Excerpt of Transcript

1977
Board of Inquiry

into

the Occurrence of Bush and Grass Fires in Victoria

Witness
John Austin HART + Technisearch reading

transcript pages 1706 to 1736

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MR. LLOYD: Is the suggestion for the alteration of the section dealing with regional advisory committees your idea or somebody else's?---My idea.

It envisages that there would be, apart from the representative, another person who would attend and observe, two from each constituent body, is that right?---Yes, that is correct. The problem if I might just enlarge on that point - - -

Just one moment. In region 13 there are 57 rural brigades, I am instructed, 14 urban brigades, and 8 municipalities. With two people from each that gives you a regional advisory committee on one view of 158, or on another view something like 74 which is a big advisory committee, is it not, 74 people?---Yes, it is.

That is not counting the instrumentalities that might be involved. Would you really think an advisory committee of 74 would be practical?---It is certainly not. I agree with that. The problem is that unfortunately a lot of municipalities tend to be rather lax as far as fire prevention is concerned. I had to try and work out some way that as many people as possible, or as many areas as possible, are going to be represented by the people.

THE BOARD: Going to be involved in the thing?---Yes. If I suggested one from each brigade, I have been in error. I suggest one from each group with someone to follow up. In other words, if one person cannot attend, somebody at least can attend and speak on behalf of that particular group of brigades.

You want to avoid the situation we have discovered in a number of instances where, through no fault of his own, the CFA officer or the S.E.C. man, somebody or other, has other duties which prevent him attending a particular meeting, and then no representative is there. That is what you want to avoid?---That is correct.

That sounds sensible to me. Thank you, Mr. Irvine. I cannot help thinking the main thing that arises from your evidence is that there ought to be more proper officers with your energy.

(THE WITNESS WITHDREW)

JOHN AUSTIN HART, sworn and examined:

MR. STANLEY: I have had the opportunity of discussing the problem with the photograph with the sergeant, and I think I have convinced him I am right.

THE BOARD: Thank you. I thought I should give you the opportunity.

MR. MARKS: What is your full name?---John Austin Hart.

Where do you live?---11 Herra Street, Burwood.

What is your occupation?---I am head of the Third Mechanics Section at the Herman Research Laboratory.

Mr. Hart is a signatory to the report that has been tabled. I shall tender this report because it has annexures to it.

EXHIBIT 216................Laboratory Report dated 13th May,1977, signed by Mr. Pleasance and Mr. Hart.

MR. MARKS: In the compilation of this report, Mr. Pleasance took part in the performance of the tests which are the subject of this report?---Yes, he did.

First of all, what are your qualifications?---Bachelor of Engineering.

What is your experience?---I graduated from the University at Adelaide.

Since then you have been with other authorities than the SEC, I understand?---I have. I worked for six years with the Australian Atomic Commission, three years with Mt. Isa Mines Limited, and three years with the SEC.

What sort of work were you involved in with the Australian Atomic Commission?---I worked in a heat transference and fluid flow group.

And Mt. Isa Mines?---I was a research metallurgist.

What is fluid mechanics?---Fluid mechanics is the study of movement of liquids and gases.

The particles are neither fluids nor gases?---No, but the movement of particles in the context we are interested in is dominantly affected by what we call aerodynamic forces which are fluid mechanical considerations.

In the report which I shall read, first of all, at the time you compiled this report, were you satisfied you had done enough tests to reach conclusive opinions?---Not conclusive opinioms.

What sort of opinions,?---We were able to establish the possibility that under conditions of extremely low probability, the particles ejected from conductors during clashes could have been sources of ignition of bush fires.

You mentioned in the report that you were able to measure the heat of particles as they left the conductors?---We were.

Would you explain in simple terms how you did that?---We were actually measuring the temperature, of course, of the particles which is a measure of their heat content. We used a method called optical barometry where an instrument is fixed on a hot pad and the colour of a wire is varied by means of the electrical current passing through it, and one seeks to match the colour of that wire with the objects being viewed.

Like the use of colour charts?---Yes, the same sort of thing. The instrument is calibrated such that you can read off, once having got a match, what that electrical current means in terms of temperature.

I shall ask you to read the report and then afterwards, you have had the opportunity of reading a report by Dr. Penman? ---I have.

You have some criticisms about that in due course?---Yes.

Would you read the report?

(Witness reads report).

THE WITNESS (Continuing reading): The general arrangement of the rig is as shown in Figure 1 which is attached.....
.....extremely low.

MR. MARKS: There is some alteration that needs to be made on the first page of the appendix dealing with Table 1.

THE BOARD: Incidentally, go back to paragraph 5, in the third line it reads, "The temperature, whilst being blown more than a few metres", "inches" has been struck out and "metres" inserted.

In the last line "exceptionally difficult" is substituted for the word "long".

MR. MARKS: Those alterations are correctly made?---Yes. They were typing errors.

(Page 1710 follows)

MR. MARKS: In Table 1 there is an error, is there not - or some errors?---Yes.

I have to point to the column headed "Initial velocity", the sixth column?---Yes.

There is some error that had not been translated back, what was the reason?-- The reason was in our calculations we used initial velocities as metres per second and the calculations were correctly made. When the results were tabulated, for consistency with the wind speed, the initial velocities were converted from metres per second to kilometres per hour, as appears in 4.4 and so on, at the bottom of the line. That is incorrect. The conversion was incorrectly made, and it does not affect calculations.

What are the correct figures there - 9, 10 and 11?---4.4 - I do not have the final version there, I am sorry.

Who has it?---Mr. Pleasance has it.

57, 57 and 40?---Yes, I have the 57s here, but not the 40.

9 and 10 are 57 and 11 is 40. In order to simplify the meaning of the results, have you, since the report was compiled, done some graphs or has Mr. Pleasance?---Mr. Pleasance prepared the graph.

But you have checked them, have you?---Yes.

Can I hand those around? (Copies circulated).

THE BOARD: They can be added to Exhibit 216.

EXHIBIT 216 (Part of)..............Graphs.

MR. MARKS: Do you wish to make some explanation of how one reads the graphs, starting with the graph on the right hand side, headed "Effect of particle diameter on final particle temperature in 75 kilometre wind"?---The intention of this graph is to consolidate all the graphs we have into a reasonably digestible form and the point and the graph you draw attention to is to superimpose the result of our experiments on the temperatures necessary to ignite grass on our calculated particle time temperature histories.

You will see that in the top right hand corner of that graph there is a shaded area bounded on its side by a partly solid and partly dotted line. That is marked "Ignition of grass", and that means within that area the particle possessing temperature rate from the horizontal scale at the bottom would ignite extremely dry grass. The boundary is partly solid and partly dotted to emphasise that only the solid part of the boundary has actually been experimentally determined; the dotted portion is a considered extrapolation of our experimental data. The lower lines on that graph are intended to show the various relevant conditions, the way in which particle temperature varies with particle size.

Can you give us an example of that? Take a one millimetre particle which is expelled from the conductors?---If you look on the vertical access of that graph at the .1 millimetre and follow on to your right, you will find it intersects with a group of curves at temperatures somewhere between 1,000 degrees centigrade and if you go to the extreme of those curves, 1300 degrees centigrate. Any of those points fall outside the shaded area and, therefore, we are able to say that a 1 millimetre particle and, of course, any particle less than that will not cause a fire.

Why is that?---Because the temperature at which it arrives at the ground and after being blown by a wind strong enough to clash conductors, is too low to ignite the grass.

