asked my Asst. Manager to get all the records together and he has done this. There is a complete record of everybody who was involved in charge hoist operations. The only thing that sticks in my mind is that the highest exposure was received by Ritson I think, but I don't know what the figure is offhand.

CHAIRMAN Could we call your assistant to give us a statement about this?

HOWELLS You could, or I could produce the information. Just as you wish. Of course, we are continuing this. What we set up was another control centre in the Pile Group for the people working on the charge

CHAIRMAN You put somebody specifically on this job?

HOWELLS Yes, Mr. Hermiston, my assistant.

CHAIRMAN I think as you put him in charge of it, I would like to call Mr. Hermiston, but not at this moment.

SCHONLAND Mr. Chairman, would he be the right person to tell us what action was taken from the health angle; the moment the emergency was known to the Gen. Manager, what action was taken by Mr. Howells? You took some action then, didn't you, on the Thursday, on the inside personnel?

HOWELLS Well, there would be a Shop Manager, who would be a professional physicist, in charge of health physics at the Pile Group. He would have been up there at the time.

SCHONLAND So, when everybody was alerted on Thursday, action took place. Who do we get some evidence from?

HOWELLS This would be Mr. Pringle, the actual man on the spot.

DAVEY Mr. Howells was present all through Thursday night, Mr. Chairman, so he can tell you, from a centralised point of view.

HOWELLS Sorry, I thought you wanted the man in direct control of this, Mr. Pringle. I was here on the Thursday night and the Friday.

CHAIRMAN We have two questions, Mr. Howells. One is, can Mr. Hermiston tell us what are the maximum doses that any man received because he was in charge of making these actual measurements? But we would like you to tell us something about what actions you were taking with respect to the W.G.M., who presumably was anxious that the men were not getting severe doses. What information were you passing to him?

HOWELLS These exposures were being controlled directly at the time by means of personal dosimeters issued to the men going on to the charge hoist and the exposures as they were being reported back at that time, were not particularly high, certainly at the start. It was only when they started discharging the channels with the molten metals in it that any activity was being brought on to the charge hoist that levels rose. These men were also provided with special films for this type of operation. In fact as far as what was being done inside the charge hoist was almost a routine operation and there was a prescribed drill laid down. Everyone would be issued, for instance, with protective clothing, respirators, fibre electrometers and special films, this would be the type of control.

DIAMOND The witness perhaps thinks this is all very simple, but I would like it described in some detail.

CHAIRMAN Yes, it's something we are not as familiar with as you are.

HOWELLS Well, if we take the operation itself, to get to the charge hoist, you have to come in through the magazine store. Now, whenever we get this type of operation, we set up a health physics barrier at that point. People going across that barrier and going on to the charge hoist would be issued with protective clothing.

CHAIRMAN So you put in one of these controls?

HOWELLS Yes, automatically, as soon as anyone would start to enter the charge hoist. This is something I would not need to initiate myself, it has been worked out over a good many years. They would be automatically provided with special films and also personal dosimeters.

SCHONLAND Just as we did this afternoon?

HOWELLS Yes.

DAVEY The man who took us and monitored us afterwards is one of Mr. Howells's men.

HOWELLS In a way, one could say that on the Thursday night, as far as control inside the Works was concerned, on the health physics side, this was a routine operation and again, apart from the fact that one was bringing metal back on to the charge hoists, one would not particularly expect high radiation levels. The reactor had been shut down for some days.

CHAIRMAN This routine operation went into effect, men got their clothing, dosimeters, film badges, and they did whatever the Operational Staff asked them to do. When they came out, their badges were collected and these went over to your Film Section for analysis. How long does that analysis take?

HOWELLS This would be developed and assessed in a matter of hours, but you would get an immediate assessment from the electrometer.

CHAIRMAN Were you concerned with passing to Mr. Davey or the Senior Officer in charge of this works what, at the moment, was happening?

HOWELLS I did not pass any information. This was being controlled in the Group there. I was told at the time that the exposures were not particularly high. I was concerning myself primarily with what was happening in the district at that stage.

CHAIRMAN But one of your officers were directly responsible and watching this and you checked there was nothing much to worry about from your particular point. What about matters other than film badges and electrometers: inhalation ...?

HOWELLS Right from the beginning, we started running air samples in the Group. When I went up to the Group, it was 2.30 p.m. on the Thursday afternoon, the air samples were being run at that time on the pile roof and areas had been barriered off as well for radiation levels. This was in the Test House on Pile 1 roof that was closed off. So right away, one of the things I asked my Shop Manager in that Group was to organise air sampling right throughout in case they got a dust hazard there. In practice we had lower air activity in the Pile Group than anywhere else in the Works.

CHAIRMAN What about checking up on what these men had? Had urine samples and things like that been done? Would that be Dr. MacLean?

HOWELLS Dr. Graham would look after this. I did ask for urine samples to be done for tritium on the men who had been in the charge hoist. I think this was on Sunday morning. One would not expect anything particular about this: inhalation on the charge hoist, because there would obviously be an exceedingly good draught up the chimney: the air movement would be away from the men working on the hoist.

CHAIRMAN Yes, but they were trying to push out the rods and the rods were being withdrawn.

HOWELLS They were all in dust masks.

CHAIRMAN We have done checks on this on urines?

HOWELLS The only one I asked for was tritium. If I might amplify a little on this, the sort of contamination levels we got on people in this operation were not higher than we have got in routine operations in the Works.

CHAIRMAN What sort of contamination are you speaking about?

HOWELLS The contamination of hair or hands, as shown by personal monitoring as they went out. They were able to clean up comparatively simply.

CHAIRMAN Is there anything you would like to call our attention to.

HOWELLS No, I do not think so.

CHAIRMAN Well, thank you very much, Mr. Howells. I do not know whether we will want you again. You will do that little sketch, just see how you can present the thing best.

KAY Won't you want a set of maps showing the plume?

CHAIRMAN I think we shall. I think the Board might well go over to the control room and see what is there, and having seen what is there, we might be able to give you a better idea of what we would like. Perhaps later today, but do some thinking on your own, please.

4.10 p.m. Monday, 21st

WITNESS: MR. FARMER

CHAIRMAN Will you begin, Mr. Farmer, by saying what your job is with the Authority?

FARMER I am Chief Safety Officer and I and my staff were set up mainly to support the technical, the new safety committees of the Authority, the Safety Executive Committee, the two Sub-Committees: the Establishment Safety Committee and Reactor Safety Committee. My duties are to carry out work to support these committees, such as the investigation of proposals for reactors, and submit these proposals to the committees with comments. In addition to that, to carry out a service for the Authority on criticality, and this is centred in a Criticality Inspector, Dr. H. Gibson?, and to provide a service to the Authority applied mainly to the Industrial Group, on matters of Health Physics through Mr. Dunster.

My staff have the job of trying to draw up Codes of Practice for the safe operation of plant, and in particular reactors, including the aspects of design, operation of reactors and the effluents emitted from reactors and chemical plants.

CHAIRMAN We would like you to tell us in your own words when you first were told of something unusual happening here, and what you did, and give us a little of the history of what you yourself did and what actions you took.

FARMER About midnight on Thursday, I had a telephone call from Dr. A.S. MacLean. He told me that he had heard, through his Senior Medical Officer, that there was some trouble at Windscale and some high air counts had been noted in the district. He asked whether we should leave for Windscale immediately, or first thing in the morning, and we agreed at the time that he should check back through a responsible man at Windscale to find precisely what was the matter.

He told me later, 12.30 to 1 o'clock, he had spoken to Mr. Ross, with the general message that there had been some trouble, the situation was under control and that there was no hazard to the public. I cannot remember the precise words. We agreed then that we should meet in the office first thing in the morning and phone Windscale again, and at 8.45 a.m. Friday morning, I spoke to Mr. Ross, who told me that the incident had proceeded overnight, was still under control, and that he was satisfied with the state of affairs in the district. I believe that when speaking to him, I said that Dr. MacLean and I were worried particularly about the district and about the public safety, and would like reassurance on that point. Mr. Ross said that he would phone back again during the day if he was in need of our services.

MacLean and I spent the morning in the office together. We informed Mitchell at Risley at 9.30 to 10. The next we heard was a message between MacLean and Davey at lunchtime... Do you wish me to talk of things that happened jointly between myself and MacLean, or would you wish MacLean to tell this?

4.10-4.40 p.m. Monday, 21st

CHAIRMAN I would like you to tell us any way you were personally concerned and that would include Dr. MacLean.

FARMER MacLean phoned Davey saying that Dunster, MacLean and I were perturbed at the situation here and felt that our services might be of use to Windscale and should we come up. He received the answer that we were not required at this stage at Windscale. Shortly after, I think about 1 o'clock, I saw a copy of the press statement which described the incident at Windscale and, for the first time, I learned that there had been a condition of red heat in the Pile. Previously, the stories I heard had been consistent with the failure of a few cartridges, such as had occurred previously. When I read of the condition of red heat, I realised that this could be a calamitous situation for the district, it wouldn't require very many red hot cartridges to release quite a lot of volatile material, and we felt then that the services of Harwell should be brought in on the assessment of contamination of milk, and phoned Windscale and asked for milk supplies to be sent to Harwell for checking that night. That was on Friday afternoon. At this moment, I cannot recall any other item of importance until 4.30, Saturday afternoon. At 4.30, Saturday afternoon, MacLean phoned me to say that he had just received the iodine in milk figure from the Harwell and this was 0.4 microC/litre. He felt that this might well be outside the reasonable limit for public consumption, and he proposed to come round to my house to discuss it immediately. By 5 o'clock Saturday afternoon, Dunster, MacLean and I spent a few hours discussing the significance of 0.4 mc/l. We could not arrive at a decision immediately because the levels quoted in

the I.C.R.P. Handbook refer to iodine intake for continuous consumption. Normally one refers to the levels for daily intake in this Handbook, but the I.C.R.P. figure for iodine would refer to continuous intake and for a short-lived isotope of 8 days there could be quite a difference in allowed intake per day as between a short interval and a lifetime. We were also aware that there was a difference in the importance of intake as between adult and child.

CHAIRMAN I.C.R.P. is for adult occupational workers?

FARMER Yes and this is 0.15 mc/day. We had a few reports, one of Scott-Russell's, on the uptake of iodine in the grass/cow cycle and we spent some time trying to decide if cows grazing over grass contaminated to a certain level, what would be the amount of iodine in milk, and then if a child drank one pint of milk per day, a child's thyroid was, we were assuming, $1\frac{1}{2}$ gm, 20% of the child's thyroid, and we would arrive at the amount of iodine in the thyroid. We then had to arrive at a basis at which we could assess a provisional level for control. We felt it should lie somewhere between the normal basis which would be that any member of the public would be permitted to have 30 mR/week, which is one tenth of the occupational dose, to either the whole body or any part of the body. The adult is allowed to have 600 mR to the thyroid because the thyroid is less sensitive. We thought that this probably did not apply to the child. On the other hand, an emergency dose talked about in Civil Defence as far as we know for weapons trials runs up to 25R as a single shot not to be repeated within three years. We thought that the consultants would not approve of any action in which we allowed 25 Rontgen to a member of the public unless it were indeed an emergency.

CHAIRMAN We do not allow 25R to a member of the public, that is, to one of our own people, for very important emergency occupations.

FARMER Precisely, that was our interpretation and whilst we might allow our own staff to have this dose if it was for the protection of the public, we wouldn't allow the public to have it. We finally arrived at a figure of about 10 rads to a child's thyroid as being the probable limit in an emergency. This came out to 0.1 mc/l in milk and this took us until about 8.00 to 8.30 on Saturday. Round about 8 o'clock we were in touch with Howells at Windscale and told him that we were of the opinion that milk over 0.1 should be controlled. We wished to get in touch with the Chairman's Office. We would like Howells to tell us what steps would be taken up here to control it; who would he get in touch with, and to be ready to tell us precisely what steps he would take at 9 o'clock.

The Chairman's Office was phoned at 8.30; I spoke to Allan and told him what steps we proposed to take. We spoke to Windscale at 9 o'clock and we were informed that Windscale would carry out this action through the Chief of Police at Whitehaven and that he would be in touch with the Milk Marketing Board distributor, and in addition with the local farmers. We advised Windscale to go ahead immediately, told them we had notified the Chairman's Office and that we would notify the Ministry of Agriculture and Fisheries. Shortly after 9, I phoned the duty officer at the Ministry of Agriculture and Fisheries, London and some 20 minutes

later, had a phone call from Sir Edward Harewood asking what action was taken, and I referred him to Windscale and then to Chief of Police, Whitehaven for the detailed mechanics. Later that night, there were many conversations with Mr. Allan of Chairman's Office agreeing on the press statement. Later that evening, there was a proposal to have a press conference next day, and I was asked to get in touch with Sir Leonard Owen. He was not available between 8 and 11, but some time after 11, I spoke to him and told him of the action taken.

CHAIRMAN This would be 11 o'clock at night.

FARMER About 11 o'clock Saturday night. I have missed out one point on the timetable. I think between 9 and 10. Dr. MacLean spoke to Mr. Ross, it may have been just before 9, and told him of the proposed action, and Mr. Ross agreed the action that was being taken. I spoke to Sir Leonard at shortly after 11 o'clock, told him of the action and arranged a meeting the next morning. That concludes the immediate actions taken. There was an urgent requirement to have confirmation of the control level at 0.1 mc/l. For this purpose, Marley arranged for a meeting of our consultants, which was held on Tuesday afternoon at Risley, at which Sir Ernest Rock-Carling, Prof. Mayneord, Loutit, Scott-Russell, Mark Marley, MacLean and myself were present. They were informed of the general story, the action we had taken and, after some discussion, agreed that the control level of 0.1 mc/l was, in their opinion, adequate.

CHAIRMAN Could I just check the names?

FARMER Professor Mitchell was unable to be present and Pochin was also invited, but was unable to come.

CHAIRMAN This group agreed; what right has this group to agree these things?

FARMER They would give it as their opinion that this level would not give rise to any harmful effects in children. They agreed that their line of argument was that 400 to 450 Rontgen was the least dose known to give carcinogenic effects in the thyroid, and in their opinion, a level of 10 rads to the thyroid would be perfectly safe. They, through their spokesman, Sir Ernest Rock-Carling, would report to the Medical Research Council. A meeting of a Medical Research Council Committee was held on Saturday morning in London. I was not present and cannot report on that meeting, but the consultants I have named are retained consultants to the Authority on matters of health.

CHAIRMAN Has it not been a matter of some anxiety to you for some time in your job that you have not got properly stated tolerance levels for all the isotopes for children covering every possible situation, so that you do not have to scout round at a moment's notice to get some experts to advise you? I know how difficult this whole subject is and how controversial and I'm perhaps being too critical, but it is unfortunate to be caught like this, and having to have emergency telephone calls all round the country. I believe you mentioned some of the people. I believe the telephone calls would go much wider than the people you mentioned. What do you think? Have you not some feeling that perhaps this is not the position we should have been in?

FARMER There is cause for concern here. I think it unlikely that we could hope to have all the isotopes written down at levels which it would be appropriate to emergencies. I think it would be extremely useful to have the principles agreed at a responsible level, such as 10 rads to the thyroid or certain radiation levels to the body and then to carry out the exercise of going through the table of isotopes as far as we could. I think most emergencies are likely to give rise to situations which aren't precisely covered by tables and there may be occasions when we would need to exercise our thoughts collectively to see how these tables could be applied in such an emergency, particularly in questions of additivity.

CHAIRMAN Yes. Well then, just to be clear. Windscale, through Mr. Howells, consulted Dr. MacLean and you, and you had Mr. Dunster available, and on the Saturday evening, you were thinking as hard as you could to set a reasonable figure for the dose to children drinking milk, having in mind, the only figure of standing that you had, was this I.C.R.P. for a lifetime dose to occupational workers and from that you had to somehow or other make a reasonable figure for children, over a few days of an emergency, and your conclusion was that .1 was the figure, the best figure you could reach, and if milk was more than .1, your advice or your instruction for Windscale should take action?

FARMER We would give it as an instruction in the sense that, if there was any question whatsoever of a query at Windscale as to whether this should be obeyed or not, we would refer back to the Chairman's Office immediately. This query did not in fact arise. But MacLean and I would have insisted on control at that level unless overridden at least by Sir Leonard or the Chairman.

CHAIRMAN So you, at any rate, gave Windscale a figure to which they must work and Windscale were asked to work through the local people here. Now, were you acting then as Chief Safety Officer of the Authority? I suppose you were. What worries me a little bit is that here we have a position of some emergency, we have now got a technical figure, we know now that the milk in this area is not acceptable for children, and somehow the Authority has got to go to somebody with the right of doing something, and what we did was through the Chief Constable and he did something. I don't know whether he acted legally or whether we did. I don't know whether it is even our question to ask that.

FARMER I cannot throw any light on the legalities. I know there had been prior consultation here as to the action to be taken and it was agreed that action would normally be taken through the Chief of Police and in the case of the Milk Marketing Board with advice given back to the Ministry of Agriculture and Fisheries.

CHAIRMAN I think this is outside the terms of our enquiry because, after all, it is an Authority matter, but it does somewhat puzzle me.

FARMER I feel that I should enlarge on the short story I gave earlier, by saying that at midnight on Saturday, I spoke to Fair at Windscale and asked him to arrange immediately for the collection of vegetables and have these assayed and for the answers to be available by 3 o'clock on Sunday. This

information would be required at the Press Conference and in any case, until we had figures for vegetables, we would not know whether control was necessary. We thought, judging by the contamination on grass, that it was unlikely that vegetables would need to be controlled. The figure actually turned out at 0.3-0.4 mc/kilo, and these figures were given to the meeting of the Consultants on Tuesday afternoon and they agreed with the action taken and with our opinion that no control was necessary at this level.

CHAIRMAN Well now, has this figure of .1 now been accepted? Is that the figure to which milk is now being controlled?

FARMER This is the level of control still applying here for this emergency. I understand, again by hearsay, that the M.R.C. committee on Saturday morning agreed this level for this emergency and would in fact officially be notifying the Authority and the appropriate Ministers. They have also agreed a figure at which milk may be released again to the public. I haven't got this figure. It was a figure to be put in the Official Report, and not communicated earlier.

CHAIRMAN So that you are not dealing personally either with M.R.C. advisers or with the Ministry of Agriculture and Fisheries.

FARMER No.

CHAIRMAN You and Dr. MacLean asked Windscale whether you should come up. They said 'No'. When did you come to Windscale, first, after this?

FARMER Could I just enlarge a little? A message was passed from Mr. Mitchell to await Sir Leonard's arrival here on Friday, and I understand that the gist was that the Chief Safety Officer was concerned and wanted to know if he should come up. The message came back on Friday afternoon that I should report here on Monday morning. This situation changed when MacLean and I attended the Press Conference on Sunday in London. We were turned back and reported to Sir Leonard between 11 and 12 on Sunday night and were asked to attend at Risley on Monday to have a discussion on the future events. I first reported up here on Wednesday mid-day.

CHAIRMAN And have you been here ever since then?

FARMER I was here until Friday mid-day, when I went to Manchester to be present at an ITV show on Friday night. I returned here last night.

CHAIRMAN I am a little puzzled why you, as the Chief Safety Officer, do not seem to be coming into the local measurements of milk here, and the relations between the Atomic Energy Authority with milk and the Ministry of Agriculture and Fisheries. You have not been brought into these details at all from what you have said. You, the three of you, gave a figure, this big Committee backed that, and then you made the point about vegetables and that was done, and the outcome of that was satisfactory. Since then you have not really been ... you have been doing Television programmes and things of that sort.

FARMER The liaison with Ministries on health matters has largely been dealt with through A.S. MacLean. This has been a function that he has carried on for many years. He is also Chairman of a sub-committee of the Health Advisory Panel on health matters, so that questions relating to public health in the Industrial Group are, in the main, routed to him.

CHAIRMAN Who is your commanding officer, so to speak? You are the Chief Safety Officer of the Authority. Who do you report to?

FARMER Sir Leonard Owen.

SCHONLAND Mr. Farmer is not the Chief Health Safety Officer.

CHAIRMAN No, he is not.

SCHONLAND And if I have followed his evidence, he is concerned with health safety as an advisor to MacLean. That is the way I see it.

FARMER The line is not very clearly drawn. I and my section carry out negotiations with Ministries on questions of effluent from our factories, for example. This introduces quite a large measure of health, that is, the levels at which fish or seaweed are safe to eat, and we discussed these matters with the Ministry of Housing and Local Government and Ministry of Agriculture and Fisheries.

CHAIRMAN Well, I understand it now. I think. Were there any other points? We have heard a lot about milk and we know that grass and vegetables were tested. What about meat or other things?

FARMER Eggs were tested. These were .024 mc/egg, maximum in this district. It would allow a child to eat four eggs per day. We imagined this was a most unusual consumption of eggs for a child and no control was established on eggs. This was reported to the Consultants. We did consider the consumption of meat, whether chicken, cattle and concluded that as 20% of iodine went to the cow's thyroid and the rest of the iodine was distributed over the body weight, this would give a factor of between 100 and 1000 to 1, and there need be no control exercised over meat.

SCHONLAND Has that been checked?

FARMER Meat samples have been taken in here and they are being checked also by the Ministry of Agriculture and Fisheries. Water samples were checked, I have not mentioned this earlier. Waters of all the main reservoirs in the district were checked. The iodine in the water was about $\frac{1}{100}$ th or rather less of the maximum permissible concentration for radiation workers.

CHAIRMAN $\frac{1}{100}$ th of what?

FARMER MPL for radiation workers.

DIAMOND You said earlier on that you were concerned with the safety aspects of reactors in addition to these other things. Are you concerned with the safe operation of Windscale?

FARMER Yes, that would be part of my job and come in my Terms of Reference. In fact, no time or consideration has been given by me or my staff to the present operation at Windscale.

KAY Am I right in saying that the Safety Branch is a relatively recent formation?

FARMER It was set up in November 1956, and staff has been joining me since the period April-May-June 1957. My work has been mainly directed to the examination of new projects. Something like 30-40% of the time of myself and staff has been directed to the CO₂ gas cooled reactors, sometimes to new proposals, the high temperature reactors, Merlin - the A.E.I. John Thompson scheme, consideration of proposals coming in from the insurance market, and no time has been spent on Windscale, the fast reactor, DMTR at Dounreay, or current Harwell reactors.

KAY Your main functions up to now, at any rate, have been to provide an advisory service for current industrial designs and developments, the sort of consideration that comes up at the Reactor Location Panel, that sort of thing?

FARMER Precisely, in that my staff is mainly serving the Reactor Safety Committee and Ancillary Establishments Safety Committee.

KAY Do you actually have any executive responsibility for the Authority's installations? Supposing you were not happy about the safety of some particular reactor being built or operated by the Authority, have you the power to intervene? What is the mechanism?

FARMER I would be empowered only to bring this back to the attention of either of the Committees or to Sir Leonard Owen. I would take it from my Terms of Reference ultimately to the Chairman.

KAY But normally it would be for a Design Office to bring a new matter to your attention or for a Works Manager to bring any problem to your attention. You do not maintain a continuous scrutiny?

FARMER Not in the reactor field. We maintain a much more detailed scrutiny of safety in the field of criticality, where a particular detailed organisation is set up and plant may be operated only after the scrutiny of the design or plant, subject to the issue of a criticality certificate which defines the conditions of operation. This certificate is issued by my staff with the backing of a criticality panel, group of experts from R. & D. Branch.

KAY Issued by you?

FARMER By my staff.

KAY But the certificate system is limited to criticality problems. Are you limited to criticality problems with enriched materials? This does not include

FARMER Enriched uranium or plutonium.

DIAMOND Is it true to say that your present post is very similar to that of the Chief Alkali Inspector?

FARMER I would not know, precisely. I would think that in the drawing up of codes of practice for design and operation, it would go beyond the interests of the Alkali Inspector.

DIAMOND But insofar as you impinge on the health aspect of the job in particular, it is similar?

FARMER In that advice is given of levels that would be safe, yes.

DIAMOND Is it one of your staff up here who No, it obviously can't be.

FARMER I have no staff up here.

DIAMOND We have heard who looks after criticality up here - he is not one of your staff?

FARMER I have no staff or technical control of people up here, but the Criticality Inspector, under his terms of reference, has the right to inspect plant onwards.

DIAMOND The Criticality Inspector, Dr. Gillison, is on your staff?

FARMER Yes.

DIAMOND Until recently he maintained a member of his staff at Harwell? Does he still do so?

FARMER He does - Mr. Wilson.

DIAMOND And at Aldermaston?

FARMER No, and no one on the staff up here.

ROSS There is a nominated officer on the staff here, and he is the Criticality Officer.

FARMER Gillison maintains a contact up here through the Health Physics staff here, but I feel that Gillison has Wilson on his staff at Harwell because Harwell have many and varied problems which require the full time attention of one man, and Gillison can make fairly frequent visits to Windscale and Capenhurst and is fairly well informed of the current developments up here on criticality.

DIAMOND Do Gillison's terms of reference involve any investigation of the safety of the installations - the piles - up here?

FARMER Not piles. Any plant which could give rise to criticality incidents. These are mainly chemical plant operations. There are some features of pile operation, such as the discharge of enriched fuel into the back of the pile, which come to his attention, but not the operation of the pile itself.

DIAMOND No such matters as what is put into the pile? Isotope cans?

FARMER No.

DIAMOND It was never the intention when setting up your Branch that that should become its function?

FARMER Not a function of the Criticality Branch. I would expect on any reactor operated by the Authority, or any reactor on which the authority had any responsibility, such as the C.E.A. reactors or A.E.I. reactor, that that reactor would be operated only under conditions which had been agreed by the Reactor Safety Committee, or by my staff, and would be subject to inspection by my staff.

SCHONLAND Your terms of reference include that much wider responsibility?

FARMER There is such an agreement with A.E.I. for their reactor and the Authority may inspect their reactor and we would expect to lay down the conditions under which the reactor shall operate.

SCHONLAND Does that extend to DIDO and PLUTO?

FARMER That has not been (missing on tape).

SCHONLAND I was asking whether the Harwell reactors, or the?..... according to your terms of reference, come under your general supervision from the point of view of safety. You say this hasn't been made clear.

FARMER Under the terms of reference, I would expect that they should fall to inspection by my group. These reactors have not yet been discussed by the Reactor Safety Committee, and recently the question has been raised whether a report on the safety of DIDO should be presented to that Committee, or whether it should be dealt with through the Harwell committee structure.

SCHONLAND Speaking from memory, I think it has been agreed, though this may not have reached you, that the safety of the Harwell reactors should be ultimately referred to you, but the immediate problems of safety would be locally controlled.

FARMER Yes, that is my impression.

SCHONLAND That has not been the case with regard to the Windscale reactors?

FARMER I have no doubt that, under my terms of reference, that I have a responsibility here. There has in fact been no consideration, because I have no time and no staff.

CHAIRMAN A fair summary of what you say is that a large part of your efforts must go on the critical assemblies and all the other problems of safety and that this Health Physics is rather a small aspect of your main job.

FARMER No, I would not say so. I would think that Health Physics would be a third of the total requirements of my strength. There are many problems arising on the siting of the reactors involving Health Physics considerations: what happens in an emergency?: does it matter if this site is next

door to certain installations? Recently, we were considering installations in the South, and needed an assessment of what would happen in an emergency. If there are oil refineries nearby, we need to consider the disposal of effluents from normal and abnormal operations into rivers and lakes. This problem is part of the Health Physics side of my work and will run concurrently with the development of standards, such as the amount of iodine that could be tolerated in certain situations.

CHAIRMAN I see, thank you.

DIAMOND How many staff have you got?

FARMER If you will bear with me, I will recall them rather than give a number. I have Dunster in charge of Health Physics. He has one man with him, who joined on 1st September, transferred from Operations Branch. I have Bowen in charge of engineering aspects of reactor assessment. He has three men at the rank of E.I and E.II. I have Dr. Gillison working on criticality matters, supported by a mathematician and a computer, who can also supply a service to the reactor assessment team. I have a complement which allows for further intake of an additional five people immediately at E.I and E.II levels and have boards currently arranged to interview these people.

DIAMOND I take it you can draw on technical section assistance at Risley if you want it?

FARMER Certainly we do on the technical section of R & D Branch on criticality, and on Harwell on the assessment of particular aspects of reactors.

CHAIRMAN Any more questions? Thank you, Mr. Farmer.

DR. MacLEAN'S EVIDENCE

CHAIRMAN Would you start by saying what your job is?

MacLEAN I am Group Medical Officer of the Industrial Group.

CHAIRMAN Please tell us in your own words whatever happened, the first you knew about the trouble here, whether you were asked to take any action or took any actions. Just give us a narrative.

MacLEAN I will start at the beginning. As far as I am concerned, the story began about midnight on Thursday, 10th October, when I had a telephone call at home from Dr. Graham, the Senior Medical Officer here. He told me that there had been a mishap on the reactor, and he understood there had been or was a fire of some sort and that there had been and was a continuing emission of activity of some sort. At that time he had no details of the extent in terms of the health and safety side. I then rang Mr. Farmer and told him of this thing and I tried to ring Mr. Dunster, but unfortunately he could not be roused. Mr. Farmer and I agreed we should be prepared to get a car immediately and come straight to the site. We proceeded to Mr. Dunster's house to pick him up.

Having to this point I rang here and spoke to Mr. Ross and Mr. Tuohy and told them we had heard of the situation and

were ready to come up. He said that initial surveys of the area were quite satisfactory and he did not wish us to come and would contact us next day. This was not a terribly happy situation for us. We had no information at all, we simply had the feeling that an accident of this sort was something that we might well expect to have some fairly seriously repercussions in the district. Next morning, we again contacted Windscale and had a number of discussions with Dr. Graham here, who gave us some further information. One interesting piece of quantitative information was a measurement of the gamma activity measured by a sister in the Separation Group surgery on a probe held to the window. On this basis, we began to make some guesses about the order of the spread of activity round the district.

Later that morning, I spoke to Mr. Davey and he told me that the survey had extended quite widely out round the works and that the gamma levels were satisfactory and that they were less than 1% of the levels which we had previously agreed are the levels indicating immediate evacuation. We had thought about this in terms of the information we had available at the time, and we had felt that in an accident involving fire, there was a strong possibility of some process of separation, so that the gaseous and volatile materials might get out much more easily than the particulates. This was especially true as we understood the blowers were not running so therefore there was quite a small draught presumably going up the stack, so the particulates might very well precipitate out. So we suggested that we felt quite concerned; although the general gamma levels were satisfactory, that the milk situation should be looked at as a matter of urgency. Early in the afternoon of Friday, we made arrangements with Harwell to have a shift of chemists put on for the week-end, and we arranged to have samples sent from here, and indeed they went that evening, that evening's milking.

I received the results of these analyses. They were phoned back to Mr. Howells; he telephoned me at home on Saturday afternoon and I received the milk analyses at 4.30 on Saturday afternoon.

I again got in touch with Farmer and Dunster and we met within an hour, and we realised from the start, as the people here at Windscale did, that these first two results we had were indicative that there would be a need to restrict the milk supply over some part of the area here, at least and we agreed with Windscale that some move of this sort would certainly have to be made, but we would have to put in a certain amount of frenzied work with this information against information from other sources that we had available at the time in an endeavour to calculate the area over which this might be necessary and also in an endeavour to reach an actual action level, because there is no level in existence for this sort of thing. It was neither the situation that we think of in terms of a continuous discharge of effluent and therefore a lifetime exposure; nor was it of the same urgency, we felt, as a very widespread contamination in war time with the need to make every possible use of food supplies. We felt it was somewhere between the two. We went through all this mental process and got in touch with various people at Harwell, Chamberlain and Marley in particular. At 8.30 or 9.00 we came to the decision jointly with Windscale that the restriction of milk would be necessary over such an area as

would be bounded by a 0.1 mc/l. of radio iodine milk contamination levels. There are many details of contacts that were made in the actual carrying out of this restriction order, some of which you may have heard from other witnesses. I do not have a clear record in my mind of every individual step because we spent 4/5 hours continually on the telephone. We arranged the details with Windscale. We made all the contacts with the London end, starting with Mr. Allen, the Chairman's secretary and from there we got in touch, later that evening, with Sir E. Harewood, of the Min. of Ag. Fish. & Food. Eventually over the next few hours, the tieback here locally between the Government end at London and the Milk Marketing Board at this end were gradually effective. Next morning, Sunday, 13th, we had a conference with our Managing Director and the Director of Operations, and Farmer and I were whisked to London to take part in one of the, in a way, unfortunate necessities which come with the job on the Public Relations side.

We had made contact with Marley and had arranged with him to call an immediate meeting of our medical consultants. Marley made these arrangements; in fact Loutit and Scott-Russell, whom we had invited along, arrived on the following day at Risley from London and we had eventually had a meeting with the consultants on Tuesday. Apart from our own Consultants, we invited Sir Harold Himsworth, Secretary of the MRC, Dr. Pochin, who is the Chairman of the Medical Research Council committee on Protection against Ionising Radiations, Chamberlain of Harwell also attended the meeting. I felt it was a good arrangement in that he was coming straight on here after the meeting, and I thought it would be valuable to have someone on the site here who had taken part in the detailed discussions at that meeting.

5.10 p.m. Monday, 21st

MacLEAN This meeting reviewed our initial level of restriction of 0.1 mcurie/litre and in fact, after a great deal of discussion, decided that this was a very reasonable level and gave it its support. They went on to consider a whole list of other possible modes of entry of iodine through foodstuffs – vegetables, eggs and so forth – and again decided that at the levels that we were able to report there was in fact no need to recommend any special action in relation to any other food stuff than milk. In the meantime, on the Sunday and Monday, the milk survey had been extended very rapidly: the facilities here for dealing with the situation were increased and they were greatly assisted by instruments and staff from Harwell so that by Monday, we really had the thing running on a basis which enabled us in the next two days to go far and wide over Lancashire, Yorkshire, up to the Borders into Scotland and North Wales.

This extended survey on Sunday and Monday showed us quite clearly that whereas the theoretical expectation of fall off of iodine levels was that the restriction might well only apply to a small area, in fact, as we moved South into the Furness Peninsula, Millom and beyond, we discovered that the levels were still in some places rather over this 0.1 figure. In Barrow the maximum figure in the Barrow Peninsula was .3 mc/l so that on Monday we advised an extension of the area to take in the whole section, down to the Barrow Peninsula about 30 miles in all in length, about 10 miles broad at the South end

and 6 across at the North end which was about 4 miles North of the Works itself.

The discussions with the consultants did not stop just at the question of radio iodine, because we felt that it would be very necessary to look at the possibility that there may be other elements, which, although they were not competing with iodine in terms of our actions in the first few days, might eventually with the decay of iodine, become of more interest or importance to us. Arrangements were made and have been carried out since to sample for other elements. I mention two: caesium and barium are materials for which we have no results but which we have evidence that there is no particular problem in terms of the overall gamma levels. However, these analyses are proceeding. But apart from these, we have been considering strontium, both 89 and 90, in particular.

We have extended our survey. Our new analytical programme caters for large numbers of samples, many of which will be analysed at Harwell, for both 89 and 90 over a grid over the area which we have agreed is a reasonable sampling pattern. The strontium levels that were measured, roughly at the beginning of this problem, showed us immediately that there is no immediate or serious strontium problem. Incidentally, they also showed us, by comparing them with iodine levels from similar samples, that there had apparently been of the order of a hundred times the iodine release that one would have expected by taking it as a straight relationship with strontium. The strontium figures are such that the largest total strontium figure we have is 2,250 sunshine units total strontium and there have been slightly differing estimates of the 89-90 ratio. I do not think this necessary. I am sure that we simply have to look back and get more detail on their radiation and cooling times.

CHAIRMAN This maximum of strontium: where was that?

MacLEAN In milk. We were comparing strontium and iodine in milk. I do not know if the court is aware that we have been very interested in strontium for at least two years here. From time to time, there have been emissions of a certain amount of material from the reactors and strontium has appeared in quite measurable, but apparently harmless, quantities in milk. As recently as two months ago approximately, the Medical Research Council set up a special committee under Sir Harold Himsworth, to consider the criteria of safety in relation to this Windscale area and they recommended at that time a maximum permissible level of strontium 90 in milk of 250 sunshine units. The maximum level we have had since this new survey started was a sample from Seascale and the maximum estimated level of Sr 90 was 90 units.

CHAIRMAN That is taking the normal fission product ratio? There is no funny business of gaseous precursor or anything, is there?

MacLEAN I am not competent to answer this question, but it has been asked and was asked from the start and our technical people have been considering it. The samples now being run for strontium are being done for 89 and 90 specifically on yttrium growth. One other thing that may be of interest, although it's getting rather far away from the incident, is that the Medical Research Council called a

meeting on Saturday morning last and they considered the whole of this problem and Sir Harold Himsworth rang me later that day to say that he was sending an official minute to the Lord President, to the Ministry of Agriculture and Fisheries and to the Chairman of the A.E.A., and I understand they expect to receive that minute today or early tomorrow, and Sir Harold suggested that it would not contain any major criticism of the levels we had adopted and of our approach to the whole problem.

SCHONLAND I ask if there was any minor criticism.

MacLEAN I think this is perhaps rushing the fences a little. I would like to see this in writing. One thing that has come home to me very forcibly is that I like to see facts, not speculation, and Sir Harold certainly did not mention any criticism, but we haven't seen the actual report.

CHAIRMAN We would like you to say a little more about the present position, the milk samples, strontium samples being done; what are the levels at the moment?

MacLEAN As far as strontium goes, we have few new levels yet, because the specific 90 estimation requires, among other things, a week for yttrium growth. The milk situation is that in the South end of the area, that is in the Barrow Peninsula and in the Eastern fringe of about a third, up as far as Wastwater, the majority of levels are now below the .1 mc/l figure and there, I would say approximately, a third of the values are marginally over, and in any case, less than .25. There are still one or two odd ones here and there that are in the range of between .25 and .3, .4. The impression we get from the rundown over the days is that the Barrow Peninsula end at least may very well be run down to the point where de-restriction will be permissible some time this week.

CHAIRMAN That's not the Authority's job, though. Our job is to advise the other Statutory Authorities of what the levels are.

MacLEAN There are two points occur to me here. Sir Harold Himsworth did say to me that the level they were recommending for de-restriction was a shade lower than the level we had adopted for restriction because they thought that as we had extended the area, there might be some people who had had a little before the restriction was applied

CHAIRMAN What is the 'shade' in numbers?

MacLEAN He would not give me the number. I put 2 and 2 together and guess a factor of two approximately. There was a meeting in London on Monday last attended by Dr. Marley. It was held by the Chairman. There were present, representatives of the Ministry of Health, the Ministry of Agriculture and Fisheries, and the Ministry of Housing and Local Government and it was agreed at that meeting that the procedure for de-restriction would be that the Authority, the Industrial Group in fact, along with the advice of its own consultants, would advise the London Office when the time came, that any particular area seemed to be free enough to suggest that de-restriction could be carried out. London Office would then advise the Ministry of Agriculture who might then take samples, make further investigations, or refer it to their own consultants and they would then carry out the de-restricting action.

CHAIRMAN I would just like to ask a question about the additivity of the rest.

The position as you described it is that iodine was really the only significant risk, the only thing you were watching somewhat anxiously and that the iodine was in the milk, there were slight traces in eggs and vegetables. You thought that .1 was about right and you applied that to milk thinking that in all the other things there were such small quantities that did not matter.

MacLEAN To find a significant source of iodine in the area which is not restricted for milk, you would find yourself very quickly in quite lurid assumptions about the amount of cabbage a child has to eat - kilograms, cwts and so on.

KAY What do you feel about the question of the definition of acceptable levels for some of these things? We have noted that there was considerable doubt on the question of the iodine contained in milk. Do you feel that any more should be done in finding in advance what are the levels at which some emergency action is necessary? I know it is difficult to forecast what form any emergency will take, but do you feel it would be possible to go further, so that one did not have to carry out an investigation of what was the level when the emergency has taken place?