But it might go any distance, might it, some considerable distance?---Yes, you can refer to the left hand side of the page, the graph there, the lower one, and you will find on the horizontal access, we have plotted the distance travelled by a particle and, again, on the vertical access, the temperature of the particle, and if you seek on that verticle access the 1 millimetre size and travel across to the lines there - -

120, is that the one?---Yes, that is the distance it would travel in a wind of 75 kilometres per hour, which is the maximum gust speed estimated for the day.

That is if it leaves the conductor 20 feet above the ground, is it?--Yes.

What does the broken line with the two dots represent?--That represents a different wind speed.

57, is it?---No, "Effective particle......75 kilometres per hour wind." That represents a different initial condition, such as you can have the particle being ejected at zero velocity from the line. In other words, it comes away from the line with no initial speed of its own, so it can be flowing out from the lines.

Just to explain what you are talking about, if there is no wind at all and you are in static environmental conditions, but the particles come within the line, the zero, the position is it would just drop dead down underneath it?---Initially it would be, but it would be blown by the wind sideways.

Assume there was no wind and you have absolutely dead static conditions, when you say it has no initial velocity, it would mean it would come out and drop straight down, is that right?---Yes.

But if you have no wind at all and some initial velocity, then you have it emitted at some speed not yet defined, have you?---Yes.

So when you are talking about initial velocity, you are talking about the speed at which it is expelled, irrespective of any wind assistance, is that right?---Yes, that is right.

So that is what you are talking about when you are talking about initial velocity, is that right?---Yes, that is right.

You were going to explain Graph B ?---That is the graph we were considering, the three lines there. I have mentioned the fact that the initial velocity can vary and according to whether it is directed, for example, vertically upwards or vertically downwards or cross-ways, the ultimate distance of travel will vary.

We have plotted the maximum distance a particle could possibly go. This is the very dark black line to the right. If it was ejected from the conductor clash with a 57 kilometre per hour velocity vertically upwards, then it would travel, a 1 millimetre particle would travel about

190 feet. If the initial velocity were the same speed, 57 kilometres per hour, but horizontally in the direction of the wind, then we would go back to the dotted line and only get a travel of about 125 feet from the line. This illustrates that it is the time in the air which mainly determines that horizontal difference because when a particle is ejected perfectly upwards, what, in effect, you do, is add to the time it has to reach its peak of travel, then come to the ground.

These graphs indicate, do they, one or two basic things: first of all, that the bigger the particle, the less distance it travels, is that right?---That is correct.

And, on the other hand, the bigger particle will tend to retain heat longer than the smaller particle, is that right?---That is right.

I do not propose to read through the Appendices.

THE BOARD: No, I have seen them.

MR. MARKS: In addition, the other conclusion, in ordinary language, is that the bigger particles are emitted very rarely from conductors that clash?---That is right, particles larger than 1.75 millimetres in diameter only constitute somewhere between half and one percent of the particles ejected from the conductor clashes.

What is the significance of there being some 1.7 millimetres in diameter - why did you take the 1.7?---That happens to be the size in the particular method used. It is an arbitary choice.

With your tests that you have done to date, what size particle do you think you would need to have in order for there to be a risk of it starting a fire - the minimum size?---Particles less than about 1.5 millimetres are very doubtful propositions as ignition sources, for the reason that their ignition threshold temperatures - and I draw attention to the shaded portion on the right hand part of that graph again - their ignition threshold temperatures are greater than 1200 degrees centigrade and they just do not reach the ground at those temperatures.

You have arrived, have you, at an opinion that the particles need to be 1200 degrees centigrade in order to ignite dry grass, is that right?---Those particular sized particles would need to be more than 1200 degrees centigrade, in fact.

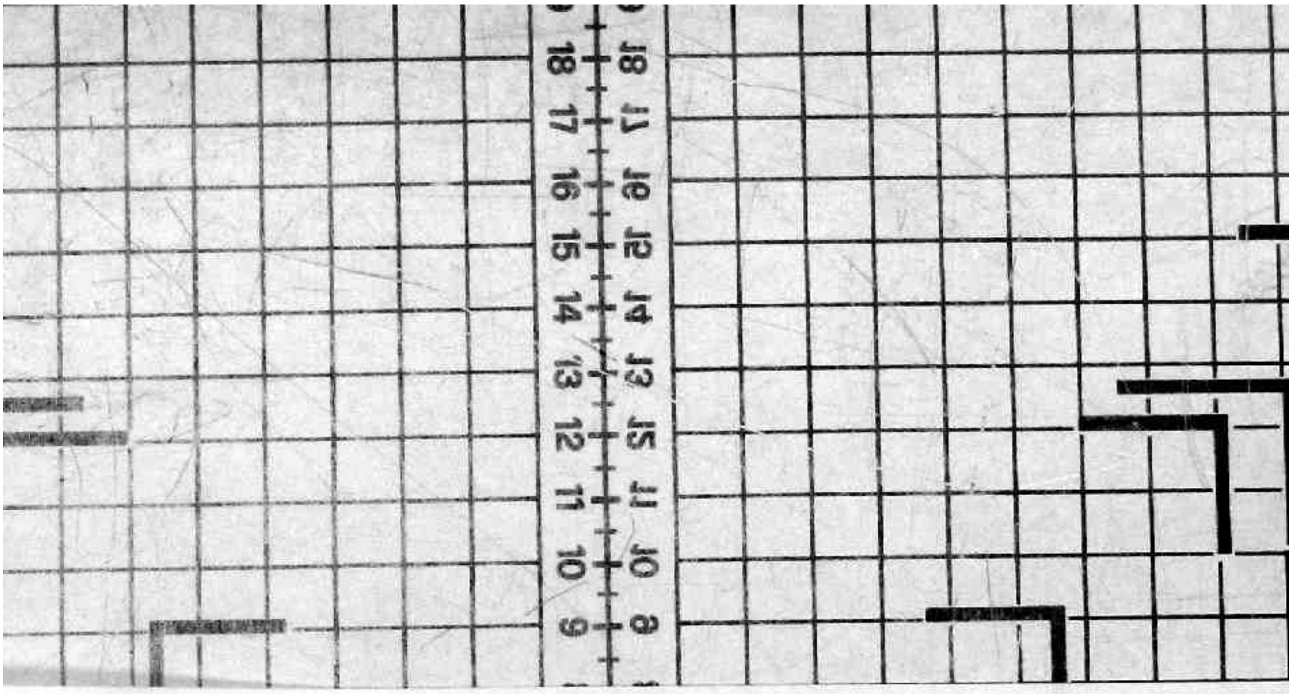
How hot does a 2 millimetre particle need to be?---A 2 millimetre particle, we can read off from here, it needs to be 1200 degrees centigrade. I emphasise that that refers to extremely dry grass, that is drier than one would expect to find in the field, so it is a conservative figure. We are looking at the graph on the right hand side of the page and reading across from 2 millimetres, through to the broken line at the edge of the hatch.

That is about 1150?

THE BOARD: Yes, almost 1200. It is 1200?---It is a little lower - say 1160 or 1180.

MR. MARKS: You have had a chance to read about the tests done by Dr. Penman in his submission?---Yes.

THE BOARD: Who presents Dr. Penman?



MR. MARKS : Mr. Dunne. To settle a discussion between myself and my junior, Mr. Chairman, would you tell us - that was an exhibit number I think?

THE BOARD: I gave Mr. Hart's report and Mr. Pleasance's, Exhibit 216, and I thought the graph could simply be added to 216, but if anyone wants an extra exhibit, we can add another number.

(Page 1713 follows.)

MR. MARKS: You have written out, for your own edification, the criticisms of the tests done by Dr. Penman?---Yes.

MR.DUNNE: Dr. Penman's report was basically obtained by me for my own information to assist me in any cross-examination of the SEC witnesses, if it happened that there was evidence given by the SEC that the sparks could not have started the Beeac fire, and I would have called Dr. Penman. That being the case, I do not think it is warranted that I call Dr. Penman, and I am happy for his report to be put in as an Exhibit.

MR. MARKS: I cannot complain about that.

EXHIBIT 217.................. .Report of Dr. T.O. Penman.

MR. MARKS: Is that the report you have seen?---Yes.

These tests about which you have just given evidence, were performed by Mr. Pleasance under your supervision?---Yes as the head of the section. Mr. Pleasance, is an engineer. I directed in the sense that a supervisor does. On this occasion I was directly involved in inititiating the tests and getting them under way. Mr. Pleasance performed the actual labour of being there at all times, and I was there only for part of the time.

He is present if anyone wishes to know about the bits and pieces he did. You could read your comments?---These are comments on the report of Dr. T. O. Penman on the start of the Beeac bush fire. In summary, although Dr. Penman's conclusion, if moderately stated as, for example, "Hot metal particles ejected from clashing aluminium conductors cannot be ruled out as the source of the Beeac bush fire" is not inconsistent with the SEC findings, the premises on which it is based are radically different and in my opinion unfairly so to the SEC. In three important respects his assumptions are unfavourable and in my opinion unfairly unfavourable to the SEC. They are:

(a) that the particle temperature which can initiate ignition is as low as $600^{\circ}F$, which is $315^{\circ}C$.

You say that is an assumption made by Dr. Penman about which you strongly disagree?---It is not exactly an assumption in that he has conducted tests which he says showed that as the temperature which could be assumed to apply to grass. However, he did not do the test with grass but with paper and merely assumed that the ignition of paper would be the same as grass.

Do you agree with that?---I do not. I was making the three points, the three important respects in which his assumptions are unfavourable, and in my opinion unfairly so to the SEC. Item (b) was that the particles can bounce large distances. Item (c) was that arc welding typically produces particles comparable, especially in size, with those produced by conductor clashes. I can give detailed comment on each of these. I also believe that the report shows clearly that Dr. Penman does not have a thorough grasp of the mechanics governing the motion analysed and I have selected four specific examples of incorrect statements on which his credibility could be challenged. It is more difficult to challenge his temperature history calculation because so little information is given, but I strongly suspect that it is grossly over-simplified.

HART.

20.C/AC.
Fire.

I then deal with the first of the points I mentioned, which is ignition threshold temperature. Reference to this material is at page 6 of Dr. Penman's report. About this material, I have said that Dr. Penman's treatment of this is cavalier and offhand in the extreme. He does not quote the temperature of the 2 mm particle when it was observed to be still hot enough to ignite paper after six seconds, and one suspects that he did not know what that temperature was, although the information is clearly necessary if one is to judge whether particles cooled at some other rate remain hot enough to do the same. In any event the ignition threshold temperature of particles would have to be considerably hotter than the quoted theoretical temperature of 451°F, or 233°C, because resistance to transfer of heat from particle to paper would require a substantial temperature difference between the two to exist, and because the heat loss from the particle to paper during the finite time taken to heat the paper and initiate self-sustaining combustion would cause its temperature to fall.

The SEC experiments show that the ignition threshold temperature of a 2 mm particle in extremely dry grass is at least 1200°C, strongly suggesting that Dr. Penman has no basis for assuming similarity between his paper and any grass. A CSIRO expert, Dr. R. Vines, advised the SEC on these tests and has stated that he can see no significant dichotomy between his findings, which were on the ignition of cottonwool by carbon particles from tractor exhausts, and ours. Work was done some years ago.

Dr. Vines was consulted by you about your results on the relationship between the heat required for ignition compared with the tests that had been done?---Dr. Vines was originally called in to consult with us how we might set these tests up and he advised on the experimental techniques. He subsequently visited the laboratory and observed our tests and he commented on our results.

Dr. Vines' name has come up before.

MR. DUNNE: Dr. Vines' tests are referred to in appendix 5 of the report.

MR. MARKS: You then went on to deal with particle bounce?---That was the second point where Dr. Penman's assumptions were unfavourable and unfairly so to the SEC. This is referred to at page 5 of Dr. Penman's report. The extent to which a dropped particle rebounds after hitting the ground depends importantly on the nature of the ground, and would in any case only be substantial from surfaces more elastic than macadamised road or natural earth. Also, the subsequent trajectory depends on the angle of impact, some of the horizontal component of motion also being dissipated, but to a different extent than that of the vertical component of motion to which it must be presumed Dr. Penman's 'other tests' apply. We view Dr. Penman's assertion that a 2 mm particle would travel 51 feet between a first and second bounce as totally unrealistic, especially as he has ignored the effect of wind during this period, despite his wind tunnel observations. I say the bouncing behaviour off the wooden tunnel floor, which Dr. Penman referred to, is in other respects quite misleading in the field context.

I could perhaps elaborate a little more on that. I have had the opportunity of doing some calculations to back up, as it were, my opinion that Dr. Penman's

HART.

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Fire.

assertions were unrealistic. We have shown, in fact, using our own calculation programmes, even if one allowed that particles could bounce from the sort of surface that we are interested in, the actual distances which they would bounce are much smaller than what Dr. Penman has asserted. He has not given any detail how he calculated that length of bounce.

(Page 1716 follows)

THE WITNESS(Continuing): The important point there is that in fact, according to his evidence, or according to his report, the 51 foot of bounce of a particle accounts for something like 2/3rds of the total travel of a particle from a distribution line from which it originated, so that fairly obviously if one accepts that the partivle could not have bounced to anything like the extent he suggests, this puts a very different figure on the lnegth of travel.

Item 3 relates to particle size, and Dr. Penman deals with this on page 2 of his report. Dr. Penman incorrectly states that the average diameter of particles collected from the clashing of aluminium conductors was 1mm and that they were solid, round, droplets. In fact, 50 per cent were less than 0.3mm diameter and less than 5 per cent were greater than 1mm; there were virtually no particles as large as the 2.8mm particles collected by Dr. Penman from arc welding. More than 10,000 particles were measured in the SEC samples.

Item 4, particle flight calculation. I go on to analyse some statements made by Dr. Penman which I say indicate his poor understanding of the mechanics involved.

a. "drag coefficient ...depends on (particle) densities". This is completely erroneous because the drag coefficient is independent of particle density.

b. "drag coefficient (of a falling particle initially at rest with respect to the velocity of air around it ... increases in magnitude as the particle separates". In fact, it decreases as the particle acquires an increasing speed relative to the air.

c. "the effect of the two relative velocities is not unrelated, and the nature of the interrelation is not known." Actually, there is only ever one velocity of relative motion at a particular point in a particle flight, and it uniquely determines the prevailing drag coefficient in an entirely predictable way from standard information."

d. "an exact calculation was made of the combined drag coefficient." The motion of the test particles relative to the tunnel airstream would certainly have been such that the drag coefficient varied from point to point as the relative velocity varied so the calculation could have been in so sense exact, and any calculated single value would be at best an average.