MacLEAN I do. I think that we certainly can go further. There are so many types of incidents one could think of. I do feel that we can go further and I think it is fair to say that so far the Authorities who have considered the maximum permissible levels who, of course, are not ourselves although we take part, have been able to cope only with continuous exposure problems, and for nuclear warfare, acute exposure problems, and here we have something which is rather between the two. I imagine if you got down to listing the possible values, you might want for so many different sets of circumstances, it would be quite a big exercise. But I am quite sure that, arising from this mishap, we must press for further thinking along these lines.

KAY And this should be initiated by the Authority?

MacLEAN It might be initiated formally by the Authority or it might just as well be initiated by Authority Staff who belong to these various committees.

KAY Who is the final arbiter in these matters? The MRC represents the they should be able to give the final medical word. But, on a purely advisory basis, they would only recommend what they would

MacLEAN On the non-medical side, agricultural interests and so on, the Agricultural Research Council in fact jointly with the MRC and the development commission make recommendations about agricultural occurrences and so on. These are recommendations; they do not carry any legal weight at all. In fact, I imagine that in practice, in a test at common law, they would carry a very great deal of weight. It may be relevant to point out that in the question of the discharging of normal effluent there is a firm, binding and absolutely clear legal requirement in the Atomic Energy Act. Gradually other interests, the Factory Department Side for example, are coming into this field and they now have the

Parliamentary draft of their first Code of Practice. We can see that very gradually, perhaps taking a number of years, the whole field of health and safety will be brought on to a firm legal basis, with some system of Government inspection operating to cover every aspect of it.

KAY But following this particular mishap you have at least some data to work on and some samples could be defined almost at once, or when you have completed your survey.

MacLEAN This is a big job to learn all the lessons that we are going to learn out of this thing, and I do not really think that we can do them all almost at once. There are some we must do at once and there are others which will take longer. Not just the Authority but lots of other people have here an opportunity of learning from experience in a way they have never had before. The Director of Research of the ARC was here a week last Monday. The MRC are also interested and they will certainly be making use of the situation here to study in practice the relationships between grass and thyroids and milk in the case of iodine, and the strontium as well.

CHAIRMAN You told us that 22 months ago the special committee of the Medical Research Council had given us the figure that the maximum permissible level of strontium 90 in milk is 250 sunshine units. Do you know what the maximum in the Windscale Area has been before the present incident?

MacLEAN The highest levels were recorded in a farm on the north side of the Works, just a little way outside, and the highest level ever recorded there, a corrected value, was approximately 100 sunshine units. This farm and this milk supply has in fact been monitored daily, or at least daily samples have been taken, and they have been looked at in the bulk once a fortnight for a long time now and the change in activity has been watched there. The levels just up to the time of the mishap had run down to approximately 40 sunshine units and in fact on the same supply, the levels, after the accident, were 50 sunshine units. So apparently very little additional strontium went into that supply. At Seascale, however, I mentioned earlier on that we had a highest figure since the mishap of about 90 sunshine units of strontium 90, whereas the highest level we had previously was only about 30 sunshine units, and just before the mishap it had run down to about 15, which was not significantly different from the milk levels from fallout over this area as a whole.

CHAIRMAN Have you any reason to suspect that the strontium 90, which has come down from this recent incident, won't be yet into the milk, so that we may get a build up.

MacLEAN We certainly can't exclude that and so, those of us who are studying this thing most closely are really very guarded about anything we may say about the future of the area, certainly the immediate area, here. We do not really know enough about the ultimate pickup of the strontium when it has been washed down and reincorporated in growing shoots next spring at this stage. Although, this is one of the points that the ARC people are working on for us. Until that is clear, we shan't be in any way dogmatic about it.

CHAIRMAN Can we be assured that this is going to be watched most carefully?

MacLEAN It is being watched. It is on our programme. Work has already started.

CHAIRMAN The other elements, caesium and barium, in some respect are similar to strontium in hazards they cause, and it will be necessary to add the risks for these. Of course, this will be done.

MacLEAN This is certainly being done. Iodine is rather a special case. In these other materials, as you say, the question of additivity does arise.

CHAIRMAN All we have to do is to note that you have assured us that work is going on intensively and will continue to do so, bearing in mind this recommendation of the Medical Research Council that at present there is still quite a margin in hand.

I would like now to turn to a different subject. We would like to know whether any of our own staff in the plant have had any exposures or risks from radiation during this recent incident. I do not know whether you will care to speak on that or suggest we take some other medical person.

MacLEAN I can do no more than give general information about the external and internal radiation side of it. In a matter of detail, or expanding this, I think the Senior Medical Officer will be a better witness than myself, because I have been interesting myself, almost exclusively, in the public health side.

CHAIRMAN In that case, we would like to take your general views and opinions and follow it up by questioning the Senior Medical Officer on the Site. We therefore invite you to tell us what you think or what you know.

MacLEAN I do not have precise figures, but in round figures, as far as the external radiation exposures went, I understand that approximately 20 people had doses over the period in excess of 1R. A few of these exceeded 3R and the highest dose recorded was 5R. This was only one man. His name was Ritson.

CHAIRMAN Only one man. That was gamma radiation.

MacLEAN That was external radiation.

CHAIRMAN What about all the other things in relation to that?

MacLEAN In the first place, the calculations which were done on the expected inhalation risks indicated that these would not be very great. We have subsequently carried out a programme which has been mainly concerned with thyroid monitoring for iodine and this work has been done for us by the Aldermaston team who came on Monday, I think. Otherwise, there has been a considerable amount of activity in urine sampling and many of these samples have been handled at Harwell. We have few results on urine; I believe the first came through today, and they show very low levels. The thyroid monitoring results likewise have all turned out to be satisfactory from our point of view. The highest levels I

have heard recorded was $2\frac{1}{2}$ times the maximum permissible level for radio iodine and this is the max. permissible continuous level for occupational people. This would seem to indicate that, for iodine anyhow, the exposures of our people on Site have been quite small. I have no detailed information about other materials, but certainly in terms of the people actually working inside the plant, there is a good deal of follow up work going on, but for people outside, generally we have the feeling, which will have to be substantiated as we go along, that there has been no serious exposure of any of our people. In planning these analytical programmes we have had to draw up priorities. We have had to identify the types of analytical procedure we wanted to have done right away and the other things could be left a little longer. In some cases the medical investigations, urine sampling, have fallen into the less immediate category.

CHAIRMAN Thank you, Dr. MacLean, for this important evidence. I think it is unlikely that we shall want you again.

5.40-6.10 p.m. Monday, 21st

WITNESS: MR. CUTTS

CHAIRMAN This morning you gave us a very interesting curve and our first request is to ask you to write out a short explanation of what it is based on and we would suggest that something that Mr. Hall did for us. Would you show Mr. Cutts the way Mr. Hall set it out for us. In the same general form as I do not think it can be improved on.

CUTTS Yes, I see the general idea.

CHAIRMAN Just your assumptions and roughly the way the calculations were done and a few words to summarise what it proves.

CUTTS Yes, I can provide you with something like this.

CHAIRMAN I believe you have got some information about the control rods.

CUTTS This is a graph of the positions to which the control rods were withdrawn, how much they were withdrawn. Now, the green line here, that is the lower one, they were in fact these three rods here and the three on the other side in the rear set. The blue lines are these two lower rods on the front set and the red line is presumably merely the fine control rods, the top ones in the front set. The complete insertion of the upper rods of both front and rear sets were maintained all the time. This, as I understand it, is the place where the second release of nuclear heat was undertaken and at that time the green control rods, that is the rear lower set, were in fact withdrawn from the reactor about 400 cms. - sorry, about 440 - leaving a space in the middle not covered by the control rods of about 400 cms. The full withdrawal from the core is about 640 cms. The blue set were withdrawn nearly all the way, in fact, about 80 cms short of being fully withdrawn and the red ones were virtually fully withdrawn. You can, in fact, see from this diagram: here is the reactor itself. The green set, the rear lower set, were withdrawn to there, leaving that much space across apparently, and the blue set was then withdrawn to there and the red set to here. Fully withdrawn would be there.

Well, I think I have shown you that upside down, but it does not make any difference to the positions. The temperature recording apparently observed was there.

DIAMOND What temperature is that; which channel?

CUTTS Horizontally 57, and vertically 25 - or is it 26/57? and since the upper sets of control rods were in all the time along here and they were withdrawn in this fashion, you would expect the flux peaking to occur in this area, so it would seem reasonable to watch that temperature. On the other hand, the ion chamber, which would presumably be somewhere up here, is shadowed by the control rods, I would think.

CHAIRMAN It would be reasonable to watch that chamber but at the front, not at the back?

CUTTS All the top control rods were in according to this information. The ion chamber was at the top and back. Now, the back sets of control rods of the lower groups were the furthest in, though they were not fully in by any means. In fact, they were withdrawn some 400 cms. That was the back lower ones and the front lower ones were all withdrawn in the region of 600 cms. with a slight variation here and there. The upper rods were fully in front and back so the ion chamber would be shadowed by the control rods. It appears to me to be more or less in the plane in which the control rods are.

SCHONLAND This word 'shadowed': as far as the operator of the control desk is concerned, what effect does that have on his control of the reactor?

CUTTS Unless he had a calibration of the ion chamber with the control rods in this position telling him what the flux distribution was and total power of the reactor was, I would think that the ion chamber was not of much use in those circumstances.

SCHONLAND But suppose he was making adjustments to control the temperature, was it of any importance that the ion chamber was shadowed or not?

CUTTS I should think not if he was just controlling slight adjustments. All I am saying is, if he was inferring from the ion chamber what the total heat output from the reactor was or what the flux was in any position in the reactor, unless he had a calibration of the ion chamber with the rods in the positions we are talking of here, then it ought not to mean much to him.

SCHONLAND The power level as indicated on the chart was probably incorrect, but if he is only told to work those rods to bring up the temperature and not to exceed a certain temperature, this point of shadowing is immaterial.

CUTTS It is immaterial because he would be watching the temperatures instead of the ion chambers.

DIAMOND I don't think this is immaterial because this is the only method of measuring the pile power during a Wigner release. There is no flow of air and therefore the total amount of energy released in the reactor is indicated only by the BF₃ counter ion chamber.

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KAY I think it is equally true that the operator is working to his instructions on temperature. not on power.

He is not told he must take it up to not more than 1 MW of power, but to no more than a given temperature. but I think that the point we want to clear at this stage is that a reading of say 1 MW on the ion chamber would mean nothing at all here.

KAY It may be immaterial to the operator to carry out his instructions. but to someone who wants to make a study of what happened, how far is it out?

CUTTS In view of its position, I think a factor of 10 would not be unreasonable. It does depend on the gradients.

CHAIRMAN It might read up to 10 low, it couldn't read high, but might be out by a factor of 10.

DIAMOND It has not been calibrated against Wigner conditions. This is an opinion, Mr. Cutts, isn't it?

CUTTS Yes, it is only an opinion. I can't be dogmatic about it at all.

SCHONLAND If the operator said that the reactor was sluggish, would that have anything to do with the power level not really reproducing the effects it should?

CUTTS I could see that you might use this term. Because if your meters were reading low, if your ion chamber were reading a low power and you were withdrawing control rods. then, because it was reading low. this thing would not respond as rapidly as you would expect.

SCHONLAND Put it another way. He is accustomed to withdrawing rods in the normal operating procedure and he would have expected the power level to change by a certain amount. He would have a certain feel. In the arrangements for Wigner release. it would be a different feel.

CUTTS Yes, that would be correct. I am looking at the distance the control rods really moved in this time. 120 cms. each side; that is the most important sets. You have uncovered quite a bit of pile in this time and the effect of these control rods in shadowing I think would, in fact. make it appear very sluggish.

DIAMOND How does this information on the control rods alter the model you showed us this morning?

CUTTS It turns it into a 3 dimensional model in that we were considering the whole back set pushed in and the whole front set withdrawn and we got this picture. It seems to me now that we have a somewhat similar picture. but this time with a section drawn perhaps vertically through the pile, where in fact we are now going to shut off this part of the pile using the control rods and opening up this part, but in the light of our discussions this morning. when we were talking about the release of heat at a point here compared with a heat release at a point here, the position is somewhat different in that the peaking you would expect to occur. would occur in the region of this temperature recording. so that now from the point of view of temperatures. you might not expect to be too far adrift.

DIAMOND But you see, this temperature point is 16' back from the front face.

KAY Where would you expect the peak to occur in the horizontal distribution along the channel, now? A little bit forward of centre?

CUTTS Looking at this one you see in doing what we did this morning, that is, the putting in all the rear control rods, leaving out all the front ones, we had moved the peak of the flux distribution approximately 100 cms. forward. Now in this exercise that we are dealing with now, we are not putting in all the rear control rods, we are leaving those out, so axially we would not expect this peak to move quite so far. So I think this picture is now an overestimate. And if we now say we are going to put in all the top control rods, looking at the thing radially, the peak would normally be in the centre or, in fact, over a flattened portion here, you would expect it now to be pushed downwards a fair way.

KAY A little bit forward, but even if it was not pushed forward in the centre at all, that at 16' from the front face, that is to say 4' back from the centre of the core, at 16' measuring point, one would be well past the peak temperature. Well past the peak power output, shall we say? Even on a symmetrical distribution, you'd be well past the peak.

CUTTS Yes. I think you can safely say this.

KAY If anything, with the present disposition of the control rods, there would be a big shift of the peak flux downwards and a slight shift forwards, and the measuring point is well of the rear. 16' from the front face, or 4' beyond the centre line but in the lower part of the pile, to the extent that it is in the lower part of the pile which is alright, but it is really, shall we say, 6' too far back along the channel to record the maximum temperature.

CUTTS 6' is the right order.

DIAMOND On the simple model which we asked you to undertake this morning, it was 100 cms. shift which is 3'. Now we are saying that it is possibly 2' but it may only be 1'. We cannot say any nearer than that?

CUTTS Other than by the experiment I suggested this morning, I do not think one could get better than that at all.

CHAIRMAN What does this mean?

KAY In distance back from the front face it means that the peak temperature will occur at between 8' and 9' from the front face. Is my mental arithmetic correct?

DIAMOND No, 10' or 11'.

CUTTS Yes, that is right.

KAY The measuring point is 16'.

SCHONLAND We must completely withdraw this previous model and disregard it with its factor of 2, if it is possible. Could we replace it by another factor accounting for a shift of 4' or 5'?

KAY I think we could look at a symmetrical case and the other one

CUTTS Looking at the symmetrical model, if you take the peak as a value of 1 then the value at the measuring point would be about 0.86, in other words, roughly two thirds of the way along the channel. If we look at our example this morning, the peak began in fact taken again as 1, our measuring point would read 1.5.

CHAIRMAN So if you split the difference, something of the order of 30% is not too far out.

KAY Yes, I would think so.

CUTTS Seems reasonable.

CHAIRMAN 25-40% that sort of range.

DIAMOND This morning it read .5 of the maximum heat release of the temperature to which it would rise given an adiabatic condition. Now in the undisturbed and normal symmetrical condition, it read 0.86, and therefore now it reads somewhere between .5 and .86.

KAY Guessing at .7.

Heat release at the 16' may be .7 of the maximum heat release.

CUTTS I think we can get an estimate of what this figure ought to be because of the type of curve is reasonably linear this way. So what we have been saying is that this peak ought to be pushed this way by the order of 1' or 2'. This effectively pushes this measuring point towards this peak of the order of 1'-2' which takes us up to about here which is somewhere of the order of between .6 and .65.

DIAMOND So that would not be unreasonable?

CHAIRMAN It would only push it halfway along. If you push the top of that curve 2' to a point halfway along, then you only push one foot.

CUTTS Yes, I had not thought of that.

CHAIRMAN Yes, Well, you know the problem now and perhaps when you are writing this out, you could include these other thoughts and end up with the bracketed figure.

CUTTS Yes, I think I could have a try, although it is most difficult to make estimates of this kind.

CHAIRMAN Could you have a try?

CUTTS Yes, I will have a try. Would there be any objection to my going back to Risley to do this and bringing it back?

CHAIRMAN How long would it take?

CUTTS Probably about a day's work to make this a presentable story for you, I think.

CHAIRMAN Could you present it here on Thursday morning?

CUTTS Yes, I think I could do that.

CHAIRMAN I think that is satisfactory.

CUTTS I would like to reflect.

CHAIRMAN You want to reflect and meanwhile we have got what we think is the order of magnitude. We will excuse you and ask you to report here on Thursday morning with your statement.

CUTTS Do you wish to keep this and extract information from Gausden?

CHAIRMAN Copy and turn it in to us. Get Gausden to prove correct statement. Mr. Gausden to come in tomorrow.

6.10 p.m. Monday, 21st

WITNESS: MR. FARMER

CHAIRMAN We understand you have something further to say.

FARMER I think my statement was incomplete in that I did not cover the movements of my staff which are relevant to the incident. Mr. Dunster of my staff left here on Sunday morning and arrived at Windscale during the late afternoon. He was in touch with the Works here during the Sunday morning getting results and at that time also in touch with Harwell with Chamberlain and Marley. During the afternoon, he arrived here and helped to co-ordinate the work of district survey, to assess whether there were additional hazards from other causes, such as polonium and also to assess whether additional effort would be required on site.

I spoke to him on Monday morning. In his opinion, a considerable amount of additional effort was required although there was some reluctance up here to take advantage of the additional assistance from Harwell. I said that in my opinion all the assistance that was available in the Authority should be brought up here for district survey work, and asked him to make the arrangements directly with Harwell advising me if there was any trouble at all in doing this. I phoned Harwell that morning and advised Marley that we would be calling on all the assistance they could provide in transport and operatives and instruments and during Monday that effort was made available up here. Mr. Bowen of my staff arrived here on the Monday evening to assist in getting together the evidence and the records. The rest of my staff have been engaged on plotting results at Risley to present the information to Sir Leonard Owen and interpret to the London Office and the Press Office.

CHAIRMAN That was the point you wanted to make.

FARMER That was the point.

CHAIRMAN Any questions?

SCHONLAND I am very glad to have had this additional evidence for the record. Naturally the people here were preoccupied with their own affairs and Farmer and his staff did a great deal to bring these other people in.

FARMER I felt I had left the impression that I had been sailing around at Risley or on I.T.V. and in fact had my staff mobilised and was fairly well in touch. Dunster in fact was here all last week and was travelling up again today.

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Staff in Compliance

CHAIRMAN I am glad you have got that in, Mr. Farmer.

DIAMOND Mr. Brown is up here to get information together?

FARMER Yes.

CHAIRMAN Thank you, Mr. Farmer.

Board of Enquiry

Tuesday 22.10.57

Evidence heard from:—

Rotherham	pp 6.1 – 6.8
Gausden	pp 6.8 – 6.10
Davies	pp 6.10 – 6.11
Graham	pp 6.11 – 6.16
Howells	pp 6.16 – 6.17
Leslie	pp 6.17 – 6.18
Moore	pp 6.18 – 6.19
Davey	pp 6.19 – 6.22
Ross	p 6.23

Tuesday 22nd October – 10.45 a.m.

WITNESS: MR. ROTHERHAM

CHAIRMAN There are two things we want to discuss with you. One is, we would like your views on the stability of the uranium cartridges in the Windscale Pile as regards temperature and time. That is one matter. The other matter is quite different: it is to get a clear picture from you of the responsibilities and the organisation of the Research & Development Branch with regard to the Windscale Piles. We don't want to go outside for this. We would like to start on the technical questions first. Perhaps I could explain here what is in our minds. One possibility is that this fire in the Pile arose from a uranium cartridge getting too hot, this is one of the possibilities, and we would like to know from you and probably you would like one of your staff later on to amplify what you say about the stability of these cartridges against temperature and against time e.g. the normal operating temperature what would it be like say at 420, 450 or 500 for 5 minutes or 1 hour, anything you say which could help us.

ROTHERHAM You cannot distinguish between the complete cartridge and its environment, and a uranium bar, a bare uranium bar.

CHAIRMAN I'm talking really about, take the situation existing in the Pile where you have got the natural cartridges and the enriched cartridges and they are surrounded by graphite, say during a period of Wigner release when there is very little air movement through the Pile and we have a thermocouple actually on a cartridge. If we had some hypothetical way of gradually raising the temperature up and up and up, at what sort of temperature would you expect cartridges to start leaking and uranium to come out?

ROTHERHAM There is evidence from Pile operation that at temperatures about 400° and upwards there is an increase in the incidence of pimpling. The temperature where pimpling would become acute and the graphite interlayer would become seriously suspect is some temperature in excess of 400°. Now much in excess of 400° is difficult to say. I should certainly think, myself, that by the time you get to 500 you would be in rather serious trouble with the interaction between uranium and aluminium. That would in fact give pimpling, and as we have seen it, it has led to small bursts in the aluminium cans. It is conceivable, although I have no experimental evidence that I can call to mind, say at 500° you would get multiple pimpling and a bigger opening up of the cartridge. It's also ...?... temperature where if you did get a lot of uranium exposed, you might expect the oxidation to lead to over-heating unless there was adequate cooling provided.

CHAIRMAN You say pimpling, that must mean that time comes into this as well.

ROTHERHAM It is an interaction between aluminium and uranium, the formation of an intermetallic compound. There must be diffusion involved here. It must be a time controlled process.

CHAIRMAN Have we any experimental evidence of a statistical nature, because it is a statistical problem, where these cartridges have been heated up in a still atmosphere gradually and kept at temperature?

ROTHERHAM I don't recall any work on the heating of cartridges in still atmospheres. There has been work done at Windscale on the heating of uranium cartridges at temperatures up to about 500° in flowing air with various rates of flow but I think the rates of flow were of the order of a quarter of the normal flow in the channel. In other words, with quite a lot of cooling in spite of the fact it was hot air...

CHAIRMAN These experiments in the flowing air were taken up to 500°?

ROTHERHAM There were various temperatures between 400 and 500° designed to study the rate of the growth of the burst under flowing air conditions as a guide to the operation of the burst cartridge scanner, to see whether at high temperatures the burst was likely to go at such a rate that it would not pick up between scannings.

CHAIRMAN I suppose that until a pimple develops and the outside of the can is punctured it does not matter whether air is flowing or not. It is the development of the oxidation that occurs once it has got to the outside, where the flowing air affects it?

ROTHERHAM Yes.

CHAIRMAN You say experiments have been done up to 500°. Do you know what they showed at 400° – was it all right?

ROTHERHAM At 500° the growth of the burst would be slow enough to be picked up by 30 minutes scanning time.

CHAIRMAN At 500°, I see.

ROTHERHAM There is a report on this which can be made available from the laboratory, and the work was actually carried out by Dr. Frank Leslie and no doubt he can give you more details.

SCHONLAND Do you know the number of this report?

ROTHERHAM No, but I am sure that Nairn has it in his dossier of reports on the subject.

CHAIRMAN How long ago was this work done?

ROTHERHAM Two to three years ago, I think.

CHAIRMAN So then you say, and let me see that I have got your agreement – at 400° these cartridges ought to be perfectly all right but if there were an occasional puncture under normal operating conditions the scanner gear should pick it up, and then it would be pushed out.

ROTHERHAM I think it would be all right at 400. But 400 is certainly just about the region I should begin to worry in still air. Held in still air for a long time at 400, I think I would be considerably worried now. I am doubtful if I would have been worried six months ago or even 6 weeks ago.

CHAIRMAN The normal operating temperature is 395°.

SCHONLAND With a good graphite layer it would not occur at all?

ROTHERHAM Uranium bars exposed to still air at 400° will rise in temperature.

ROTHERHAM With any good interlayer, not necessarily graphite, it would not occur.

CHAIRMAN I think I must give my colleagues a chance to follow up the questions on this particular point.

SCHONLAND If there is anything defective in the layer, then the time occupied in the pimpling and break through stage could be anything from hours upwards?

ROTHERHAM There have been some instances which I have been able to pick up by enquiries, but they are only hearsay instances which I can refer to. It has been said that in hot air tests at Springfields, which were a standard test at one time, sometimes carried out at 350°, sometimes at 400°, that over the years, bearing in mind there must have been some hundreds of thousands of cartridges tested, I can recall two instances when in fact cartridges may have burnt. This probably refers to something like 100,000 cartridges anyway, held at 400° for 48 hours in an oven and two instances of burning are recalled by people, but this is hearsay and nobody knows whether the furnaces went... We have had two instances of cartridges behaving similarly in sagging bar tests at Springfields at 450 and 500°. that would be on a much smaller number, which rather suggests that 400 is beginning to look like a critical figure.

ROTHERHAM A really bad interlayer should have been picked up from the proving tests at Springfields. There should not be in the pile thoroughly bad interlayers, and I would not expect there to be. The evidence of how good they are must be derived from operational experience – how many bursts do we get from this cause and at what temperatures – they do occur more frequently at high temperatures.

SCHONLAND I would like a little more information about the pimpling process and its subsequent oxidation. Supposing you had a uranium cartridge and it was at 450 for 48 hours in still air, or nearly still air, can you give me some idea as to the time scale of various processes. There would be a first stage in which there would be an interaction between aluminium and uranium; a second stage in which the can would puncture, that would be a rapid stage and it would be cumulative, presumably then there would be a stage at which oxidation took place. I would really like to know what sort of rough limits one could fix for these periods, if it is at all possible.

SCHONLAND Let me explain what I am driving at. I am trying to interpret a slow process, and I have got from you the first stage of the process could occupy any time from an hour to infinity, depending on conditions. In the next stage, the perforation is rapid. I do not know much about uranium oxidation, this would proceed at a rate depending on the temperature, currents of air, flow of the air, size of the hole. Now, at what stage would you expect fission products to be emitted from an irradiated bar?

ROTHERHAM Almost as soon as the hole is formed.

SCHONLAND This is very important to me. You would not expect ...?... to be delayed. I would like to concentrate on this point.

ROTHERHAM The pimpling stage is entirely dependent on contact between the aluminium and the uranium and with a graphite interlayer the detailed mechanism of what occurs is certainly not known to me. There is a correlation with temperature. It occurs more readily at high temperatures which may be dependent on diffusion through the graphite layer rather than the mechanical break up of the layer, but once there is contact with aluminium then the process of interaction with uranium is rapid... a few hours I think, is sufficient to perforate a can once there was contact.

ROTHERHAM No, not here anyway. We have picked up defective bars where we have had great difficulty in finding a hole at all. In other words, certainly fission products are released from defective cartridges through very minute holes, if there are holes.

SCHONLAND I want to get this quite clear. Your answer is quite unequivocal; once oxidation starts in an irradiated bar, if you have an equipment capable of picking up...

ROTHERHAM Once the uranium is exposed to the air stream fission products can leave the uranium lattice through the hole in the can, but I would say it would be picked up almost at once.

SCHONLAND The can is held at 450 or some temperature at which pimpling is going to occur. Break-through, bursting will be rapid, as you say. Could the previous process occupy a considerable time – say, days?

SCHONLAND For instance, if I had a uranium bar in a stream of air and I had in the outlet stream, the discharge stream, a thermocouple and an ionisation chamber and oxidation took place, the thermocouple would indicate a rise in temperature, but it is inconceivable that the ionisation chamber should not indicate it at the same time.

ROTHERHAM The ionisation chamber would pick it up much quicker, by several orders of magnitude, in sensitivity.

ROTHERHAM I should say months, if you have got a good graphite layer. It must be so, because we are operating at 395 and very few bursts occur through this cause.

DIAMOND Do you know of any evidence of the rate of heating as distinct from the absolute value on the incidence of bursting?

ROTHERHAM I do not know of any experimental evidence of it, no.

SCHONLAND Would you expect a sudden increase of temperature to be more damaging than a slow one?

ROTHERHAM Only through its mechanical effects, thermal expansion effects and things of that sort. I can't think that chemically it would...

CHAIRMAN On the mechanical effects, we have been talking about temperatures of 400 and upwards. Would the mechanical effect be severe if the temperature of the whole cartridge was raised from 300 to 400° or thereabouts in a matter of a minute rather than very very slowly?

ROTHERHAM I do not think it would be considerable at all, not during the heating. After all, the aluminium would tend to expand more rapidly than the uranium and the stress on the aluminium would not rise during heating: presumably the stress could rise more rapidly during rapid cooling.

CHAIRMAN Supposing this heating were done by being in the pile and the power were put on, then the heating is being developed inside the uranium, and now the heat has to get from there to the aluminium. Would that stress the aluminium?

ROTHERHAM Yes, it would, the stress is not important, it is the strain that is important. The aluminium would in fact accommodate itself, apart from any local regions where the strain might be concentrated which is a design defect we have seen in some cartridges where you tend to get the strain concentrated near the ends. This is a different type of failure from the pimpling failure.

KAY Could I ask about the incidence of pimpling failures during normal operation with the maximum temperature of 395. Are there any figures available of the total number, and also the proportion of bursts that could be attributed to...

ROTHERHAM This is data which I could imagine is completely available here at Windscale. I think there must be hundreds of different types of failure which have been examined here at Windscale. Some are due to pimpling. Generally speaking I would think that the cartridge in which a failure occurred... and there will be data to show what the temperature was. Perhaps you could consult Mr. Ross on this. We have recently had two types of failure, Mk. 5 and Mk. 10, and one of these was pimpling, but I can't recall which.

KAY This was the most ordinary type of failure of the cartridges examined?

ROTHERHAM Yes, that is so. It occurs mostly when operating at high temperatures.

CHAIRMAN For the record, Mr. Ross has provided the documents for which Dr. Schonland has asked. I would like to follow up the possibility, a remote one, that when the power was put on there was as you know, a first heating and then a second nuclear heating. During the second nuclear heating when everything was warm, the uranium cartridges were

perhaps 300 or 350 and thermocouples showed that some of them went up by 80° within 3 minutes. Does the uranium have any peculiar expansion coefficient or anything odd about it in this region?

ROTHERHAM No. The only peculiarity which could arise would be in going through a phase change at 650.

CHAIRMAN It is a point you would not normally think about, instability of a cartridge where the heat suddenly came from inside and the inside was expanding before the outside had got very warm. Therefore there is a bit of a bursting pressure which may put local stresses and cause a local failure?

ROTHERHAM Yes. The expansion coefficient of aluminium is higher than that of uranium, and ordinarily one would assume that since they are reasonably tight to start with, that at the operating temperature there is a gap between them. That would have to be taken up first. It is difficult to think that a rise in temperature of say 50-100° would impose sufficient stress on the aluminium to lead to a failure. We can do some calculations on this without difficulty.

DIAMOND It is possible that under long irradiation the uranium would have grown in length and was taking up, and putting the aluminium skin in tension?

ROTHERHAM This is the second type of failure that is due to concentration of strain in the aluminium. It ordinarily occurs in the cartridges which are operating at low temperatures. Obviously there is a possibility that you could have growth at a low temperature and in this particular operation you might possibly have brought the growth up suddenly to a high temperature, when the ends of the can are tight on the bar. There is that possibility.

DIAMOND What sort of temperature is this likely to occur?

ROTHERHAM The growth of uranium occurs mostly at 200°C.

DIAMOND So it is possible to conceive that some of these fuel elements at least are pressing against the ends of the aluminium keeping it in tension, and then if you impose a sudden increase in uranium temperature, so sudden that in fact the temperature had no chance to get out to the aluminium, it might push the end off.

ROTHERHAM Yes, I think you must consider that possibility. I would like to work out just how much strain is associated with this particular heating which you are postulating before I was definite about it.

CHAIRMAN The growth at 200° - is it appreciable at 300 MWD/T material or does it occur at higher temperatures. Is it linear with dose?

ROTHERHAM It is roughly linear with dose. It increases progressively with irradiation. The extent of it of course in a completely random orientation of crystals in the uranium is negligible. We are producing cartridges with a completely random ...?... but statistically there must be some preferred orientation.

CHAIRMAN If we want to have a little calculation done on this, who is the best man to do it?

ROTHERHAM I think it would best be done by the group at Springfields under Hardy who have been set up to do exactly this kind of calculation. In the Physics Group at Springfields there is a group who spend their time calculating stresses and strains on cartridges.

CHAIRMAN If we reflect on this and find we would like to have something done we would ask Dr. Hardy to get some of his staff to work on it for us.

SCHONLAND I am worried about the question of fission products. We have an oxidising bar of aluminium, a low air stream and a thermocouple and ion chamber at the other end. Mr. Rotherham said that in his opinion the ionisation chamber would indicate even more rapidly than the thermocouple that oxidation was taking place. Would this not depend on the rate of the air flow. Is it possible that certain fission products at that temperature...?... during oxidation?

ROTHERHAM The detection of a burst depends on the transfer of activity through gaseous fission products. You have to have enough air stream to take the fission products from the point where they are originating to the detector.

SCHONLAND If you have enough air stream to affect the thermocouple you must have enough to affect the detector.

ROTHERHAM I think not necessarily. You could have an air stream which was not taking the fission products from the point of origin to the detector; it could be an air stream in the opposite direction. It would still affect the thermocouple.

SCHONLAND Could you explain? Here I have my experiment in my channel and I've got an oxidising fuel element at the other end of the channel, I'm under conditions of very small air flow. At the other end of the channel I have a thermocouple.

ROTHERHAM My point is that in fact gaseous fission products are released through the hole in the can wall. They could be swept away from the detector by the air stream. There would have to be an air flow in the right direction to take them to the detector or they wouldn't be picked up.

Tuesday 22nd October (10.10 a.m.)

CHAIRMAN You have put a new thought in my mind. If a little burst occurs in a cartridge and fission products start coming out you say it is gaseous products that come out first. But in due course it will be followed by oxide particles which might not be carried off by the air, but the first thing that comes is something gaseous, is it?

ROTHERHAM I don't know if the first thing that comes out is gaseous but the detection depends on the transfer of activity through gaseous fission products. There are other ways of doing detection, but what exists in the pile does depend on that.

DIAMOND This is a scanner gear.

CHAIRMAN But the Scanner Gear was not being used. The only detection of activity we have got is on the pile stack filters.

ROTHERHAM Is that the only one? Are you sure the one at the bottom of the chimney is not operative? Well then, it's a different question altogether. That presumably does depend on the transfer of particulate activity. I think I am being led to a different question from the one I was asked. Given the appropriate detection gear which is the scanner gear, the answer I gave was correct. I do not say you would pick it up on the top of the stack more readily than a thermocouple would.

CHAIRMAN There is a point on the activity that is very puzzling. The air was all very stagnant, we do not know what the currents were. But at a certain time the meters on the filters at the top of the stack showed a fairly rapid rise of activity. Then the next thing was had it decreased rather appreciably over an hour or two until later on it went right out. What intrigues us is why did it decrease?

ROTHERHAM What was the detection method used there?

CHAIRMAN It is an ordinary ionization chamber.

ROTHERHAM Which would detect what?

CHAIRMAN Gammas. The thing that is puzzling me is, if it is particulate and sticks to the filter and the ionization chamber is recording that, I would not have thought it would decay. It would stay there.

ROTHERHAM If it was particulate you would expect it to mix fission products ...?... up there. It would decrease slowly.

CHAIRMAN If it was something gaseous it would go through it.

ROTHERHAM Well no, gaseous fission products decay, giving solid fission products, which are collected and...

CHAIRMAN Yes, but if it takes only a few minutes leaving here to get to there then it would go through the filter, and maybe all that ionisation chamber was reading was a passage of gases past it which gave gammas. But then the difficulty is to understand why it is only gases and not particulates.

ROTHERHAM You could only get very small particles up the chimney because it's such a small draught.

CHAIRMAN Yes that's right.

ROTHERHAM It's surprising to me that there's not more particulate activity being emitted, because the filters are not very good on very small particles.

CHAIRMAN I am sorry to press my point but I did ask the wrong question and we got on to scanning gear. I will put my question again: I have a slowly oxidising uranium bar, the air flow is small, and there is a thermocouple and ionisation chamber, but this is only responsive to gamma rays.

ROTHERHAM It is a question which cannot be answered unless you put it in real terms. What is the detector and where is it? And what does it respond to?

SCHONLAND The detector is this ionisation chamber in the filters.

ROTHERHAM I do not think I know enough to be able to say that that would be more sensitive than a thermocouple. I do not know what the sensitivity would be.

SCHONLAND In this way you would expect both particulate matter and gaseous matter to be emitted during the slow oxidation.

ROTHERHAM I think this is unquestionable.

CHAIRMAN You have given us this valuable information about cartridges and I should like to know if any of your staff could amplify this. Can you get more information by calling your staff, or have you given all that there is.

ROTHERHAM I feel sure I have not given you all that there is, but not being clear on the particular piece of information you want to pursue it's difficult to say where you could find it. The only systematic experiments and they are not very systematic, as you will see on reading this report, relating to heating of cartridges, is in that report. Most of the information about cartridges is derived from operational experience.

CHAIRMAN Well we will read these reports.

ROTHERHAM That was only an experiment carried out for a very limited objective and is certainly not directly related to the generalities of Pile operation.

CHAIRMAN Now I would like to turn to the other question. I would like you to tell us what are the responsibilities of the R.& D. Branch with particular reference to the operation of the Windscale Piles.

ROTHERHAM Well formally the relationship between my Branch and the Operations Branch is quite clear. In any operating plant, i.e. any plant which is in the hands of the Operations Branch, we do no experiments except at the request of the W.G.M. Indeed he pays for all that work against his overheads. That is why the responsibility for the work ultimately rests with him. That is the formal arrangement. That does not mean that there is not a good deal of give and take. I do not think my staff would answer any questions, feel in any way inhibited in commenting on the operating techniques and that would be done at the Works Technical Committee. The WTC would decide whether any experiment that we might propose was acceptable or not, in which case it would be sanctioned by the WGM. If in fact my staff felt it was sufficiently important and it was declined by the Works Technical Committee, they would report it to me and I would take it up with Mr. Ross. If we did not agree I would take it to the Managing Director. Formally that is the arrangement but there is a good deal of give and take, and a good deal of informality in the relationships between the two Branches.

CHAIRMAN I understand that. I would like perhaps to take an example to see how this operates. Over the last two or three years the temperature at which the cartridges have been running has been raised several times. When the decision was made to increase the running temperature many sections of the I.G. must have been concerned in that decision. How did your branch contribute to that?

ROTHERHAM Again I recall very well that the proposal to run these piles, I think to take it up to 375, and that did come up at the Production Executive. It was a proposal put forward by Mr. Stewart who was then working on the Operations Branch Staff. It was discussed by the Production Executive. In theory we jointly agreed it was a satisfactory thing to do. In practice of course the Authority is organised in such a way that what the Managing Director says goes. The Production Executive is not a corporate body; it is in fact an advisory body to the Managing Director and the ultimate decision is taken by him. For quite the wrong reasons, now, I recall that I opposed the decision. Afterwards I thought it was the right decision. Now I'm quite sure it was the wrong decision again. But that was the sequence of events in my mind.

CHAIRMAN I would like to take a second example. The Wigner Release of energy in the Pile is an operation with peculiarities in it and had to be done very carefully. What was the responsibility of the R. & D.B. in giving advice about the way in which the Wigner Release was to be done?

ROTHERHAM The operation of the Pile is the Works responsibility. We have always tried to guard against the situation where we took away from the Operating Staff the technical responsibility for what they did. I'm sure they wouldn't have wanted it and I'm sure they still wouldn't want it. I would say therefore that our responsibility was to provide such additional technical service as they felt was necessary to carry it out safely, and the definition of what was necessary would finally fall on them rather than us. I am not clear that the detail of this release of Wigner energy is considered in detail anywhere outside the Operations Branch. But, if so, my staff would be in exactly the same position as any other operation: to give advice and discuss the question.

CHAIRMAN There is one point here that we have noted and has caused us some anxiety. [during] The normal instrumentation of the Pile, the instrumentation that is there, with the thermocouples at different places and so on, all of that makes good sense as regards the normal operating conditions of the Pile. But it does not make good sense during a Wigner Release. We can see why this was, it was put in in the early days when the Wigner Release was not known, and things like this have happened:- that the uranium temperatures are measured 16' back from the pile where the temperature during normal operation is greater. But during Wigner Release they tried to make the energy of the pile come forward to release it where the Wigner Release is, and there are no thermocouples on the uranium fuel elements in that part of the pile. Therefore we can say that the instrumentation is not right for a Wigner Release. Now what worries me is that it seems clear that the instrumentation should have been reconsidered. Now I think you said you would have expected the Operations Branch to have requested you to look at this and advise them. You did not

have any right or you did not go into them and say "look this is not the best way"?