Dr. Penman appears not to have taken account of the direct effect of particle temperature on its motion (because the surrounding fluid is heated by the particle), nor of its indirect effect through oxidation changing the size, shape, density and surface roughness, but our experience is that overall these effects are small and our calculation corresponding to his time of about 1.4 seconds for the free fall of a 2mm particle from rest 23 feet above ground is 1.45 seconds.

Item 5, rate of cooling calculation. Nowhere near enough information is given to appraise the adequacy of Dr. Penman's temperature history calculation but it appears likely to have been grossly over-simplified. The main deficiency is undoubtedly the neglect of particle exodiation, but this is of course "conservative" in the sense of underestimating the resulting particle temperature because heat is released during oxidation. However, we would need to know that radiative heat transfer, which Dr. Penman does not mention, had been correctly taken into account, using an emissivity appropriate to the particular particles, before we could be sure that the neglect of heat generation was not offset by neglect of an important heat loss.

MR. MARKS: Those are your comments on Dr. Penman's report?---They are.

Your experiments were done with aluminium conductors?---That is right, only aluminium conductors.

You know, of course, that many conductors in the SEC installations are copper and also aluminium and steel?---I do not know from my personal experience.

Assuming that to be so, would you expect very much difference in the results if you did the tests with proper conductors?--I cannot say, because I have got no idea of the extent to which copper would oxidise during its flight. I can only say that my own experience is that aluminium oxidises much more rapidly than copper, and therefore one might expect that copper particles heated up less than aluminium particles, but we would definitely have to do those experiments to determine that.

I cannot remember what the conductors were at Beeac. What about if the conductors were aluminium and steel?-- These were the actual conductors of the type in my report, these were the actual conductors used in the laboratory simulation.

You call them aluminium when they are aluminium and steel?---Yes, that is the actual conducting part, the aluminium, the external part.

MR. NIXON: When did you carry out those tests, what month?---In April, going into May.

How long have you been with the SEC?---I have been with the SEC since November 1969.

How did these tests of conductor clashes compare with other tests conducted by the SEC since you started?---Other tests of conductor clashes?

Yes?---There have been no previous tests in which any detailed data of the type we obtained, measurements of particles, filming of particles for measuring velocities, measurement of particles and so on, there have been no tests in which data was collected.

Were you aware that after the fires on 8th January 1969 the SEC was severely criticized for, amongst other things, conductor clashes?---I am only aware of that recently by hearsay.

So that none of this type of test has been conducted at the SEC?-- Not to my knowledge, but they would not have been done by my section.

You would have access to the records of these tests if they had been done?---I would.

Have you made enquiries as to whether any such tests have been done in your time with the SEC?---No, I have relied on the statements of the people that would have done them, that none of the information we required to complete these calculations was available from previous tests.

MR. NIXON: Reference No. 6; particle diameter 1.5 millimetres, given the wind speeds and other details you have there, given a temperature of 1566 degrees on reaching the ground?--You are right.

And would travel a distance of 83 feet. In fact travelled 83 feet on that occasion?---That is correct. These are the result of calculations.

That you believe, if your calculations are correct, it would travel that distance?---That is right.

Whilst 2 millimetres diameter would have a temperature of 1580 degrees on reaching the ground and would travel 62 feet?---That is correct.

Those temperatures of 1566 degrees and 1580 degrees for those particles of those sizes would be sufficient to cause a fire?---I cannot say for certain with 1.5 millimetre particles because that would lie in the extrapolated range of our results. On a reasonable extrapolation of those results - no, I would not say that - our results on ignition temperature could be reasonably interpreted to suggest a particle of 1.5 millimetre diameter could never cause a fire but until you actually did the ignition threshold experiments for that size of particle then you cannot make that statement, nor can I make the statement that at a temperature of 1566 degrees it would cause a fire.

It is possible?---In the sense anything is possible. It cannot be ruled out.

What about the 2 millimetre diameter particle?---We have done the ignition threshold temperature measurements. They are not quite to 2 millimetre diameter but to a point reasonably close above it because we would have to be reasonably competent of our threshold temperature and that temperature could cause a fire. However, I have to emphasise this is a fire in extremely dry grass, that is less than 1 percent moisture compared with 3 percent which is regarded by the Forestry people as being in their terms extremely dry.

Have you seen the area where the fire started at Beeac?--I have not.

Or at Balliang East?---I have not.

MR. BARNARD: Do I take from your answers to Mr. Nixon you have available to you information as to what research has been carried on by the State Electricity Commission in the past in relation to conductors clashing?---It would be available to me. It is not within my department.

You have made enquiries in relation to this type of matter?---No, I have relied on my conversations with the officers of the department who were concerned on this question and it was mentioned there was no point in looking into this information because the data required to complete our calculations had not been obtained in the earlier tests.

There were two factors involved in fire risk associated with the clashing of conductors. Firstly, size and heat of particles that come off. But the other question whether particles come off at all depends on the amount of energy generated as a result of clashing?---We have not examined in great depth that sort of effect. One from general experience could assume there might be some effect. What we did was to vary the electrical conditions associated with conductor clashing and we showed they did not have any great effect on the character of particles, size of particles, that were ejected. These electrical conditions were chosen to be representative or to cover a range including the estimated field conditions.

I understood your description of taking the rod away and letting the wires clash together. Do I understand that to be when they clashed together they stayed together?---No, they usually come together and fly apart a short distance, come together again and occasionally, particularly at the lower voltages, they would eventually stick together but usually in most of the tests some particles were ejected from the initial and subsequent clashes.

In fact this was just not one clash that was being tested but a series of clashes involved in each test?---Yes. 79 tests in all and each test consisted of several clashes.

Perhaps more than several occasions?---It depends on what you mean by "several", but the order of 5 clashes.

The time during which the fault current ran, was that in any way measured?---There is an attachment in Appendix 1. You will see A.1 presents typical voltage in current recordings.

That indicates clash duration but does that give us a measure of time?---It would. Up the top of that graph you will see 20 milliseconds which would enable you to scale off there.

I follow that. You have said you tested the aluminium. Is there any reason why you chose aluminium as the metal on which you carried out your tests?---Yes. The reason was the conductors of the fires of immediate concern to us, Beeac and Balliang East, the ones we were asked to give an opinion on, those conductors were aluminium of this type listed in the report.

Were they wholely aluminium; is that your understanding?---My understanding is they were not wholely aluminium.

A mixture of aluminium and steel?---This is not my field of expertise. The tests in that sense were not set up by me but set up by Mr. James, a witness here before. It was the province of the distribution engineering department to supply the clashing rig. It was our only brief, as it were, to make the measurements on the particles which they produced.

The chemical properties of metal is something about which you do have knowledge?---Only in the sense anybody doing a course in chemical engineering would have acquired that.

If a conductor was composed of both strands of aluminium and strands of steel you would expect that the aluminium would be the metal which is more likely to cause fires than the steel, is that not so?---As I understand it, and I am relying here on hearsay from Mr. James and his department, these conductors are steel cored. The actual conductor is wrapped around it so it is only the aluminium which can clash together as it were. The steel cores cannot come into contact.

THE BOARD: The only metal extruded is aluminium?---The only metal exposed to the clash that is carrying most of the current.

 1720. LUNCHEON ADJOURNMENT.

UPON RESUMING:

MR. BARNARD: Mr. Hart, the significant feature in the capacity of particles of aluminium to retain heat and cause fires is the formation of their energy on the oxidisation of the aluminium, is that not so?---Heat and reaction, yes.