ROTHERHAM No we did not. I think that is the position. I would certainly expect it to be the position.

CHAIRMAN I am not criticising any individual man, would you have expected your staff up here to have thought these things out and gone to Mr. Davey in his Works Production Committee.

ROTHERHAM I think certainly I would and certainly I do not think it has been done. There is a lot of history of this of course and I think it is quite clear that there is something seriously wrong here: but how it has arisen is a matter of history.

CHAIRMAN There is one other point. When a Wigner Release has been done a great many records are in existence; the wall is covered with them, and these records must contain valuable knowledge. Graphite temperatures, Uranium temperatures, and what is happening in the Pile. We have not discovered here any indication that those records, after a Wigner Release, have been studied in great detail by either your staff or by Production staff to see if there are any lessons to be learned for the next time.

ROTHERHAM I am sure that they have been studied by both staffs, but not sufficiently intensively. I don't think there's any doubt that they are looked at. There are a lot of records in the Authority which are never seen after the day they are taken. These are not in that class, they are looked at. Some lessons are drawn as a guide to the next.

CHAIRMAN In the November Wigner Release of last year in the Pile, Graphite temperatures of 410 or 420°C were recorded, and we have found that Harwell and Amersham people who are submitting cartridges to go into the Pile seem to think that maximum Wigner temperature was 350°C.

ROTHERHAM Yes I think we all thought so.

CHAIRMAN I think you have answered my questions Mr. Rotherham. Any questions?

SCHONLAND I should like to ask Mr. Rotherham a question, his Branch does study Wigner release later. Can he name an individual in charge. Is it Bell?

ROTHERHAM I should say Bell is the man who does most of the work.

SCHONLAND Is there no one to see that Mr. Bell is doing the job properly.

ROTHERHAM Well there are people above Mr. Bell whose duty is to see what he's doing and that he's doing it right.

SCHONLAND Could I have the names.

ROTHERHAM He would fall in the Group now managed by Mr. Moore, who has been recently transferred here. Previously we didn't have a research man at this site.

SCHONLAND First, if this Board found that for some reason or other the Wigner Energy in a proportion of the Pile had been allowed to accumulate over 80,000 MW days, and wished to ask why had this point not been taken up or wasn't considered important, we would ask Mr. Moore.

ROTHERHAM Yes I think so. I think we should realise here that there has never been a strong Reactor Physics Group at Windscale or anywhere in the Industrial Group. It has customarily been a responsibility discharged by Harwell. Bell was the best man I had. About three years ago they brought in Curtiss who was found unsuccessful, and 18 months ago I brought up Mr. Moore and since that time we have strengthened the RPG here but the main interest of that Group has been in future reactors and not Windscale reactors. This is an attitude of mind I would think in my Branch which is perhaps most open to criticism: that they have thought more about the future than the past, which is a natural thing. We still have not got a strong Physics Group.

SCHONLAND But apart from Reactor Physics, which is a very wide subject, you do admit, agree, Mr. Rotherham that your branch is responsible and is the only branch responsible for full study of the actual operations of Wigner Releases in the I.G.

ROTHERHAM That is the position. We are not the only Group which is concerned with the study of Wigner release. Operations Group and the Engineering Branch are concerned with it.

SCHONLAND There are three Branches that study it.

ROTHERHAM The I.G. is responsible for this value.

SCHONLAND And in the IG three Branches have diagrams like that on their walls?

ROTHERHAM No, Operations Branch have diagrams like that on their walls.

KAY Could I ask a question? At the start of the Wigner Release arrangement, I think I am right in saying that the first Wigner Release took place in September, 1952. Did the R & D Branch come into the picture completely at that time? The subsequent procedure is based on what happened in that accidental release. Did the R & D Branch take a full part in working out the procedure?

ROTHERHAM No I would say that they had a very minor part in the original ones.

CHAIRMAN Then who dealt with the technical aspects of Wigner release in that first occurrence?

ROTHERHAM The Technical Group in the Operation Branch at ...?.... and the Scientific Service was provided exclusively by Harwell. I think it was shared at that time.

KAY So that the first full technical studies of the implications of Wigner release were not made by R & D Branch but jointly.

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ROTHERHAM We had no responsibility whatever at that time for reactor physics at all, and it was subsequent to that it was felt to be an improvement to build up gradually a team here to take that responsibility. We had no one at the time who had special knowledge of Wigner energy. We had no experimental work at that time.

KAY What date did the R & D Branch here actually take responsibility for giving advice on Wigner Release?

ROTHERHAM It is very difficult looking back into records, but it must be two or three years ago that we started gradually taking over the experimental work from Harwell. The experimental work was to some extent done up here under Harwell supervision.

KAY But your Staff inherited a technical method for Wigner release which had in fact been evolved by a different party.

ROTHERHAM Yes.

10.35-11.05 a.m.

DIAMOND Mr. Rotherham did suggest that calculations might be done on the rate of strain in an aluminium cartridge due to sudden heating of the uranium, supposing the two were in contact before the heating began. I think it would be useful to have some calculations done on this if it wouldn't take too long.

CHAIRMAN That was in my mind when I said to Mr. Rotherham we would think about that and he then said the man to get in touch with was Mr. Hardy. If we want it done we have already said that we will get in touch with Mr. Hardy.

DIAMOND Sorry, I missed that. Then there is the question, Sir, of anything Mr. Rotherham could tell us about isotope cartridges.

ROTHERHAM Yes, I can tell you that they are dangerous!

CHAIRMAN Could you make the question more specific?

DIAMOND How many of these isotope cartridges which were in the pile originated from the R & D group?

ROTHERHAM They don't originate through us, but its procedure in evolving a new cartridge is through one or other of the Technical Committees which exist. There is the Cartridge Working Party on the engineering side and the Works Technical Committees which ultimately determine what form the cartridge will be.

Again, we are required and do carry out experimental work to justify whatever design is put forward. If we take the specific Mark III AM cartridge, if I am not mistaken that was originally considered by one of the Engineering Branch Working Parties and I think I am right in saying that the engineers did enquire into what the compatibility of the alloy and the aluminium can in which it is contained would be. Again, I am fairly confident that they were given the answer that there is some diffusion of aluminium and magnesium at quite low temperatures 250 or 300°C. but this is insignificant and not until a temperature in excess of 350° is reached is there likely to be any serious interaction between aluminium

and magnesium. That I am pretty certain was discussed in detail at the Cartridge Working Party. At some stage the deliberations of that Committee were turned over to the Works Technical Committee but I do not know whether that is so. Did you know that?

DAVEY I cannot answer that offhand.

ROTHERHAM I don't think it was. I think it was always the responsibility of the Thermal Reactor Cartridge Working Party.

DIAMOND Is that Working Party still in existence, and the Chairman?

ROTHERHAM Yes. The Chairman these days, I think, is Turner.

CHAIRMAN Name of the Committee?

ROTHERHAM The Thermal Reactor Cartridge Working Party.

DIAMOND And as the different Marks of these cartridges come up as they are evolved, are they all considered by this Working Party?

ROTHERHAM In theory certainly yes.

DIAMOND Has anyone got the job of checking experimentally the behaviour of all these cartridges as they are evolved?

ROTHERHAM The function of any design committee, and this is in fact a subsidiary of a design committee, is to ensure that the Engineering Branch has got adequate evidence on which to base its conclusions. And the Working Party would consider data presented by the Engineering Branch and data presented by my Branch to determine whether they thought adequate data was available. In this particular case they decided that there was adequate data available to give this particular cartridge clearance to temperatures up to 350.

DIAMOND Am I asking the right question?

CHAIRMAN Please proceed.

DIAMOND Does the Working Party consider cartridges originated elsewhere, outside the Industrial Group?

ROTHERHAM I think the answer to that is no.

DIAMOND You said they were very dangerous these isotope cartridges.

ROTHERHAM Well, there is the reaction between magnesium and aluminium which occurs reasonably rapidly at the eutectic temperature 436 which is not much above the operating temperature.

CHAIRMAN And, of course, during an earlier Wigner release the temperatures did go up to 420. That is a fact.

ROTHERHAM It has been known for a considerable time that temperatures in excess of 320, the melting point of lead, had been exceeded in the Mark II AM cartridge. It appears to be a rather critical temperature from experimental work we have done since this incident.

We have not had cartridges perforating at temperatures below 440 in the work at Springfields, but whether you would at longer times I do not know.

DIAMOND The work going on now since the incident on these cartridges, I think we asked for a progress report on this did we not? Saddington's work.

ROTHERHAM There is work going on here and at Springfields.

DIAMOND Saddington here, who is it at...?

ROTHERHAM Work falls under Hardy, the work is being done by Dr. Lawton.

DIAMOND Yes, it falls under Hardy. I think that is the lot, thank you.

CHAIRMAN Dr. Rotherham, you are in a position to have seen a lot of the ideas and thoughts that have come up. Are there any technical points to which you would like to draw our attention. We have a very formidable analytical problem here, and if any points seem to you of importance, we would like to know about them.

ROTHERHAM I personally have been looking into the work we have initiated, such as how an AM cartridge will perforate, and the similar work you have laid on here up here since the enquiry started. I feel that the work you have asked Hall to do, the analysis of what might happen in these channels does, in fact, give a very clear indication of what the sequence of events was. I think that is the right work.

SCHONLAND Mr. Rotherham, what is the sequence?

ROTHERHAM Well, Hall I think shows... I think you have all the papers... but it seems to me that it does show that you can get an AM cartridge which will be perforated at temperatures of 440 anyway which is within striking distance of the temperatures you have got: if perforated it will burn under the conditions in the pile. If it burns, the temperature in the channel will rise, locally, to something of the order of 700-800°C, and again I would have thought with graphite under the conditions, the pile might very well burn under these conditions.

CHAIRMAN Is there anything further?

ROTHERHAM No, I think not.

CHAIRMAN Well, thank you very much. I can see no further questions that we might want to ask.

ROTHERHAM I shall stay up here all day and go back tonight unless you ask me to stay over.

CHAIRMAN I think that unlikely, but if we do...

ROTHERHAM I'll be here. Probably until 5 p.m.

12.10 a.m.

CHAIRMAN Good morning Mr. Gausden. This is getting a bit like perpetual motion! We asked you to get a drawing or statement about the position of the control rods during the

second nuclear energy release - can you produce this information?

GAUSDEN I have it here.

CHAIRMAN Could you tell us what it means?

GAUSDEN This is the second heating here, with the control rods running out. It is done in three different colours, representing the front rods, the back rods and the fine rods.

CHAIRMAN That shows the distances as a function of time with the time along an ordinary "x" axis and distances there. I think that is satisfactory. Are there any questions?

GAUSDEN This is the control rods running out to reach criticality. At the point where we had a temperature peak, this is the immediate reversal of the rods shown here in green - 2 and 4 banks - to control that without moving any further on 1 and 3 rods and no movement on the fine rods. (Using diagrams) These controlled on that. Then to keep the temperature, these rods were kept more or less steady and these rods gently run out in this fashion.

DIAMOND And does each of these lines in the diagram at the top represent one rod?

GAUSDEN These represent the rods in question. Three pairs of green representing 2 and 4. These are the back ones.

DIAMOND Does each line represent two rods?

GAUSDEN The green line represents three pairs of rods.

DIAMOND These lines here.

GAUSDEN Each line there represents one rod.

DIAMOND One rod - so there are 12 altogether.

GAUSDEN 12 at the front and 12 at the back. Coupled with that is this power level, drawn out to a linear scale.

CHAIRMAN That is done as accurately as you can measure it off the records? Thank you very much, that is very useful. Now there was one other question which Dr. Schonland asked you, which related to some activity records. Would you like to frame your question, Dr. Schonland, and Mr. Gausden can answer it.

DECLASSIFIED

SCHONLAND The question I would like to ask it about the argon meter.

GAUSDEN I have the record here.

SCHONLAND Does it show any indication of activity, when did it show an indication of activity?

GAUSDEN It is a completely steady record right up to half past one on the Thursday afternoon.

SCHONLAND That is 1330 on Thursday afternoon.

CHAIRMAN A long while later than when the stack meters were showing it?

GAUSDEN Yes.

DIAMOND Then this records a rise a long time after the stack meters?

SCHONLAND The stack meters started at 0420 hours.

CHAIRMAN The stack meters started 9 hours before.

SCHONLAND I would like some information about this meter. What does it do and how does it operate?

GAUSDEN It extracts air from the chimney through into a hollow tank with an ion chamber sitting in the centre of it, and then passes the air straight back into the stack again.

KAY By what mechanism?

GAUSDEN This is done by the exhausters, the same air as draws the air through the burst cartridge detection gear: the same pull.

KAY It's sucking on a point in the chimney?

GAUSDEN Yes, quite separately but...

KAY Is this exhauster on the whole time?

GAUSDEN No. It was not switched on until just about this point here.

KAY So the fact it didn't read before merely indicates that the exhausters were not working?

GAUSDEN There was no airflow through the chamber.

KAY So even if there had been air going up the chimney, if the exhausters were not switched on, ...so we cannot say anything about the argon activity prior to the point where the exhausters were switched on.

CHAIRMAN It showed activity at the moment they were switched on?

GAUSDEN I get the impression, in fact, that it showed it before. I think from the records it was put on about 2.30.

DIAMOND The records show that the four shut down fans were switched on at 1345, (*NOTE: this statement of 1345 may have been contradicted*) and the turbo exhausters at 1430.

GAUSDEN That is where we get an immediate rise right up the page here. One hour prior to that is this increase here.

KAY Suggesting the accumulation of activity in the chimney which was percolating through into the...

GAUSDEN I think that is the most likely explanation, yes.

SCHONLAND One would not have expected anything before, but there is nothing before?

GAUSDEN Nothing before at all.

DIAMOND Could I ask Mr. Gausden for a list of these various fans and exhausters, to make quite certain I've got them right. There are the main blowers, shut down fans, and booster fan? That is shut down plus booster is it?

GAUSDEN They run in conjunction, the booster is never put on without the shut down fans. These are all on the inlet side.

DIAMOND Then there is the turbo-exhauster.

GAUSDEN There is one on the extract side, there are in fact two, but one is a spare. This is pulling air either through the scanner gear, or through the argon chamber - both, in fact.

DIAMOND This is steam driven?

GAUSDEN No, electrically driven.

DIAMOND I just wondered why they were called "turbo".

KAY It refers to the fans, as opposed to reciprocating gear.

SCHONLAND Mr. Gausden, under normal operation you detect burst cartridges with the scanner gear?

GAUSDEN Yes.

SCHONLAND In the case of a small burst, and you have had them, does it show in the stack meters?

GAUSDEN No.

SCHONLAND It never shows? I have one other question, about Mr. Toole, who took over the controls for the second heating. Has he ever been connected with a Wigner release before?

GAUSDEN Yes, he has. He is one of the longest standing foremen in the group. I have one other small piece of information. I took the opportunity yesterday of removing a couple of charge plugs from Pile 2 and took an irradiation measurement, flush with the face of the pile at the entrance to these holes at approximately the same position as a reading was taken on Pile 1 when we first went up to the charge hoist. The reading I got yesterday was 35 r/8 hrs. The reading we got on Pile 1 was between 20 and 40 r/8 hrs, which I think compares pretty well. I tried to compare the shut down conditions on Pile 2 with conditions on Pile 1 at the time.

CHAIRMAN You have compared how Pile 2 was when you took out these plugs. You put the meter right into the plug hole?

GAUSDEN Right up to the face of the hole.

CHAIRMAN And you have got 35 r/8 hr and that compares with 20-40 on pile 1...

GAUSDEN When we first extracted plugs on Thursday.

KAY And saw that the uranium was glowing?

CHAIRMAN I do not know whether we can deduce anything from this, although there was glowing, there was only a comparable amount of activity. The fact that it was inside the can doesn't shield it from gammas does it?

GAUSDEN Oh no. At the back of my mind, I was interested in saying well, we know that Reactor 2 is shut down and is completely dead, if one gets a comparable reading on Reactor 1, one should be able to say the same for that.

SCHONLAND You will recall that you told us, and Mr. Toole confirmed, that the pile was sluggish.

CHAIRMAN One said sluggish and one said touchy.

SCHONLAND When he did the second heating, what did you mean by touchy?

GAUSDEN My version of it is - I admit I was not in the control room at the time - by touchy I mean slow to respond initially and then a quick response after that, which was not quite the normal response you would expect.

SCHONLAND Following on that, I can understand that it would not be normal, because in normal operation you have not got these back rods in. And reactor physicists can explain to us that the power meter is shadowed by the back rods, it would not behave normally. That's why I asked if Mr. Toole had had previous experience, because if the arrangement of the rods on previous occasions had been similar on this occasion, Mr. Toole would have had experience of this touchiness or sluggishness. Have you used the rods in these same general positions on previous occasions?

GAUSDEN The same general positions, yes.

SCHONLAND The object being to push the heat forward.

GAUSDEN Yes.

KAY When you are doing this operation, pressing buttons to move out control rods, approaching the point where you expect it to diverge, what meter or instrument do you look at?

GAUSDEN We are doing two things - the man at the desk is looking at his main power level. Also he has an operator looking at one temperature, and a man standing at the other temperature recorders in the main panel, telling him as each point prints what the temperatures are.

KAY At what point does the man pressing the buttons, on the control desk, say "Ah! The pile has diverged?" When he sees a movement on the power meter, or what?

GAUSDEN Yes, because that always comes before a temperature increase.

KAY He sees the power meter go up and then he...

CHAIRMAN That is all, Mr. Gausden.

MR. DAVIES' EVIDENCE

CHAIRMAN We don't know what it is you are going to say to us, but we did say if anyone in the Works thought he'd noticed something, they would call it to our attention. You have been good enough to come forward, and will you please tell us in your own words.

DAVIES I was on leave when this incident happened, and I got back on the Sunday after the incident. Actually I went to see my nearest contact in Seascale, my foreman. We had a chat about things and he mentioned to me that during this shutdown one of our jobs was to take a liner tube out of O.C.3 on the pile which is the PIPPA 2-rig. This is an open cycle rig where air is discharged into the pile over a cartridge, and then joins the main air flow through the pile except for the sampling arrangement, which takes air from around the cartridge and samples it just to see if we've got a burst. He mentioned that when they took this liner tube out (they were taking it out because the rig is finished), there was a small film of oil on the side of the liner tube. We wondered whether there was anything in this. This rig has been running for about three years under the same conditions and I more or less discounted it. However, thinking about it, I decided to have a word with Mr. Gausden about it, to see if he thought it had any bearing on it, and then spoke to Mr. Ireland. Since the rig has been running for about three years, and the conditions have not changed, I naturally did not think there was very much in it. We knew a very slight amount of oil was passing over into this rig, and I thought it might as well be mentioned to Mr. Gausden.

CHAIRMAN What is your actual job?

DAVIES I am Group Engineer, Maintenance, on the pile.

CHAIRMAN Have you seen oil on it before?

DAVIES I have not seen oil on this liner tube before, because this is the first time it has been out since the installation of the rig. The situation applied to the rig is as follows: we get our compressed air from the main group compressors and I would assume there is an oil separator at the main group, because oil is supplied from these compressors to all parts of the factory. It comes from the compressors up to the pile group, which is a distance of about half a mile of travel, and then it comes into what we call the east rear court and from there it goes up the side of the pile. It goes then through two filters. I forget the name of the filter but the general principle is that incoming air passes through a fine gauze and the outgoing air is led off from a pot which surrounds the gauze. It then goes through a series of reducing valves and control valves until it enters the valve chamber on the rig itself. About three years ago, when we were starting this rig up, we did have some trouble in throttling the air supply because the pipes were a little too large for the quantities we required. We changed a section of the pipe and

put a new throttling valve in, and the shop manager on experimental rigs noticed a small amount of oil on this valve at that time. Nothing was done about it and the rig carried on a further two years operating under these conditions. Recently we did further work on the pile roof in putting in a ring main so that any rigs could be supplied by this compressed air supply. Because we were leading air to other rigs, we increased the pipe size and we put new filters in. About six months ago, when we examined these filters we noticed the inlet gauze was sticking. My foreman noticed it and said what should he do about it. I saw the shop manager responsible for rigs, and told him of the situation. We all went down to examine the filter. There was obviously some oil there - very little, just enough to make the inlet filter sticky and a little had dropped into the pot below. As a precaution we cleaned it out and put it back and the shop manager O.K.'d this. That was the last we thought about it until at the shut down, when this liner tube was brought out. It came out fairly easily and oil was noticed as a thin film on the outside of the liner tube. The rig was closed down on the Monday before the Thursday, (I was not there at the time - this is secondhand information) so that means to say that oil on the liner tube would come out with the liner tube, and no further air would get into the pile from the Monday to the Thursday. There have been cases before of slight traces of oil from the blowers, but most of that oil comes out in the inlet cascades. It would not be more than about a cupful in six months.

CHAIRMAN This particular tube had been in hole C.3?

DAVIES It was installed when the rig was installed. I can't remember the exact date, it was the second rig that was put into the pile, about three years ago.

CHAIRMAN Thank you for calling this to our attention. We will consider it among the other facts we have to consider. We appreciate your coming to us with this information.

DIAMOND I want to confirm that the air discharges having been through the rig into the main cooling air.

DAVIES I'm not sure what the arrangement is at the bottom of the ...?... that this liner tube comes in to, but it must be discharged into the air stream somewhere in order to get away. The only time we take anything back is when we have a half-inch sampling pipe connected to vacuum pumps, which sample a small percentage of the air in the vicinity of the cartridge.

KAY Can I check the position of this hole and the depth of penetration into the core?

DAVIES The hole is No. C.3; we would estimate that the liner tube is about 32 feet long, with an $3\frac{3}{8}$ " outside diameter, and maybe 1/16 to 1/8 wall thickness. This is a 4" hole, but these points should be checked.

DIAMOND Have you examined this experiment - have you taken this out?

DAVIES The experiment failed about six months ago. I am not sure when air was last put on this rig, but the cartridge jammed in the pile I would say at least three months ago and it was wound out into the concrete. It could be that we have not had much air down there for about three months. The

only work which has been done since then is the top section of the rig that is the shielding and so on has been removed. The liner tube will have been removed and I should think slung over the side of the pile. This is the normal practice before disposing of it.

DIAMOND Is the PIPPA cartridge still there?

DAVIES The PIPPA cartridge will have been disposed of by now. The principle is that if there is anything the matter with a rig, and we sometimes get winding trouble. We always bring the cartridge first into the concrete, then brought into a coffin, the shutters are closed and the coffin is taken away. I cannot just remember the sequence of events. The likelihood is that air might not have been on that rig for the last couple of months.

11.35 - DR GRAHAM

CHAIRMAN Sit Down Dr. Graham

(Introduction to members of the Committee)

Will you please begin by saying what your job is in the Atomic Energy Authority.

GRAHAM Well, I am called the Senior Medical Officer, my duties are connected with Windscale Works primarily. I have a Medical Staff who have duties connected with the health of the workers. In addition to ordinary occupational medicine, I also have a great interest in the radiation side of things. From time to time I attend outside meetings and I have contacts with the other factories in the Industrial Group and also in the other Groups.

CHAIRMAN You are Senior Medical Officer here, at Windscale? Probably the best way to proceed would be to ask you to tell us what you had to do when you knew that something was going wrong.

GRAHAM I received a phone call from the Deputy Works General Manager at approximately 9.15 p.m. on Thursday, 10th October. The message was to the effect that there was some difficulty in Pile 1 and could I come in if possible before the shift changed in half an hour's time. I arrived about a quarter to ten on Thursday night. I think the prime purpose of calling me in was that we expected that there would be a lot of contaminations to be dealt with and I might have to organise extra help or accommodation for doing this. This did not happen though. We did not have to do so. What I did was to sit in the office with the others and hear the story and take part in the discussions which went on until about midnight, and then I went home. I checked with my male nurse who was on duty at that time. He said he had had one case of skin contamination which he had dealt with quite easily so I could see there was no immediate problem in that direction. The other health implications of course one thought of all the time.

Before I went home I rang Dr. Maclean to tell him that something was up and that I was going to give him information as soon as I could. That is all that happened that night. I am not quite sure about the next one I think Dr. Maclean rang me back later. It was merely to confirm that he had spoken to others and to have a general check on things. I

came in in the morning. There was a traffic diversion. I went to Research and Development to see the research people I'd been talking to the previous night and I was informed that a modified Site Emergency was in progress and the general idea was to stay indoors and carry on working without respirators.

CHAIRMAN You did not put these people under cover?

GRAHAM No. I had no direct hand in that. The next thing was I went to my own department and found my colleague Dr. Blakeley had instituted the emergency procedure by shutting the doors and doing various things inside the building. Each member of the staff was standing by in his position, just waiting. We had this arranged in detail before he had put this procedure into operation. When I got inside the Medical Department I simply stood by like the rest of them; I was in telephone contact with Dr. Maclean several times. I do not think that anything terribly important happened after that. I just stood by and stayed in the picture, heard as much as I could about the situation and had various queries which I tried to answer. I gave instructions for measurements to be taken, at the request of Dr. Maclean, up in our smaller surgery in the Chemical Group plant, Separation Group Surgery and I phoned these measurements to Dr. Maclean.

The rest of the time from then on was spent answering queries. It's rather an ill-defined story. I don't think it could be put in the form of a programme. One dealt with things as they arose.

CHAIRMAN This was the Friday?

GRAHAM The days have tended to run into one another. I am not clear on that point. It could have been. I began to get queries from outside people, notably the County Medical Officer of Health.

CHAIRMAN And then the next few days?

GRAHAM One of the problems that did arise, I was asked about feeding arrangements, canteens. Was it alright to open the canteen or not. I checked with one or two other people to find out what had been done already, and the arrangement was made that the Separation Group Canteen would not be used for feeding purposes but the Main Canteen was alright. The Canteen Manager rang me to ask about certain foods. For the sake of giving him something to do, more than anything, I told him that it would be useful to wash the crockery and cutlery and not to use fresh salads. I asked him how long the milk had been under cover and he told me that there was milk in the 'fridge' from the previous day. I told him that it was alright to use it in tea but not as a beverage, a few homely instructions of this kind. He asked about potatoes and I told him that they would be alright if they were peeled. Then there was no problem from then on. The Calder people rang up too about the Canteen and I told them what had happened in the Main Canteen and they followed suit as far as I know.

CHAIRMAN When you gave these homely instructions, you had something to go on, of course, you had the readings of the air activity.

GRAHAM Yes. I had some of them. I knew that summaries were in progress and that the level was being watched over the Site. Some counts were being obtained from the interior of the buildings, table tops and so on and it was on those grounds that I said the thing we had to watch were surfaces that had dust on them.

CHAIRMAN These instructions to the canteen obviously fall within part of your job, to look after the health and safety of the people in the Works?

GRAHAM Health, certainly, safety is rather..., hygiene if you like. Radiation is only part of our interest.

CHAIRMAN What about the people who have been dealing with the incident, as regards their health and hygiene? How did you come into that?

GRAHAM We have an arrangement whereby people whose skin becomes contaminated have preliminary washing in the ordinary change room. If this fails we have a medical arrangement for bringing them into the small surgery to be decontaminated in a systematic way by nursing staff. If the nursing staff do not succeed they tell me and ask about special cases.

CHAIRMAN If one of Mr. Howells people in monitoring of the film badge had some evidence that the man is contaminated somewhat and the ordinary first aid precautions at the place failed he then would be passed over to you?

GRAHAM Yes. We do a body survey if it seems necessary and we have a chart and the Sisters have instructions about shampoos and nostril washing out and chemicals of various kinds to clear the contamination. We also do routine urine checks in all cases referred to us.

CHAIRMAN How about the exposure of staff to gamma radiation. How do you come into that side of it?

GRAHAM Well, the mechanics of it is done by the Health Physics people. They refer to me cases that have exceeded the 13 week total and these people are referred and I arrange for them to be taken off contact for a suitable period of time and I inform the labour office or their management if they are Industrials. For the purposes of this event we agreed that things were happening so fast we might not have paper work but we were quite agreed that the procedure would carry on. That means that people who had gone over the mark would be taken out.

CHAIRMAN Do your records show which people had doses of any kind?

GRAHAM Yes, the ones that have been referred to us do.

CHAIRMAN Can you give us an idea of the scale of this - so many people, so many Rontgens?

GRAHAM Yes, do you wish to know in any exact detail? I can tell you in a rough way.

CHAIRMAN Yes, we do want it in some detail because we have to assure the public, or at least the A.E.A., of what has happened.

GRAHAM I told Dr. Maclean we had seen about 10 people who had had more than 1R and I understand that the highest dose was 5R to one individual. I now believe that there is some doubt about that. A contaminated film badge has now been brought into the picture, so this may not be true.

CHAIRMAN You mean that the 5R may be less.

GRAHAM Yes. I know of two other chaps who got 4.5R, and one other man got 3.3R. These people are now off the Contact Plant and working elsewhere.

CHAIRMAN Now, they got these doses during the incident. Have those men had some roentgens in their normal work period before the incident in their normal work.

GRAHAM This was their total figure I was told. Some of this radiation could possibly have been sustained in previous work. It has not been separated from the incident as far as I am aware. I think there was something like 17 people who I heard of and they were graduated in this way:

There was one who might have had 5R; subsequently a contaminated film badge was discovered and this may be changed. Below him were two people with 4.5R and a third man with 3.3R.

The position this morning is that apart from the ones I have mentioned, we have three individuals who have exceeded a 10 week local limit that we have for safety reasons, but have not exceeded the 13 weeks that is internationally recognised, and apart from these the rest of the pile staff are all below the 10 weeks mark and I understand that they are now working to a maximum of 3R in 13 weeks and I imagine that several of them will be off the job, having taken up the rest of their 3R.

CHAIRMAN How about hazards other than gamma. Have you any reason to worry about urine samples or blood counts.

GRAHAM Yes, we did for good measure blood counts on all people above one R. I would not expect to get any change in the normal blood picture at that level, but it is certainly useful to have a base line, and to say that we have done this.

CHAIRMAN There is nothing of any significance?

GRAHAM No. I have not treated a patient in connection with this episode, and that sums it up.

CHAIRMAN Urine samples?

GRAHAM We have urine samples. At least I gave instructions for them to be taken from all persons involved in this thing, including the people who had come through the Separation Group surgery, and they have now been sent to Harwell and are being done by a quick method; we are not radiochemists and we want to know as soon as we can. The other thing we did was to include these individuals working on the job in our thyroid iodine survey which we started on Wednesday. This is in addition to the district survey people.

CHAIRMAN It is not part of your responsibility to see that every individual that is going into a hazard actually has a film badge.

GRAHAM No, this is not directly my responsibility.

CHAIRMAN As far as you know then you dealt with the measurements that have been made and supplied to you. In other words you deal with people you are notified about.

GRAHAM Yes. Although one takes an interest in a general way.

CHAIRMAN One of the particular features of this incident has been the iodine, radioactive iodine, which has caused the trouble with milk and so on. From a medical point of view, is there any reason to be worried about the operatives in this plant; from what they have had in the plant, we are not at the moment dealing with what they did at home. But inside the plant is there any reason why iodine here should have...

GRAHAM No, I do not think there is. Of course, at the time one didn't know and I have done some thyroid measurements on plant operatives and they are all very low and I am quite happy about the thyroid and iodine story.

CHAIRMAN It is possible to measure the thyroid activity in a man without saying anything to him?

GRAHAM Yes we do it through the skin with a collimated ...?... window counter. We can get down to .05 microCuries in the gland. This is the limit of detection on a two minute count.

CHAIRMAN And this is well below anything you would say is hazardous?

GRAHAM Yes. It is far below.

CHAIRMAN At what figure would you be worried? Is there an agreed standard?

GRAHAM You would have to calculate back from a dose of 200R to the glands. I haven't got the figures readily available but the arbitrary level that we set for this episode was .1 of a micro curie in an adult gland. This works out in fact to the dose which an ordinary process worker would receive during the agreed 13 weeks. He would have gamma radiation to the whole of his body, his thyroid would catch part of this and that portion which his thyroid would get, translated, comes to .1 microCurie of radioiodine and so it was on that basis that we fixed this.

CHAIRMAN What was the maximum you measured in any man?

GRAHAM The maximum was in a child from outside.

CHAIRMAN Let's take our own people first.

GRAHAM The maximum was in one of my own staff, a nursing sister who had 2½ times this level, .25 microCurie.

CHAIRMAN Can you tell us how many were found with more than .1 microCurie?

GRAHAM Do you mean site personnel? I have not got that separate. It might take some minutes to have it separated, but I can give you the total now of all the thyroids we have done.

12.5 p.m. - 12.30 p.m.

GRAHAM 76 positive out of total 144 examinations. this was the position yesterday.

CHAIRMAN What was the highest of all?

GRAHAM 2.75 times - this is mCi in the gland. This was a child of 6 who came from Ravensglass.

CHAIRMAN So that really about half of the people you measured had more than this .1.

GRAHAM Yes. .1 is reckoned to give an integrated dose of between 6-7 R to the gland between its decay time for an adult. In the case of a child this would be somewhat higher. The carcinogenic dose we regard as in the neighbourhood of 200 R. I speak without the document, but this is what we think of in terms of carcinogenesis.

CHAIRMAN So that really you have got a figure of .1 which you fixed on 6-7 R, but you didn't really have any group of experts or any international advice or agreed standards to which you could work. You had to do the best you could.

GRAHAM It was not readily available in this form. The figure was given to me by Dr. McLean, who had consulted with people like Dr. Loutit and the Medical Research Council generally - it was not simply a locally agreed figure. It was a British figure.

CHAIRMAN I know - a British figure worked out on the spur of the moment by a lot of brilliant people who knew what was involved.

GRAHAM Yes. I don't think it is laid down in any accepted code of practice.

CHAIRMAN Were you worried that one out of two people that you were measuring seemed to have more than this .1?

GRAHAM I was worried in the sense, not that I think that any of them will develop disease of the thyroid. I'd like to be clear about that, but it is a rather high proportion, and is a measure of the amount of iodine that did get out. It checks with the other measurements. Milk for instance and grass. Although this is not my special job. I understand other district survey levels check with this and it was expected. It is disturbing, but I do not think anyone will suffer thyroid disease from it. It is not a thing that one would wish to happen very often. A further thing one has to consider is the future exposures, particularly to personnel. This will have to be fitted in somehow to their radiation record, of which we keep a running total for every man.

CHAIRMAN This will have to be watched very carefully so that men and women in future are given coverage as far as possible, that they do not come in for total doses.

GRAHAM What we will attempt to work to, of course, is the 5 R/year which has been suggested in the new factory regulations, which have not come into effect yet.

CHAIRMAN Do these new regulations give permissible levels for other than Roentgens?

GRAHAM No, it is expressed in terms of rads to the tissue and you have to work out...

CHAIRMAN Somebody has got to do a lot of calculations from these new regulations to say if there is iodine, or if there is this or that, and we have to add these risks together.

GRAHAM This will have to be decided by a panel of experts - Medical Research Council probably have done a lot of this already.

CHAIRMAN This will not merely be useful to you, it is necessary for you to have these.

GRAHAM It is essential for us to have these sometime in the future, the sooner the better.

CHAIRMAN Are there any other radioactive materials other than iodine which may have come down in the plant and affected our workers.

GRAHAM Well other fission products, to a much smaller extent must have come out - exactly to what extent one does not know. The urine samples which I took from factory personnel were sent to Harwell primarily, not for iodine, but for strontium 90. The method by which the analysis is being done is a quick method and we hope we will be able to calculate in a short time from one of the stronger isotopes to give us a reasonable strontium figure. Caesium, for instance, we may be able to count quickly and easily if we can get rid of the iodine from the samples.

CHAIRMAN So that, of today, although you think everything is alright, you have not got the information as yet on which you can make really reassuring statements.

GRAHAM The long term information about the long-lived isotopes is not yet available.

CHAIRMAN But this is being done with great urgency. When do you think we shall have enough information to be clear?

GRAHAM It depends how quickly they can work and how much instrumentation they can bring to bear on it. The district survey people outside the fence are being done by radio-chemistry, which is a very long drawn out business. The material has to be chemically treated and the ash left for a time to allow yttrium 90 to grow, so it could be weeks until we know about the district people. The site people we hope to know about this week.

CHAIRMAN I have asked a lot of questions about our own staff inside the factory. I would like to give my colleagues a chance to ask questions on this subject.

SCHONLAND Is every possible pressure being put on Harwell?

GRAHAM I think so, Sir. They have been flooded with samples from Windscale, and our own chemical staff as well. We are doing many at Windscale.

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SCHONLAND They have been asked to give it highest priority and they are doing so.

GRAHAM I think so. The sampling programme as a whole has been treated as a special operation. We have special offices, and special phone numbers, and special transport to do this on a big scale. Something like 300 a day we are handling ourselves and many more going to Harwell.

CHAIRMAN I would like to explore whether you have any responsibility outside the factory perimeter. What are your responsibilities there?

GRAHAM I do not think they are clearly defined in any written instruction, but one accepts certain responsibilities. Cordial relationships with the outside doctors, for instance we know the general practitioners of the hospitals very well, and we regard it as part of our duty to keep in touch with them, see them from time to time and visit the hospitals. This is straight medicine and good relations, and this applies to some extent to the County Health Authorities who would not normally deal with us directly on matters of effluent and so on but in this particular episode I have been in touch with the local M.O.H. on many occasions. He comes to me as a colleague although I have no direct responsibility towards him.

CHAIRMAN When the district surveys were being done on the milk you were aware of what was being discovered, did you play any part in giving advice?

GRAHAM Yes, I was rung up a number of times. Points arose in dealing with specific farms and the M.O.H. frequently rang me up to ask 'what should he do?' In a very short time all such queries were channelled to a special office with Ministry of Agriculture and Fisheries officials and other people who knew the facts of the thing, but in the early stages had a number of queries of this sort.

CHAIRMAN As I understand the position a certain tolerance figure was set for the iodine in milk. Dr. McLean and a number of people from MRC came into it. Once that figure had been set it was a question then of measuring whether milk had that, and if it had then something had to be done. But you played no direct part in that. You did not actually take any responsibility.

GRAHAM It was not my recommendation that started the local milk ban. I merely helped in implementing it in matters of detail. Locally on site as soon as Dr. McLean told me that there was a hazard in the milk I notified the Shift manager and told him to tell the canteen people about it. The official announcement was not mine. I understand the machinery of the sanitary inspectors and local police actually implemented it. I contacted the local policeman who wanted some help about the details of small farms which were not big distributors. I gave him some help in that.

SCHONLAND I think one of our terms of reference is to ascertain that everything possible was done to safeguard the health of our staff and of the public. We are only entitled to call Authority members of staff as witnesses. We can't call the County Health Officer or doctors, and it would be very awkward for us if we were to say from our investigations - and

it certainly would indicate that today - that every precaution was taken, I think you did a fine job, if there is somebody outside who is going to say no. Now, I can't ask you to repeat hearsay, but have you had any complaints from outside, on the medical side, that the right things were not done?

GRAHAM I have had inquiries from local doctors about specific patients asking if it was alright. I have explained the position as best I could to say that they would not be seeing cases of radiation sickness; the levels were far too low. They had told me symptoms in a patient which are nothing to do with radiation. This has happened a number of times. In the matter of complaints, I have had none from the medical... I've had enquiries for information but that is all.

CHAIRMAN Is there anything you would like to call to our attention, Dr. Graham?

GRAHAM I think probably my biggest problem in dealing with extraneous queries was that so much information was not in the medical line, and I could not be expected to know what the grass levels were in a certain district. I had to pass on a lot of queries of that kind to the central enquiry agency which was latterly set up. I did feel a little embarrassed at saying I did not know what the level was. I think there was a feeling outside the fence that perhaps I had all these duties in addition to my medical duties. For instance, schoolmasters rang me up to say was it alright in my playground and would I send an instrument to measure it. I had to answer this question that a district survey was being done in a very detailed way and it was quite alright. As far as I knew his area had been taken in and I answered the questions in this way. This could be personal contact. I was probably the only person they knew. I have nothing helpful to say in the matter of difficulties.