Arising from the oxidisation?---Yes.

And I think you mentioned your tests demonstrated an oxidisation of between 20 and 64 per cent, is that not so?---Yes, in the laboratory tests, that is right.

I think in the calculation you assumed 30 per cent?---That is right.

If one put it the other way, aluminium, on oxidisation, produces a very high amount of free energy compared to other metals, is that not so?---I am not in a position to compare them directly. Certainly, I can say that in this case the heat of reaction contributing to the oxidation was a very important, even dominant, consideration in the heat balance.

Do you say you are not in a position to compare them?---I have not looked up the figures for, for example, copper, or other metals.

And it is only a matter of looking it up, is it not?---It is.

I see in Appendix 6 on page 2 you have a figure of 16,500 KJ/KG, does that represent kilojoules per kilogram - what does the figure represent?---Kilojoules per kilograms, that is the heat form of aluminium oxide, and that is the amou t of heat. The number of kilojoules which are given out per kilogram of oxide form.

And that is calculated from merely looking up tables of chemical properties, is that not so?---Not calculated, merely extracted from looking them up.

And those tables are usually expressed in kilojoules per mol? ---I could not say whether they are usually or not, people often use mols, people often use kilograms.

I suppose it is for the aluminium oxides, that figure is arrived at by multiplying the product of your Delta H factor and the molecular weight, that 16,500?---Delta H being the heat content per mol.?

Yes?---Yes, you would need to know the molecular weight of aluminimum oxide which I think is 48 and use that factor to get from mols to kilograms.

102, is it not?---I cannot say offhand, I think it is 96.

Would you accept that the Delta H factor is 1676 - minus 1676? ---I could not accept that. I accepted your reading it out of a book, which I presumed is the sort of book I would look up.

"Chemical data" - Aylett and Finley?---I would accept that. It could be the figure based on that.

MR. BARNARD (Continuing): If one looks to the copper oxides, it would be the cupric oxide, the factor for cupric is minus 157?---Again I accept your word.

You agree that the heat generated on the formation of the copper oxide is much less than the heat generated on the formation of the aluminium oxide?---Per unit weight.

THE BOARD: There is less heat created, is that the word?---Yes.

In the case of aluminium oxidation?--No, in the case of copper oxidisation.

Is less in aluminium?---Yes. If you oxidise one pound of copper to an equivalent amount of oxide it would heat up less than it would with aluminium.

MR. BARNARD: Substantially?---Per unit weight.

Looking at the 1 mm particle, it would be a substantially less heating up on the oxidisation process of the copper than it would be of the aluminium?---Not necessarily so because the reaction rate comes into it. You are assuming they would oxidise to the same degree. The reaction rate data is additional to what you are putting.

The reaction rate in copper is very much slower than aluminium, is it not?---That is my suspicion, but I cannot say that. There would have to be tests done similar with aluminium to determine that.

It is a matter of common observation?---Not necessarily.

They are extreme cases, aluminium is an extreme case?---You quoted reaction and of course there are extremes. The reaction depends very much on the physical circumstances surrounding oxidisation. We were, for example, quite unable to make use of text book data on reaction rates of aluminium oxides simply because those reaction rates are so dependent on the surface of the particles, the physical circumstances in which oxidisation takes place. As more oxide forms the rate of reaction slows down. I cannot say what the equivalent circumstances with copper would be.

You really have no feeling about that?

THE BOARD: He has really got a suspicion, he says.

MR. BARNARD: You are talking about the rate of oxidisation. Is it not a well accepted fact that one never sees aluminium in fact, the normal person, because it oxidises so quickly?--That is right, but what you also have to point out, is that a film of oxides forms very quickly and prevents further oxidisation. At high temperatures our results indicate that oxidisation was not as much inhibited in that way as it is at low temperatures. You have to know the copper-oxygen system in that particular physical form before you could make a transference or analogies.

You might agree then that it could well be in the case of copper, the copper particle of equivalent size to the aluminium particle, the difference in the heat generated by oxidisation may result in a conclusion that copper could not cause fire; in other words, there would be insufficient heat increase and the heat loss would have had cooled bythe time it got to the ground?---Provided you allow me to say it may do the opposite. I do not know.

You are not prepared to offer any view having regard to the different factors of heat generated on oxidisation in the two metals?---May I suggest say our observation on aluminium particles took us in a way completely by surprise. We did not appreciate the extent of oxidisation which was involved before. None of the people we talked to about that could have predicted the sort of results we found in the way of temperature and oxidisation. I think the same is true of copper. One would have to be extremely chary of the sort of knowledge I have of making a prediction.

There may have been some people before this inquiry who have not carried out tests and suggest that copper wires clashing could cause or could lead to fires. Would you agree at least in this they have seen this in the case of copper wires from their own experience that that statement could not be made as a result of any tests that were carried out on aluminium or on steel?---I do not quite follow your first premise. Would you go back and repeat your question?

I will put it in another way. If one has not actually seen fires caused by copper wires clashing or established that that could happen by tests such as you carried out, there would be no basis of concluding from tests such as you carried out in relation to steel or aluminium that copper wires also would cause fire in the same way?---I do not think that there is any sound basis. I think educated guesses could be made, but they would not be investigated in the same way as we did with aluminium.

What I am suggesting is a substantial difference in the factor of free energy formed on oxidisation means an educated guess that they behaved in the same way, copper and aluminium?--I would prefer to say that I would have to sit down with a pencil and paper. You have quoted a premise on energy. Obviously molecular weight of copper oxide and aluminium oxide are different, the densities of the material are different for me to say that a 1 mm particle of copper would heat up less from this point of view than 1 mm particle of aluminium requiring me - - -

I will interrupt you for a moment - I am suggesting that the molecular weights would help you?---It would help me but I would not be prepared to say that I accept it without calculating with pencil and paper.

What I suggested to you is that in the oxidisation of aluminium, if all the heat was absorbed into a 1 mm particle, the temperature might go up as much as 2000 degrees, is that not so?--I very much doubt that based on my own observations because we have particles which ended up 60 per cent oxidised and they did not exceed 1600 degrees centigrade.

I am suggesting if all the heat is absorbed into the particle itself?---If you imagine the precise way heat is lost.

The absorption of heat into the particle may be as little as one per cent?---Under what circumstances?

On the oxidisation, the amount of heat from oxidisation only one per cent may be absorbed into the particle and the rest lost into the atmosphere or lost outside the particle?---In the field?

Yes?---That is looking at it in one particular way. All the heat is initially absorbed into the particle and then it is lost in these processes. I differ from your picture a little bit. The point you are making is radiation heat loss is high so that heat tends to be got rid of almost as soon as it is formed; yes, I would agree with that.

I am suggesting the fact that there is not such a chance of heat gain with copper means that copper must be looked at in an entirely different way to aluminium, and you would not draw any conclusions or tests with aluminium by comparing tests?---There are other factors, of course. Copper melts at a higher temperature than aluminium so that it is going to leave the conductor at a higher temperature. I do not have these figures in front of me, so I cannot say more on that.

I will take you to another matter. You mentioned that copper melts at a higher temperature than aluminium which I take it within it involves more energy produced, electrical energy to cause that, is that not so?---That depends partly on the heat capacity of the two and the density and heat content of a particle at a given temperature is the product of the temperature multiplied by the density, multiplied by the heat capacity, multiplied by the volume of the particle. I would have to know those individual figures to compare the two. I suspect that the higher melting point of copper would be the dominant factor, and the particle at melting point would have more heat than aluminium at melting point.

(Page 1725 follows)

MR. BARNARD: To achieve it with copper you would need more volts by amps, would you not?---Again, that depends on how many particles are produced by a given clash. I do not know the way copper behaves under these circumstances. It may be that it produces very few large particles or a lot of small ones, or very few particles at all.

It is possible that it may not produce particles at all. In other words, it may deform the conductor rather than produce particles, is that what you are suggesting as a possibility? ---Well, it could produce less particles, or it could produce more, and obviously the difference could be related to other deformations, as you say. It is pure speculation on my part.

Carrying on into the speculation, it may well be that in the field situation the sort of energy required to produce particles big enough from copper may cause a flow of current which would blow fuses before you would get particles which would be capable of falling to the ground and causing fire. That would be another factor?---You are moving outside my area of expertise. I am not an electrical engineer, and I cannot say what currents are required to blow fuses.

In your tests, you were not relating them to the protective devices and whether it would be possible, having regard to what protective devices were fitted into the electrical system?--- We were using fault currents which we were told could have occurred in the field. These currents, of course, only occur for a short time, and I assume this is not sufficient to blow the fuse.

This was in the field at a particular place?---I understand that the fault currents are believed to have varied in different circumstances. It depends on the length of the line involved and how much resistance there is, and so on, and we covered a range of fault currents for that very reason.

In your tests on aluminium when the clashing took place, with each clash was it a matter of one particle coming away or more than one particle?---Each time a rod was pulled from between the wires there was a clash from which a number of particles were produced.

From the one clashing?---Yes. The conductors tended then to bounce back slightly on each other, or be repelled by some means and clashed again, and again as many as 5 or 6 times from a single rod pulling and each time they clashed there tended to be some particles ejected. I would say that unless the conductors stuck together there would never be a clash in which there were not any particles seen on film anyway.

You were using two voltages, normally 240 and 480, is that so?--- 240 and 450 - 415, I am sorry.

Is the conclusion from your tests that with higher voltages particles are more likely to come off?---No.

Can that conclusion be extrapolated in this way to say if you were using, in fact, 22,000 volts or 66,000 volts, are you able to draw the conclusion it would make no difference? ---No, I am not able to draw that conclusion either. Of course, you would not have the currents at those voltages.

You would not normally. I suppose you could, could you not. It depends on your protective devices?---I could not say.

HART

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Fire

Just as a matter of general knowledge, I would not think so. From our tests I could not say anything about what would happen at the voltage you have mentioned.

MR. DUNNE: Mr. Hart, in the acknowledgements in your report you set out nine different sections of the S.E.C. and an independent photographer involved in the conducting of these experiments, is that so?---Yes.

Quite a substantial undertaking?---It was.

Quite a number of manhours involved?---There were.

Any idea how many?---The tests, those particular tests lasted about four days, and there would have been 12 people, say 10 people involved on average, so that would give us 40 man-days.

Then relating to the information and the results?---Yes.

You were using quite expensive equipment?

THE BOARD: Mr. Dunne, I do not want to discourage you, but what does this have to do with it?

MR. DUNNE: I am putting a question regarding Dr. Penman's report. That is an explanation for many of the things in Dr. Penman's report, that he just did not have the manpower, or the money, or time to conduct the sort of experiments you conducted. That explains many of his deficiencies, does it not?---It would certainly explain why he preferred to produce particles by going to an arc welding workshop rather than set up the actual clashing of conductors. I do not think it can account for what I have pointed out are incorrect statements as far as, for example, the particle mechanics are concerned that one would expect from an expert working by himself.

In conducting your ignition experiments, you used grass hay?--- Dried grass hay, yes.

Is there any explanation as to why you did not use phalaris grass which I think would be more relevant than grass hay?--- Well, the experiments were done - firstly, of course, we had to work with a time limit, and we looked around for what the Forestry people advised us would be the most representative timber to use, and I selected dried grass hay on that basis.

I am just wondering how the Forests Commission was able to advise you in that regard?---If I said Forests Commission then, I meant Forestry officers both within the S,E.C. and the State Forests people.

Do you know whether they were made aware of the sort of grass in existence where the Beeac fire started?

THE BOARD: Mr. Dunne, the miracle of Beeac was that the thing jumped the phalaris, and the fire started in the paddock where it was grass hay.

MR. DUNNE: Well, that is one view of the evidence.

THE BOARD: I would have thought the only view of the evidence. It seems to me the selection of grass hay as against phalaris was the proper thing to do. I would be fascinated to know what happens with phalaris too, but if you had a choice of one or the other, I would have thought grass hay was the correct choice. In Mingay it started in the phalaris, but that was another matter altogether. That was the burst surge diverter. As I recall Beeac where it was, I forget how many feet, inside the paddock, it was in the fodder in the paddock, the ordinary grass in the paddock rather than the phalaris.

MR. DUNNE: That is certainly what the witness said, it was two or three yards into the paddock, and it was on a 10 foot front.

THE BOARD: Yes. I do not know that the experiment could be criticised for using one material more than the other. If you had a choice of one or the other, I would have thought the choice they made was the right one.

MR. DUNNE: With respect, that is something with which I do not agree.

THE BOARD: I cannot make you agree. All I can do is tell you what the Board's view is.

MR. DUNNE: Even inside the fence the material was not grass hay.

THE BOARD: It probably was not, it was grass, was it not?

MR. DUNNE: Probably eaten-out phalaris.

(To witness): The other thing in your ignition tests, which you have ignored, is the possibility of there having been some foreign substance such as paper which might have been ignited by particles?---Yes, that is true.

THE BOARD: I take it you were criticising the use of paper rather than grass, do you contend or agree that paper does light more readily than the dried grass?---Based on Dr. Penman's statements, I accept his statement as to what he did and he apparently found paper lit at a low temperature. I am only pointing out the contradiction between the two experiments.

Yes, I follow that.

MR. DUNNE: You have taken Dr. Penman to task for over-emphasising the significance of the particle bounce?---Yes.

It is certainly not mentioned in your report, but I think you said in answer to a question before that you had made some calculations in relation to bouncing and found there was some bouncing but much smaller than Dr. Penman had suggested, is that so?---We have done some calculations which take Dr. Penman's assumption on their face value, and have then done some calculations determining how far a particle would bounce. But, of course, we do not accept his assumption that we considered at face value, and that is that the particles bouncing on a macadamised road or natural earth would bounce to the same extent that he mentioned in the laboratory.

(Page 1729 follows.)

THE WITNESS (Continuing):

He does not appear to have taken into account the effect of the two materials together of what we call the co-efficient of restitution. That is the essential point. We dispute they would bounce in the way he says they do. We also say even if that were true your calculations were true, they show they would only bounce the order of 20 feet rather than 51 feet.

MR. DUNNE: If 20 feet is correct rather than 50 feet, it is a fairly significant factor?---Yes, we do not even accept that is correct.

Do you consider bounce would be a significant factor at all?---No. In our view of things the particles are likely to be molten anyway when they hit the ground. Anything above 660 degrees Centigrade, the particle is molten and if covered with a crust of oxide we have observed in our laboratory tests where they were falling on to Masonite boards just little bounces because they tend to splatter in some cases and generally deform. There is very little bouncing of that type of particle.