CHAIRMAN It is understandable anxiety.

GRAHAM I did visit a number of schools in the neighbourhood and saw the headmasters and told them of the things mentioned. The main worry was that future pupils would dwindle in numbers because of this.

SCHONLAND Something was reported in the newspapers about one of our staff who kept wearing gloves in a pub.

GRAHAM Yes, I know this man. His name was Ritson? He was one of our contaminated people and he had had such a terrible scrubbing I gave instructions to Sister to let him go home wearing gloves and to sleep wearing a factory cap and we gave him a pillowslip in case he contaminated his own bedclothes. It was mainly hair and hands which were contaminated. The levels were such that I was not bothered about the dose to his skin, and the idea was for him to come back next day for a check and we would start again. If we scrubbed too hard we would break the skin and ruin the whole thing. What I hadn't reckoned with was that he would go out wearing these things. He talked rather freely and got a lot of publicity which I think embarrassed him more than it did us. He was very angry the next day to find out what had been done with his story. He is now quite clear and we didn't regard him as a special problem. I saw him afterwards and we did a blood count and he is quite well. The skin is now clear. It was unfortunate he went to this particular place. He should

have stayed at home, although I did not instruct him to stay at home.

KAY Have you provision for keeping a case like that in your sickbay or under observation until...

GRAHAM We have emergency beds which we use occasionally.

KAY But you didn't feel it necessary in this case, only with hands and head, and it was just a question of limiting any further re-contamination.

GRAHAM Yes, contamination of his food and bedclothes. He was very tired. He had been scrubbed for several hours and we knew a lot of it would decay. It was only a matter of time before the count would come down, very largely, and we decided to let him go. There were one or two people like that: Ritson was the only one that was picked up as a national figure, but the rest of them - nothing happened about them.

KAY Did you find it necessary to keep anyone back under observation.

GRAHAM No, we didn't detain anyone. If the contamination had been widespread we would have kept him in. He is off contact. He is working in a non-active area now.

CHAIRMAN Any further questions?
Thank you very much. I do not think we shall want you anymore.

SCHONLAND Could we ask Dr. Graham to let us have those urine results?

CHAIRMAN They have not got them yet.

GRAHAM All I have is intelligent guess work of two or three cases. I do not think I would like to quote these just now. When I have them perhaps I could submit them.

CHAIRMAN As they come through perhaps you had better send a copy to me at once. Until you hear to the contrary keep me informed of the results you are getting.

2.30 p.m.

WITNESS: MR. HOWELLS

CHAIRMAN Mr Howells, I asked you to prepare one or two maps and a few figures. I would like you to tell us what you have done and then submit the papers.

HOWELLS I have made a list here of the places we visited. During the night we worked out something like four different routes for survey purposes working on the system that we had an inner circle first of all where we went round making measurements. This was going out of the main gate, going up the public road and then back in through the pile gate at the top and then back down the side of the site. Then we took another ring going further out. These places are listed. The

idea was to work out from the works and find the levels at various points. We did have difficulty with the ionisation chambers in the survey vans, due to the fact that the vans became contaminated, so that the levels we got were not really reliable. We were forced to adopt some expedient to get round this and one thing that we did was to take swab surveys of square yard areas, and we also did probe surveys with the contamination type of monitor. From the readings you get with this type of monitor it was fairly evident again that the radiation levels were not particularly high. On the basis of what ionisation chamber measurements we were able to get, the swab results and the contamination survey meter measurements, we prepared this map here which gives sort of rough contours, which was all we could do at the time of what we estimated the deposition was in curies/metres². This was done by converting the radiation measurements we got on the ion chamber into deposition figures.

CHAIRMAN Assuming the factor for normal fission products? Was that right?

HOWELLS This was all we knew at the time. We didn't know that this was iodine. We did the same thing again with the probe. Again you can convert the counts per minute as you get it on the probe. The probe is held one meter above the ground and then related to the...

CHAIRMAN What sort of figures were you getting for microcuries/metre?

HOWELLS The highest figure that we got...

CHAIRMAN This is what you thought it was, not what it really was?

HOWELLS No, this is what we thought it was.

CHAIRMAN That was in terms of what - pile fission products?

HOWELLS Yes. From the contours we got the impression that the areas where the levels were between 10^{-4} and 10^{-5} C/m² was quite a narrow zone in the centre. This was the information as we deduced it on the Friday.

CHAIRMAN Did these figures look serious?

HOWELLS These figure here? No, these don't. The impression I had at the time was that the level was probably 1/10th the level where we might get stoppage of milk. This is just my impression on the information we had.

CHAIRMAN Nothing like evacuation level?

HOWELLS Certainly not, no.

CHAIRMAN Thank you, while you are here we might just ask you one or two more questions. I think you told us it was not you who told people to go under cover during the Thursday night.

HOWELLS I passed the message to the shift manager.

CHAIRMAN Oh, it was you – Howells passed the message to the Shift Manager. Do you remember when?

HOWELLS This would be in the early hours of the morning – about 1 to 1.30 on Friday morning, something like that.

ROSS In the log I kept a note of this – 1.33.

CHAIRMAN You said there was a certain activity in the air and you thought it was advisable that people should stay under cover?

HOWELLS Yes, there was air activity as you can see from the results I've presented, but the levels of air contamination in the building were not abnormal, so that when they stayed indoors it was obviously better than being outside.

CHAIRMAN That was only very locally; it was not outside the fence?

HOWELLS No, these are the ones we had inside, even inside the factory they changed quite rapidly. One minute you would get a high sample at one end of the works, and then it would move to the other end of the works. But there was not a constant high level at any particular point. I make this point because on the list I have made a selection of the air samples. For any particular time we have only recorded there the highest level that was got out of these and we were sampling in 10-15 locations.

CHAIRMAN There is one other question, and I am asking simply because it may catch public attention.

In our first press release we did say the direction of the activity was out to sea, and you have now produced conclusive evidence that there was a plume going this way, another this way and a third one this way, and I am wondering just what the explanation is for these three directions. The wind you saw, from the way the flags were flying and so on, was out to sea when you first...

HOWELLS We demonstrated this by that radiation measurement. When this was done, they were sent off in the direction of the ground wind. There is a weather vane outside, and they went along in the direction that was pointing at the time, with a radiation monitor. When they obtained the highest reading, the 4 millirads I quoted to you, they were under the plume, and this was not very far east of here. We kept records of the wind every ten minutes, and we had a monitor in the control room reading the direction as given by the weather vane outside.

CHAIRMAN Does that show changes that match with these other plumes?

HOWELLS We did not have time to make a correlation, but there may have been a different wind direction between 500 ft. and ground level, particularly under night conditions.

CHAIRMAN I was wondering whether it was that the wind up to a few hundred feet was always more or less in one direction at any one time, and then the whole thing swung round, making two changes over the next ten hours or so, or whether it was always more or less that way near the ground and a bit higher up went in the other direction.

HOWELLS Over the night of the 10th and the Friday morning it kept blowing in one direction at ground level, but the wind velocity was low, a speed of 2-3 knots, or possibly less, then there was some variance on it, in that it was swinging slightly east and west, as was shown up by the air samples.

CHAIRMAN So you would, subject to you looking closely at these wind records, be inclined to think that it was winds of a few hundred feet up might have been blowing the other way, and that is why there were two or three plumes?

HOWELLS The wind speed could have been a lot different too.

CHAIRMAN Have you got the records with you, that you took at ground level?

HOWELLS Yes, I have these. I have been looking at the met. report for the Thursday-Friday-Saturday period, and this is what set us off on some of the gamma surveys in following up the direction where these plumes might have gone.

CHAIRMAN Could we ask you to let us have by Thursday morning a table giving the wind directions and times as observed by your people on the weather vane and anything you have got further on the met. report.

HOWELLS Yes, I can do that.

CHAIRMAN I really want it from the time that you started to get on guard and for, say, a 24 hour period, that sort of thing.

HOWELLS We have them from about 3 o'clock on the Thursday afternoon and right through to now.

CHAIRMAN Suppose you give us the first 24 hours. 2.40 p.m.

WITNESS: DR. LESLIE

LESLIE I will try to be as brief as possible. The first point I would like to give you is that the fallout at Seascale on the Friday morning was nearly the same as the fallout at Calder. At Calder on Thursday afternoon a site emergency started at 4 o'clock. When people came to work on Friday morning they were told there was a site emergency. At 11 o'clock or just before, all non-essential personnel were sent home, just before 12 o'clock the buses left, taking the people away. Now, applying Sutton's formulae to the Windscale stack, you would expect that the air activity at Seascale, assuming the particles are very small, would be rather higher than at Calder. On the other hand, of course, if you have fallout due to rather larger particles you might expect the activity at Calder to be higher than at Seascale. As I say, my measurements taken on Friday morning suggest that Seascale and Calder were very much the same.

CHAIRMAN Can you tell us what you measured?

LESLIE I merely used a B.6 counter, similar to the 1021. It is battery operated. Well, that is the point, that's all I wish to say. I could comment on this, but I think I should just give you the facts.

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CHAIRMAN Can you give us the reading?

LESLIE It was 30 counts/second at Seascale and at Calder 25. I think you should bear in mind the thickness of the grass, which must obviously come into this. If you were to do this on very thick grass, you'd undoubtedly get a less reading.

CHAIRMAN On grass?

LESLIE Yes. Do you wish me to comment on this?

CHAIRMAN You have called our attention to it, we will think about it.

LESLIE I would like to stress that there was a warning given at Calder but not at Seascale. The second point I would like to bring to your attention is about the warning system. The external warning system. I have been looking at the factory instructions and see that the civil authorities will be informed, but to the best of my knowledge there has never been any practice to determine how efficiently the civil authority's arrangements would operate in such an event as this. Thirdly, the warning system at Calder, I have nothing to object or complain about. In the recent incident we were warned promptly by telephone by the Safety Officer, but there is no warning system at Calder other than by word of mouth or by using the telephone. It is a fairly large plant and obviously under certain circumstances cannot possibly be entirely satisfactory. The next thing is the sampling equipment on Pile 2. In 1955 I made a number of proposals concerning the sampling of the gaseous effluent from the Windscale stacks. This was because I felt we did not really know what was going out of them, and of course the Authority has an obligation to see that nothing dangerous goes out of them, and I felt it really was not carrying out these obligations. My proposals were that you should fit small sampling cyclones, but above the filters and below them. I made my proposals in a letter to Mr. Saddington, it was never issued as a paper. Subsequently I was transferred to other work, to the Calder start-up and was taken off this, I designed a cyclone before I left and I believe it was fitted. I discover nothing has ever been fitted on Pile 2 and I am quite sure it ought to be fitted if the reactor starts again. I believe that the order had been put in for some equipment. My fifth point concerns the fallout in 1955. I think you may feel that this is irrelevant but it concerns the operation of the plant and it may be that in your findings you may wish to do something about the filter. In 1955, in August, I found a number of particles in my garden, the largest $\frac{1}{3}$ microcurie. In my notes I see that I picked out 23, which were easily removed. That is all I have to say.

CHAIRMAN Thank you very much. We will look at what you have said and give it careful consideration. Tuesday 22nd October - 3 p.m.

WITNESS: MR. MOORE

CHAIRMAN Good afternoon Mr. Moore I believe you have some data for us on the analysis of the filter material.

MOORE Yes that is right sir, shall I distribute these now?

CHAIRMAN Yes please, perhaps you would take us through it briefly.

MOORE We have the cyclone sample unit fitted above and below the filter. In the cyclone we collect particles from the samples in the gas stream and estimate that the sample is representative certainly down to 15 microns, particle size, quite reasonable at 10 microns, but down at 5 it is beginning to be a bit doubtful.

The sample taken underneath the filter; the catchpot was installed at 16.00 on October 8th and taken off at 15.30 on October 10th. This sample was only in position during the incident. Unfortunately it was taken off half-an-hour after the air was turned on for the first time. So it is highly probable that most of the material collected in this sample was collected in the short time between air being turned on for the first time and the cyclone being taken off.

The one above the filter had been on for a longer period - it was installed on the 27th September so some of the material in that sample will be from previous history of the pile when the pile was running.

The figures show particle size distribution in the cyclone sample in the two of them. We have had some of the samples analysed for lithium and magnesium content. In section 1.1 I give the results for lithium and magnesium based on the magnesium analysis. This gives an estimate of three grammes passing up the stack prior to the sample being taken off.

The sample above the filters, item 1.2. The estimate there is 3.4 grammes of A.M. passing up the stack. I think really as far as the A.M. is concerned all that the sample units do is to show that some A.M. had passed up the stack before 15.30 on October 10th.

CHAIRMAN But very small quantities?

MOORE Very small quantities, yes.

CHAIRMAN How much uranium do you think had gone up the stack, corresponding with the 1.6 grammes of A.M.?

MOORE The last column is the estimate in the stack.

CHAIRMAN In the stack there is .5 grammes you reckon of uranium and about 3 grammes of A.M. bars.

MOORE Yes.

CHAIRMAN I suppose there is no experience of anything like this before that gives us any clue?

MOORE The cyclone sample unit below the filter has been used for some time as a monitoring device to detect excess activity going up the stack, but this has been more on a qualitative basis than a quantitative basis. There is some error here in estimating how representative the sample is under these conditions because the velocity in the stack is much

lower than normal. I do not think we can be out by more than a factor of ten though.

CHAIRMAN Well there is the statement. Any questions from Committee members?

MOORE I should say at the moment we are using this information to calculate how much uranium and activity left the stack. I will present a note of this some time tomorrow.

CHAIRMAN Up to this time?

MOORE Throughout the whole incident. We are estimating how much activity and uranium there is in the filter at the moment. We have estimated the filter efficiency ...?... particle size distributions, and using this particle size distribution, we can estimate how much has left the stack.

CHAIRMAN It has gone through in 5 microns, or less?

MOORE No, this is an impaction type filter. At these flow velocities it is much less efficient than normal, in fact not far off 50% goes through in total and you must add to it that that goes through at small sizes that we cannot detect in the cyclone.

PIERSON Mr. Moore said the filter bag was on for only half-an-hour after the air was on, that is the shut-down fans. According to our log they were switched on at 13.45, so that gives 2½ hours, I am sorry 1½ hours.

CHAIRMAN All right, any questions at all?

MOORE There is an erratum to the summary of events which I wrote of the incident. I can distribute these and just briefly comment on it. The second one is a typing error I am afraid.

CHAIRMAN Is there anything else Mr. Moore?

MOORE No sir.

CHAIRMAN All right, thank you very much.

At this point Mr. Davey again gave evidence

CHAIRMAN I think it is only fair to give you a few minutes chance to answer some questions something like the ones I put to Mr. Rotherham, like what did Mr. Rotherham see as the responsibility of R. & D. Group to you. What did you see the responsibility of R. & D. to you.

DAVEY I think I would say that at Windscale there are two separate organisations, which are each answerable to a directorate, but I would not stress that unduly because on a day to day basis I would say we work as one team. Again organisationally I think one has to say that on the operations side we do not carry experts in a particular field. We claim to have general experience as operators and, of course, we do require a great deal of backing which to a large extent one gets from R. & D.

I am quite clear that I am responsible for the operation of this works but I think I could illustrate my point, all this by the way represents the Minutes of the Windscale Technical Committee, and the first example is 21st July 1955, in which there was a fairly detailed discussion of Wigner release on Pile

2. The concern was with the safety of this as an operation. It was discussed Committee-wise and of course there was representation of the various branches other than the Operations Branch. I could turn the pages and the same thing would be true all the time. On another occasion the R. & D. representative was a Mr. Mossop, the Harwell representative was a Mr. Mummery but all I am saying is that one must approach this team-wise and whatever discussion takes place it is a free discussion between representatives of the various interests or branches and out of that a decision comes. This is the final illustration, but these are the Minutes of the Technical Committee dated 19th September 1957 in which Wigner release was discussed in some detail, and in fact a proposal was made that we should go to irradiation of 50,000mW days before a release. Present again were R. & D. and Harwell representatives. R. & D., Mossop and Greenough and Harwell, Mummery, and out of the discussions came the proposal to compromise at 40,000 mV days, and I think, Mr. Chairman, I would merely use these as examples to illustrate the running of the pile is through the Technical Committee and that is how we have gone along. But I think I should say too that if someone waded through these Minutes in detail that Wigner release is a recurring theme. It has been in my mind fairly constantly because I have never been too happy about it. I think it might be worth saying that in a conference in Chicago June 1956, I tried to raise this as an item with the Americans. They were not prepared to pursue it, but I discovered that they themselves did not appear to be at all clear and in fact the answers from the various representatives differed quite considerably and I came back from that meeting with the feeling that as we were committed to the graphite moderated type of reactor, considerable effort should go into the whole question of Wigner release.

CHAIRMAN You are the Chairman of the Windscale Technical Committee of course. Did you ever consider that sort of record or who on that Committee would you look to to bring you a digest of what those curves meant?

DAVEY I must say quite fairly I would expect R. & D. to bring that, perhaps not to me but certainly to the Pile Manager because at every Wigner release which has taken place shall we say for the last 3 to 4 years, there have been R. & D. representatives present at the release. In fact, R. & D. have done the calculations and predicted in round terms the energy which would be released and the maximum temperatures which one could expect. I am not trying to shed responsibility, but one does rely on R. & D. to produce this type of effort because the Gausdens of this world are not equipped to do it, I am only mentioning Gausden as an operator, but he is not a specialist in graphite problems, but each Wigner release has been planned as between Operations and R. & D. and the results of these releases have been studied in some detail but I have never seen anything as elaborate as that.

CHAIRMAN Could I ask you the same question that I asked Mr. Rotherham about the instrumentation of the Pile. The instrumentation of the pile was put in in the early days and, in the light of knowledge then existing, it was very good, adequate instrumentation, but once Wigner came into the

picture it might not have been. In fact we now begin to see that it was not. Would you have expected some representatives either from R. & D. or Harwell to advise you that it was not satisfactory instrumentation for Wigner release. How would you have expected to be warned? Who would you have looked to?

DAVEY I would have looked to the Technical Committee as a whole, and in fact I think even speaking from memory I am entitled to say that members of this Committee have referred to the inadequacy of the thermocouple provision. I can call to mind discussions in the Technical Committee where people were rather concerned that 2,000 tons of graphite were covered by the odd 70 thermocouples.

CHAIRMAN Having referred to that, nothing was done?

DAVEY Again speaking from memory I think there was the whole difficulty of doing anything at that stage.

CHAIRMAN It was an elaborate job to do it?

DAVEY And it was a very difficult one too.

CHAIRMAN The same idea about the possibility of inadequate instrumentation might arise at any other time about something which we do not understand or something we have not yet realised, something perhaps that would stem from some scientific fundamentals. You would again expect or hope that the Committee, or that some of the Committee members are of the right type to bring that to your notice?

DAVEY Yes.

CHAIRMAN How did you choose the members of this Committee? Did you invite members of this Committee? It is your Committee I suppose?

DAVEY I do not know if this is what you want, but so far in this organisation in the precommissioning stage, there has been a meeting which has devoted itself to design and construction. At the appropriate point of time, a commissioning committee is set up, which is composed of representatives of interested parties, and when commissioning is complete in effect this Committee goes on and changes its name and becomes the Technical Committee, but interested parties continue to be represented and on this particular Committee from the beginning, there was Harwell representation. In fact in the early days it was quite strong because R. & D. was weak at that time. Now I think over a period of years the R. & D. representation has tended to increase and to some extent the Harwell representation decreased, although it is true to say that at no time has Harwell representation ceased, so on this Technical Committee, one has the works people, R. & D., Operations Branch HQ, Headquarters, Harwell and originally there was a member of the old Pile Design Office, but that office went out of existence 3 or 4 years ago.

CHAIRMAN Could I turn for a moment to your accepting isotope cartridges, not fuel elements, to go in the pile. Somewhere in the Authority somebody wants to put something in this pile. How do you accept them? What assurances did you get? Did you do this on the Technical Committee? Whose say-so did you accept on this?

DAVEY I certainly cannot answer that simply. Any proposed cartridge certainly comes to the attention of this Technical Committee but it might there as a proposal, it might come almost as a finished design. There is no well defined route. Having said that, one then goes back to Springfields on the one hand and Harwell on the other, and cartridges which are manufactured at Springfields to a specified design are inspected by Windscale inspectors. We have a resident team at Windscale who certify stuff coming off the line. As far as the design of the Springfields cartridge is concerned I think I am right in saying that without exception that will go through a Cartridge Working Party. I think that is right. But certainly there is a fair amount of detailed discussion in the Cartridge Working Party before a final decision is produced, and then out of that comes the specification and as I said, the production line at Springfields is inspected by Windscale inspectors. Frankly I am quite vague about the Harwell isotope cans, except that we have always said that they must be satisfactory in the sense that they do not prejudice the operation of the reactor.

CHAIRMAN I was surprised to get from the Harwell and Amersham witnesses who told us about their cartridge, that they all seemed to think that the maximum temperature in the Wigner release was 350°C and we know, because you have given it to us, that it was 420°C on one previous occasion and somewhere in between. There is a gap. The 420°C information should have got back to them.

DAVEY Well I am aware of the gap. I am aware of quite a few other things now, but to try to do justice to this, distinguish first of all between normal operations and a Wigner release. Over a period of years the cartridge temperature during normal operation has been raised through the Technical Committee. Every proposed increase has been brought forward for discussion and after all the salient points had been dealt with a decision has been taken. I do agree that in one or two cases it actually became a P.E.C. decision which I would think was ratifying the decision of the Technical Committee. But we have taken these discrete steps up to about 395 through this Technical Committee. As far as Wigner release is concerned I am quite sure that what has happened in the past is that we tended to regard Wigner release in terms of the uranium cartridge, and because its normal working temperature was of the order of 395, a Wigner release approaching that temperature has not been regarded as hazardous.

CHAIRMAN Yes I see.

DAVEY I am trying to be quite honest about this and I think it is true to say that Wigner has been regarded in terms of the uranium cartridge.

CHAIRMAN That is a fair point to make but it does not quite meet the criticism that temperatures of over 400°C have been recorded in a Wigner release and the isotope people at Harwell and Amersham seem to think 350°C was all right. That is still an outstanding point, that somewhere in the Authority a criticism can be levelled. We have understood your end, we've understood their end. Now somewhere in between, there is a gap.

DAVEY Yes. I admit that quite freely.

CHAIRMAN But as far as you are concerned, you are running the pile. If the Harwell people want to put that in you allow them to do so? [NO REPLY] Well that is all the questions I have to ask you on the organisation side, but I must give the other members a chance to question.

SCHONLAND I would like to ask some questions, starting off with this point. Mr. Davey says that after the Conference in Chicago in 56 he decided that considerable effort should go into the question of Wigner release, so what was done about it?

DAVEY My direct action was to speak to Rotherham about it and point out that there seemed to be very little information from America and what there was appeared to be conflicting and as we were major users I thought we should be putting more effort into this.

SCHONLAND In fact there is no evidence that it was.

DAVEY I would not go quite that far because a serious attempt has been made, particularly in the last 12 months or so. I would not say a successful attempt but a serious attempt to strengthen up on this side. This man Greenough is a comparative newcomer.

SCHONLAND It takes time to collect staff. Would the minutes of the Technical Committee be available to us to browse through?

DAVEY Yes.

DIAMOND How difficult a job is it to install extra thermocouples over and above what you have in now?

DAVEY The difficulty arises really because one can work only from the charge face. Again I do remember a discussion on this. I would not put a time to it, but 3 or 4 years ago, and the actual statement was, what would we not give now to have a lattice which had not been irradiated and which we could get at and put thermocouples where we wanted. That was the general theme, but in fact to feed additional thermocouples in from the charge face presents all sorts of difficulties and I think there is a limit to what one could do. This is particularly true of graphite; cartridges are not so difficult.

DIAMOND Cartridges are what I have in mind. Is it very difficult to put in uranium thermocouples nearer the front than 16 ft.?

DAVEY There is no difficulty about that but again I think one distinguishes between normal operation and this Wigner release and certainly I can see no reason why additional uranium thermocouples could not be put in.

DIAMOND Obviously your mind was directed on the uranium temperature being the important thing or the indicating thing in Wigner release and the temperature went up as the uranium temperature went up. I just wondered whether, knowing that the flux is squeezed towards the front, one could have put in extra thermocouples in front of 16 ft.

DAVEY I agree that that could have been done, but so far the picture of Wigner release has been the heating of graphite, and any heating of the cartridges would be due to conduction or convection from the graphite. In fact up to now the emphasis during a Wigner release has not been on the cartridges themselves but on the graphite mass, and what people have said in the past is, what they would not give to have additional information on temperature in graphite.

DIAMOND That is very fair. Every time the fuel elements are discharged new elements are put in with thermocouples in them?

SCHONLAND Perhaps I have got this wrong but this may be true of people who are interested in Wigner release, but during a Wigner release you have no cooling. Was your attention not drawn to the fact that your cartridges were in danger at these temperatures because there was no cooling?

DAVEY I as an individual am going to say that I missed the AM cartridge in the picture but if I am allowed to confine myself to the uranium cartridge, then all my experience or evidence goes to show that if the graphite heated the cartridge to about 400°C, one was not concerned because that is so near to the normal operating temperature of the cartridge. And you see, if you go back to Pair's instruction, which has been quoted several times, I think again the scheme for putting in plugs, opening dampers, and in the end bringing on shut down fans is related to 400°C as a limiting temperature. I am not speaking for him, but I am sure the interpretation is the uranium cartridge.

SCHONLAND Yes. But I must be missing something. If the interpretation is the uranium cartridge, and if we are dealing with a situation of Wigner release, then the Pile Manager has not got information about the uranium cartridge; his only information comes from 16 ft. back and therefore the instruction is wrong.

KAY I think we are at cross purposes. Davey is talking about heat transfer from the graphite to the cartridges, and I think you are talking about the period of nuclear heating in promoting or initiating Wigner release.

DAVEY I am confining myself at this stage to the picture, having initiated the release, which is essentially an exercise in graphite heating.

KAY But I think this is Dr. Schonland's point. Was any consideration given to the period of nuclear heating when you had no coolant available and when you altered your control rod connections.

DAVEY Yes. Consideration was given to this and you may need to wade through the Minutes of this Technical Committee, but I did say that Wigner energy release has been a constant theme in this Technical Committee and part of the theme of course is that the release has become increasingly difficult to carry out, and I am sure I must be going back more than 2 years because I go back to the time when Phillips was in charge and there were occasions already when the anneal was incomplete or difficult to initiate. You do know now that later on substantial pockets were left unannealed, we had out that ... and through this Technical Committee we had

calculations done to satisfy ourselves that we were not in any immediate danger. These calculations were done and the answer was that we were not terribly concerned about the odd pocket, which would go off next time. but not raise the graphite to a dangerous temperature.

SCHONLAND I come back to my point. There is an instruction that neither the graphite nor uranium must exceed 400°C? Now if you take that situation in a second, a double Wigner release, a second nuclear heating: the Pile Manager has got available graphite temperatures which are not, let us say, exceeding 400°C. He is therefore satisfied in terms of the instruction, but the uranium temperatures refer to uranium 16 ft. from the face, and the region where the uranium would be hottest is, let us say, 6-10 ft. from the face. Therefore he is not getting the information which he needs in order to protect his fuel elements and therefore the instruction is wrong.

DAVEY I know that now.

[NOTE: Other members asked if they wish to question]

KAY Only that point that when the temperature 400°C was exceeded in November 1956 was this discussed or investigated or was anxiety expressed at this Technical Committee?

DAVEY Could I come back to that one because I would like to look up the Minutes. I was not here. but I think you'll find it was discussed.

CHAIRMAN That is all the questions on organisation and responsibility. Now there were one or two members who had something more on the technical side. I cannot remember what they were. Was it you Professor Diamond?

DIAMOND I asked for a drawing, but it doesn't have to be formally laid on the table. does it?

CHAIRMAN I don't think it does.

DAVEY It's here. But the drawing of the channel we'll have to make for you - it's being done.

5.05 p.m.

CHAIRMAN Well there is one last thing I would like to say Mr. Davey, you have heard a great deal of the evidence, you have studied all the records, and you are the Senior Officer in charge of this Pile, we would be very interested to hear any views you have to make on the incident, you may feel that you could leave that to us, but I will give you a chance to say anything if you wish.

DAVEY I still cannot claim to have done much more but sit in here from time to time, and have a look at a few bits and pieces. But under the heading of trying to be helpful, on the Saturday morning after the incident, I had a look at what was available then in terms of Wigner Release. It did not compare with what is available now, and I had a look at some of the temperature/time charts, and I had a look at this activity/time chart and my own opinion is that the incident started about 2.00 on the Thursday morning. I think it was missed because they were engaged in an energy release exercise. My picture is

of a group of people watching temperatures and when one temperature rose they took the normal action to deal with it, but my opinion is that the first time the air went on, the incident had started.

CHAIRMAN It had already started, you mean.

DAVEY Yes, or was initiated.

The first time they allowed air into the pile they probably supported oxidation. Because the stack activity meter is concerned there is this time interval between them and the appearance of substantial activity and I am still on the offering opinion, but this seemed to look as though there was overheating in the first instance which was not carrying fission products to the meter, but fission products were carried to the meter at some time later. If I may summarise that as an opinion I believe the beginning of the incident was masked because we were dealing with a general temperature rise on the pile. If I could put that slightly differently, although I think I have just one doubt, if this pile had been operating, the story would have been completely different.

CHAIRMAN I thank you Mr. Davey.

PIERSON I think there was something Mr. Davey said about isotopic loading. Sir William did say if Harwell or Springfields asked you to put something in the reactor you would and you said yes.

DAVEY Yes, but I did say that we always have said the cartridge must be satisfactory in terms of pile operation. I cannot say that there is a streamlined organisation for this, but in the past I have known discussions between Harwell people and the Pile people here which have led to a satisfactory cartridge.

PIERSON But the decision to accept isotopes for the pile is with the Technical Committee here, and you might say no.

DAVEY Yes and we have said no. I can't remember the precise isotope, but I do remember distinctly an occasion when a proposal was turned down.

SCHONLAND ...?... a corresponding business ...?... the Springfields organisation who I take it are responsible for the AM cartridges?

DAVEY Yes.

SCHONLAND Do they have power to instruct you to take the cartridges or do you have the responsibility of saying whether you will take them?

DAVEY We have the responsibility of saying that we will take them.

SCHONLAND But since you can't be omniscient, surely this is really their responsibility to say these are alright. How can you know whether these are alright.

DAVEY I agree but in the end that is what it has to be, but I think I am entitled to say this, the only way to define the responsibility for this cartridge, is to say the Industrial Group is responsible.

CHAIRMAN I think this is a fair summary.

SCHONLAND Yes.

CHAIRMAN For the record Mr. Rotherham has sent four reports on fuel elements and destruction tests.

4.20 p.m.

WITNESS: MR. ROSS

CHAIRMAN Mr. Ross, you have heard Mr. Davey's evidence. I do not know whether you have any comments or additions from your point of view on this matter of responsibility.

ROSS Yes, I feel I have. I feel that Mr. Davey is quite right that the I.G. is responsible for the cartridge, but I have for a long time thought that the Operations Group we were not strong enough technically, because although theoretically I have the call on 20% of Mr. Rotherham's effort in each works, (this is a figure that we lay down, that I have the right to call on 20% of his total effort in each works), this is not the same as having technical people attached to my operators. People like Gausden have not had time to do the development and technical side of the job. On the other hand your development people quite naturally tend to be interested in the new stuff rather than the running stuff, and consequently you have not got this technical cover, which I have in the past been very used to having, and in our organisation we don't have it. I have raised this both with the previous Managing Director and the present Managing Director because I do feel I ought to have a small section which you can call a Technical Section or a Development Section or what you like, but I have always been told that "for God's sake don't let us have another Development Section", but this incident has brought it more strongly to my mind. That is all I have to say on the control side of the organisation.

CHAIRMAN Very well.

DIAMOND In the 20% call on the effort available in the works, that is 20% of the R. & D. staff allocated to Operations?

ROSS It is really worked on an expenditure basis. Rotherham tries to give me 20% of his budget for each works.

DIAMOND So it is 20% of the whole of R. & D. Branch?

ROSS No, his establishment budget in each works.

DIAMOND I see, and the establishment of R. & D., that's a different and separate thing to be discussed?

ROSS Yes, you see the other 80% is mainly engineering and fundamental research. There are different ratios for these again. I am supposed to be able to call on 20%: actually at the present, I think it has ranged between 10 & 15% depending on the Works.

DIAMOND So that that 20% is 20% of the manpower which R. & D. feel they wish to put into Operations Branch.

ROSS No it is 20% of the man power engaged in each works, for R. & D.B. The point is that there are no men engaged in R. & D. outside the works. It is 20% of each laboratory, Capenhurst, Springfields, Windscale, etc.

KAY Each Works can draw on 20% effort available at that Works.

ROSS It isn't a right, it is an arrangement, a gentlemen's agreement.

DIAMOND The total allocation of R. & D. effort to each Works is not controlled by Operations requirements at all, it's something entirely separate.

ROSS Yes. When I first got to this branch there was no definite allocation, and it was just that I felt the need of development effort in running operations that gradually Rotherham and I came to this arrangement, and the 20% evolved as a reasonable figure.

KAY What form the technical strengthening to the Operations Branch would take given a free hand. Is it a question - take the pile for instance - of having a greater number of technically qualified men dealing with the management of the Piles, or would you have a separate Group who would be a highly qualified technical group studying the performance and behaviour of all the plant.

ROSS What I would like would be a P.S.O. who at some period in his career in R. & D. would be definitely transferred to Operations for say, three years. He would be under the Works Manager (Pile) and he would advise the WM(P) on the technical side of the pile and be a liaison between the operation of the pile and R. & D.B. He would be the man who would call for work from R. & D. Branch. He would be the man who would have time to study these curves. While we are discussing these curves I think it is a fact that this is the first time Wigner Release has been set out in this way; it has been looked at on chart papers but I think I am not wrong in saying that this is the first time that the Wigner release incidence has been plotted out in the way we have in this room.

CHAIRMAN Any more questions on that point? Well I feel that as Head of the Operations Section you should be allowed a have a chance to say that if you have formed an opinion you would like to tell us. Of course we shall make our own opinions.

ROSS I feel very much the same as Mr. Davey and am sure that the incident started before Thursday and my contacts during the Thursday night made me feel that there was something which had not been picked up early Thursday morning, and I think that is all.

CHAIRMAN Thank you very much. I think that it concludes our evidence for today.

Board of Enquiry

Thursday 24.10.57

Evidence heard from:—

Hamer	pp 7.1 – 7.7
Butler	pp 7.7 – 7.8
Morgan	pp 7.8 – 7.9
Cutts	pp 7.9 – 7.11
Hardy	pp 7.11 – 7.14
Robertson	pp 7.14 – 7.20
Shackcloth	pp 7.21 – 7.22
Mowatt	pp 7.22 – 7.23
Goodwin	pp 7.23 – 7.26
Howells	pp 7.26 – 7.31

24th October 1957 - 10.05 a.m.

WITNESS: MR. HAMER

CHAIRMAN Will you please say what your job is with the Atomic Energy Authority?

HAMER Pile Control Engineer, D. Shift.

CHAIRMAN The next question is, can you tell us what shifts you worked in the period from Monday, the 7th. up to Friday noon?

HAMER That is the week of the incident. I was on mornings and the hours for the morning shift are 8.30 until 5. On the Thursday in question it was rather different. I stayed on until 9 o'clock at night, purely as a mutual arrangement between myself and my relief, who had some business.

CHAIRMAN So you came in the morning at 8.30 a.m. and worked until 5 in the afternoon all those days, with the exception of Thursday, when you stayed until 9 o'clock?

HAMER That is wrong - with the exception of Wednesday. Wednesday I stayed on. Thursday was the day of the actual discovery, and we were all working right through to the small hours.

CHAIRMAN On that Wednesday when you stayed until nine, you came on again the following morning at 8.30 a.m.?

HAMER Yes.

DIAMOND Which of the other Pile Control Engineers did you relieve? Whose shift you did?

HAMER A man called Mr. Butler.

CHAIRMAN What we would like to do then is, we will mention a particular shift period when you were working and we would like you to tell us in your own words the sort of things that happened, whether it was all normal in that shift or not, and whether anything unusual happened, so just to clear the record, anything on the Monday?

HAMER No. Now you realise we are speaking of what is a purely routine job and it is very hard to remember what are routine things. I cannot remember anything untoward until Wednesday.

CHAIRMAN In that case, we will go right on to the Wednesday. On the Wednesday you came in from 8.30 a.m. and worked until 9 in the evening? Tell us what happened in that day that you remember.

HAMER Nothing at all on Pile 1. There was some trouble on Pile 2. Are you interested in that at all? The stack activity went full scale. In the light of subsequent events, we thought it might have some bearing on Pile 1. Whether it has or not, I do not know.

CHAIRMAN Do you mean then that you were working on Pile 2 rather than Pile 1?

HAMER No. The Pile Control Engineer looks after both Piles, with a foreman stationed on each pile.

CHAIRMAN What did you think was the trouble with Pile 2?

HAMER The stack activity reading was reported as going full scale which is a reading of 30 curies. In such conditions as that, we shut the pile down and inspect the discharge face. Before we do anything drastic about that, we check that the instrument is not lying. So we had the instruments out and it took about a couple of hours to decide that the instruments were not all that they ought to be. After making adjustments, the reading came somewhere near where it ought to be, and of course the following day it went full scale again, so whether the instruments were telling the truth or not is something I cannot say now. You do see what I mean, don't you? Except for the business in Pile 1, this would have been a relatively routine sort of thing and one would not have had to remember it, but in view of what later occurred, the instruments may, in fact, have been telling the truth. That is the reason it sticks in my mind.

CHAIRMAN The instruments that were giving this activity in the stack of Pile 2 - you don't think they had anything to do with Pile 1?

HAMER No. There was something going on in Pile 2.

CHAIRMAN You suspected a burst cartridge?

HAMER The only thing that may tie it up with Pile 1 is if Pile 1 was "chuffing" anything out, it might have been sucked in by the blowers into Pile 2 and registered on Pile 2 instruments.

CHAIRMAN But would you not in that case have expected something on the instruments in the stack of Pile 1?

HAMER Yes.

CHAIRMAN When did you first get this activity in the stack of Pile 2?

HAMER In the afternoon of the Wednesday.

CHAIRMAN It took several hours to get these instruments checked?

HAMER Yes. It was at 7.30 at night when finally they told me that the instruments had, in fact, been wrong. You see, the reason it sticks in my mind is because Mr. Gausden, the Manager was made aware of this position, which is quite a serious one, and we shut the pile down and examined the discharge face.

CHAIRMAN Do you think now then there was nothing wrong in Pile 2, other than these instruments recording wrong, or do you say maybe they were right?

HAMER I would not like to give an opinion on that. You made quite a strong point there. You said why didn't the Pile 1 instruments show it and that would seem to rule out that Pile 2 had nothing at all to do with Pile 1 instruments.

CHAIRMAN Would any member of the Committee like to follow this through?

KAY I am not clear. Was Pile 2 in fact, shut down or not?

HAMER Running normally.

KAY And, in spite of this high stack activity you say you would normally shut down the Pile on getting this reading?

HAMER Had that been confirmed we would have shut the pile down.

KAY I think you said it was confirmed.

HAMER No, I didn't say it was confirmed. We had this reading and before taking any drastic action, we had the instrument department check that the instruments were in fact telling the truth.

KAY What checks?

HAMER Checks on the ion chambers, the recorders and wiring connections at the top. They said it is not full scale at all, they said the wiring was rather ropey and it is listed for re-wiring now and when they finished finally the reading, well it had not gone back to what it was, but it was reasonable and acceptable.

KAY And you did not feel justified in shutting the Pile down?

HAMER Oh no, the Assistant Foreman does this himself and you are bound to accept his advice.

KAY What about the burst cartridge scanning gear? Was there any sign of trouble?