What you are saying, if the particles did bounce then the heat would be so low ignition would not be a problem?---That is correct.

Would you agree with this proposition; the survey sketches prepared by the SEC in relation to the Beeac conductors show a sag of something like 3.5 metres or 11 feet 6 inches?---I have not examined those sketches. My sections took the information from Mr. James.

If you can take it a plan has been exhibited showing a sag of 11 feet six inches, being my calculation, that would have an effect on the horizontal distance that the particles would travel from the normal line of conductors? ---They would be released at a lower height than if the conductors were not sagging.

If the conductors had been blown out with the wind that could well send them on their way up to 10 feet before the particles were actually released from the conductors?---No, really if they are blown one way they could not be clashing.

We have evidence they can be out of sag five degrees?---The conductors coming together, they might be somewhat to one side of the centre line of the distribution line but I cannot imagine they would be ten feet out. You are taking the full sag and adding it horizontally but then both the conductors would be out that amount.

I am taking one foot less, assuming they are that apart?---I could not assume that.

It would add a few more feet to your calculations?---Yes, if they are sagging that much they are ejected at a fairly low point in relation to the ground compared to conductors that are not sagging. The time above ground is the most critical factor of all because of the time they take to fall.

Looking at the graph now annexed to your report, can you explain to me the significance of the 57 kilometres per hour initial velocity?---When the conductors were clashed we have high speed films taken of the actual event. When I say high speed this was a special camera operating at 500 frames per second in the laboratory. We were then able to run that film through frame by frame and plot the position of a particular particle. We know how long a frame takes to move from one position to the next so we were able to measure the speed of the particles not exactly as they leave the conductor because there is a flash that obscures everything, but as soon as we saw a particle emerge from the clashing we measured the distance from two frames in a small unit of speed and that gave us the average speed between those two points.

That graph shows quite clearly that a two millimetre particle with no initial velocity will travel a horizontal distance of approximately 65 feet and have a temperature of nearly 1600° C?---Yes.

Which would be sufficient to ignite dry grass?---This is grass much drier than normally found in the field and these conditions relate to 75 kilometre per hour wind which is the maximum experienced and there is an order of half a per cent of particles of that size. We are talking of an extremely low probability occurrence but nevertheless if those circumstances all came together our data indicates possible ignition.

Similarly if you have 57 kilometre per hour winds in the horizontal position then the horizontal distance travelled would be in the order of 150?---No, you are thinking of it vertically.

57 kilometre per hour vertically up horizontally 15 feet and a temperature of 1500° C?---That is correct.

Again capable of igniting very dry grass?---Extremely dry grass.

Is it likely the initial velocity would be given any more impetus by a swinging of wires with the sort of sag I have described to you?---You are suggesting the wires themselves would have a higher velocity than the wires in our experiments?

Yes?---For the conductors to clash their velocities are opposed. The effective ejection velocity has something to do with the difference between those two. I do not think I agree per se that just because the conductors in the field were clashing from a longer distance apart that the velocity would be higher than we imagined.

They do not have to have opposite velocity if you have two wires of differing sags. They could clash whilst travelling in the same direction?---It would be a pretty gentle clash compared with the ones we are dealing with.

THE BOARD: You were dealing with opposed clashes?---Yes. Furthermore in one series of tests we did increase the velocity those conductors came together and did not find any observable effect on the number of particles or possible velocity.

You have some qualifications in extrapolating the results you have obtained into the extremes you have not been able to measure?---Certainly.

Despite that do you think the extrapolation of your results would be fairly accurate with a relatively insignificant margin of error?---The problem here is there appear to be two factors which govern a particle which will act as an ignition source. One is the temperature. It has to have a high enough temperature. The other is its heat content. If a particle is small even if it has a high temperature it looses such a big proportion in heating up a little bit of tinder with which it is in contact that the temperature drops so rapidly that the whole process is extinguished. The results on the graph you have drawn attention to do suggest that area of particle size, this range of particle size immediately below where our experiments finish, this was the range where the minimum heat content of a particle was likely to become the dominant feature. We do not know what that is. We do not know whether to extrapolate our results parallel or keep them going on.

Those examples after 2 millimetre particles - - -?---They were just outside of range of the experiment but the extrapolation would have less doubt with the two millimetres than the half and we would be more confident of our extrapolation as to the 2 millimetre size.

With a relatively smaller margin of error?---We have made a reasonably based informed estimate which I would be prepared to defend . on the two millimetre partiles. Below that I would have to say your guess might be almost as good as mine.

Although the number of 2 millimetre particles produced in 10,000, measured particles, was quite insignificant, less than one per cent?---Unfortunately not insignificant, but certainly very small.

On any given flash there is a likelihood of a 2 millimetre particle being produced?---Mathematically in order of one in 200.

One of the witnesses was taken to task because he said he saw sparks being produced off the conductors but could not say where they were going. Would you agree his inability to see where they were going is explainable by the loss of incandescence of the particles as they leave the conductor and get further away?---I do not know that is so much the reason. Our calculations suggest any particles that were still at 1600 degrees would still have been visible. A lot of particles do lose their temperature; the smaller particles cool off very quickly and we would lose sight of them.

This was a witness who was 100 to 150 yards away. That makes it more difficult?---He would only see the larger pieces of ejected material anyway from that distrance. The speeds involved are fairly high. When you look at a clash you do not see all that many particles per clash, even though we collected 10,000. I am sure nobody would have seen anything like 10,000 particles.

In page 3 of your report, Section 4, you talk about the conditions obtaining on 12th February, 1977; 45 mile per hour winds. That is the only place where you have mentioned non-metric matters on their own. Is that a mistake?---No, it is correct. I would prefer to see it in international units but unfortuanately the data was quoted to us as such and it seemed better to re-quote it. That is equivalent to about 75 kilometres per hour.

MR. LLOYD: Were you, in the course of your experiment, trying to reproduced as well as you could all the factors which you believed to have been present out in the field on the 12th February?---All of the factors which we believe would affect the result.

In other words all the relevant factors?---I have to qualify this because I did, right at the beginning of the report, emphasise we broke the problem into three parts. In the experiements we were only attempting to simulate the origin of particles. Obviously we made a judgment that the effect of wind on the production of particles as distinct from causing the conductors to cla sh was not relevant to those experiments.

Would a filure to reproduce in your experiement a relevant factor that was present in the field be likely to undermine the validity of the experiments or its usefulness?

(Page 1733 follows)

THE WITNESS: It is a hypothetical question you are asking me?

MR. LLOYD: Yes?---Certainly in any model exercise it is possible to ignore a vital factor.

Did you heat the grass?---I think you misunderstood this laboratory experiment; we were clashing conductors and catching particles, we were not trying to burn grass then, it was a separate experiment where we were dropping heated aluminium particles on dry grass. The way that bed was prepared was that the grass was dried in an oven to a very low moisture, it was taken out of that oven immediately, so it was heated, but the main purpose was to dry it.

Do you know what the temperature was when you took it out?---No.

Unmeasured?---Unmeasured.

Of course, the hotter the grass was in the field, the less likely it would be to dissipate by absorption the heat of the molten metal?---Is that right.

You are talking about such high temperature levels - whether it was 20 degrees, 30 or 50 degrees, the difference between those two is normally the same, so I would not agree.

Have you measured experimentally the difference in behaviour of molten metal particles in grasses of temperatures that vary by even small amounts?---No.