HAMER No.

KAY So that was really your evidence. The Pile stack meter was not supported by any other indication of a burst. You continued to do your normal scanning and there was no trouble at all.

HAMER Yes.

DIAMOND Mr. Hamer, when the Pile stack activity goes full scale, which I take it as on No. 2, what do they call it, No. 2 static burst cartridge detecting gear, is that what it's called?

HAMER No.

DIAMOND It doesn't matter; this is the one at the top of the stack which monitors the filters. When you have had a burst cartridge, does it normally do that?

HAMER As you just pointed out, we have several lines of defence, three main ones: the scanners, the statics and the stack activity. We have found within experience, missed the first two lines and been caught on the third. Even without the corroborative evidence from other meters we can sometimes pick it up on the stack activity, thence Mr. Gausden's decision to shut down the pile had it been confirmed.

DIAMOND Would it normally go full scale?

HAMER No, full scale is very high indeed. Full scale is 30 curies. We have shut the pile down on 25 and thought it was very shocking indeed.

CHAIRMAN Coming back to Pile 1 during this shift, things, as far as you were concerned, were running normally for a Wigner release.

Any questions on that? And that went right the way along until 9 o'clock when you went off duty? What about the next period when you came on in the morning, Thursday, at 8.30 a.m.

HAMER Well again until lunchtime things were quite normal. You have probably seen the programme. After the Wigner release was completed we had a loading programme and things had apparently been going quite according to plan. They said what an excellent release it was and I was looking forward to getting the teams under way to start on the production side of it. Just after lunch I was on the roof myself seeing that things were more or less winding up and all of a sudden the stack activity down below in the Control Room - they rang me about 2.15. Stack activity in Pile 1 had gone full scale.

CHAIRMAN Stack activity at 2.15 p.m.? The Pile activity reported to you was very high? And who reported that to you?

HAMER The process worker in the Control Room.

CHAIRMAN The process worker in the Control Room? Actually, on the record we have here, it went very high not long after noon, 12.30, so there is a bit of a delay there somewhere.

HAMER The reason I am so clear it was was after 2 o'clock, the change over is at 2 o'clock, it was the afternoon shift that reported it.

CHAIRMAN What happened then?

HAMER On Wigner releases, as you probably realise, when we have a fair amount of heat and no cooling, we have on re-starting found bursts, and it occurred to me that the cartridge had burst.

CHAIRMAN May I get that absolutely clear? You say that on re-starting after Wigner releases you have on previous occasions found a burst.

HAMER Burst cartridges, yes.

DIAMOND After previous Wigner releases you have found burst cartridges which are additional to those which may have shown up before the Wigner release started.

HAMER Yes. If one had shown up before a Wigner release started, it would have been pushed out right away. We shut down with a clear pile.

CHAIRMAN Has this happened several times, that you have found a burst cartridge after a Wigner release?

HAMER Coming up to power, it is quite a common thing.

CHAIRMAN When you come up to power? It's not just at a Wigner release.

HAMER I see I have given the wrong impression. It is on the return to power, that is when we find it. We don't find the burst during the Wigner.

CHAIRMAN No, no. When you have done a Wigner release and then you cool off, I suppose, then you bring it up to power and almost invariably there is a burst.

DIAMOND Almost invariably.

HAMER I think that is fair, yes. I can't remember instances, but it is quite a common thing - after Wigner, burst.

SCHONLAND During Wigner release, on this occasion at least it was not possible to use scanning gear. The heat prevented its movement.

HAMER That's true.

SCHONLAND Do you ever use the scanning gear during a Wigner?

HAMER Not during the Wigner.?..... static condition completely.

CHAIRMAN May I just amplify this question? If you shut down the power for any other reason and cooled it off and then brought it up to power again, would you also get these bursts? Do you think that it's actually the ... well this bursting, it's just bringing it up to power?

HAMER The Physicists can tell you more about this, but it does seem to me that the metal does get fatigued with the contraction of the cooling. It is down for three days for a Wigner usually on the average - it is up and down and it just doesn't seem able to stand it at all.

KAY Without discussing, for the moment, how this might happen, do you confirm the finding of burst cartridges on return to power is much more common after a Wigner release than after a normal shutdown for some other purpose?

HAMER No, no, when the power shuts down and we cool off, for a discharge or anything like that, when the metal has time to come down to 25-30°, then coming up again we expect bursts.

KAY So you expect bursts whenever you come on to power after a shut down?

HAMER Yes.

CHAIRMAN We were previously at a point where it had been reported to you that the stack activity was very high and that was 2.15 Thursday afternoon.

HAMER Yes, as I said, we do not normally have the scanning gear running so I tried to get it put back into commission to see if I could do a scan of the discharge face, in order to pinpoint one particular area which might be the source of the high reading. Well, the scanners had been?..... for maintenance too and I tried these things by hand first and found they had absolutely stuck, so I couldn't do that. What I did do was to get the turbo exhausters running to try to find out on my statics, altogether apart from the stack, on the statics and as soon as I got the turbo exhausters on and the gear running, I found on an instrument which we call the MX119, that went full scale deflection.

CHAIRMAN Are there many of these - the MX119s?

HAMER One.

CHAIRMAN The MX119 gave immediately a full deflection?

HAMER Yes.

CHAIRMAN What time would that be?

HAMER It must have been 3 p.m.

CHAIRMAN What did that mean, Mr. Hamer? An instrument has given you a full scale reading, where is it reading?

HAMER The statics are normally sampling the air, that is, sampling the gaseous effluent from the channel. If an end cap or really large hole comes in the cartridge you get uranium oxide, and that isn't measured. Now that can be caught in the filter and this peculiar thing which you've the MX119 actually measures the activity of any oxide, indeed any solid body that is caught on the filter, so if there is a solid body giving a full scale deflection on this particular recorder, it means that there is quite a serious burst.

CHAIRMAN And that filter takes air from all channels, or only from one?

HAMER The statics. You know, the discharge: the statics. The air from the pile comes round from two sides, one side covered by static 1, the other covered by static 2. Similarly, this MX ?, that covers two sides, both 1 and 2.

CHAIRMAN So that, although you now have this evidence of activity of solid particles on the filter, you still are not in a position to say where it has come from.

HAMER No. We are in a position to say there's a serious burst somewhere.

CHAIRMAN You know there is a serious burst but you do not know where?

HAMER Yes.

CHAIRMAN What happened?

HAMER Mr. Gausden, the Manager, was here by that time and there was one point on the graphite points. Everything else was normal except this one point and it was reading at this time about 415.

SCHONLAND That would be 20.53.

CHAIRMAN Yes, we have that recorded here.

HAMER That was puzzling, and I couldn't tie the two things up. We decided we would go on the charge face, find that particular channel, have the plug out, and see if anything could be seen there.

CHAIRMAN So you have to take out a lot of plugs?

HAMER No. Again, during Wigner, the normal uranium is disconnected and the graphite thermocouples are taken and connected to the various terminal blocks, so, shall we say, a terminal block which is normally associated in this particular instance it was 1761, might have, with the uranium disconnected, been tailing down to 2053, which is indeed a hot one. Can you visualise that now?

CHAIRMAN On this 2053 you have a high reading on that thermocouple in the graphite in channel 2053 and you thought. Well, let's have that particular plug out and have a look.

HAMER Yes. We were looking for something, we were not very clear except there's a bad burst there.

CHAIRMAN You thought. Well of all the most likely places where a thermocouple reading is high.

HAMER That plug was pulled out and we do not normally spend much time looking up the channel, because of the radiation - you'd get readings of 40R, things like that, so a quick glance up in this particular instance, it was like looking at four setting suns. You know, you have one large hole, you look across the air gap, it is then subdivided into four pans.

CHAIRMAN Four glowing suns in the fuel element channels? What about the graphite itself, was that just black?

HAMER I have been asked about that. The best thing I can say is that as you looked up the channel, it appeared to be almost incandescent and it appears that the source of this light was the metal. It was in the centre of the channel.

Now, I had no reason to think it was graphite at the time. I thought then myself it was the metal that was glowing. The source definitely appeared to me to come from the metal. From the metal in the centre of the channel.

DIAMOND Did it look rather like an eclipse rather than the setting sun?

HAMER Like the noonday sun. I said setting sun because normally you can look at the red glow.

DIAMOND There wasn't a black spot in the middle?

HAMER I did not see any black spot in the middle.

CHAIRMAN Were the fins glowing?

HAMER No, there was just a white glow in the centre.

DIAMOND What sort of intensity was it? Was it painful to look at?

HAMER It was frightening, but not dazzling. It was definitely white heat.

This evidence was taken at 10.35 a.m. on 24th October 1957.

DIAMOND White heat.

HAMER As one went further to the outside, it grew less in intensity. When we found that this unprecedented thing had occurred we pulled another plug out to see.

CHAIRMAN Yes. On the one plug - the first one you looked at - there were four white hot suns which were the fuel elements - the uranium part - and they were all about the same - and the next thing you pulled another plug.

HAMER Obviously something untoward had happened and we wanted to see the extent of this. I can't say which, it wasn't the neighbouring one. It was two or three along.

CHAIRMAN Yes.

DIAMOND Left or right?

HAMER Right.

DIAMOND On the same level?

HAMER Yes, and it seemed to be that this glow was along about 10 holes - I think 51 to 61. Again, I am going from memory of a week ago. After that, we got blackness again. And the centre part appeared to be brighter than the outer ones.

DIAMOND Do we know which channel this was or can't you remember?

HAMER There will be a record because after this, when we'd got ourselves sorted out, there was a plan of the Pile and I started to discharge all the way round it. My initial discharging is marked with crosses.

SCHONLAND It is 21/53.

CHAIRMAN Yes, it is.

DIAMOND 21/63 - sorry, 21/53.

HAMER (indicating on chart) On the left hand side. Somewhere down here. We started going along on the left here.

DIAMOND Were channels to the left of that affected?

SCHONLAND Is it possible to see up an isotope channel?

HAMER Sorry, we started coming along to the right here. As we came along, it seemed to get brighter here towards the centre – an area of 10 holes by 8 down, at least when I was on, it extended later, but I worked along here to the right, it got brighter towards the centre. As you went along here it tailed off to something like the same intensity as it was here, and if you went sufficiently far out you got blackness.

HAMER Normally, no. One only sees a little dark circle in the centre, and I saw nothing in the centre at all.

DIAMOND There is no gap – there is nothing like the same gap round the isotope plugs as round the fuel elements?

HAMER No – they are much smaller.

DIAMOND This one was black?

CHAIRMAN Now at this time Mr. Gausden is there, and is he directing things at this time? Has he taken over responsibility?

HAMER Yes. You could look straight up there and I realise now I was looking at the concrete on the discharge face which was illuminated by the glow from these channels. The back side is of course concrete. I am looking at the back of a channel which is perfectly ordinary and I can see the metal.

HAMER Very soon afterwards. At this particular point, Mr. Gausden asked me to try and locate the area of the heat while he went to get the machinery into motion as obviously something was screwy, and I was doing that when Mr. Gausden came back with Mr. Hughes, and after that, of course, he took over command completely.

DIAMOND This is an uranium channel. You can see the back concrete.

CHAIRMAN We have heard how you attempted to try and push out and how you could not get the middle bits out and then a fire break was put in. I think we are all clear about that. Is there anything that sticks in your mind?

HAMER Normally you can't, but in the flow from the channels you could see the concrete of the discharge side glowing, reflecting the heat. Normally you can't see the concrete, but with the glow from the channels you could.

CHAIRMAN The first one you looked at was very hot but you were not right on the maximum. The maximum was over to the right looking at the front of the Pile. Two or three channels over.

HAMER No. We tried the canes; we tried heavy gear and we got a soggy section. You see, we tried to get the fiery stuff out. We got about two out and then tried the heavy discharge gear again, but it appeared that the force of the blow was not being transmitted right through.

HAMER Something like that.

CHAIRMAN We have had a very long account of that. And then, how long did you stay?

DIAMOND What time?

HAMER Around about 3.30-ish.

HAMER Until about 3.15 in the morning.

DIAMOND 3.30 on Thursday afternoon.

CHAIRMAN A quarter past three. Are there any questions on this actual story?

HAMER That is an approximate time.

CHAIRMAN Then did you go to the left?

DIAMOND May I ask whether ? you mentioned that if an end cap comes off it is another way of describing a burst. Do you find this is what normally happens?

HAMER Yes, until we limited the area of what later on we began to call the fire.

HAMER When an end cap comes off or a can splits, invariably we get it on MX.119.

CHAIRMAN How far?

DIAMOND Yes, I just wondered if an end cap coming off was a more common form of burst failure.

HAMER At this stage I'm afraid I can't tell you precisely. It did appear to be about 10 holes on that level. Then we went up and down to try and find the vertical limitation, which was about 8 holes.

HAMER No – by far the most common form of burst are those we pick up on the scanners which are gaseous bursts, a minute hole, a pinpoint, so?..... it isn't even found.

DIAMOND By holes, you mean holes in the outer face?

DIAMOND If we can go back a little bit – were you in charge during the installation of the graphite thermocouples before this Wigner release?

HAMER Yes – holes in the outer face.

SCHONLAND Before we leave this point of the holes, can Mr. Hamer say anything about the isotope channels. Were they also glowing?

HAMER That would take place on all shifts. I was on morning shift. Some were definitely put in on my shift. I can't tell you the day, I'm afraid.

HAMER I have no memory at all of the isotope channels. They must have been perfectly normal, otherwise I would certainly have remarked upon them.

DIAMOND What is the procedure for installing graphite thermocouples before a Wigner release?

HAMER There are three different types. The stringers of graphite, one of which is a thermocouple, that is inserted in the graphite. There are four positions, four feet, six feet and ten feet measured from the front of the pile (Mr. Hamer only quoted three figures at this point, not four). The double cotton covered wire.

DIAMOND Just a moment ... on the graphite stringer, is that in during normal running?

HAMER Some are left in, yes. If we can use them again we try to, if not we discharge them. Some are left in. If there are any graphites left in – double cotton covered wire which is the insulation for these things, double cotton is the insulation for the wires, sometimes they survive and sometimes they don't. But in the area which is going to be used, any which are left in are tested by the Instrument Department and if in fact they are alright, we use them and if not we discharge them into the water and load up with fresh ones.

DIAMOND Fresh graphite?

HAMER Yes.

CHAIRMAN Just one more question. When the pile has been shut down for any reason it is all cold and you start it up again, how long does it take to get it up, is it a matter of hours?

HAMER I take it you mean power rather than temperature. The temperature would rise at a steady 2° a minute, standard rate.

CHAIRMAN You raise the uranium cartridge temperature by 2°/min.

HAMER That is the standard rate for increase and decrease.

CHAIRMAN Do any members of the Committee wish to ask any more questions?

SCHONLAND About the second period of nuclear heating, that was during your shift was it Mr. Hamer, on Tuesday? The second nuclear heat was put on at 11 o'clock and ended at 5 o'clock in the afternoon?

HAMER Yes.

SCHONLAND Can you tell me two things. First of all, what are the instructions for a second nuclear heat, and to what extent do you actually control the matter at all?

HAMER During a Wigner release, we operate completely on the direction of the physicists, the technical staff. We merely operate to instructions.

SCHONLAND Could you explain a little more? There is a physicist on the spot who takes charge?

HAMER Yes. That is a?..... requirement.

SCHONLAND Who was it?

HAMER During the day it was actually Mr. Robertson. I think I have the period rightly – it was when instructions were given to increase the temperature from 250 to 300, I believe. We wanted an extra fifty, no it was 280 to 330.

SCHONLAND Who was that given by, Mr. Robertson?

HAMER Mr. Robertson.

SCHONLAND Which temperatures?

HAMER The highest uranium temperature, it doesn't matter which one. The highest one.

SCHONLAND 280 to 330. But under this particular regime, when a nuclear heating is ordered, who decides when the thermocouples are coming down and the second nuclear heat should take place, is it the physicists?

HAMER Yes, definitely. We operate the pile to their instructions.

SCHONLAND And it doesn't concern you whether they are doing it at the right moment or not?

HAMER I do not think we are qualified to say.

KAY Does this standard rate of heating of 2° a minute still apply in those conditions, or not?

HAMER Yes. In this instance the pile had not diverged so the rods were running out, so there would be no increase of temperature for a start, and as I say I was not present but you have heard the story of how it did diverge, and went up quickly I'm told.

KAY The normal procedure would be that the physicist would say, I want 50° temperature, I want to take the temperature to 330. The pile foreman would operate the control rods, make the pile diverge and he would take it up to that temperature whatever temperature he was told, without questioning that, but following the normal drill and normal limits on rates of rise so far as he could?

HAMER That is quite correct.

SCHONLAND The records show us that the temperature reached during the second nuclear heating was 380. You were not informed of that?

HAMER I was not informed of that.

SCHONLAND There is another question; in connection with the temperature in the stack: the stack temperatures were round about 40° until 7 o'clock on the Wednesday morning and then they started to rise all the time throughout the day. Would you like to see the curve of stack temperature?

You see round about 7 or 8 o'clock Wednesday, the stack temperatures start to go up and all through the day they rose. Well, there is a lot of heating going on during a Wigner release, but would this be normal or is there anything abnormal?

HAMER It is not particularly abnormal, not at that point. That jump is definitely abnormal, but the other is quite normal.

DIAMOND This jump here before the damper was opened was abnormal?

HAMER Oh. This is the opening of the damper? No. But anyway that jump, indeed any jump at all, is something queer. The thing should be gradual, whatever it is.

DIAMOND You would expect it when the damper is opened, but not when the damper is shut.

HAMER No, only when it is opened. That is typical up to that jump.

DIAMOND Yes.

CHAIRMAN Any more questions? Thank you very much, Mr. Hamer.

Thursday, 24th October - 11.5 a.m.

WITNESS: MR. BUTLER

BUTLER My job is Pile Control Engineer, Technical Grade I and I am in charge of 2 foremen. I am responsible for the efficient working of both piles, and the pond, and the cartridge shop. My job is to make sure that we run the pile to the maximum efficiency with the least shutdown time possible. I am of course responsible for all the stock and stocktaking as well, in the cartridge shop, to make sure that the equipment is there, and in fact to make sure that the pile is working efficiently.

CHAIRMAN We are interested, Mr. Butler, in the whole period of the incident, that is, from the Monday until Friday noon and we would like you to tell us which shifts you worked during that whole period.

BUTLER From Monday?

CHAIRMAN From Monday until the middle of Friday.

BUTLER I went on the afternoon shift 17.00 hours to 24.00 hours each day until Thursday, with the exception of Wednesday and on Wednesday I commenced work at 21.00 hours and finished 24.00 hours.

CHAIRMAN You said 17.00 until midnight except on Wednesday when you went on at 9 o'clock. So this means you took over from Mr. Hamer?

Well now, what we want to do is just to go through those shifts as they come, to see whether while you were on duty anything unusual happened. Anything that gives us some light on the event. Now on the Monday shift, anything unusual?

BUTLER On the Monday, the programme was a little delayed, behind schedule, but as far as my shift was concerned, there was nothing unusual.

CHAIRMAN Well, I see. The Tuesday you came on.

BUTLER The Wigner was continuing.

CHAIRMAN On the Tuesday, had the second nuclear heating finished or not when you came on?

BUTLER As far as the re-heat was concerned, I had no knowledge of that.

CHAIRMAN On the Tuesday, that shift when you were working, was there anything unusual?

BUTLER Nothing particularly unusual on the Tuesday.

CHAIRMAN Well, let us come to the Wednesday at 9.00. What was happening?

BUTLER A good part of that time I spent on the roof of the pile with the junior physicist. Just watching him more or less take the reading. There were one or two occasions ... one occasion when we were asked to open the dampers.

CHAIRMAN The junior physicist, who was he?

BUTLER Mr. Goodwin.

SCHONLAND The shift on Wednesday. Could I ask about the opening of the dampers?

CHAIRMAN Yes.

SCHONLAND Was the damper opened during that shift?

BUTLER Yes, on the Wednesday between 9 and midnight.

SCHONLAND Was it opened at 9.00 and left open?

BUTLER It was not left open, no.

SCHONLAND A bit more about this damper: the Wigner release was still going on and who ordered the damper opening?

BUTLER The physicist.

KAY On the Wednesday night.

SCHONLAND I beg your pardon. We are speaking of the Wednesday night when all the dampers were opened, not the Wednesday morning.

KAY 10.00-10.15 p.m.

CHAIRMAN And the dampers were opened and shut, and you were on the pile roof looking at what? Thermocouple records?

BUTLER It was a general check round. I only started at 9.00, and I usually do a round (I am attending a Technical College). I usually make a round and during one of the rounds, I stayed on a little while on the roof. Of course I stayed until probably 10.15 and then I like to see the other shift, my own shift, going off. I usually see them in the P.C.'s

Office, and at 11.00 p.m. I started gathering the details for the log.

CHAIRMAN And you went off at midnight and everything was more or less normal.

BUTLER At that time as far as I knew, the Wigner was going satisfactorily.

CHAIRMAN The Wigner was going well. Did you think that any of the thermocouples were coming up rather fast or?

BUTLER There was one reading at 405°C at that time, I think. 5°C an hour, I think.

CHAIRMAN And during the Wigner release, you look to the physicist to tell you how to operate the pile.

BUTLER Definitely, yes.

CHAIRMAN That would be Mr. Goodwin, or is there a more senior man there?

BUTLER Mr. Goodwin was in at that time. I don't think there was any other physicist in at that time. I am not positive.

CHAIRMAN I see. Right then. At 12.00 off you went. Thursday, what happened?

BUTLER I reported for duty at 4.45 mainly because I had been to the dentist to get a tooth filled and I arrived at 4.45 p.m. I did not know anything was going on and I simply went over to the pile and was met in the magazine store by Mr. Hamer and Mr. Hughes and they simply said that the pile was on fire and they wanted some men to push the channels out.

CHAIRMAN And then you went in to give a hand.

BUTLER Yes. I worked there until Friday morning. I was on duty from that time, although Mr. Hamer until the same time. He stayed until 2 or 3 in the morning.

CHAIRMAN And were you there when the?..... were pushed out?

BUTLER I was there, we were discharging channels.

CHAIRMAN But by the time you got in there they knew there were red hot fuel elements in there, and they were starting to push them out.

BUTLER About the first job I did was to get extra equipment.

CHAIRMAN And you worked - how long did you work? When did you go off?

BUTLER 2.30 in the morning.

CHAIRMAN Did you notice anything that you thought was particularly important? Very odd?

BUTLER I am afraid at that time it was rather hectic and I was in charge of the teams discharging and while we did look up one or two channels, we were trying to build a fire break of course above the fire, and we were working pretty hard in masks.

CHAIRMAN I am sure you were. And you and all these men were properly dressed, in coveralls, plastic sheets and masks? Did you have all that to start ...?

BUTLER We had masks. There may have been one or two people who rushed up there who did not have complete coveralls. As a matter of fact, I first went up there all my own clothes on because I did not know what was happening until I got there.

CHAIRMAN What happened to those clothes?

BUTLER They were contaminated, but I suffered no contamination to the body, well, nothing to worry about.

CHAIRMAN Any questions? Well, thank you very much.

BUTLER I have actually been up all night and yesterday as well. If my answers have not been as clear as they could have been -

CHAIRMAN That's alright. We understand what you've told us.

WITNESS: MR. MORGAN

Mr. Morgan Pile Control Engineer, Technical Class Grade I.

CHAIRMAN And you are in a very similar capacity to the other two gentlemen, Mr. Hamer and Mr. Butler. You three are very much alike.

MORGAN We are the same.

CHAIRMAN We are of course interested in the period of this event from the Monday until the Friday and could you start please by telling us what shifts you were working during that period?

MORGAN I came on on Wednesday midnight. I was on from 12.00 until 8.00 and then the same on the Thursday and Friday evenings, on night work.

CHAIRMAN And you were not on on the Monday and Tuesday.

MORGAN I am the relief engineer and when I am not relieving on shift for people on holidays and sickness, I come on the normal day shift. I was on normal day shift Monday and Tuesday.

SCHONLAND Those times, please.

MORGAN 8.30 - 5.00.

CHAIRMAN I don't think we need worry about Monday, but on the Tuesday while there was a day shift there was a second nuclear re-heat which is one of the things we are interested in. Were you around while this was going on?

MORGAN I had nothing whatsoever to do with that.

CHAIRMAN So that we really start with you at midnight on Wednesday. Well, perhaps you would like to tell us what happened. Anything unusual?

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MORGAN I came on at midnight and went across to the pile, and I think it was about 1.30, the shut down dampers were opened on the instructions of

CHAIRMAN 1.30 a.m. opened the dampers.

MORGAN And closed when the physicist thought his temperatures had gone down sufficiently to warrant the closing of them.

CHAIRMAN Which physicist was that?

MORGAN Mr. Jenkinson.

CHAIRMAN So he said open them, close them, or whatever it was and he was watching thermocouples. You were seeing that the actual job was done as he wanted it.

MORGAN Nothing further of any consequence happened on my shift to my knowledge. I was called across to Pile 2 at something after 5.00. We had had some trouble across there with the stack filter activity and the pressure dropped. I was across there until 7.45 and at 7.45 I came across to the Office towards the end of my shift to write my log and at 8.15 my relief came in and the normal day shift people and that was the completion of my shift. On Thursday night when I came on at 12.00, I am afraid the place was absolutely heaving with people, and I got in and was told what had happened. I had a full change and went straight up on the charge hoist and I spent the whole night on the charge hoist on Pile 1.

CHAIRMAN While you were up on the charge hoist, did you see anything unusual that perhaps you spotted and someone else might not, or was it just a dreadful thing happening?

MORGAN I don't think I saw any more than anybody else. I did see plugs out from time to time when we were changing our discharge tube over, and looking down the channels as I remember, you could just make out in the channels that were alright, the finning of the first cartridge in the channel, but from there you could not distinguish really what the states of the cartridges were further down the channel. It just seemed the heat was more intense towards the back end of the channel, the channel itself.

CHAIRMAN You had the full clothes on and film. Everything that was required for the job?

MORGAN Yes, film badges, QFE, dust mask, everything that was required for the job, complete full change.

CHAIRMAN Any questions?

DIAMOND The discharge tube, Mr. Morgan, this is on the front face. This bridges the gap?

MORGAN It bridges the gap between the charge face hole and the actual pile face, the face of the graphite.

DIAMOND And it is taken out each time you want to put another ...

MORGAN You don't have to take it out each time, you serve four ... That's what I mean, it's an eccentric ...

DIAMOND But another charge face hole ...

MORGAN You have to remove it.

SCHONLAND Mr. Morgan, when these dampers were opened, they were opened on the instructions of the physicist for the purpose of cooling. The physicist did not remark to you that instead of cooling that in one case the temperature went up in two places, the temperature went up, and in the other case, the rate of fall was reduced. He did not tell you that?

MORGAN In the first case, I mentioned to the physicist that the temperature seemed to me to be rather high and I was told that it was giving a particularly good release in that area and there was nothing to worry about.

CHAIRMAN Any more questions?

SCHONLAND I have one which is from Prof. Diamond. Do you find that after a Wigner you get a more frequent rate of burst cartridges, when you start up again, than usual?

MORGAN Normally after a start up, we have always taken it that there usually are a few more bursts than we do normally get. We always more or less expect it after a start up.

SCHONLAND Do you expect it to be any worse after a Wigner start up than after a normal?

MORGAN No, nothing has shown it to be worse after a Wigner than normal times.

CHAIRMAN Thank you, Mr. Morgan.

Thursday, 24th October 1957 - 11.40-12.10 p.m.

WITNESS: MR. CUTTS

CHAIRMAN You were going to try to do what you could when the pile power took place and do a calculation for us.

CUTTS I have written up what I did last time and have discussed also questions of the flux distribution in the vertical direction and perhaps I had better run over what the report said. There's a little preamble about the difficulty of doing the calculation. I then described two problems which I think can reasonably represent the state of affairs in the reactor. No. 1 was the one we discussed last time with the rear set of control rods fully in and the front set fully out. The second problem essentially is with the front upper and rear upper control rods fully in and the lower ones withdrawn to some value to make the reactor critical. And I have looked at a vertical cross section through the reactor and made some attempt to

determine a flux distribution. Figure 1 is what we saw last time and I have shown the approximate position of our measuring point of interest in channel 25/57. The next diagram shows the side elevation of the reactor with notes on showing where the control rods really were. I have only quoted approximate positions. Figure 3 shows how I think we might idealise that state so that we can calculate a vertical flux distribution. I have turned the reactor into a sort of layer cake; in fact, completely rectangular sections, and the flux distribution I get out of that is given in Figure 4 and I think this would be a reasonable flux distribution to obtain towards the centre of the reactor, though as you moved away from the influence of the control rods, it would tend to depart from this kind of construction. Also I think that the actual position of the lower sets of control rods would not particularly influence this flux distribution. It would tend to make the flux distribution more or less peaky according to whether the control rods were further in or further out. The actual position of the control rods would essentially be determined by the necessity to counter-balance temperature effects etc. That was problems 1 and 2. Then I discussed what one would expect the peak flux distribution to be, accepting these diagrams, relative to that flux occurring at the measuring point. And if you accept these two diagrams as representing the axial and diametrical flux distributions, we find at the first part of para. 4 that the peak flux would be about $2\frac{1}{2}$ times that at the temperature measuring point.

CHAIRMAN Yes, I see.

SCHONLAND I have got para. 4.

CUTTS At the top of the page. The end of the first part of the paragraph.

KAY The peak points would therefore be 2.27 times that at the

CUTTS Yes. If you accept these two diagrams as being completely realistic.

I have gone on to point out that the first calculation is not particularly realistic, because this would demand that all the rear control rods were in and all the front control rods withdrawn. But, in fact, the lower rear control rods are in* slightly more than the lower front control rods.

*[NOTE 'withdrawn' may make more sense, but he did say 'in' - perhaps it is a later correction?]

All the other control rods being fully in, this would tend to make the flux distribution rather more symmetrical in the axial direction. In other words, it would pull peak more towards the centre as we discussed on Monday. Now, if you said let us suppose that this flux distribution became completely symmetrical, you then get a lower limit to this flux ratio and the peak flux would then come out at about $1\frac{1}{2}$ times that at the measuring point. Now the vertical flux distribution I regard as being much more representative than the axial flux distribution I have calculated. Because in the course of doing the calculation I had to take into account that there is an enriched ring of uranium round the flattened region to provide the necessary reactivity to support the flattened zone in the reactor. In doing the calculation I noticed that this

reactivity is so high that the flux in it would almost certainly be near the peak flux, and the peak flux would almost certainly occur in the enriched ring, though not necessarily, but it would be very near to that ring, so in Figure 4 the shape of this flux distribution is really determined by the position of that enriched ring relative to the measuring point.

DIAMOND Enriched ring?

CUTTS Enriched ring. So that I would feel much more confident about the flux ratio in that diagram than in the other. So I came to the conclusion that the peak flux would lie between 1.3 or 2.3 times that occurring at the temperature measurement position and although it would be very difficult to be specific here, I would incline to the lower end very much, say an outside value of $1\frac{1}{2}$ times, perhaps.

I have gone on in para. 5 to discuss the ion chamber activity that would occur as a result of this type of flux distribution. It would be very strongly influenced by this vertical flux distribution that I have produced because the ion chamber is located towards the top of the reactor and to some extent in the plane of the rear control rods. It is near that plane. Now the activity measured in the ion chamber is strictly speaking a measure of the current neutrons out of the reactor in the direction of the ion chamber, and in Figure 4 I have shown what is the normal flux distribution you would get in this type of flattened reactor. And of course the type of flux distribution we've got with the upper control rods fully in. And as you can see, there is a very great deal of difference between those gradients. I don't want to put a factor on this because the presence of the control rods as discrete points in the control half of the reactor does affect this flux distribution seriously, but it shows the trend that I have got there. I think perhaps the best deduction that one can make is that in normal operation of the reactor when you are withdrawing all the control rods perhaps uniformly (I am assuming this would be done), the activity in the ion chamber would rise on two counts as you started up the reactor. In the first place, it would rise because the general activity of the reactor was increasing, and in the second place, it would rise because the shadowing effect of the control rods is being removed. In the case we are talking of here, where the upper control rods are always in, these are the ones that determine the shadowing of the ion chamber. If you withdraw the lower control rods the ion chamber would only respond to a general rise in activity and not due to removal of the shadowing, so that the response of the ion chamber would be much slower than normal. I have put a final short paragraph in this paper pointing out that I have carried the calculations out under?..... neutron theory but this is generally permissible for large systems and is certainly valid when the points of interest are quite a long way from the black bodies or boundaries and this is the case; our measuring point is of the order of 10' from all such boundaries. Well, that is where I have arrived at so far.

CHAIRMAN Any questions?

KAY The ion chamber activity - while I appreciate fully your hesitation to put a figure on this, really, one cannot make a definite figure, would you be prepared to put a bracket - a range of ratios?

CUTTS Well, I think I might. This diagram here in fact shows an order of something like 10. I do not think it would be greater than that necessarily. A lot depends really on whether the ion chamber is precisely in the planes of these control rods. Certainly I do not think it would be more than 10 but I think it will be quite definitely of the order of 3. You can see the very marked effect this thing has on the reactor.

KAY It can hardly be less than 3, and is unlikely to be more than 10.

CHAIRMAN Is this 3-10 on the power of the pile or on the peak?

CUTTS It is on the peak flux relative to the activity.

DIAMOND Peak.

CHAIRMAN So, if you went on to power, you'd have to integrate it everywhere and the factor of 3 or 10 goes down quite a lot ...

DIAMOND But not on peak flux.

CUTTS Not on peak flux, no. On this diagram you see we have a factor, if we take this as the 'slab' reactor, the layer cake, of 10 on this current. The area under this diagram is about $\frac{1}{3}$ for the controlled flux distribution as compared with the normal one. So our factor of 10 becomes a factor of 3 on that.

CHAIRMAN Yes, I understand.

DIAMOND Well, Sir, we are very grateful to Mr. Cutts. I would prefer to read this before I think everything we want is here.

CHAIRMAN You mean you want to read it and ask Mr. Cutts to wait a little while in case we wish to ask questions.

DIAMOND I think that might be the sensible thing to do.

CHAIRMAN Can you stay until after lunch?

CUTTS Yes, I think I can now.

SCHONLAND This idea of pushing the flux forward was initiated many years ago and evidently examined in some detail. Do you know of any reports, memoranda or calculations on file of this same character?

CUTTS No, I do not know of any. I didn't know that this was in fact done in this reactor. There seems to be no real objection to it providing you know what you are doing and you know what instruments to watch when you operate a reactor of this kind. But then I have not necessarily been associated with committees to which these reports would be sent.

CHAIRMAN Thank you, Mr. Cutts. We will read this before lunch and if we do have any questions to ask you, we will ask you to come in after lunch.

Thursday, 24th October 1957 - 11.55-12.10 p.m.

WITNESS: DR. HARDY

CHAIRMAN We are interested at the moment in the effect of raising the temperature of the fuel inside the can and seeing whether the can is likely to stay intact. The fuel in it of course may not be new. It is the fuel which was in the pile and the point we have in mind is that if the pile is producing energy suddenly, the temperature of the fuel goes up and it must take time for that heat to be diffused to the can or it may be just straight pressure strain put on the can due to the expansion inside and yet not much expansion on the outside. This must be a matter which is related to the rate of rise of temperature of fuel in the can. We would like you to tell us of anything you know which has been done in the Authority.

HARDY I take it you are postulating something like a 100°C difference possible between the fuel and the can.

CHAIRMAN We mentioned that figure which just to be definite, we don't say that it actually happened, but what sort of ...

HARDY We can take that as a notional figure?

DIAMOND The temperature from which it rises being about 300.

CHAIRMAN Yes, starting at about 300°C and going up to 400 in a matter of a few minutes.

HARDY First of all the coefficient of thermal expansion of aluminium at about 150 is 25.5 times 10^{-6} and of uranium at the same temperature is close to 15.5×10^{-6} and these figures both increase as the temperature is raised, but they increase at about the same rate. So I have in fact done my calculations on the basis of these notional figures. We know from experience of the pile operation that a certain number of failures are what we term end cap failures or shoulder failures.

CHAIRMAN Are they the same thing?

HARDY Yes, they are identical. The term shoulder failure is really more appropriate than end cap because the part that fails is the part between the end of the fins and the actual end cap itself. There is in the mark 10 approximately 4 mm of what in our tests we have called this free length, close to 4 mm.

During the past six to nine months we have been carrying out tests designed to measure how much extension this free length will accept before it fails and to determine how the end detail can be changed to increase the amount of extension which can be accepted before failure, and all except one of our tests has been carried out at a temperature of 200°C, since this is the temperature at which wrinkling and, we believe, growth of the uranium is most serious.

DIAMOND Does it diminish above that?

HARDY It tends to go through at a maximum at about 200° and our tests take the form of having a standard can which is sealed at one end. It is heated up to a temperature, in our case of 200, and there is a plunger passing through the inside of the

can up to the closed end cap. and the end cap is pushed off at a constant strain rate and the strain rates we actually used are close to 0.010"/hour.

SCHONLAND Give me that figure again.

HARDY 0.010"/in/hr. Well, we have results on standard mark 5 and standard mark 10. I should explain that the ends of the can, we place a square grid on the outer surface of known dimension so that after the test, we can see what has happened and how the strain has been accommodated. You realise that in this test the end cap is pushed off completely, so in order to determine the extension at which failure occurred, we have to work backwards to some extent because you can't fit the pieces together, and at the position where the end cap and the can is furthest apart, we measure the strain along that line, and we take the strain, the breaking strain close to the fracture is 35% because you cannot measure it; the grid has been destroyed at that point, whereas it might rise to 70% over very small regions. I will give you the consequences of that in a moment.

Well, we have done four standard mark 6 and three standard mark 10 cartridges under these conditions, and I am introducing the mark 6 because the design is identical except for the bore diameter, and since we are dealing with uniaxial strains, we can treat them as identical. Now the cap extension before fracture in each case was identical over these four tests on mark 6, and three on mark 10, and it was 1.1 mm ± 1.5 mm. The uncertainty due to lack of knowledge of the actual strain at the point of fracture is about .3 mm, so to sum this up it means that the can at this end will accept something like between 1 and 1¼ mm before it fails.

I will pass this drawing round which shows the end detail magnified and the actual plot of the state of strain along the shoulder. That is typical of both mark 6 and mark 10.

So at a temperature of 200 we can expect a relative expansion of the uranium relative to the aluminium of between 1 and 1¼ mm before failure occurred. Now, yesterday we did a similar test on a mark 6 cartridge at a temperature of 300°C and in this case the extension before fracture was 2 mm, and to this figure we can only say that it was about 2-2¼ mm but at a temperature of 300 of course, the extension before fracture has gone up because the aluminium is more ductile. So I think the figures to remember are something between 1 and 2 mm and we now look at the sort of relative expansion you can get in the uranium, in length.

CHAIRMAN Yes.

HARDY Now the most dangerous position, well let me say first of all, the least dangerous position would be that in which the uranium and the aluminium were gripped together so as the uranium extended it pulled the whole of the aluminium can with it and this would put almost zero strain on this free length. Against that the most dangerous condition is that in which uranium and aluminium are gripped at one end together and all the extension of the uranium is thrown on the shoulder at the opposite end. So if we now take the exact length of the uranium bar 29.2 cm, and the thermal expansion as 15.5×10^{-6} for a 100° rise, this gives an expansion of the uranium of .45 mm or point 5 mm as a round figure and this is less than half. This can will extend before failure. So if one

has a cartridge in which none of this extension of the shoulder has occurred during service, then you would expect it to be quite safe. But we know of course, from the failures that have occurred in the pile that the uranium can extend: or, alternatively, you can get wrinkles at the end of the bars, which comes to the same thing, and therefore some of this extension could have been taken up during the service of the pile before the Wigner release, and if something like half of that extension had been taken up or half or three quarters of it then you might be able to create a burst under the conditions you postulate.

CHAIRMAN In your discussion you have not got a time factor at all.

HARDY No, Sir. The only time factor we have is the rate at which we did our tests. Aluminium is a very reasonable material and I would not expect it to show brittleness under very low rates of strain. In any case, the rate of a strain you were talking about is going to be quite fast so I think that these extensions are not unreasonable figures to work on.

CHAIRMAN You have done less than a dozen tests so far.

HARDY Mark 6 and 10 – we have done a total of 7 tests.

CHAIRMAN Do you think if you did a thousand you would start finding trouble?

HARDY You would certainly get failure if you were to extend the range over which you had to ... You would extend to a lower linear extension the value at which fracture occurred, yes. Aluminium is a very reproducible material and you would not expect this to come down by more than a factor of 2 at the most.

CHAIRMAN Now apart from the possible wrinkling of uranium inside its can when it's in the pile, are there any other things? Does the aluminium change because it has been in a neutron flux?

HARDY No, the aluminium does not change. There are three factors which can lead to bursts – 1. Chemical reactions between the uranium and the aluminium:?..... graphite film, but we know it occurs; the second defect is due to irradiation, the growth extension of the uranium. The third one would be the thermal effect arising from the differential thermal expansion.

CHAIRMAN We are told that after the pile has been shut down and then started up again, it is by no means unusual to find cartridges failing.

HARDY Well, I have been told that as well, Sir, but I have also been told that a statistical examination does not confirm that, and the rate of burst after a shutdown is in fact very close to the normal rate of burst.

CHAIRMAN Probably like to hear the evidence.

HARDY The person is Mr. D. Williams from R. & D.B., Windscale. He has made a study of this particular aspect. He could also quote burst rates if you don't already have them in the zone of interest during the period immediately preceding.

CHAIRMAN We shall almost certainly want Mr. Williams this afternoon.

HARDY The burst due to chemical reaction would go on at the same rate under Wigner release as in the pile because they are most likely to occur in cartridges that are running hot as normally. Bursts due to irradiation effects will cease of course.

DIAMOND Why do you say that?

HARDY Burst due to chemical effects? Yes you see, the peak running temperature of the can is 395 which is not very different from the temperature during the Wigner release and growth of the uranium will not continue because the pile is in effect shut down from the neutron point of view.

DIAMOND But from this sudden increase of heat, the most suspect cartridges would be those that had normally run at 200?

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(Continuing Dr. Hardy's evidence)

HARDY In my opinion, yes.

DIAMOND What if ... this rate of strain that you postulated as 0.1.

HARDY This is the rate of strain we used in our tests, because it enabled us to put them in overnight ...

DIAMOND How long does that take to give you the breaking rate of strain on the cartridge shoulder?

HARDY In about 4 hours in round figures.

DIAMOND I wonder if Dr. Hardy would summarise this report for us, so that it can be tabled.

HARDY You want me to write it out? I can do that.

DIAMOND There is the time factor, of course.

HARDY When do you want it?

DIAMOND Is it in a form we could understand now?

HARDY We do have at the moment a report in draft but I do not think they are in a suitable form for covering these results, but I will write it out this afternoon here and submit it to you before I leave.

CHAIRMAN Thank you.

DIAMOND To go back to another point. When you were here before, you gave us ... I want to make quite certain I've got this down correctly. I have down here a defective cartridge at 400° may oxidise completely at 12 hours.

HARDY Yes.

DIAMOND How big a hole is that defect supposed to be for those conditions? Is it a pinhole?

HARDY I am afraid I cannot answer that question. You see, you can tell a defective cartridge and it is quite likely that you would heat it for 12 hours at 400° and nothing would happen, but we have in our experience had cartridges which were examined in the furnace and apparently nothing had happened to them, yet 12 hours later when we went back there was just a mass of oxide present.

DIAMOND Was the furnace on or off?

HARDY The furnace was on all the time.

DIAMOND At what temperature – 400?

HARDY Yes.

KAY So there have been a few cases, not many.

HARDY There are only two or three cases where this has happened so severely.

KAY When an apparently quite satisfactory cartridge oxidises complete at 400.

HARDY We have had two or three cases; we have probably had something of the order of 10 to 50 cases where we have heated cartridges which have only burst partially and have not oxidised completely.

KAY Was this anxiety felt with the raising of the normal working temperature up to 395°, or was it simply felt that it was only a statistically small number of failures occurring in this way, and they would be picked up on the scanner gear?

HARDY Your last sentence is correct. We would expect them to be picked up and the number not to be larger because the temperature is larger but ...

KAY And you would expect them to be picked up quickly provided the scanner gear was in operation.

DIAMOND If a defective cartridge with bare uranium were given a sudden increase in temperature from about 300 to about 400 or something above that, for a short time, say 10 minutes, and then the surroundings were reduced to 300°, do you think that the oxidation which would start would be maintained?

HARDY If the temperature reduced to 300 I do not believe it would be maintained, but if oxidation were proceeding at a self-maintaining rate, the heat liberated inside the oxide, and the oxide as a fairly poor conductor, the temperature at that point would not be reduced.

DIAMOND This kick of 10 minutes might start a self-maintaining reaction?

HARDY Let me put it this way. If a self-maintaining reaction were started in that period of 10 minutes I believe it would still be maintained when the temperature was reduced to 300.

KAY Have you any information about the rate at which this oxidation proceeded. I think you mentioned 12 hours the total time in the ... have you any evidence of when this happened? In the first hour, or spread out?

HARDY I have no more specific evidence.

KAY Just a heap of ash at the end.

HARDY And there were only one or two such cases.

DIAMOND Do you know of any experimental work now proceeding on the combustion of uranium in still air? In fact, any experimental work in the past?

HARDY The only experimental work I know of in the past I have reported. Consideration is being given to further experimental work to be carried out at Culcheth and I believe channel experiments are planned at Windscale.

DIAMOND This furnace work was in still air, wasn't it?

HARDY Air which was slightly moving: circulating air.

DIAMOND Free to the atmosphere, but not a forced draught?

HARDY It is not still air and is not moving very fast. Air is being churned up in order to equalize the temperature in the furnace and so the movements are fairly slow.

Mr. R.I. Robertson's evidence

CHAIRMAN Will you please begin, Mr. Robertson, by telling us what your job is with the Atomic Energy Authority.

ROBERTSON I am the physicist in the Pile Group at Windscale and my job is to look after the physics side of the operation in the Group.

CHAIRMAN You have told us what your job is, perhaps you could amplify a little with particular reference to your job during a Wigner release.

ROBERTSON I am in charge of the initiation of the release and in charge of keeping an eye on what goes on during the release. I cannot stay there all the time, so for part of the time this is delegated to my two assistants.

CHAIRMAN Who are these two assistants?

ROBERTSON Mr. Jenkinson and Mr. Goodwin.

CHAIRMAN Do you have the responsibility of saying when a Wigner release is becoming necessary?

ROBERTSON Yes. We are advised by R. & D. on the necessity of a Wigner release but the actual date is usually fixed by myself in consultation with the Group Manager having regard to the fact that we have more direct information as to the total power output of the Pile during a given period.

CHAIRMAN We have been told that the last Wigner release was planned at 40,000 MWD power in the Pile. Did you take part in that decision?

ROBERTSON Yes. The decision was taken after we had had a number of abortive attempts to anneal at 30,000 MWD/ton especially on Pile 1, where at the end of 1955 and the beginning of 1956 we had three attempts to anneal and in the end even a third attempt did not anneal all the energy out. It was becoming obvious that the graphite was saturating and becoming more difficult to anneal because there was less energy present that would anneal out. Work by R. & D. by Bell and Simmons and their people showed that this was, in fact, the case and a report was published which recommended in fact that we should not anneal more frequently than 50,000 MWD. This was thought to be rather too long and a proposal was made to the Windscale Technical Committee that we do it at 40,000. We thought that going from 30 to 50,000 was too big a jump.

CHAIRMAN You have said that the Wigner release was becoming more and more difficult to initiate and that you had had several abortive or not completely successful attempts. Did that mean then that on this occasion some of the graphite at any rate in the Pile had not been annealed for some long time. It may have escaped being annealed on earlier occasions when it was not completely successful.

ROBERTSON As far as we could determine there was a very small region at the very bottom of the Pile which had not definitely indicated an anneal, at the previous anneal which was in November last year. The rest of the Pile had definitely indicated an anneal on that occasion.

CHAIRMAN What do you think was the most MWD that any graphite had had when this present anneal was attempted?

ROBERTSON Perhaps 80,000.

CHAIRMAN You think that was right at the bottom of the Pile and presumably in the front. How did you decide that?

ROBERTSON We withdrew graphite specimens and checked by annealing them in the laboratory.

CHAIRMAN Did you also consult the thermocouple records of previous releases?

ROBERTSON Oh, yes. When I say I considered that there was only a small region at the bottom of the Pile which had not annealed, the first indication was that the thermocouples had not shown a rapid rise in temperature, and we then took out samples from regions which had not given any indication of rapid rises in temperature.

CHAIRMAN As the physicist in charge of this operation, what rules do you have? What instructions do you give?

ROBERTSON The instructions given are not really very hard and fast because one cannot ever say precisely what temperatures are likely to be attained. The general instructions are that during the initiation, the first initiation of the anneal, a temperature of 250° will not be exceeded in the uranium. That is during the initial period of nuclear heating. What happens after that largely depends on how the anneal goes. In 1955, I think, the Group Manager issued a piece of paper which gave maximum temperatures, and which laid

down operations which should be performed if these maximum temperatures were exceeded, and I think the absolute maximum temperature laid down was 415.

CHAIRMAN Is that the document signed by Mr. Fair?

ROBERTSON That would be the document. There were other actions to be taken at lower temperatures than that.

CHAIRMAN So that the Pile Control Engineer who starts this operation, the first thing that he does is to bring the uranium temperatures up to 250.

ROBERTSON Normally I do not like him to bring them up to 250 straight away. I usually have him hold them at about 180 for a while and then gradually ease up to 200 and then in gradual steps up to 250.

CHAIRMAN Then at 250 the Pile is shut off?

ROBERTSON No. The Pile is maintained at power maintaining that temperature. The control rods have got to continue to run out. In order to maintain the temperature at 250° one has to put a little more reactivity in because of temperature poisoning. The Pile is not normally closed down until the uranium temperatures are maintaining themselves from graphite heat.

CHAIRMAN In the hypothetical situation which we are following now the Pile is tapered off more and more and the graphite heating is now maintaining the uranium temperatures. This is at 250, at this stage?

ROBERTSON At that stage, it is at 250, yes.

CHAIRMAN Then what happens?

ROBERTSON It depends on whether it is a good anneal or a bad anneal. In a good anneal the uranium temperatures will rise. The instructions in that case are that the Pile Control Engineer or Foreman on duty will run in control rods in an attempt to maintain the temperatures at the 250 level. However, if the graphite near the thermocouples is releasing, they go up and one cannot do anything about it. So no matter how quickly they run in control rods, the uranium temperature still continues to rise purely from graphite heating and perhaps a little fission product heating.

CHAIRMAN And in a bad anneal?

ROBERTSON In a bad anneal the Pile appears to be losing more heat than it is gaining and one may have to raise the nuclear heating temperature of the uranium, and in fact this occurred on this occasion.

CHAIRMAN And when it is decided by you or whichever of your assistants is physicist in charge, that the anneal is not going satisfactorily and therefore a bit more nuclear heat is required, how does he come to that decision? Does he look at the thermocouple readings?

ROBERTSON He looks at the thermocouple readings, yes, and by having a general look at the picture of what the thermocouples are doing, especially if they are falling. In the early stages of the anneal the falling temperature is a bad thing because it means that some parts of the Pile are cooling off too rapidly and the release might not spread.

CHAIRMAN If you have got the position of the odd one going up and all the rest going down, what do you do?

ROBERTSON If there was only the odd one going up it is almost certain that I would heat up again.

CHAIRMAN This is a matter of judgement at the time?

ROBERTSON Yes.

CHAIRMAN On this particular occasion, were you there when the release was begun.

ROBERTSON I was.

CHAIRMAN When was that?

ROBERTSON On the Monday evening, the Pile diverged at 7.30 and the first indication of an anneal was somewhere between midnight and one. I am afraid I cannot say more definitely than that.

CHAIRMAN You were there?

ROBERTSON Yes, I was there.

CHAIRMAN How long did you stay?

ROBERTSON I stayed until approximately 2.0 a.m. and then went home as the anneal appeared to be going, slowly, but it appeared to be going.

CHAIRMAN Was one of your assistants there then?

ROBERTSON Jenkinson was there.

CHAIRMAN It appeared to be going but then you or one of your assistants thought it was losing pace and wanted a bit more heat.

ROBERTSON Yes. On the Tuesday morning, the temperatures in the lower half of the Pile were in fact falling.

CHAIRMAN Who decided to put some more heat on?

ROBERTSON I am not very clear on that point. I was in the midst of flu and was in bed the next day.

CHAIRMAN It wasn't you, then?

ROBERTSON It may have been me. I was in on Tuesday, but I wouldn't like to say definitely. If someone else said definitely it was me, then it was me, but I think I came in somewhere at about 9 to 9.30 on that morning. I am confident that if it wasn't me who decided to do this, if it had not been decided, I should have thought it worthwhile doing, and if it was me then obviously I did think it was worthwhile doing.

CHAIRMAN When did you come back from your flu? Were you in bed with flu during the rest of the period?

ROBERTSON I was in bed with flu on the Wednesday and Thursday.

CHAIRMAN What time did you leave on Tuesday with flu?

ROBERTSON At 5.0 p.m.

CHAIRMAN You had it coming on but stayed here until 5.0 p.m.

ROBERTSON As far as I can say at that stage, the anneal was going quite well.

CHAIRMAN When you were in bed with flu you had only now two assistants to cover a 24 hour day?

ROBERTSON Yes, I presumed the Group Manager would take over my part of it.

SCHONLAND What I would like first of all is to take some of the history of a Wigner release. How long have you been associated with this business, say on the Technical Committee?

ROBERTSON Since the beginning of 1955.

SCHONLAND By that time the technique which you are now using had been established. the forward heating?

ROBERTSON Yes.

SCHONLAND One thing that puzzles me is that when the forward heating technique was first introduced, the fuel element thermocouples were moved forward to the 6' position, but they are no longer in that position.

ROBERTSON I hardly think that is correct. in fact as I remember the situation from the reports I have read, the fuel element thermocouples were loaded into the 6' position in perhaps two of the very first anneals and later this was discontinued.

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SCHONLAND That is my point: when it was first done ...

ROBERTSON When the very first anneals were done, but I don't think at that stage there was any particular emphasis on forward heating.

SCHONLAND Well, I can find that if you give me a moment. It is the 1953 minutes but you were not there then.

ROBERTSON No, I was not there then.

SCHONLAND It is on the record that in view of the fact that the temperature in the graphite was greater than the temperature of the cartridges, the thermocouple cartridges were placed in a 6 ft. plane.

ROBERTSON Yes. I do not think that indicates that forward heating was used particularly. My impression of the first couple of anneals was that the control rods were just run out normally. This may not be so, but this was my impression and the technique was gradually developed.

SCHONLAND In other words, it was not realised that placing thermocouples at 16 feet in the fuel elements did not give the maximum fuel element temperature in the channel. Is that what you are saying?

ROBERTSON No.

SCHONLAND You agree it did not give the maximum fuel element temperature.

ROBERTSON Yes, it need not give the maximum fuel element temperature.

SCHONLAND There was no consideration given to the point that to push the flux forward would push the temperature forward in the channel.

ROBERTSON As you say, I was not there then and the point has not been raised since I took over. On previous anneals, on the early anneals, in which I was involved the thermocouples in the cartridge No. 14 appeared to give adequate cover.

SCHONLAND The anneal went on alright?

ROBERTSON We had no trouble either then or afterwards, in fact we have never had any trouble until this.

SCHONLAND I have another question. There seems to be ... We have a report which was issued showing that the frequency of bursts increased with the temperature of the fuel and with the irradiation, and after 400 MWD it rose rather rapidly.

ROBERTSON Yes, Sir.

SCHONLAND In this particular instance, were you acquainted with the total dose the fuel had had?

ROBERTSON I was, but it varies considerably from zone to zone. The Pile is divided into 8 zones, loaded individually and the maximum irradiation of any fuel in the reactor in bulk was of the order of 460-470 MWD/T. That was not, however, showing a rapid increase in burst rate at that stage.

SCHONLAND In the Minutes of the Technical Committee some months ago you promised to report on Wigner release but they never got round to it.

ROBERTSON Well, in fact, quite a lot of the information you have been fed came from the work which was done on that report. It has not yet been published for the main reason that we have been all this year under strength in the group and myself and my assistant have both been trying to do more than normal.

SCHONLAND If you have written it, I think the Committee would like to see it.

ROBERTSON It is not yet completed but the information we have given you on previous anneals is in fact taken from the work which we are doing on it.

SCHONLAND If we may go back to the second anneal, it is easy to have hindsight, but one sees that the temperatures were rising quite rapidly at that time in at least two channels.

2053 and 1960. You say nuclear heating started about 11 o'clock. 2053, 1960 were rising, 2053 was rising slowly, 1960 was rising rapidly and in the others the release was either complete, or had not yet been initiated. Now if you turn to Chart 6, 1960, now at ten feet, was rising rapidly. 2148, a new one, was rising rapidly and the other two, 2661 was rising slowly.

ROBERTSON Actually these temperature rises as seen on the actual chart are comparatively slow compared with the sort of temperature rises one gets when the energy is released.

SCHONLAND The interval of time between the two heats is 11 hours. Do you still feel that if you had to order the second nuclear heat, with hindsight, you would have ordered it at that point, or you would have let the thing go on to see what these rises were going to do.

ROBERTSON I think that I probably should.

SCHONLAND You see, the only guide that you could have could be experience; there is no written record of what you should do or what you should not do, and in the Minutes of the Technical Committee of some years ago, they were very careful to see that all temperatures had fallen. Now you seem to be more confident and you do not wait that long, or you may find it is so difficult to anneal that you cannot wait that long.

ROBERTSON This is the point: we have done this re-heating several times before, with no serious ill effects.

SCHONLAND You spoke of 80,000 MWD down at the bottom end of the pile. Were you aware that 2053 which is in the centre, had not been released for 80,000? This comes out of the record.

ROBERTSON I was not aware of this. I was under the impression that it had in fact annealed at the last November anneal.

SCHONLAND You will find it did not.

ROBERTSON What was the temperature to which it got at that anneal?

SCHONLAND It is on the wall. (Studied chart on wall). So in your opinion, 2053 did anneal at the last time.

ROBERTSON The six foot thermocouple certainly indicates an anneal.

SCHONLAND Yes.

DIAMOND Continuing that last point, do you think it conceivable that, although 2053 last time did indicate an anneal, the graphite near it might not?

ROBERTSON It is always possible.

DIAMOND You agree I take it that the annealing is very patchy.

ROBERTSON It can be patchy. We have never had evidence of patchy annealing in the centre before. We have had it at the edges.

DIAMOND On previous reheats ... you did mention this point, but had the pile always been allowed to cool?

ROBERTSON No.

DIAMOND There have been previous reheats on a rising temperature.

ROBERTSON Yes.

DIAMOND And although the graphite temperature is falling there may still be a Wigner release.

ROBERTSON I have never seen a release indicated in a region of falling temperature, and it is my opinion that in a region of falling temperature this cannot occur because the graphite has already been hotter than the so-called triggering temperature. If one heats up graphite and then cools it down, I cannot see any mechanism for initiating a release on a falling temperature curve.

DIAMOND Then on Sheet 6 No. 2550 - do you think that Wigner release was complete at that levelling off temperature of about 315.

ROBERTSON In that region - yes.

DIAMOND And that the subsequent rise of that temperature is due to heat flowing in from elsewhere.

ROBERTSON Probably heat from uranium. We started nuclear heating just where the rise starts.

DIAMOND On Sheet 8 you mentioned earlier on that the temperature taken for guidance in uranium was about 250°.

ROBERTSON Yes.

DIAMOND But when the second nuclear heating was started the temperature of 2557 was already 300 and rising.

ROBERTSON This is so, but on Sheet 7 and Sheet 5 the graphite temperatures were falling fairly rapidly, and it was my opinion that I would have to heat the uranium in order to maintain temperatures of the graphite at a reasonable level in that region in order that the anneal might be caused to spread.

DIAMOND So that the prime consideration in your mind, I know you had flu, would be the graphite temperature and not the uranium temperature, unless the uranium were at what temperature.

ROBERTSON If the uranium got up to 395 or somewhere around 400 perhaps, if it had been up there I should not have dreamt of doing any heating. The indications were that in the region where the heat was being inserted nuclear heating was being started, the graphite had annealed and there appeared no danger of energy coming out of the graphite in that region and so giving a rapid rise in the uranium temperature.

DIAMOND Yes.

CHAIRMAN How did you know graphite energy had come out in that region?

ROBERTSON 2550, 2661, 2655 had all gone up to ...

CHAIRMAN The same channel but one is 16' back and the others only 4' and 6'.

ROBERTSON Well, we never get any energy out right at the back.

CHAIRMAN You are not worried about the back.

ROBERTSON No - about ten feet back is the maximum distance that energy really comes out.

DIAMOND Nobody asked, I take it, for a sudden heat input on this occasion.

ROBERTSON No, we never ask for a sudden heat input.

DIAMOND Was this a usual arrangement of control rods during a Wigner release. This arrangement of control rods has been done before?

ROBERTSON Frequently. In fact, this is the arrangement of control rods which has been used ever since I took over and was being used before that.

DIAMOND One last question. You told Dr. Schonland that the maximum dose rate of the uranium anywhere in the pile was between 460 and 470 MWD/T.

ROBERTSON That is bulk; there may have been an odd channel, but we always have an odd channel.

DIAMOND What might the odd channel have been up to?

ROBERTSON About 600 perhaps. It is almost certain that these were in zones 5 or 6.

DIAMOND These 600 MWD/T fuel may have been in zone 5 or 6.

ROBERTSON It is most unlikely for them to have been in the central region of the pile.

DIAMOND And the 460-470 MWD/T refers to zone ...?

ROBERTSON Zone 4 which we have got partly marked on the chart - the red blocks here. (Mr. Robertson indicates Zone 4 on the chart).

CHAIRMAN Is it circumferential?

ROBERTSON It is a ring.

KAY During a normal Wigner release in the first nuclear heating, you set a limit of uranium temperature of 250°; that does not apply if it is found necessary to carry out a second re-heating?

ROBERTSON Not necessarily.

KAY Can you tell me what instructions you give to the Pile Control Engineer if you decide to carry out a second re-heat.

ROBERTSON Yes, they are very similar to the ones given in the first instance, in that one says, run out the lower control rods, these being the only ones he can run, the upper ones being disconnected, until you get a maximum temperature of?.....

KAY You give him a temperature to bring the pile up to so many degrees.

ROBERTSON 330 or something like that. In fact, that was the instruction given in this occasion.

KAY Do you give any instruction with regard to the rate of heating or do you assume that he would work to the normal drill?

ROBERTSON I assume that he will work to the normal drill which is a maximum of 2° a minute temperature rise. I doubt in fact if he could get that. I doubt in fact if he could, in general running, get much more than 2° a minute rise in temperature with the control rods he has available.

KAY I see. Do you set any other limit? Do you limit the power indication in any way or not?

ROBERTSON No, in this sort of instance, in both the initial and the possible reheat the power indication is not very good. The ion chamber which gives the power indication is in the wrong part of the pile with this configuration of control rods to give this figure. The emphasis is most certainly on temperature.

KAY You would assume that the power meter would give you a false reading which would read low because of masking by the control rods so that you are watching out for temperature and you give the Pile Control Engineer a temperature to which he should take the pile.

KAY Will the Physicist be in the Control Room?

ROBERTSON He might and might not. There are other things to look at.

KAY And it is the responsibility of the foreman in the Control Room to actually move the control rods.

ROBERTSON Yes. It takes a long time for the pile to diverge after one starts to run the control rods. I think on this occasion the rods started to run about 9 o'clock in the morning and the pile did not diverge until 11.

KAY Was any comment made about the rate at which the pile diverged, at which the temperature was built up on this occasion?

ROBERTSON I do not recollect. I noted that some of the temperatures had gone up to a peak and had been brought back. I looked at another lot of temperatures and did not seem to see any peak. I was somewhat puzzled at this I must say.

KAY But this was not your prime concern. Your main concern was with the graphite and the release of energy in the graphite.

CHAIRMAN One question I would like to ask and that is on the Pile Stack Activity Meter a little drawing (Reference to chart). Now if you are on duty or if your assistants are on duty, have they any responsibilities to watch the Pile stack activity meter, or is this just another instrument?

ROBERTSON In general, it is another instrument. It is rather a peculiar instrument in many ways, in that a large number of things can affect it.

CHAIRMAN You were not on duty at the time on the 9th and I cannot ask you about that, but if you had seen a peak of that sort, what would you have thought was happening? I am talking about Thursday morning at 5 o'clock, there was a sudden rapid jump and then a fall away, that was when some dampers were opened, no fans on, just dampers.

ROBERTSON I would have been a bit suspicious of that. I must say. I do not know how suspicious I would have been. This sort of scale is so very different from the scale one has on the instrument and this may look much worse than it indicated on the instrument.

CHAIRMAN But it is not part of the normal routine things, to do anything in a Wigner release, if you get these blips up, you make a decision when they occur.

ROBERTSON One has to. I do not recollect having seen a blip like this on a Wigner before, or having it brought to my attention.

CHAIRMAN So there is nothing in these pile stack activity meters that gets anything like the attention that the thermocouples are getting. As far as Wigner release is concerned, it is the thermocouples you watch all the time. You have a set of rules which apply, but on the pile activity meter, no rules.

ROBERTSON Well, they are read at the normal times.

CHAIRMAN Have you ever had blips before.

ROBERTSON I do not recollect a blip like that before.

CHAIRMAN You have had some blips, though.

ROBERTSON I cannot recollect.

CHAIRMAN If you had been on duty and seen this blip, and thought it was something to worry about, what would you have done? Would you have reported this to somebody?

ROBERTSON I feel I should probably have reported it to the Group Manager, if I felt there had been anything to worry about.

CHAIRMAN I am asking a hypothetical question, after all you were not there.

DIAMOND On sheet 6, 2148, have you seen that before as a typical Wigner release.

ROBERTSON Do you mean the temperature rising suddenly and then continuing to rise afterwards? Yes, I have seen that before.

DIAMOND I notice it did occur last November.

ROBERTSON This has occurred on quite a large number of occasions. This seems to be rather a large rise, but we have had them as much as 70° to 90°. I think the largest I have ever seen was about 90°.

DIAMOND Where from?

ROBERTSON More or less from the end.

DIAMOND From the point of inflection, another 90.

ROBERTSON I am afraid I am going on memory now.

DIAMOND When we discussed 2550, in the November, you thought that rise after the maximum must be due to external heating.

ROBERTSON I think that was due to nuclear heating.

DIAMOND And yet 2148, which is even more so, is not.

ROBERTSON Well partially I think the increased slope at about 12 o'clock was probably due to nuclear heating, but I have certainly seen temperature rises of this nature, slow rises of temperature of the odd degree of say an hour, frequently before.

DIAMOND That is normal Wigner release?

ROBERTSON Yes, I think this is due to energy coming out from somewhere else and affecting that thermocouple.

DIAMOND So that the shape of that 2148, if you were to see it at that time, in that form, would not give cause for alarm.

ROBERTSON No, not that shape at all.

SCHONLAND You do not think it unusually long. Looking at the previous records, I have never found anything that lasted longer than about 25 hours. This rise goes on about the same.

ROBERTSON About 25 hours.

SCHONLAND No, from the beginning.

ROBERTSON Well, there is a sudden rise and then there is the gradual rise which goes on perhaps 25 hours. We have had the pile boxed up for longer than that after the last anneal has taken place and temperatures have still been either rising or not falling.

DIAMOND So that your experience indicates that neither the shape nor the temperature to which that record goes is novel.

ROBERTSON Not very novel. I think it is perhaps a rather higher temperature increase than we have previously seen. a rather bigger slow increase.

SCHONLAND Could Mr. Robertson comment also on 2053 in the same way?

ROBERTSON Well, that of course is a very similar shape. and until about 1400 on the 9th when the rate of increase got rather larger. Certainly up to midday on the 9th it would not have perturbed me at all.

SCHONLAND But if you had been present at midday on the 9th, round about that period, ? the sheet in front of you, you would have known there was something wrong at once, wouldn't you?

ROBERTSON Not necessarily.

SCHONLAND Not necessarily? Take your triplet at 2550, these three together, all shooting up.

ROBERTSON They were going up rather rapidly. Again it is rather hindsight I am afraid. You have seen the recorder charts, and they do tend to be a bit difficult to interpret.

DIAMOND Do you think that this saddleback on that triplet is unusual?

ROBERTSON I think it is.

SCHONLAND Following that up now with hindsight - looking at chart 5 - would you say that whatever started this fire, took place either at about 0800 or Wednesday or before?

ROBERTSON Well, should I say that perhaps it may have done. It would appear that those thermocouples were receiving quite a large amount of heat from somewhere else. and this probably started about 8 o'clock on Wednesday morning.

DIAMOND On sheet 9 two thermocouples. 1745 and 1769. have an extremely gradual steady rise for the best part of two days. Is this due to Wigner heating?

ROBERTSON It may be, I have seen it before; these are uranium thermocouples.

DIAMOND I beg your pardon, so they are.

Thursday, 24th October 1957 - 2.50 p.m.

CHAIRMAN We are not clear about Tuesday morning when you first began to know you have the flu. You said you were in the works - were you actually over at the pile?

ROBERTSON I visited the pile a number of times during the morning.

CHAIRMAN Yes. I see. And you did say that because you had the flu you could not remember whether it was you or not that said put on the second nuclear heating. Anyhow, having looked at the records again, you reckoned that was the right decision anyhow.

ROBERTSON Yes.

CHAIRMAN You also said that it takes a long time to get the pile going again and you talked about something like putting it on in the region of 9 o'clock and it did not diverge until 11 o'clock. Is that correct?

ROBERTSON That is correct.

CHAIRMAN What about the instructions about putting the pile on from the rig ? heat was that the uranium temperatures must not go above 330. But they did, in fact, go to 380.

ROBERTSON Yes.

CHAIRMAN Did you see that?

ROBERTSON After it had happened, yes.

CHAIRMAN You were not there as it happened?

ROBERTSON I was not there when it happened. I cannot tell you where I was.

SCHONLAND I must come back to the question I asked earlier. In 1953 since the maximum graphite temperature reached ? 6' from discharge base, the cartridge thermocouples were put in the 6' place. Now, during your time, Mr. Robertson, have there been any 6' cartridge thermocouples?

ROBERTSON No Sir, this was discontinued before my time. I think either the last anneal in 1953 or the first in 1954.

SCHONLAND It surprises me that when the Technical Committee reached the conclusion they should have the cartridge thermocouples at 6', they should ever have been moved back without it being reported in the Technical Committee minutes. You have nothing to contribute on that point?

ROBERTSON I am afraid not, I was not involved at that time. I should think it probable that this decision was taken because the loading of thermocouples at 6' in the uranium would have involved the discharge of a number of uranium channels and further re-loading.

SCHONLAND This normally would not be of interest to the operation of the pile?

ROBERTSON No. The normal loading of the thermocouples is at cartridge 14.

SCHONLAND You spoke I think of a rise of temperature of 2°/min.

ROBERTSON That is the normal temperature rise at which we raise power on the pile.

SCHONLAND It would appear from the temperature records that the second nuclear heating involved a much more rapid rise.

ROBERTSON I have not measured it in fact Sir, but I was given to understand that it was not very much more, perhaps 3° at the outside.

MR. SHACKCLOTH

CHAIRMAN Please tell us what your job is.

SHACKCLOTH Training and Safety Officer in R. & D. Branch, Windscale. Training and safety officer – as it implies my duty is, from the training point of view, to ensure that the staff are trained, and to organise and arrange extramural training and, from the safety point of view, to ensure the safety of the department, to establish safety regulations and to ensure that the department is being run safely.

CHAIRMAN I believe you have something to tell us.

SHACKCLOTH Not really. I made this report to Mr. Mackie and I understand Mr. Mackie has sent it on to you. The report is purely a summary of what actually happened on the morning of Friday, the 10th October. Would you like me to relate what actually happened?

CHAIRMAN Well, perhaps we will give you the chance but we have a copy of it.

SHACKCLOTH Well, there is nothing much to add. I was rung up at home at 7.45 by the foreman in R. & D. and told that there was, to use his expression, some sort of modified site emergency, and he asked me if I could get into the factory as early as possible. It was 7.45 so I could not get in much before normal time.

When I arrived, he told me that the shift manager had instructed him to keep everyone indoors and to instruct everyone not to go out of buildings and I merely proceeded to implement that regulation. But quite soon it transpired that quite a few people were contaminated. The first I heard was when a chap came to me and told me that his hair had a fair amount of contamination and in a matter of $\frac{1}{2}$ – $1\frac{1}{2}$ hour it transpired that there were quite a few people, so we instituted a sort of quick monitoring of all the personnel in R. & D.

Obviously it had to be done on a sort of rush basis and we did not keep any records or anything like that. We merely checked people's hair, trousers, overcoats and so on and in the case of hair, we advised them to wash their hair, where it was very bad. If counts on trousers were high, we gave about 3 pairs of factory trousers where we thought it warranted a change. Where the count was not excessive, we just ignored it.

CHAIRMAN What guide had you on the level of this?

SHACKCLOTH We have our own factory. We merely worked on 600 beta gamma counts as being the tolerance. The actual counts which I have quoted here were of the order of 3000 to 5000 which was roughly 5 times the tolerance levels, but we did not know then, of course, but it transpired fairly soon that it was short-lived and we did not regard this as dangerous. It is the sort of thing we considered unpleasant but not dangerous.

CHAIRMAN What time was that?

SHACKCLOTH Between 8.30 and 10 a.m. We had pretty well got everything straightened out by 10 o'clock.

CHAIRMAN Any questions?

SCHONLAND Do you feel that everything you could have possibly done was done for these contaminated people?

SHACKCLOTH Within my own section, yes. It is a matter of judgment as to just whether to warrant the change of clothing but as I say, this is the sort of level which we consider unpleasant, to be avoided, but not dangerous. You will notice that in my own case, my trousers were contaminated to 3000 at 9 a.m. in the morning. By lunchtime, that count had dropped to 500, which is below the permissible tolerance level.

CHAIRMAN And you mean to say that something stuck to your trousers, and it was reading 3000 when you first monitored them and a few hours later had dropped to 500. Something has a very short half-life. Did you not brush them or wash them?

SHACKCLOTH I did not brush them, but bear in mind I wore them, and I was conscious of it. For 3 hours that morning, I was sitting in my office conscious of this stuff on my legs. I wasn't shaking my legs; I was careful where I put my trousers so to speak. I knew it was there and I think it safe to assume that there was probably some dusted off. I do not think that we could honestly say that the drop from 3000 to 500 was entirely decay. I think that would be rather an unreasonable assumption.

CHAIRMAN Have you got any other measurements on anything that showed a rapid decay?

SHACKCLOTH No. Now, nothing more happened on Friday at all. The air count was done. It was publicly stated that about lunchtime that the air was down below contamination was down and no further monitoring was done that day.

DIAMOND How did your trousers get contaminated?

SHACKCLOTH Well, it is assumed that it was picked up riding on the track from Seascale. This is the crux of the point. I'm sorry, I did mention in the report here, these are people who come from the south, predominantly from Seascale. Now how many of these people I quote came by cycle and how many by train I cannot quote, but I can say from memory that the major contamination was picked up by cyclists on the track, including myself. I ride a mechanised bicycle and I had picked up my contamination there. There is one point I churned over in my mind; the meaning of the drop from 3000 to 500. No, I'm sorry.

I went home at lunchtime and my trousers were still 500 when I came back, which means I did not pick up any contamination during my journey home. I took this journey home purposely. I used myself as a guinea pig – as an experiment of my own and I did not pick up any contamination during lunchtime. The sort of argument that arose was whether this was lifted up from the track or whether

it was airborne. It was known by some people that the track had been monitored on the Thursday evening and was known to be contaminated. I myself doubt very much whether this contamination was actually picked up from the cinders of the track, I think it is more reasonable that it was purely airborne.

CHAIRMAN Why was it only your trousers, and not your jacket?

SHACKCLOTH I don't know. Actually my coat was slightly contaminated but not to the same extent.

CHAIRMAN Were your shoes all right?

SHACKCLOTH The soles of my shoes, but then I had walked a little on the road inside the factory and I assumed that what was on my shoes had come from there because my shoes never touched the track from Seascale to the factory. There is the second part of my report here. On Monday, quite a few people came to me quite concerned about contamination in Seascale. They had taken swab samples from window sills, grass, and gave counts which I have quoted here of the order of 3000 counts/min. One chap brought in a pair of shoes belonging to his child and monitored them in the factory on the Monday morning and they registered 5000 counts/min on the soles of the child's shoes. The child had been for a walk on the golf links at some period during the weekend. Another case I've quoted here was 5 m.p.l.'s; the chap could not remember the count, he merely quoted 3-5 m.p.l.'s on baby's clothing, actually napkins that had been on the line overnight Thursday and Friday and were swabbed on the Friday morning and measured on Friday evening. Although I quote figures they are not scientifically accurate, a question of $\pm 10\%$. I think this evidence should be qualitative rather than quantitative.

PEIRSON Did you get a feeling among the people in Seascale that something which should have been done had not been done?

SHACKCLOTH Yes. When I got home on Friday evening (my wife of course knew about the incident because I had been home for lunch on Friday) and it was fairly well-known in the village and the question of contamination was discussed. Obviously I felt myself in an awkward position and I decided, for the time being, to obey the security regulations and not pass any comment. The usual sort of questions were asked - why wasn't someone told, why were the people of Seascale warned on Thursday evening for instance, to arrange to shut their bedroom windows and keep their children indoors. A?..... was raised by various people, why we were allowed to come to work on Friday at all. These are questions which have been asked at various times. The feeling seemed to be that had the staff been told on Thursday evening that there was air contamination, had they been told that it was not dangerous but slight precautions, such as staying indoors, should be taken. I think the feeling is that they would have liked to have been told. Of course one can see the other point of view, would it have created more panic? That sort of question I can't answer, but there certainly was that feeling among residents in Seascale.

PEIRSON So far as you are aware, there was no communication to the inhabitants of Seascale, as inhabitants, apart from the people who work in the factory.

SHACKCLOTH No. No official statement not as far as I am aware. The first official statement that anyone knew as far as I know was the 1 o'clock B.B.C. News (on Friday), other than the two notices that Mr. Davey has put out. I cannot think of any other, I am not aware of any Seascale people or anyone outside the factory having been warned in those early hours (24/48 hours from 4.30 Thursday onwards). I was asked an awful lot of questions at the Windscale Club where I was with friends and my attitude at that time was that it was obviously not for me to pass comment. I played it down as much as I possibly could. At the time I thought it was the right thing to do.

CHAIRMAN Any other questions? Thank you very much.

MR. MOWATT

CHAIRMAN Would you please be good enough to say what your job is in the Atomic Energy Authority.

MOWATT I am Senior Scientific Officer in R. & D. at Windscale.

CHAIRMAN We believe you have something to tell us.

MOWATT Shall I make the salient point or take the things in order?

CHAIRMAN We want to understand what you are saying, so please do it in whatever order you wish.

MOWATT On Thursday evening I cycled home along the track beside the railway. I noticed a Health Physics van near the railway bridge and thought nothing in particular of it.

On Friday morning I cycled in by the same route. I was a few minutes late and I remember remarking to the Policeman at the southwest gate that it was a pity it was not a south wind or I would have been on time, and he said nothing. I got to Building 229, the R. & D. Building, and found people monitoring themselves, so I followed suit and found beta/gamma activity on my jacket, trousers and shoes, cap and moustache.

CHAIRMAN Did you note your readings?

MOWATT It would be about 30-40 counts per sec. at the worst. I think my shoes were the worst.

CHAIRMAN Your shoes were the worst? That was on Friday morning in the region of 8.30?

MOWATT Yes, about then.