Would you say a thing is of a low order of probability, what does that mean?---I am using that in the mathematical sense, but you might use it in the sense of the odds, for example, there is a one in one hundred chance of something happening, that would be a low probability, a one in one thousand chance is probably extremely low.

I think I put this to another witness: if you have a thing that is of an extremely low order of probability, in the sense that it only happens once in one thousand times and if you try it one thousand times, you will get it happening once?--There is a probability of that happening.

We have one million wire spans throughout this State and the probability is the low one, the one in one thousand, that we might on a hot day get one thousand fires, as a matter of probability?---Those terms are purely hypothetical, as a matter of illustration. I would not know enough to suggest any numerical probability to that.

Would you say a probability figure of one in one thousand was extremely low? Mr. Nixon would, but would you?---"Extremely" is a word you use, it is like "several", it means different things at different times. In the context you have used, no, it is not extremely low, but in the context of one thousand likely tosses of a penny not turning up a tail, I would say that is an extremely low probability.

MR. MARKS: There is a difference between saying one in one thousand and saying it is one thousand times one thousand. That is different.

MR. LLOYD: I suppose you could say it is an unlikely thing to happen for a stopped clock to tell the right time, is it not, but, in fact, it does it twice a day, does it not?---I would not accept that as an analogy.

I notice that Dr. Penman, in his report, says the literature has many references to fires starting in this way, to shorting wires; did you make any examination of the literature or did you stick to the experiment?---We did stick to the experiment. We did not have the time to institute a survey of this material.

THE BOARD: I cannot say I have encountered any such literature, but it does not mean that it is not there.

MR. MARKS: He said he was talking about inside buildings, actives and neutrals clashing in installations, in factory sites, in buildings.

THE BOARD: I believe that was right.

MR. LLOYD: His statement says, "the literature also has many references ......grass forests and a peat bog." I agree it is not helpful to try to determine it if he is not here.

Mr. Hart, could you tell me whether, to your knowledge, experiments of this type that you conducted have been conducted anywhere in the world?---Not to my knowledge - when I say not to my knowledge, that simply means I would not be in a position to know.

THE BOARD: Unless someone had done it and published the result?---Even then, I would probably not know about it; this would be published in the journals on firefighting.

Which you would not look at?--That is right.

MR. LLOYD: Does the section of the SEC which you are in have the opportunity to contact its opposite numbers in other States, such as New South Wales?---Yes, we can; we occasionally do on some subjects. I did not on this, I do not know if anyone else did.

THE BOARD: I am still interested in that sentence of Dr. Penman's, "The literature also has many references ...." - what sort of literature is he talking about?---He means technical literature.

The scientific literature?---Yes, it is a phrase used in that sense.

When you come to look at the statistics on causes of fire, which you get from books about fires, that would not be supported?--I could not say.

(THE WITNESS WITHDREW)

MR. NIXON: I propose to tender and read into the transcript the conclusions of a report prepared by Technisearch Limited in association with Mr. Sol Freedman and his Associates. I will read the conclusions into the transcript, rather than the whole report.

It is a report following a conference at which the following persons were present:

Mr. S. Freedman	Consulting Engineer
Mr. W. Sidebottom	Manager - Technisearch
Dr. J. Higgins	Metallurgist
Mr. T. Flavell	Metallurgist
Mr. J. Ellis	Metallurgist S.E.C.
Mr. R. James	Construction Engineer S.E.C.

They had the samples that were available from the fuse at Wallindue and the conclusions take the form of answers to questions which were raised at that preliminary meeting. They read:

1. Q. How far does the top cap of the fuse screw onto a new unit?

   A. For the particular design of fuse in question, it was found from the examination of the Dowling Forest fuse that the cap screws down until it contacted the button of the fuse link. At this point, the threaded portion of the nose is just fully covered.

2. Q. What is the nature of the metallic material used in construction of the fuse nose?

   A. X-ray fluorescence analysis in the scanning electron microscope indicated that the material was a brass, possibly a leaded brass, coated with an electroplated layer of tin. Full analysis would need to be carried out to determine the exact nature of the brass. It was not felt that this was necessary. As indicated earlier, the conclusion that the tin layer was put on by electroplating was deduced from an examination of a new fuse.

   The corrosion products on the fuse in question obscured the original coating and the possibility that the coating was produced by hot dipping cannot be discounted.

3. Q. Was there a top cap ever screwed on this particular fuse?

   A. The evidence suggests that a cap has been missing for a considerable portion of the life of the fuse, if one was ever fitted.

   It cannot be said for certain, however, that a cap has never been present on the fuse.

4. Q. Was a cap present on the day of the fire and, if so, how far was it screwed on?---

   A. The evidence of the corrosion pattern on the fuse in question strongly suggests there was not a cap present on the day of the fire. No obvious fresh score marks were present on the threaded surfaces at the time of inspection.

   It is unlikely that sufficient corrosion would have occurred in the interim period between replacement of the fuse link and removal of the fuse assembly for examination. The exposure trials mentioned earlier in this report should verify this conclusion.

5. Q. Has a cap ever been blown off the threaded end of the fuse nose?-

   A. There is no evidence to suggest that this has occurred. Severe plastic deformation of the threads would occur in such an event. None was observed.

6. Q. If so (5) how far was the cap screwed on?

   A. The answer to question 5 makes this question unnecessary.

7\. Q. Is there any evidence to indicate that a cap was not present when the fuse operated on the day of the fire?

A. An examination of the three fuses indicated that a significant difference between the capped fuses (i.e. Glenfine and Dowling Forest fuses), and the fuse in question was the presence of arcing on the inside surface and top edge of the fuse in question. As already indicated, the opinion of an electrical engineer should be sought in this regard.

8\. Q. Was there any other evidence to indicate whether:

(a) A cap was present on the day of the fire?
(b) A cap was present but had blown off?

A. The answer to both questions is "no".

EXHIBIT 218 ....Conclusions of a Report prepared by Technisearch Ltd. in association with Mr. Sol Freedman & Associates.

THE BOARD: The conclusions will be put in the transcript. It is plain enough, Mr. Marks, that you do not have any objection to that report going in?

MR. MARKS: On the contrary, it was I who offered it. We agreed at the time it was not necessary to call the people. I should point out that the Technisearch people were present when the other tests were done, they have simply added their name, but they were an outside body.

MR. NIXON: I propose to call Mr. Nicholson, and Mr. Wilson will be recalled in relation to surge diverters. I have in my possession details arising from shattered or broken surge diverters and I think Mr. Dunne should have an opportunity to peruse those details over the weekend before cross-examining, so I propose to call Mr. Nicholson and make him the last witness of the day.

(Page 1737 follows)

ERRATA

Page 1706, line 46 - "11 Herra Street, Burwood" should read "11 Parer Street".

" " line 47 - "Third Mechanics Section" should read "Fluid Mechanics Section".

Pages 1707/1708, line 10 - "Three years with the SEC" should read "Seven years with the SEC".

" line 34 - "Barometry" should read "Pyrometry".

" line 35 - "Hot pad" should read "hot object".

Page 1709, line 10 - "Exceptionally difficult" should read "exceptionally dry".

" 1710, line 50 - "Vertical access" should read "vertical axis".

" " line 50 - ".1 millimetre" should read "1 millimetre".

Page 1721 line 4 - "Heat and reaction" should read "Heat of reaction".

" " line 25 - "heat form" should read "heat formation".

" " line 27 - "oxide form" should read "oxide formed".