CHAIRMAN Did you try and do anything?

MOWATT I changed my own clothing for factory clothing and had a wash. I went home in factory clothing.

This evidence was taken at 3.20 p.m. on Thursday, 24th October

MOWATT I handed in my jacket and trousers for decontamination but by the time the laundry had got round to it, it had either decayed or fallen off and they were returned to me without treatment.

CHAIRMAN When did they come back to you?

MOWATT I think it was the Tuesday, it may have been Monday.

CHAIRMAN Monday or Tuesday.

MOWATT I have been to the Medical Department to have measurements made on my thyroid and have their results in my pocket.

CHAIRMAN Perhaps you'd like to give them to us. We have a lot of records coming from a lot of people.

MOWATT It was only when I knew you were going to call me that I went along.

CHAIRMAN Are there any questions?

PEIRSON You were coming from Seascale - in fact going to Seascale on the Thursday night this was - was this Bailey Bridge?

MOWATT No. It was this side of the narrow arch under the railway by the Calder.

PEIRSON At about what time?

MOWATT A little after 20 past 5.

(Entrance of Mr. Goodwin)

CHAIRMAN One or two points before we start. (Introduction of the Committee). Would you be good enough to say what your job is in the Atomic Energy Authority.

GOODWIN I am Shop Manager in the Pile Group - one of Mr. Robertson's staff. Technical P.III is my official grading.

CHAIRMAN Shop Manager - what are you then, an Engineer?

GOODWIN Physicist.

CHAIRMAN You are therefore one of the assistants of Mr. Robertson, and you watched - you three watch - rather carefully the behaviour of the thermocouples during a Wigner release.

GOODWIN That is so, Sir.

CHAIRMAN When were you on duty during the period of the last Wigner release?

GOODWIN Nominally on back shifts which is from 5 p.m. until midnight and I came in on Monday at that time; Tuesday, I stayed a little longer, till approximately 1 o'clock; Wednesday again - 5 until about 2 o'clock; and I came in again on Thursday at noon and stayed in until Friday morning.

SCHONLAND Can I have them again?

GOODWIN Monday evening - 5 to 12.
Tuesday - 1 a.m.
Wednesday - 2 a.m.
Thursday - came in at noon

CHAIRMAN So that if I have understood this right, you were present when the second nuclear heating was proceeding.

GOODWIN No, Sir - that was Tuesday morning. I understand. I was in Tuesday evening.

CHAIRMAN So it was all over when you got in.

GOODWIN The second nuclear heating - yes, Sir.

CHAIRMAN Was there anything unusual when you got in on the Tuesday evening? Did you see anything? Were any of the records funny or did anything strike you as odd or did it seem going well?

GOODWIN No. We did not do anything or observe anything until Wednesday evening.

CHAIRMAN What did you see or do on Wednesday?

GOODWIN On Wednesday one of the temperatures was coming up quite well. I think someone has a chart which will show that.

CHAIRMAN Sheet 4 or Sheet 5?

KAY I have Sheet 5 here.

GOODWIN On Wednesday night, you will see the one labelled (these are graphite, aren't they?) - 20/53 levels off some time before midnight - about ten o'clockish. There is a script somewhere which contains quite a bit of my roof log. About that time the chimney base was closed up and the inspection holes were closed up and then, at these periods marked, I opened the dampers. I started by opening one damper for five minutes, noting the effect, and then followed that by two periods of about a quarter-of-an-hour or 10 minutes each. There is a sheet somewhere with this on. My last one was at midnight, I remember. They are the first two black lines on the graph, labelled "Dampers Opened".

I left then, or at least some time afterwards. I was in the devoid after that. I had been officially relieved but was actually doing some work elsewhere.

CHAIRMAN On the Pile?

GOODWIN Yes. I was installing, that is helping to install, new thermocouples in the devoid wall until about, roughly, 2 o'clock. And then I next come into the picture at midday - which is Thursday, at 12.00.

CHAIRMAN May I just finish off this damper opening and shutting. Each time you opened it, it dropped a little and when you closed it, it came up again.

GOODWIN No, not quite. The period of cooling was not really sufficient to show any marked effect until after, in fact, I had closed the dampers, and then in the succeeding half

hour or so, the temperatures would take a turn down. I remember noting at the time the temperature went down on the average $\frac{1}{2}^{\circ}\text{C}$ for every minute I left the dampers open. This was just a useful figure I had in mind at the time.

CHAIRMAN The temperature on that particular one which was oscillating between 405 and 415 – that is 20/53 is a little high but perhaps not unduly high? It did not give you cause for anxiety?

GOODWIN No. We had had a similar temperature; I think you will find it on the graph in the November '56 Wigner energy release. There is a graph of that somewhere which will give that.

CHAIRMAN Are there any questions about this part or can we proceed?

DIAMOND The damper opening and closing; let me turn to the air temperature.

CHAIRMAN Stack temperature, do you mean?

DIAMOND Stack temperature, yes. This rise in stack temperature at that point, is that when you closed the door and the inspection holes?

GOODWIN Yes, that would seem to be it. If you look at the published memo of what happened – it should be on here somewhere; no it isn't, 21.00, approximately 9 o'clock – that is about right. That would be approximately when I was shutting the chimney base and the inspection holes to increase the draught through the Pile.

SCHONLAND What time is this?

GOODWIN Approximately 9 at night.

CHAIRMAN What did you mean when you said 'shut the chimney base'?

GOODWIN There is a hatchway large enough to allow people access to the bottom of the chimney. This is normally, during a Wigner energy release, this trap is normally open to allow the chimney to draw cold air through that aperture and reduce the pressure drop across the Pile, so that any leakages in the blower system and so on which would normally allow cold air to be drawn through the Pile, the pressure drop being reduced, the amount of leakage will also be reduced. This enables the Pile to remain hotter for a longer period. You are attempting to box up the thing and keep it totally enclosed.

CHAIRMAN So this hatch goes right into the air which is coming out of the Pile.

GOODWIN Yes, Sir. It actually goes up into it that way: one climbs up a ladder. There is a very strong draught up through that hole.

CHAIRMAN Was it open right the way through, from the start?

GOODWIN I cannot say categorically that it was open the whole time, but it was certainly open all the time as from Tuesday when I had it inspected.

CHAIRMAN So presumably you were not there so you can't say, but it might well have been open during the second nuclear heating.

GOODWIN I presume it was so.

KAY This is the normal state of affairs during a Wigner; that you seal the inlets of the pile and you open the chimney base to destroy the draught from the pile, so you have as little air flowing through the pile as is possible to have?

GOODWIN That is so.

CHAIRMAN You shut that about 9 p.m. on Wednesday?

GOODWIN Roughly, I can give you more exact time if I get a copy of my roof log, if you think it important.

CHAIRMAN I think it is near enough, it is right within half-an-hour, I take it?

GOODWIN Yes, Sir.

CHAIRMAN Can we come now to the next period you came on duty, which was Thursday and you came in at noon?

GOODWIN Yes.

CHAIRMAN All right, what is happening then?

GOODWIN I came in then and observed that (a) the stack activity had gone up during the night. That I saw in the control room. I went to the roof, and one of the temperatures, in fact it is this self-same 20.53, was (a) rather high, and (b) was increasing more rapidly with time. I think you have the original charts; you will see I have marked every hour the rate of increase and it is getting sharper. It is going up more and more steeply. It does not seem to show so well here, but it does on the original Kent chart which you have somewhere. This being the case, I immediately opened the dampers; it is marked here with a black line, and then I went round making a survey of the position to see what had happened since I was last on.

The increase in activity led me to believe that, as in fact I reported later, we had a burst. On this basis, I requested permission to blow the pile cold.

CHAIRMAN Who did you ask?

GOODWIN Mr. Gausden. I think I may have introduced a red herring there. Anyhow that is what I said at the time. I considered we had a burst.

CHAIRMAN About noon on Thursday?

GOODWIN No Sir, it would be after then. I'd already opened the dampers for $\frac{1}{4}$ hour. I spent half-an-hour at least going round the charts trying to see what had happened since I had been away.

SCHONLAND About 1 o'clock.

GOODWIN That was when you requested permission and Mr. Gausden gave it?

GOODWIN That is right, I asked permission and was given permission to cool it down on the dampers to start with, and as soon as the graphite temperatures had turned over and started to come down, to turn on the four shut-down fans, that is labelled there as "air on", the bigger black line.

CHAIRMAN Right.

GOODWIN When the dampers were opened and then the fans put on, the graphite temperatures turned down, as expected, and the maximum uranium went up as expected. Somewhere we have a maximum uranium temperature.

CHAIRMAN Why did you expect the uranium to go up?

GOODWIN The pile essentially being like so in section and very hot in front perhaps 6, 8, 10 feet. and with a uranium temperature thermocouple in the middle. as soon as we start blowing and moving the hot air. (blowing in cold air which is heating up) and moving the whole of the heat in the pile backwards, it is spreading into the pile. and uranium temperatures show a turn up. This is almost always observed except in cases where the metal channel has no hot portion at the front of it and they then show a decrease. I think there is one here somewhere.

DIAMOND Quite right.

GOODWIN The maximum temperature was on this continuous recorder.

PEIRSON 21.53.

GOODWIN 21.53, is it? The decrement there. as far as I can see, on that graph, the first little spike as from 12 noon is me opening the dampers.

CHAIRMAN Which chart are you looking at?

DIAMOND Eight.

KAY Channel 21.53 at 12 o'clock.

GOODWIN There is a point I will make in a moment about the numbering on that channel. Do you think we could have the original, I far prefer the original on this. the single point recorder.

CHAIRMAN If it seems important we'll send for the original.

(NOTE: A copy was fetched from the office next door) after which ...

GOODWIN The first spike, I think you will find. is somewhat narrower than shown on that graph, which is due to air blowing through the pile when the dampers were opened.

The next occasion, the dampers were opened, and closed momentarily while we started up the motors (it is necessary to shut the dampers first so that the motors are started on a light load), then the shut-down fans were in operation. This took up the metal temperature, as expected, to approximately 400, which is just about operating temperature - 395 is specified. There it levelled out, on this graph it looks quite steep; that is why I wanted the original, and showed a low rise. This was not quite as expected, especially as the graphite temperatures were then coming down. So, what time are we?

PEIRSON About 14.00.

GOODWIN Mr. Gausden would be up there with me by then looking at this and several people joined us. We had Mr. Hughes up there shortly afterwards as well, they having come because of the high activity measured on the Met. Station. This was watched to rise; it was fairly linear at this portion.

(Blackboard work) The sort of thing it had done was to go quite sharply, there was a drop as I closed the dampers and then it went up again and was levelling off, that sort of thing. about 400. But after it had gone to 405, 410, I cannot say exactly. it began to do this, and creep up, and at that stage Mr. Gausden said it looked as though we had something radically wrong. He wanted that channel out. Previously also Mr. Howells had done a calculation to see if the activities we had got could be just from a single burst cartridge, and at that time it still seemed as though all the activity could have come from a single cartridge split open. We were still thinking in terms of a burst in one channel, and we thought that wherever it was it was jolly near this thermocouple. There was a delay then due to an error in the numbering system between the actual number of the channel as we understood it at the time, and the actual channel as located by the thermocouple wire that went into it on the chargehoist. This was sorted out, again the time is stated somewhere, and the plug was withdrawn from the hoist, from the wall of the charge face, which disconnected the thermocouple and that disconnected at 450. It had gone up almost sort of exponentially to about 450 and then the instrument just went U.S. as the wire was broken due to the withdrawal of the charge plug. At the same time, the people on the hoist, having pulled out the plug. seconds later reported that the pan was glowing red hot. The cartridges were red hot.

The exact decisions then I am not so certain on, because I was no longer with Mr. Gausden or Mr. Hughes. They were down below on the hoist. All I know now is here as regards the discharge programme.

CHAIRMAN You needn't worry about the rest of the story.

DIAMOND The account of the opening and closing of the dampers is a correct record as you understand it.

GOODWIN All that is on recorder "I". I thought we should have one. Here we are.

These are the series of rises in temperature as I opened the dampers. Here is the first trial run, that was when I just opened one shut-down damper, and that is the increase in

uranium temperature. Then I tried four and then again at 24.00, another go. There was one during the night, no, two during the night, and then I came on at noon and opened four, and this is where I opened the dampers, closed them momentarily, started the shut-down fans, and that is the sort of thing the maximum uranium temperature was doing. It was crawling up. On this scale, it doesn't look anything like as vicious as that one there, but from here onwards it is beginning to turn away.

CHAIRMAN What are these blips?

GOODWIN Standardising points of some kind, I'm sure, some instrumental kicking. I do not think they are significant ones. There is where the channel was finally discharged, when the instrument went off scale there. So up to about here, everything seemed about normal, but from there onwards when it continued to go up, and the graphites were lower than that, and also a little later, it began to rise more rapidly. Those were the signs we had of trouble.

Thursday, 24th October 1957 - 3.50 p.m.

CHAIRMAN Any questions?

PEIRSON Could I ask Mr. Goodwin why he came in at noon on Thursday?

GOODWIN Because on Wednesday evening I heard that Mr. Robertson was ill, so I decided that it would be better, even though it looked as though the Wigner had come very near to its end, I came in to make sure all was well.

PEIRSON Did you and Mr. Robertson and Mr. Jenkinson take shifts during a Wigner?

GOODWIN That is why I was on this peculiar hour, 5.00-12.00 at night.

PEIRSON Normally you don't work Saturdays.

GOODWIN Normally just during the day.

CHAIRMAN Any more questions?

DIAMOND You went on in this period here I take it?

GOODWIN Wednesday, during the day, no Sir.

DIAMOND You weren't.

CHAIRMAN Well, thank you Mr. Goodwin. I don't think we shall want you again.

GOODWIN Would it be presumptuous for me to bring your attention to a point which might not have been observed before, on the stack filter activity. This is it here, it really does not show very well on this one. It does a little better on the original. From 0400 onwards, there is a very slight increase. It looks as though, as regards to that particular point, it changes from just a background to a very small positive signal. The three points on the graph are beginning to open out. I think this has been obscured considerably by the portion immediately following when the dampers were opened. But it does

look as though something is beginning to occur just then, but what it is I don't know.

SCHONLAND It did so just before?

GOODWIN Perhaps the original shows a little more.

SCHONLAND Yes, we have seen the original.

MR. HOWELLS

CHAIRMAN Well, there are quite a lot of questions we have dug up for you. I think several members of the Committee would like to ask you questions. I would like to start. I just want to check that people in the vicinity here that were under the plume have not got several Roentgens. Can we make any sort of estimate on this?

HOWELLS We are carrying out thyroid measurements right round the area, and we are making an assessment of what the thyroid dose is in the district. This calculation is going on at the moment.

CHAIRMAN I think I may not have made this point clear. This is another point if I may say so. You remember you said that at the Bailey Bridge at Seascale you had got 4 milliR per hour and the ground contamination was a lot less than that, so that most of the 4 milliR were coming from a plume that was going overhead. I just wanted you to check that the plume passing overhead, over anybody's head, could not have given them a gamma dose that amounted to nearly as much as a Roentgen.

HOWELLS From the measurements we have made there was no reason to believe that anything like as high as that would have occurred.

CHAIRMAN You would say that if there was someone sitting on the Bailey Bridge at Seascale during the whole time ...?

HOWELLS We kept a van there. It was at 5.30 we took this reading. There was a van in that position taking readings up to 9.30 p.m. and none of them was as high as that, i.e. which I quoted.

CHAIRMAN So even there it would be 4 milliR x 9 hours or whatever it was, it was still a very small ? So we can at least say that no member of the public has had an exposure of gamma from something in the air, external radiation. We are absolutely alright on that?

HOWELLS External radiation. I am quite certain on that point.

CHAIRMAN Now a second one you were looking into for us, was the direction of the wind.

HOWELLS Yes, I provided that.

CHAIRMAN Could you just glance at it a moment.

HOWELLS I have three. The ground measurements and also the data which we had collected subsequently from the Met. Office at Preston.

CHAIRMAN Now, when you first realised something was wrong; the flag and the Met. Station wind direction.

HOWELLS This was measured on the wind vane at the control station here.

CHAIRMAN I noticed this morning the steam coming out of the condensers was going one way and as it got up a bit, it seemed to be bending round more or less what appears to have happened then.

HOWELLS Both the wind velocity and wind direction were different at 500 ft. to what we were measuring at ground level, and then as is mentioned in the met. report, there was this frontal trough that came in at about 3-4 in the morning and probably the movement of this would have broken up the wind system, round here might have changed quite considerably during that time.

CHAIRMAN I see, so that what we have appears to be a good met. statement here, it does not really cover all the fine structure which was very complicated.

HOWELLS Every 6 hours they give a forecast. It will only be a general picture. not a detailed one. Then you have got the problem of micro-meteorology and the forecasting is giving a general picture: in a small area, it may be quite different.

CHAIRMAN And we have never before had need for micro-meteorology in the Works. Now you were saying something about thyroid counting. Are you doing that?

HOWELLS This is being done by the Medical Dept. We ? calculating on it. There is a team from Aldermaston doing some of these calculations on thyroid dose.

CHAIRMAN So that really you are not the man to ask about this. It would be Dr. Orchard?

HOWELLS Yes. There are no figures available yet. It is something which has just started. We have had to call in people capable of doing this work for us.

CHAIRMAN And the object of this is to determine by measurement the quantity of iodine curies or microcuries of iodine in the thyroids of our own workers and of volunteers from the public.

HOWELLS That's right. We have had public officials coming in and their friends. This is the basis on which it is being done and we have made the measurements here, and from the present iodine content of the thyroid we hope to calculate the integrated dose that has been received.

CHAIRMAN Perhaps this is not a fair question for you. Dr. Graham said he had got some measurements a couple of days ago. Were these preliminary?

HOWELLS I have seen these measurements. It is just a straight measurement of thyroid content in microcuries, but this won't give you the answer one needs. One has to calculate this in terms of Rads to the thyroid.

CHAIRMAN And how are we going to do that?

HOWELLS Well, this will have to take into account the decay and then do an integrated dosage for the period involved. You see, you know how much iodine is in the thyroid at present; you can estimate how much was there originally and then integrate the exposure involved.

CHAIRMAN Have you now got a medical figure; that so many Rads is alright, and above so many Rads.

HOWELLS There is a figure which is quoted by the M.R.C. that the threshold for carcinogenic effect is about 250 rads. This is the sort of figure they quote. We would like to see a figure of about 25, about a tenth of this. Because if you go to 250, this is where you would expect some clinical effects to rise.

SCHONLAND There are no results available on this at all, not on one case?

HOWELLS I have not seen any myself on this. They have just started the calculations on it and I have not the full details at present. I shouldn't think there will be any fully calculated out as yet. One obviously has to relate this to the geography of the area, tying down what was the exposure at various places in the district. I think they are tracking down patients that we have measured and where they were, and doing the calculations afterwards from this.

CHAIRMAN But somebody will measure the activity of their thyroids and ...

HOWELLS And a bit more than that. They will calculate the dose which was actually given to the thyroid, because the activity measurements ... lots of these have been done, but this does not necessarily tell you a great deal unless you calculate the exposure.

CHAIRMAN I am not absolutely clear. You know that the exposure must have started not sooner than the incident and you now have got a measurement. You mean you have to fill in when it got into the thyroid? You have the rest of the picture.

HOWELLS If you can calculate microcuries say, at the beginning of the incident, this is what got to the thyroid, this will decay, and the dose given to the thyroid per day gets less as time goes on, so we have to integrate in that way in order to calculate what the total exposure is.

CHAIRMAN And does the iodine come out of the thyroid anyway by natural changes? I think it is from

HOWELLS I am not competent to answer this fully, but I would have thought that body metabolism will change the iodine 131 and replace it ...

CHAIRMAN This part of the thing is not your business at the moment.

HOWELLS Only indirectly, by looking after the people doing the ...

CHAIRMAN Now I am sure there are some other people who would like to ask questions.

PEIRSON Mr. Howells has given us another ... (this is yours, isn't it?) of air contamination that is yours? Yes, you told us I think that the background or normal level was of the order of 200-300, so you had figures very many times that obviously, at different times. I see 718,000.

CHAIRMAN Some millions here.

HOWELLS There is one, yes, down at the bottom, 1.2M.

PEIRSON And these were the times when men were taken off work at Calder and were taken out of the Chemical Plant.

HOWELLS This is in the morning. I would like on this one ... that these figures look impressive. When one takes the figure that one operates for mixed fission products is something like 7,000 D.P.M./metre³. This is what we normally operate to. If there is an air contamination level higher than that, people are put in some sort of respirator. This is assuming a level for continuous exposure as such, and the ICRP report for instance, quotes that one can go to something like 10 times this level without the slightest cause for concern (I think this is exactly their wording) for a number of days, so this immediately takes the figure to 70,000 and for a short time probably to 140,000 D.P.M./m³. This is one aspect. There is another aspect which doesn't really come out on this table. These show the highest levels at the various places and if you look down the table, it is not always at the same spot, due to the fact that we had a variable wind condition. At one moment you would have a high sample at one place and then it would move over to another. These measurements are also made outside the building. We had air sampling inside the building and we did not have air contamination inside the buildings themselves.

PEIRSON In the night people were told to put on dust masks and later they were told to have them standing by. Obviously later they were very much higher figures, in the morning of Friday, but presumably at that time everyone was staying indoors.

HOWELLS Yes. There was an instruction to stay indoors at that time. In the early hours of the morning we told them to put on dust masks, this was in anticipation that something more serious might happen at this stage when we were sampling.

PEIRSON May I ask about the contamination outside the factory. Did you do sampling for that?

HOWELLS We took some air samples but I was concerned primarily, since the method for measuring the gamma level is more sensitive than air sampling, to carry on that type of monitoring rather than take air samples in the district.

PEIRSON But would the one cover the other?

HOWELLS Yes. I should explain that if you take an air sample you obviously have to stop the van and start a petrol generator to supply the power to operate the air sample device. In order to get a sample which will give you anything from which you can conclude what the air activity is, you have to draw in air for a period of about half an hour. This immobilises the van in one position for that time. I was really concerned in trying to sweep as big an area as we could during that night to see what radiation levels there were and what the ground contamination level was rather than the inhalation. One would not expect there to be any inhalation risk as far as this type of incident is concerned.

PEIRSON Could you amplify that? When you had such very high figures in the morning why would not expect to have any such hazard outside the factory?

HOWELLS The further you move away from the factory, the bigger the dilution you would expect as far as the air contamination is concerned. We have made an estimate of what the inhalation was at Seascale during this incident.

CHAIRMAN It's an estimate, is it?

HOWELLS Yes, based on these air samples results and extrapolating to Seascale. The value that we get from this was, I'll quote the technical unit, and then explain it. It is 100 microcurie seconds/metre³. To put this into perspective, it is about equivalent to drinking about half a litre of milk at a contamination level of 0.1 or 1.0 (not clear) microcuries per litre.

CHAIRMAN If you are breathing in this air, for how long?

HOWELLS During the time of the incident this is estimated as 12 hours.

KAY The integrated effect? of breathing the air at Seascale for 12 hours equivalent to ...

HOWELLS Drinking $\frac{1}{2}$ pint of milk at the level we would accept for infants as such.

DIAMOND Is this gaseous activity or particulate?

HOWELLS This will be in terms of the activity whether it is particle or gaseous. You just measure the amount of activity that goes in. You do not discriminate here between particles which go in and come out and those which stay in.

CHAIRMAN Now there is a point here which I would like to clarify. I forget the exact details of how smoke coming out of the chimney diffuses in the wind and comes down. You start off here with the great advantage that the thing is 400ft up in the air, whereas of course downwind it has been diluted but it has actually got right down to the ground.

HOWELLS Yes, I follow your point. You would expect the maximum concentration about 3 miles away.

CHAIRMAN It is about 8×20 times the effective height of the stack.

HOWELLS We have a check on this. It is one thing we looked at. We know where the argon 41 plume normally comes down and this is usually in a region 3 to $3\frac{1}{2}$ miles away from the Works under normal weather conditions, at something about a 10 knot wind.

CHAIRMAN Why was not the air activity then more there than it was in the Works?

HOWELLS You will still ... there is another point one considers here. The wind velocities during the night concerned were particularly low so under these conditions you would not expect the plume to go so far. This might quite easily be turning over and coming down.

PEIRSON At the time of the highest figures here you had a much stronger wind, you had 10, 13, 17, 12-15 knots.

CHAIRMAN The highest activity was in the region at 10.30 in the morning of the 11th.

PEIRSON And you had a wind of 13 knots.

HOWELLS I still think that the risk associated with this ... the inhalation side which is the thing we are concerned with at the moment, is still not the thing one was really concerned with. When you come to this time in the morning I was more concerned with collecting biological samples than anything else. But this still comes back primarily to an ingestion rather than an inhalation danger.

CHAIRMAN I don't disagree with you. What I am saying though is that the maximum air activity was probably not in these Works, but was a mile or even two miles away from here in the direction of the plume from the top of the chimney and we have not got any air activity measurements at that time, for a very good reason as you have explained that it takes $\frac{1}{2}$ hour to get a sample.

HOWELLS There is another point. The assumption I was taking at this time was that the wind was going towards the sea, which means that we could not get the 3 mile range so that is why I sent the van down to the Bridge to take an air sample there and the radiation readings.

CHAIRMAN Which direction is that Bridge from the chimney?

HOWELLS That would be approximately NNE.

CHAIRMAN It is just the opposite to what you say, so that in fact the wind was going pretty well in that direction.

HOWELLS Yes. That is about the line.

CHAIRMAN So that we have got one reading of the air activity at the Bailey Bridge at Seascale. What was it?

HOWELLS This was ... I haven't got the figure on this list because I only gave the figures inside the Works. This would have been about 2,000-3,000 D.P.M. per metre³.

CHAIRMAN We would like you ...

HOWELLS I could say it was definitely below 7,000.

PEIRSON Could you give us the time of that?

HOWELLS It will be sometime between 5-7 when we took that sample on the Thursday night.

CHAIRMAN Perhaps you would be good enough to see if you have actually given us these figures correctly. You have been quoting them from memory. Could you just pass in a note saying what it really was and if there are any other air activity measurements outside the Works, we should like them.

HOWELLS I think I had one taken at Seascale. This was at one of the points, Moffat's Corner perhaps.

CHAIRMAN Hunt them up and give us the activity, where it was, and the time.

HOWELLS There certainly were not more than two or possibly three that we did take in this way.

PEIRSON I don't know whether it is correct to ask Mr. Howells, but I should like to ask you whether you could comment.

Thursday, 24th October 1957 - 4.40-5.00 p.m.

PEIRSON We have had a suggestion that the inhabitants of Seascale ought to have been informed of some state of emergency and advised at least to stay indoors. Would it have been part of your responsibilities to consider that point?

HOWELLS Yes, I think so. If I had been asked this, I did not see any reason on the basis of the measurements we had taken why this would be necessary. Certainly on the Friday and the Saturday there was no reason why we should have done this. If there had been an irradiation risk at any time, this would have occurred at the time the plume was coming out. If we had passed any message to Seascale under these circumstances, people would probably have come out of doors rather than staying indoors, and during the night the best place for them to stay was in their houses, which they were doing during the night.

On the Friday morning one would still not expect there to be any inhalation risk at Seascale. You would not expect any real contamination risk on the basis of the measurements that we had made. It still comes back to the question of what they are going to eat afterwards as a result of contamination.

PEIRSON Were you doing radiation measurements in and around Seascale on the Friday morning?

HOWELLS In the area we would have been going round, we wouldn't have stayed in the Seascale area. We were actually going round both sides of the Works. We would not have been in Seascale specifically all the time, we would have passed through that area. We had one point at the Seascale Golf Course which is right down on the front and one at the other end of Seascale at Moffat's Corner, two of the points at which we were taking measurements.

CHAIRMAN These are gamma measurements.

HOWELLS Well, these were both swab measurements and gamma measurements and we were using contamination monitors as well. I explained earlier, we were having difficulty with gamma measurements in getting precise information though we could get an upper limit, due to contamination of the vehicles themselves.

CHAIRMAN Were you using swabs then?

HOWELLS Using swabs over one square foot area.

PEIRSON We have been told that people who came in on Friday morning, having cycled along the track from Seascale, had to change their clothing. They found a high level of contamination on their clothing picked up on the track.

HOWELLS Depends what they define as a high level of contamination.

HOWELLS I certainly had telephone messages from people on the Friday morning saying that they had counted 6000 counts/minute beta gamma on their hair. I responded - "Well, wash it off". This is not a level which would cause concern as far as health was concerned. It would not be permitted permanently in an area where people were working, but in an incident such as this, the level is not high enough to cause concern.

PEIRSON Over how many days would you say that?

HOWELLS You could probably go certainly a week with getting this sort of level without any difficulty.

PEIRSON And you regard that as reasonable for the general public as well as staff.

HOWELLS Yes, perfectly so, only in circumstances such as this, you would not want to maintain these levels.

CHAIRMAN What sort of counts per minute would you have said Yes, by golly! We must do something - 6000 you say, once, is alright. At what sort of figure would you start showing anxiety?

HOWELLS I would say ... I really need to do a full estimate of this. This is getting up to the top end of the range - 120,000 counts/minute.

CHAIRMAN I see, 100,000 counts or so ...

PEIRSON It is never easy to put yourself back in the place you might have been. If you had this again, which heaven forbid, do you think you would let the inhabitants of Seascale know anything at all if only to stop this kind of talk.

HOWELLS I do not think so, not with an incident of this nature. To start off with, one could not be quite certain where the main contamination was, until one has done a survey of this area. I see no reason at all to notify the people of Seascale unless one has a certain amount of evidence to support the idea one has. We had no evidence to justify the assumption that there was any hazard at all at Seascale.

PEIRSON Well, I suppose by the Friday morning, the wind had changed and it was going down the coast.

HOWELLS Well, the wind at ground level was certainly blowing out to sea on Friday morning. This was probably the only wind which would affect the Seascale area.

PEIRSON I thought the activity was first taken out to sea on the Thursday, but these winds, if you look at the long table you gave us, it goes right round from NE, ENE and so forth, so that on the 11th it is all NW, NNW and so on.

HOWELLS At 9.30 in the morning it is NNE.

PEIRSON No, you're looking at Thursday. Sorry. Well, it's veering, but it seems to me that mainly throughout the 11th it got more westerly than easterly.

HOWELLS This is the later part of the day, from 10.15 onwards.

PEIRSON Anyway, the measurements that your vans were taking around Windscale were such that you would not have felt it necessary to advise people to take in their washing or to keep indoors, even if you had known at the time where the wind was.

HOWELLS No, even if it was blowing directly there.

CHAIRMAN Dr. Schonland?

SCHONLAND I have no more questions in this field - I have more general questions. Mr. Howells has been a member of the Windscale Technical Committee for a good many years, I see from the minutes.

HOWELLS You are confusing me with Mr. Gordon Howells. This is a natural confusion.

PEIRSON There is another table from Mr. Howells. I think this is your table?

HOWELLS Yes, that is right.

PEIRSON This is the irradiation of the charge hoist operatives. I think the summary bears out the figures Mr. Howells gave us.

HOWELLS One point in this table. This is not quite complete, we have some other 20 people but all these people have received less than .3R. We are still getting the records.

CHAIRMAN Present permitted dose over 13 weeks is what?

HOWELLS 3.9R. And we are going to change that to 3R for 13 weeks.

CHAIRMAN 3.9R for 13 weeks and we are going to change that to 3R for 13 weeks and what do we do about betas? These are all Roentgens. What about beta activity? We don't add that on?

HOWELLS No, not on these.

CHAIRMAN These people you have listed here, the Roentgens they had over a 24 hour period. Have any of these just had some more than normal ...

HOWELLS They will have, from previous exposures in their normal duties, but these will have been kept below the 3.9 figure. Except there will be some exceptions on this.

CHAIRMAN What I am worrying about now is that one of these men who may have had 3 or 4R, may have had three or four in the present 13 week period already from normal plant work.

HOWELLS Yes, we will have ... we haven't got the figures at the moment, but we have got the detailed breakdown, both of the exposures which have occurred here, and for the week up to last Sunday, with a previous 13 week total as well included.

CHAIRMAN Will that information be available in due course?

HOWELLS In fact it might be ready this afternoon.

CHAIRMAN That is something we would very much like to have.

DIAMOND How are these exposures?

HOWELLS These are measured with a film badge. These 24 hour ones: the ones which go up to the end of the week, some will include personal dosimeter measurements on quartz fibre electrometers.

CHAIRMAN Am I right in saying that the maximum is 4.5?

HOWELLS That is the highest.

PEIRSON Highest here. Mr. Ritson is not on this list.

HOWELLS He should be.

PEIRSON Oh sorry. 1.74. You did mention the other day one doubtful 5R.

HOWELLS I think this was Ritson, in fact it was not 5R, there was some confusion about Ritson. We normally get a list made out and we report these in weekly MPEs, and it is

put down as a number of such, you can get this written down as 17 weekly MPE, which is 5R as such. If you see a number 5, this might mean 1.5R. I think some confusion arose in the case of Ritson on that basis.

CHAIRMAN Well, we should like to have it, Mr. Howells, as soon as you have got it, the extra information which you have just mentioned, so that we can see what is the very worst case of all.

HOWELLS Including the past 13 weeks exposure?

CHAIRMAN Yes.

PEIRSON May I ask that the highest of these figures – if the man has been taken off contact.

HOWELLS Yes. This should follow automatically. We are working to normal standards that no-one is to receive more than 3R in a 13 week period. As I say, it has been exceeded in some cases where we had an emergency as such.

PEIRSON And they are automatically taken off.

CHAIRMAN You are working to 3R not 3.9.

HOWELLS Yes.

CHAIRMAN When did you start the 3R?

HOWELLS Just introducing it now.

CHAIRMAN Just introducing now – any more questions?

Thank you, Mr. Howells, you will be working here for the next day or two.

DECLASSIFIED

Board of Enquiry

Friday 25.10.57

Evidence heard from:—

Williams	pp 8.1 – 8.5
Gausden	pp 8.5 – 8.9
Leslie	pp 8.9 – 8.10
Graham	pp 8.10 – 8.12
Leslie	pp 8.12 – 8.13

Friday, 25th October

WILLIAMS Yes.

WITNESS: DR. WILLIAMS

CHAIRMAN Say what your job is in the United Kingdom Atomic Energy Authority.

WILLIAMS I am responsible for a small section engaged on operational research. One of my specific responsibilities is to determine what operating factors determine the burst rates in the Windscale piles.

CHAIRMAN This burst rate is a matter of great interest to us and we are interested in knowing when and at what rate they do occur; whether there is any extra rate of bursting when the pile is started up again after a shut down and any other interesting features of that kind which you may have come across in your work.

WILLIAMS Taking your first point about the incidence of bursting before shut down and after, I have done some work on this, the effect of thermal cycling on the burst rate of cartridges, and I found that it appeared that the burst rate was quite independent of the number of thermal-cycles suffered. The thermal cycles were caused by either an automatic shut down due to a power failure or an instrument fault, and also a controlled shutdown due to a main discharge. In particular, I could give you some figures of the burst rate before and after a Wigner release. I could put these on the blackboard.

(Proceeds to demonstrate)

1 month After W.R.	1 month Before W.R.
7	5
4	3
5	6
4	4
6	9
8	6
4	10
6	4
av. 5.5	5.9

All these figures apply to Pile 1. The same sorts of figures apply to Pile 2.

WILLIAMS I would conclude from the figures that the burst rate is quite independent of the Wigner release. There is no real difference.

CHAIRMAN This goes right from the previous to the present incident?

DIAMOND To be quite clear, this is the burst rate during the month preceding a Wigner release and during the month succeeding it.

SCHONLAND After a Wigner release the pile is scanned for the start up.

WILLIAMS Yes, I think so. I am not quite sure about this point.

SCHONLAND Is it not possible that before they start up after a Wigner release they remove these cartridges. Is that so?

CHAIRMAN I think you had better ask Mr. Gausden this.

WILLIAMS A Wigner release usually takes place and coincides with a main discharge of a zone. I am not sure about this point but after a Wigner release they run up the pile to power and the normal scanning apparatus will still scan the pile to see if there was a burst. A burst would be recorded in the normal manner.

KAY You would not include in this table any defective cartridge that came out during the main discharge because they wouldn't be recorded as burst cartridges. You would miss that.

WILLIAMS Yes.

SCHONLAND So they clear the fuel after a Wigner release? They discharge it?

WILLIAMS No. After a Wigner release it usually coincides that you do get a discharge of a zone. The Wigner release takes place when they are about to discharge a particular zone. In other words it coincides within terms of time. They have already shut the pile down for the Wigner release and this coincides in terms of time when they are discharging a particular zone which has reached its normal or standard irradiation.

SCHONLAND Then it is of no interest to anyone to know if there was a burst cartridge or not because it goes into the pond. Do they report burst cartridges?

WILLIAMS All cartridges which burst are definitely reported.

KAY But not if a burst cartridge is pushed out in a refuelling operation. Only if it is picked up in the scanner gear that it is recorded.

WILLIAMS That is quite true, yes. If it is picked up in the scanning gear it will definitely be recorded.

KAY But during a Wigner release discharge period, the scanner gear is not being used. Can I be quite clear that the Wigner release is carried out and then a certain zone, or two zones, are re-fuelled, and any defective cartridges after the Wigner release in these zones would be discharged with the others and would not come on the records of burst cartridges?

WILLIAMS I am not quite sure about that.

WILLIAMS Yes, approximately one-eighth of the pile. A zone might contain 300 channels, or up to the largest zone of about 600 channels.

KAY Where did you get the figures? From the records of the scanning gear?

CHAIRMAN And at 400 megawatts, just to get an idea of the figures, is that 10 or 50?

WILLIAMS Yes. The actual bursts are the bursts as recorded by the scanning gear.

WILLIAMS Can I just show you a few plots of these? This one was rather unfortunate in that this was a rather exceptional zone which went to higher than standard irradiation, but this is the sort of curve we get normally: we discharge round about this level here.

DIAMOND This may be anticipating your evidence but I take it that the incidence of bursting increases with irradiation time?

SCHONLAND Normally your Wigner releases take place in that region, ... with the sharp rise?

WILLIAMS Yes, may I...

WILLIAMS For a particular zone, the Wigner release might occur here, but other zones, because they went in at a different time, the Wigner release might occur in this part of the curve here.

DIAMOND Therefore in the period one month before Wigner release, you have a greater probability due from that cause, than you have in one month after Wigner release, because during the release they are discharging the most irradiated cartridges. Therefore, you would expect, if Wigner release had no effect, for your second column to be less than your first.

CHAIRMAN What is the maximum cumulative total? Can you give me the X and Y co-ordinates?

WILLIAMS May I illustrate my point, which will answer your question.

WILLIAMS This is an exceptional zone, but shall we take more or less average conditions at round about 400 megawatt days per tonne. The total number of bursts was about 15.

(Blackboard)

CHAIRMAN Per zone?

WILLIAMS The normal pattern of bursting in a particular zone, if we plot the total number of bursts, or cumulative number of bursts, and plot along here, the average megawatt days per tonne irradiation on that zone, then this pattern is quite distinct for the Windscale cartridges. If I number these 1, 2, 3, 4, 5 etc. up here, let's take a value of 300 MWD here, where normal discharge occurs approximately about the 325 MWD/tonne ...?..., the pattern is that we'll get an odd burst here, and maybe an odd burst there and there. My scale doesn't quite show this up. Then we reach what I call the saturation point, where now there is a reverse trait (?) of the frequency of bursting; bursting becomes really high. So after this we get a very rapid burst rate for irradiations going from this saturation point here to say 100 MWD/t above the saturation point, this line here is more or less linear. The rough interpretation of this curve is that there is an incubation period here and the bursts which occur in this incubation period are what we call the manufacturing oddities. They are the extreme ones which due to some possible manufacturing defect have actually burst. In this period growth or irradiation damage is building up and the effect really occurs after the saturation point. During a Wigner release it may in one zone occur here, in which case it will make no difference to the figures before or after. In this region here it is ...?... linear, and also therefore will make hardly any difference between before and after.

WILLIAMS Per zone. This is for a particular zone.

CHAIRMAN That one was exceptional. What do you mean - exceptional? It was a lot more than 15?

WILLIAMS No. This would normally discharge round about 350 MWD/t. This zone went to 500 megawatt days per tonne.

CHAIRMAN Give us the figure at 500.

WILLIAMS At 500 the total number of bursts was 58.

DIAMOND What are the figures at 600?

WILLIAMS The irradiation did not reach 600. The zone was discharged round about 500 megawatt days.

DIAMOND Going on linear, still?

WILLIAMS Well here I should point out that there is linear up to this region here; most of the graphs show a more or less linear slope up to this region here.

SCHONLAND That's up to 400?

WILLIAMS Mostly about 400.

CHAIRMAN You have drawn a linear scale. Can you put a figure on that. It is one zone - that is to say, it is one-eighth of the pile?

CHAIRMAN This means in fact that one-fifth of the channels or so, each had a burst in it ...at least, 1 in 5 of the channels had a burst somewhere in it by the time you had got to 500 megawatt days per tonne.

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WILLIAMS Yes, that is quite true.

CHAIRMAN 58 in about 300 or 400.

WILLIAMS I want to stress this point – we have only had two zones going up to this irradiation.

CHAIRMAN Yes I understand that. How many channels did this particular graph refer to?

WILLIAMS This graph would refer to about 300 channels. What might be relevant here is the actual pattern of bursting for the affected zones. I could show you zones 1, 2 and 3 of Pile 1, the pattern of bursting for these zones and what condition they were in when the incident occurred.

DIAMOND Is there any other statistics for the position along the channel length where these bursts?

WILLIAMS Yes, we have statistics on this, but it depends on the type of cartridge used, are you interested in any particular one?

DIAMOND Mark 10.

WILLIAMS The Mark 10 are the more recent type of cartridge ...?... enriched material. Several bursts were examined and the channel position located. Bursts occurred in No. 3, that is from the charge face outwards. I should say that No. 3 is approximately running at a central ...?... temperature round about 200°C. The channel positions were 3,3,3,3,4,5,4. These are for Mark 10 cartridges.

DIAMOND So they are grouped between Nos. 3 and 5. But the temperature is fairly low, and that is about between 4" and 6" in.

WILLIAMS Yes.

CHAIRMAN As much as that?

WILLIAMS Yes, the cartridge length is...

DIAMOND 12" plus spaces. Can you give us the figures for Mark 6?

WILLIAMS We have had very few bursts in Mark 6 cartridges and I cannot give you any figures.

DIAMOND Mark 5?

WILLIAMS Yes. There are two types of Mark 5 cartridges. We normally put them in two categories although they are more or less identical, in that the first Mark 5 had a ?thinned/finned? roll over each. I had better concentrate on the second batch. In the Mark 5 cartridges which contained beta-annealed metal, the bursts occurred predominantly at central channel positions. The type of burst was rather different in the Mark 5 in that there was definite evidence for UAL₃ formation which penetrated the can wall. I will read out the channel positions of what we call the second series of Mark 5 failures. The positions were 15, 13, 13, 12, 4, 14, 12, 15, 12, 9, 4, 17, 16, 14.

DIAMOND Could you tell us what the normal type of burst was in Mark 10?

WILLIAMS Seven were examined and they all showed the same mode of failure. This was a circumferential split at the shoulder with only a small amount of oxidation occurring at the end. When I say a split at the shoulder it is really in the region where the finned part ends.

DIAMOND How many were examined?

WILLIAMS Seven were examined.

DIAMOND A small amount of oxidation?

WILLIAMS Yes.

DIAMOND These were picked up on the scanner?

WILLIAMS Yes.

CHAIRMAN A circumferential split – you mean it went all the way round the edge?

WILLIAMS Yes, sir.

KAY Pretty well pushing the end cap off?

WILLIAMS Yes, the mark 10 bursts were due to longitudinal growth of the uranium of the order of 2 mm.

KAY Pushing the end off, and failure occurred just at the end of the finned position?

WILLIAMS Yes, the strain was taken up in the unfinned position.

CHAIRMAN What megawatts were these, do you know?

WILLIAMS I have not got the irradiation values for these particular bursts, but they were of the order of 300 megawatts days per tonne.

DIAMOND One more question: what evidence is there for a burst not being picked up prior to a Wigner release and being picked up within a day or two of start-up again after Wigner release?

WILLIAMS I don't think I can answer that question.

KAY You have not investigated the immediate period after start-up.

DIAMOND Your evidence is consistent with these bursts detected after Wigner release occurring during that month from normal causes?

WILLIAMS From normal causes – yes.

DIAMOND Do you think it possible that a burst may not be picked up before the start of Wigner release, but be picked up after?

WILLIAMS I think it is quite possible that at any period, certainly for the zones 1 and 2 more or less where the incident occurred, the graphs there were of this sort in that they had reached a saturation point and they were in this region of the

curve. Therefore I would say that there is quite a possibility of a burst or a cartridge about to burst at any time, but the time at which it bursts I cannot say. But there is a probability of a cartridge about to burst or could be already burst.

DIAMOND May be already burst?

WILLIAMS It depends really; if they shut down before the scanner sensitivity was not great enough to pick up the burst, they might have shut down at this period. This is purely conjectural.

DIAMOND What radiation was that? You are talking of zone 1 and 2 in Pile 1 before this last incident?

WILLIAMS Yes.

DIAMOND What radiation was it?

WILLIAMS Zone 1 irradiation was about 280 megawatt days per tonne. The type of graph is this, and this is the start of the burst. We had a burst here and a rapid burst rate there and it as suddenly slowed up here, where the incident occurred in this region.

CHAIRMAN And Zone 2?

WILLIAMS Zone 2 the irradiation was ~ 240 megawatt days per tonne.

DIAMOND You told us that zone 1 was 280, but you have drawn it on the steep part of the curve. I thought you put 300 at the knee.

WILLIAMS You will always get a structure in these results. When I quoted a figure of 300 megawatt days per tonne for the saturation point this is more or less a nominal value, in that you always get a structure. One zone might go at say 250, 250 megawatt days per tonne, the saturation point, and another zone might go to up to 320 or 340 megawatt days per tonne.

DIAMOND Then that curve is particular to this pile and this zone.

WILLIAMS It is particular, but not unexceptional.

DIAMOND It is specific to this zone in this pile on this occasion, and therefore it is more accurate that that general picture you have drawn.

WILLIAMS Yes.

SCHONLAND If there are no more questions on irradiation times I would like to pass to temperature. Is that included in your figures for position along the... No, we have laboratory tests on temperature? Have you any laboratory test information? The effects of temperature on rate of burst.

WILLIAMS No laboratory figures, but we have some figures on the actual operating conditions in Windscale, the effect of temperature on burst rate at Windscale. Can I again illustrate with the zone graph. This has not been established, but the general tendency is that if the zone operating temperature increases, then this saturation point moves

further out. In this region here where the burst rate frequency is high then the results obtained on 2 zones where the operating temperature was changed during this period here, showed that the burst rate increased with increased operating temperature.

10.50 a.m.

CHAIRMAN Is it a marked effect, or rather small?

WILLIAMS The effect is certainly detectable. It is best to illustrate in this graph here where the operating temperatures were changed. I did actually quote a figure in this report here. I stated here that the effect of increasing the operating temperature from 350°C to 390°C was to raise the burst rate by a factor between 2 and 5.

CHAIRMAN That is the wrong way isn't it?

WILLIAMS The effect of increasing the temperature increased the burst rate.

CHAIRMAN It increased the burst rate. I see. But it moved the threshold outwards.

WILLIAMS That is true, but that mark really applies to this region here. (Referring to chart).

DIAMOND Do you mean that, or do you mean that that yellow line is the equivalent of a larger number of MWD/tonne?

WILLIAMS One cannot distinguish ...it is difficult to disentangle whether it is the effect of radiation or the effect of increased operating temperature, but our conclusion from analysing this work was that the effect of temperature alone was to increase the burst rate by this factor.

CHAIRMAN Once you have gone above the knee? You move the knee upwards but you make the curve steeper.

WILLIAMS I should also add that these results were based on the Mark 5 Beta annealed vector. The number of results on the Mark 10 are really insufficient for us to form any conclusion at present.

CHAIRMAN Returning to the Mark 10 we seem to have evidence here that there is a preferred region for bursting between No. 3 and No. 5 cartridges where the central temperature is about 200°C, of the order of 300 MW D/t.

WILLIAMS Or less.

DIAMOND At 280 I think in zone 1. It must have been from that curve you've produced.

DIAMOND Supposing you were to raise the temperature quite rapidly above 200°C of one of these cartridges.

WILLIAMS Does this assume that the cartridge had not burst. We have no operational experience on that. Certainly as far as growth... we think that the phenomenon giving rise to Mk 10 cartridges is growth of the uranium. Growth is dependent on the temperature. Raising the temperature would decrease the growth rate.

DIAMOND The growth of the uranium relative to the aluminium depends on the rate at which the temperature is raised.

WILLIAMS Yes, this is the differential expansion between the uranium and aluminium.

KAY The bursts you get in the pile in the ordinary way, with the Mk 10 cartridges are due presumably to the growth of the uranium under irradiation pushing, extending, along the cartridge and eventually pushing the end cap off. So you would after a certain period of irradiation expect to have the uranium pressing more or less up to the end, and beyond that you get a much more rapid rate of bursting ...?...

WILLIAMS Yes.

CHAIRMAN Any more questions?

CHAIRMAN I would like to return to the question I asked very early, that Dr. Williams has no certainty that after a Wigner release, burst cartridges are found from channels which are about to be discharged but are not reported because the channels are discharged. You said you were not quite certain of that.

WILLIAMS That is true.

CHAIRMAN Any more questions? Thank you very much Dr. Williams.

You have given us some figures and we have jotted them down. I do not know whether you could put them on paper for us. Could we have them. Could you leave that curve please. The things you have quoted to us and drawn on the board, have you got copies of them here?

WILLIAMS Well here are a set of graphs for Piles 1 and 2. This is a Tech. memo on the actual burst rate with increased irradiation. The figures on this paper here show the number of bursts before and after a Wigner release. I would point out, if you examine these figures, when you come to Pile 2, there is one spot here where the number of bursts before a Wigner release was 20 and afterwards was 5, but this can be explained in that the Wigner release occurred when this zone was showing a really high burst rate and is really quite exceptional.

CHAIRMAN Yes. Will you leave all these for us please. Thank you very much.

WITNESS: MR GAUSDEN

CHAIRMAN I think several of you have questions for Mr. Gausden and I have one little one. Is there anyone who wants to say anything?

KAY I have two or three really small ones.

CHAIRMAN We are trying to clear up a few small points and they are rather scattered. They are not on one line. What I want to do is to give each member of the Committee a chance to ask a question or two.

GAUSDEN Yes.

CHAIRMAN The question I want to ask is that when you are planning a Wigner release, do you take a last minute check up with the scanner gear to make sure that there is not any cartridge that is just about to go.

GAUSDEN The scanner gear is on right the way through while we are reducing power for the shutdown until the main blowers go off, and then at the point where we put on the shutdown fans the scanner gear is switched off. So we do check right the way down.

CHAIRMAN So you keep it going as long as you can, and keep looking?

GAUSDEN Yes.

KAY What is your experience of the occurrence of burst cartridges on re-starting after either a Wigner release in particular or a shutdown in general.

GAUSDEN First of all I do not think you can differentiate between a burst rate after a Wigner release and after a normal shut down.

KAY Yes.

GAUSDEN We always get an odd burst after a shut down but my impression is that we would probably have got that anyway if we had continued normal running. The incidence of bursts after a shutdown does not seem to be particularly high compared to normal running.

KAY You do not think for instance that the heating up and coming back on full power is the final blow to a few incipient...

GAUSDEN There does not seem to be a trigger effect, so that we find half a dozen bursts on our hands.

KAY It is not uncommon to find one or two bursts on starting up.

GAUSDEN Oh yes, that is true.

KAY How quickly do you pick that up? On the first scanning run?

GAUSDEN No, it isn't immediately. This is over a matter of two or three days. We don't start up and immediately have to come down again.

KAY On a normal shut down you discharge a zone, presumably the most irradiated zone, and reload. Any defective or burst cartridges in that zone which had not been picked up on the scanner gear, which were discharged, would escape registration as burst cartridges.

GAUSDEN Not necessarily, because these cartridges are all discharged with boats on. One of the first jobs is to bring all the metal in and take the boats off and if we find a cartridge which has burst this is reported to me. In fact I have never had such a report, but the drill is laid on for that to be done.

KAY Would you register them as burst cartridges?

GAUSDEN Any that were obviously so, but a pin point might escape detection.

KAY Quite often when you get a reading on the burst cartridge detection gear and discharge a channel you have difficulty in finding the burst cartridge if it is a small defect. You would have to examine them very closely.

GAUSDEN That is true.

KAY My second question is, when you are carrying out your Wigner release, you are not using the burst cartridge detection gear – there is no reason why you should – but what about the activity meter in the exit duct? You have to run the turbo exhauster and you would normally shut this off when you cease to use the burst cartridge detection gear.

GAUSDEN Yes.

KAY Is there any reason why the exit duct should not be sampled during the period. for activity.

GAUSDEN You cannot split the two unfortunately. If you put the turbo exhausters on you immediately start drawing air away from the discharge face through the scanner gear. so. we've always considered that to be undesirable. You cannot do one without the other.

KAY During the Wigner release you do not want to withdraw air from there and you switch the turbo exhauster off and it is normally off for the whole period.

GAUSDEN Yes.

KAY On the incident... it was in fact switched on when you realised you had a burst or some considerable amount of activity, but there would be no reason whatever for running that turbo exhauster normally.

GAUSDEN No.

KAY In fact it would cause of other difficulties with withdrawal of air through the burst cartridge detector gear?

GAUSDEN Yes.

KAY My only other point is a small factual point. Where exactly is the chimney base door which is opened to prevent the chimney draft from becoming effective. Where do you get to that door from?

GAUSDEN Is there a diagram that might help here.

CHAIRMAN Can you draw a diagram?

GAUSDEN Not very well. There is a trap door in the floor leading to a shaft about 10-ft in depth with an external door at the bottom of the shaft

leading out to the open air, but the trap door would be roughly in the centre.

KAY You admit air to the base of the chimney there?

GAUSDEN Yes.

KAY Finally what are the sizes of the damper openings on the inlet air ducts? What is the actual area when you open the dampers.

GAUSDEN I am not absolutely sure of the sizes here. I can give them to you roughly or, alternatively, I can go back and send you down the details.

KAY Roughly.

GAUSDEN Roughly each main fan damper is about 4' x 4'. The shut down fan opening is around 2' x 2'.

KAY And how wide does it open? Is it completely or a flap?

GAUSDEN Virtually completely. You are opening flaps from a vertical to a horizontal position.

KAY Some of the area is obstructed by the flap but the greater part is open.

GAUSDEN Of course you can control this. You can open them halfway to get a greater obstruction.

DIAMOND How many of them are there?

GAUSDEN Five.

KAY Going back to the Wednesday evening, when dampers were opened, were these just the shut down dampers.

GAUSDEN Yes they would be.

KAY So there were...

GAUSDEN There were four shut down fans.

KAY There are four shut down dampers, each approximately 2' x 2'. They would normally have been open fully.

GAUSDEN Not initially. We use the term "cracked".

KAY The term cracked has come up and it was not clear whether it meant open fully or just a crack.

GAUSDEN I see. Initially one opens them a small amount and they are gradually opened up to their full extent.

KAY Over a period of ten minutes? So when we say, air dampers opened for ten minutes, it would mean a quite gradual opening up of the shut down dampers.

GAUSDEN Yes.

KAY Ending up with them being fully open at the end of 10 minutes?

GAUSDEN Yes.

CHAIRMAN And when you shut them do you shut them slowly?

GAUSDEN They go straight off.

CHAIRMAN When you say the dampers were opened do you mean the 2' x 2' or the 4' x 4'?

GAUSDEN Not the main blower dampers.

CHAIRMAN So the entrance in was four partly obstructed things because you have the louvres; four of them, and they were approximately 2 x 2. They open rather slowly and they shut rather fast.

GAUSDEN Much faster than they open.

DIAMOND Have you any experience of what the rush of air is when you open these dampers.

GAUSDEN It is not very perceptible. In other words, if you stand by a shut down fan and expect to feel a great gale rushing past you, you do not get that.

CHAIRMAN Even after ten minutes.

GAUSDEN Yes, you do not get the impression of a lot of air flowing through.

CHAIRMAN No. Nothing like the amount of air that is pushed through by the shut down fans. They really push it through.

GAUSDEN Well, even with the shut down fans running you do not get the impression of a lot of air being drawn through so it is difficult to judge. Personally I would say that with all dampers open you are probably getting almost as much air through as you would with the fans running.

KAY That's only with the full draught from the chimney.

GAUSDEN Yes.

KAY It takes time for that.

GAUSDEN Yes.

DIAMOND One other question, on isotope loading. There are two channels here, in fact there are a number of channels unloaded with isotopes, 1953 and 2153. Are they normally loaded or is there some reason for them being kept empty.

GAUSDEN There is a reason - the A.M. which is the bulk of our flattening material. We do try to leave a number of channels so that we can trim, if you like, and these are in fact

two of the channels so that sometimes we would put A.M. in those channels and sometimes we would not.

CHAIRMAN Dr. Schonland?

SCHONLAND Just two questions. Before this last Wigner release on Pile 2 the pile was shut down and the fuel element channels were scanned.

GAUSDEN Pile 1.

SCHONLAND Sorry, Pile 1. If there has been no incident would you have scanned again before discharging Zone 1 and Zone 2 to the pond.

GAUSDEN Normally we would not.

SCHONLAND You would have discharged, re-fuelled and started up?

GAUSDEN Yes.

SCHONLAND Then, in the operation of the pile, raising it up in temperature during the Wigner release, there is a rise in temperature that we have been given to understand is usual as 2° a minute, but the records show the operator went to 6° to 8° per minute. Is not this a surprisingly rapid rise in temperature on the fuel element thermocouples.

GAUSDEN It certainly is. I have not looked up that point at the moment, but normal operating procedure is to not raise any fuel element temperature at a greater rate than 2° per minute.

SCHONLAND Would you like to see the curve?

CHAIRMAN I think we ought to get Mr. Gausden to look at that to make absolutely sure we are not making a mistake.

SCHONLAND Which is it?

CHAIRMAN I am not sure - the fuel element temperature chart - someone whipped it off us first thing and I've been trying to get it back most of the morning.

DIAMOND Well, could I ask another question?

CHAIRMAN Yes.

DIAMOND When did you discharge uranium cartridges on this occasion?

GAUSDEN We have not discharged any. Well, we have now, but...

DIAMOND None were discharged before the Wigner release?

GAUSDEN No. During the normal programme it is to operate the Wigner release, get that finished and then do the discharge and unloading.

DIAMOND Yes.

PIERSON It is not available, this record.

SCHONLAND I would like to see it.

PIERSON We could get it back from Mr. Hall, but we would interrupt his work.

CHAIRMAN Are there any other questions except that one?

I think we should have a recess for a few minutes until the record comes over. We would like you to look at this. It is a very important point. I think it would be a mistake to go on to Dr. Leslie. I would rather hear Mr. Hall's first.

11.20 a.m.

CHAIRMAN Now Mr. Gausden, this is the point. Time is going this way and this is the period of the second nuclear heating and these are the various channels, and you see that 300 to 400, so that is 50 degrees, one sphere (?). I do not know whether you would like to look for a moment and to tell us what you think the maximum rise of temperature per minute is. We will give you five minutes to look at it.

GAUSDEN Well as far as this chart shows us, it looks to me as though there was about a 50 degree rise in $6\frac{1}{2}$ minutes.

CHAIRMAN 50 degree rise in $6\frac{1}{2}$ minutes?

GAUSDEN Yes, that was the sort of order.

CHAIRMAN And although we could no doubt fix better values by a very careful estimate – a careful measurement of that – it is of the order of 8° or so per minute?

GAUSDEN Yes.

CHAIRMAN Right. Any questions?

SCHONLAND Could I ask a question?

GAUSDEN I was going to point out here that it is, in fact, a rise which took place between successive stampings of that point. In other words, the temperature was down at one stamping and right up at the next stamping.

CHAIRMAN And that is a three minute period?

GAUSDEN It is $3\frac{1}{2}$ minutes.

CHAIRMAN A period of $3\frac{1}{2}$ minutes between stamping. Dr. Schonland, you have a point?

SCHONLAND Isn't a somewhat abnormal way of operating a pile to have such a rise of temperature.

GAUSDEN Well, we certainly would not want that sort of rise. It should conform to our normal specification of 2 degrees a minute, in other words, over that period say, 13 to 14 degrees at the most.

DIAMOND Why don't you want it to be more than 2 degrees a minute.

GAUSDEN Mainly due to the effect on the can, we do not know that, or we have reason to believe, I won't say categorically that we know, we have reason to believe that at an automatic shut down, where the whole thing is shut down very quickly and the temperatures drop quickly, that there is a certain amount of stress set up in the cans because we have seen an extra one or two bursts appear after such a shut down. Looked at statistically, it doesn't look at all significant, but looked at from a practical point of view you do seem to get this odd one or two extra.

PIERSON Your operating experience makes you believe that if you heat your fuel too quickly you get a burst.

GAUSDEN You stress the can more than you should.

KAY It is the other way round. It is on the rapid cooling you have no evidence on the rapid heating.

GAUSDEN No, that is true. The rapid cooling produces the stress in the element. Rapid heating, you are quite right, of course, we have not had a similar experience.

DIAMOND Is it not true that the conditions during a rapid heating with no air on are the same as the conditions as during the rapid cooling with the air on.

CHAIRMAN I do not see how Mr. Gausden can answer that.

GAUSDEN I couldn't give a very definite answer to that, No.

CHAIRMAN Any other questions?

PIERSON May I ask one? Is there a polite description of the NX119 suitable for a report.

DIAMOND I asked for this.

GAUSDEN Mr. Bowen is getting you the precise details.

DIAMOND We can ask this after lunch.

PIERSON The other thing is how long would it take to motor out the rods ...?... the second heat? I mean is it ten minutes, or two hours or ...?

GAUSDEN Oh no, of the order of two hours. I cannot be quite specific but certainly not minutes. It is about two hours.

PIERSON The log told us that the pile diverged the second time about 11.5, so that the decision to have a reheat must have been taken...?

GAUSDEN Soon after 9 o'clock.

KAY Is this the actual time required in motoring the rods out?

GAUSDEN Yes, this is the actual physical time required, because of gear down ratio, that they will not move out any quicker. They move at 9 centimetres per ten minutes. That is the maximum speed.

KAY You do not have to stand with your finger on the button all this time?

GAUSDEN Well, you can either keep your finger on the button or operate a master lever at the bottom. In fact, what is done, is the lever is operated to bring the rods out to just going on to critical.

KAY Automatically: motoring them out at a steady rate, then you bring it on to diverge on the push button?

GAUSDEN If I may come back to Dr. Schonland's point on this, I believe Mr. Toole in fact did give you a description of what he did at the time and how this temperature did jump up quickly. I do not want to repeat that, obviously.

CHAIRMAN Well, we have what Mr. Toole said. No further questions?

Well I believe this is the last time we shall be calling you Mr. Gausden, is there anything you want to say to us before you go?

GAUSDEN I feel I have told you everything I can.

CHAIRMAN Allright. We just want to give you the chance in case you had thought of something.

GAUSDEN I have been over it many times but I really believe you have covered all the ground.

WITNESS: MR. LESLIE

CHAIRMAN One of the points we have been thinking about, among a lot, is the oxidation of a fuel element that has failed for some reason and we are in a position where, on the assumption that perhaps there has been a failure of the fuel element during stagnant air conditions prevailing in the Wigner Release, that the temperatures of that fuel element might be anywhere say from 300, 350, 400, goodness knows just where, and we want to know what might happen at these various temperatures. Well, we know that you have done some work on the oxidation of fuel elements of this sort and we are wondering whether there is anything that you have done?

LESLIE I have put it in my report.

CHAIRMAN To be perfectly honest we have not done more than glance at the report. We thought we'd get it much quicker from you.

LESLIE The aim of that work was really to see what happened in the event of a burst developing and not being detected, and what we did, very simply, was to make a hole in the can with either a screw driver or a .03 drill I think it was, and put it in the oven, and measure the volume of oxide in a given time, and you will see pictures at the back at 500 degrees.

LESLIE Yes. There are some other experiments I will tell you about. Well the other ones were done in an oven and you can see the volume of oxide developing. We also did a couple

of experiments, we took cartridges that had been heated in an oven and had developed a pimple of oxide and we put two of these in the pile in an experimental rig and continued to heat these up, and there is also a picture of that at the back showing the can, and in one case the can opened out quite considerably, and then the other experiments that were done with cartridges that had already had the cans fairly widely burst. These are the first pictures, I think, in the report and these were heated up in a pyrex tube at about 400 or 500.

KAY 400.

LESLIE They just continued to oxidise and eventually bits would blow off.

CHAIRMAN Blow off? There is an air stream through the furnace.

LESLIE This was in the ones that were heated up in the pyrex tube.

KAY In what you describe as a channel simulator?

LESLIE Yes, that is right.

CHAIRMAN So these are really trying to simulate normal operating conditions, not stagnant air.

LESLIE Yes, really aimed at finding out how much oxide you would get liberated from a really severe burst, in case the scanning gear failed to pick it up for some reason or other.

KAY You wanted to know how much time you had in hand and you came to the conclusion that in the event of the scanner gear being in normal working order you could pick up a burst and discharge before any severe oxidation had set in?

LESLIE Yes.

CHAIRMAN The temperatures must be very critical in this?

LESLIE Oh yes, there is another report, also on oxidation of uranium, and the oxidation rate as a function of temperature. It is really a collection of the literature on this.

KAY By Shaw?

LESLIE By Bell I think.

KAY In the course of these experiments Mr. Leslie, did you investigate heat developed from the oxidation. Did you consider the problem of a self-sustained oxidation or not? That would not have been within the terms of reference of this particular study, but I wondered whether you do know.

LESLIE I don't know, I think the oxidation rates were very very low. For instance, when you put it in the pile, the cartridge dissipation would only be about 1 kW or something of that sort, and the heat from the oxidation would be very very small.

KAY Your experiments here extended ...well you were concerned with damage that would occur over a period of about 24 hours?

LESLIE Yes, it was aimed at finding how serious the damage would be if the burst was missed.

CHAIRMAN I do not know whether you could give us a picture of what might happen. Suppose that in this stagnant air, and the temperatures, to be definite we'll say 350, suppose then that an end cap failed so there is a little crack round there, and the air is stagnant, temperature 350.

LESLIE It would be a very long time.

CHAIRMAN A very long time?

LESLIE Yes, a very long time, at 500 I think. Well you see here, that is a cartridge 500 degrees after five hours, it is a very small amount of oxide developed.

SCHONLAND Can you be more specific about "A very long time" - is it in weeks, days or months?

LESLIE I would say between 300 and 400. I would say that it would take say two or three days before you get a gramme of oxide or one or two grammes of oxide.

KAY This would go up rapidly with the temperature?

LESLIE Oh yes, very rapidly.

CHAIRMAN When do you think it would start really, at what temperature...?

LESLIE Here we are - here is a cartridge - 1 k

KAY I have the impression quite a lot of oxide is formed at 400 in your experiment there.

LESLIE There is actually a picture, though it may be slightly misleading, of an end cap failure after 27 hours at 350. There may be some doubt about this because it says "burst No. 7". Well there was a great deal of doubt in those days about temperatures, and also they are put into the pond which obviously ...

CHAIRMAN It must have knocked them about.

LESLIE And also there is water reacting with the oxide to make it develop.

DIAMOND Burst No. 9 was rather more difficult to explain, wasn't it?

LESLIE I do not remember No. 9, I did examine some very early bursts but I do not remember that one.

CHAIRMAN I do not want to put words into your mouth, but I just want to understand...

LESLIE I think if you want the oxidation rate I can give you a report, which will give you mg per cm² per hour.

CHAIRMAN I would like that if there is a report in existence. Now that was our most general point and we are all very interested in that. Any other questions?

KAY No, I think I would like to see that report.

CHAIRMAN Could you get that report Mr. Leslie please, and we have one other thing, could we say 12.45. Could you come at 12.45 please.

DR. GRAHAM

CHAIRMAN Now this is a last minute interview - we want to get the latest position, you have probably had some measurements in since you spoke to us.

GRAHAM Yes, I have been waiting to get the complete story, but...

CHAIRMAN Please proceed.

GRAHAM Well in general I can say there is no strontium problem at all to the district people and with regard to the site people I got enough quick answers to show that it is not at all serious.

CHAIRMAN I am delighted to hear that. Could you give us a little evidence. What are the statements based on?

GRAHAM Well, with the district people we sent samples for radio chemical analysis, doing them in the proper way, and out of the thirty or so that we sent we have only got five results back yet and the highest of these, taking the worst possible calculation, constitution: strontium 89, 0.02 micro curie, which is 1/100th of maximum body volume. Strontium 90 is .0005 micro curie and that is 1/2000th the maximum permissible body burden. So that is the district survey and I have only five answers in that series.

CHAIRMAN This is the radio chemical analysis of what?

GRAHAM Of urine. (Asked to repeat figures) strontium 89, 0.02 micro curie, is 1/100. Strontium 90, .0005. Yes, that is 1/2000 and I have taken the worst interpretation I can.

CHAIRMAN Does it take time? Will it go up and up with time?

GRAHAM It will be coming down. It starts... The time lag is no more than 24 hours before it starts coming out.

CHAIRMAN Something goes in by the mouth and comes out, does it go up in the body?

GRAHAM Yes. In the district people, this would be true to some extent, but would have reached equilibrium... On site it was different. The site problem was an inhalation one and the district problem, roughly speaking, was an ingestion one, so that the metabolic things are slightly different in the two cases.

CHAIRMAN The figures you gave us of the radiochemical analysis just now, were they site, or are they district.

GRAHAM District.

CHAIRMAN District people, and that is ingestion. It may be going up a little bit simply because they keep on ingesting.

GRAHAM Yes but they will reach equilibrium in a very short time.

CHAIRMAN The site problem then?

GRAHAM We did these by the quick method again, the scintillation... you know, by energy discrimination, and 25 of these have been done and I only have the answers to 8 of them. The way they were calculated, barium and lanthanum was looked at and it was reckoned back to Strontium 89 and 90. The comparison of the two sets: the site people if anything are a shade higher than the district people. One would expect that. Taking the worst figure I have – I have to guess at this but it is of the order of 0. I don't see how it could possibly be higher than one-tenth maximum burden of Strontium 90. That is a very broad statement. When we get time we will do each individually.

CHAIRMAN That is the Strontium. Anything further on the others?

GRAHAM I got some figures on urinary iodine which came up. They were extra; we didn't particularly want them at this time, but they came out of the thing and I have not done any work on them. The difficulty about that was that they swamped the spectrograph quite appreciably and we had to adjust the machine to look at ...?... and lanthanum. At some point we must relate this urinary iodine to the thyroid measurements, but I haven't had time yet.

CHAIRMAN You are not really in a position to go on record about urinary iodine.

GRAHAM No, sir. I am afraid not.

CHAIRMAN When you were giving your account last time of the thyroid activities and something like 70 out of 150 were coming out at a bit above the figure that you had deduced from the M.R.C. figure, I was not quite clear whether you were on safe ground or not. Would you please just tell us again how you argued?

GRAHAM Yes. I have looked at this one again since. I had a draft I was going to send. At this time we had done 193 people, I think the figure is now more like 250. Out of 193 we detected radioactivity in 40%.

CHAIRMAN These are people outside the works?

GRAHAM Unfortunately they are mixed, inside and out. I think Dunster is analysing all this now. He will probably give you a better report. Of the total number tested, 12.4% showed a level which was more than .1 micro curie per gland, and this .1 micro curie per gland is the I.C.R.P. limit for continuous exposure.

CHAIRMAN In other words they can have this for a lifetime and the worst you were getting was $2\frac{1}{2}$ times this?

GRAHAM The highest reading I had then was .275 micro curie. I have had a higher one since in an adult. Speaking from memory, 0.5. The integrated dose coming from these sorts of levels is being calculated at the moment. The only one I know is this child of 6 with a level of .275 micro curie. We think this particular gland is due to get 7 rads while the iodine was on.

The MRC limit with respect to cancer – the threshold dose is regarded as 250 rads over a lifetime.

CHAIRMAN There is no way in which you can give them some iodine pills or something and swamp the thyroid and take it out?

GRAHAM Psychologically I think it would be very bad to do that and I do not think it would give you much of value.

CHAIRMAN You do not want to do it. However at the worst for the adult you are 5 times worse than for the ICRP for a lifetime and for this one child of 6 you are $2\frac{1}{2}$ times that.

GRAHAM You scale it down because of the short period of exposure.

CHAIRMAN Is this child presumably... and in fact everyone is not now taking in any more iodine?

GRAHAM That is so. This ICRP figure implies a continuous topping up.

CHAIRMAN So that really if you had the child of 6 and an adult here the adult was having the ICRP figure and the girl, the ICRP man would pass the girl in a few months.

GRAHAM That is so. This is not the whole story, you understand. There is a tremendous lot of scientific interest in this stuff. It will be done in a very full way later.

CHAIRMAN No doubt in due course it will have to be published.

GRAHAM But from a quick survey of the thing, that's what it looks like.

CHAIRMAN You are quite satisfied on medical grounds that it is only the iodine and the strontium we need to worry about? The caesium, have you any doubts about that caesium?

GRAHAM I had none until you mentioned it. I ought to be absolutely certain about this but the maximum body burden is very high for caesium. The ICRP limit is 98 microcuries and the excretion rate is high as well. I do not think there is any caesium problem.

CHAIRMAN There are no other problems?

GRAHAM There is the question of the people on the charge hoist. The question of particular exposure to the head. I have not looked at the question of cataracts in one or two individuals due to looking down the holes, a neutron beam or a gamma beam to the head as a local dose. I could not guarantee that nobody's eyes have been damaged by this. It takes such a long time to develop.

CHAIRMAN Now that it is done there is no way of doing tests on these people to find out?

GRAHAM An ophthalmic specialist could look and see if there are any seedling cataracts there. Many people have them in the ordinary way, but do not know. It would be of value to know if there were any in existence now so that we could measure them later.

CHAIRMAN Is there any remedial action possible if there are?

GRAHAM No. Not at this stage.

CHAIRMAN The Authority will have to consider this because there is a question of causing panic, once you start remedial measures. It might be possible to detect them but if you detect them you can do nothing.

GRAHAM No, and you can't guarantee the patient will ever notice he's had them. 90% of them don't become clinically...

CHAIRMAN We do have records of the men who looked down these holes. We know which men they are.

GRAHAM Yes and they will have cap films. This is one of the things we have to do. I do not know whether we will do immediately.

CHAIRMAN My point is that we do have the records. Is there anything else Dr. Graham?

GRAHAM Anything else would be elaboration, greater detail. You don't want that.

CHAIRMAN I think we have got what we want provided, through ignorance, we are not missing some important point.

GRAHAM I have not heard of any others mentioned and we have discussed this in all sorts of quarters. Psychologically I have had one or two neurotic responses to this. About half a dozen - people with imaginary sore throats and things of that kind. Reassurance is ...?...

CHAIRMAN Anything serious?

GRAHAM No.

CHAIRMAN What do you do with a neurotic case?

GRAHAM Talk to them to start with, then you take measurements to show willing - you take the blood count and tell them it is perfectly normal. We were getting one or two who have to report for duty to the decontamination squad on the charge face. There have been one or two who say they are not very well and should they go in. Could they be excused? We have taken these off even although we found nothing wrong, even though it is only psychological. We leave them on the normal job. There are people like storemen and group office clerks, etc. who have reacted in this way. It is not a problem.

CHAIRMAN Anything come to your notice about similar cases in the district?

GRAHAM Not on any big scale. One person, I have heard, went to the local doctor asking whether he would be all right because he had been digging in his garden on the day. The local doctor sent him up to me and I told him it was all right.

CHAIRMAN The local doctors will work with you on this?

GRAHAM I am sure of that. I have been in touch with the ones we know best and I think they are helping us in this way. There was a question asked in the local hospital about human breast milk and about whether it was fit to give to new-born babies, and I said yes, and I think that clinic is now quite happy.

CHAIRMAN You explained that ladies don't eat grass?

GRAHAM I didn't say that. I didn't think they would appreciate it.

CHAIRMAN But that is the reason why the iodine is in cow's milk and not in human milk.

GRAHAM Yes.

PIERSON Was the man Ritson scrubbed in the surgery on the night of the incident?

GRAHAM Yes. Several times.

CHAIRMAN Were any others similarly treated?

GRAHAM Oh yes, it is quite a common thing to have this done. He was not the only one.

CHAIRMAN When you have to do scrubbing it is done in the surgery?

GRAHAM Yes. We use things like detergents like Cetavlon? and potassium permanganate. We have one or two special gadgets for washing special places and we take swabs. We do it sometimes in stages. Let them go home and come back and have a second go. Sometimes we can't do it all at once. I don't know the result of Ritson's urine sample. It will be coming along.

CHAIRMAN Thank you very much Dr. Graham.

WITNESS: DR. LESLIE

LESLIE ...showing the oxidation rate as a function of temperature, and got a number of results. There is quite a bit of scatter on it; it depends exactly how you do it. You were interested in...

CHAIRMAN I am really interested in three temperatures, 350°, 400° and 450°. Is that covered?

LESLIE Yes. A sort of average value; there is some spread on the thing, but at 350° from the curves it looks as though it is about .002 grammes per c.m.² per hour and then at 400° it is about .02 and then about 450° - .2. Actually there are no values going as high as that but it is going up quite steeply.

CHAIRMAN In the region of .1 or .2. Is that about right?

LESLIE Yes.

DIAMOND What were the air conditions.

KAY Can we have the tables so that we could study.

LESLIE Well I will have to look through. We did one or two experiments here. There was another paper where we were purposely trying to find out whether the oxidation rate was different in moving air. This was a very simple experiment. We had a small uranium foil in a glass tube and blew air over it. We measured the change in weight and we found that at 350° it was about 0.1 g/cm²/hr.

KAY There was quite a big variation in the conditions.

LESLIE Yes. You can see from the scatter...

CHAIRMAN At the same temperature if you move the air you put it up 400 times. Is that a fair deduction?

LESLIE I do not think so really. It's probably just the scatter on this. There's a value quoted in this paper we got from the original Chalk River workings on this, and it says 0.1 g/cm²/hr at 400°C, and at 300°C, 0.022. In our experiment we found 0.1 g/cm²/hr at 350°C. There is certainly a lot of scattering. You cannot really apply these to cartridges, can you, because the oxide is building up and the can is being prised off, and it is also acting presumably as a self seal, having a self sealing effect. Certainly that applies to body bursts where the can is packed tight with oxide.

CHAIRMAN So really apart from looking at one set of experimental results and another set there is no direct evidence on the effect of merely making the air move over it, instead of being stagnant.

LESLIE I do not think so. We feel that in this experiment there did not seem an enormous difference, and I don't think you would expect there to be, unless it has some eroding effect which may take the oxide away and leave more surface.

CHAIRMAN I was startled by the difference between the value you quoted in one curve and what you quoted again: at the same temperature there was a difference of 400.

LESLIE There is quite a bit of scatter there. If you look you can see. I think it is very difficult to apply any results directly to exposed uranium in a can.

CHAIRMAN Yes, it is too variable, too undefined.

SCHONLAND Do you carry in your head the heat of combustion?

LESLIE I'm afraid I don't.

CHAIRMAN We shall have to look that up. Are there any more questions?

This is the last time we shall want you, is there anything you want to say to us?

LESLIE No, I do not think so.

CHAIRMAN We are right down to the last one. I think that is Hall. I think we had better leave him to the end of the afternoon because he has some work to finish. I think we are now in the clear to work on our own for three or four hours.

13.00 hours. 25th October, 1957.

