INTERNATIONAL CONFERENCE

ON

ELECTRIC FUSES

AND

THEIR APPLICATIONS

Liverpool Polytechnic

7th, 8th and 9th April, 1976.

Transcript of Discussions

SESSION 1. Wednesday, 7th April, 11.00 - 12.15.

PREARCING PHENOMENA

Session Chairman: Dr. A. Wright
(University of Nottingham).

Paper No.

2 "Steady-state thermal phenomena in fuses with variable cross-section", by I. BARBU.

3 "Survey of numerical methods for solving time-varying fuse equations", by P.M. McEWAN and L. WARREN.

4 "A decoupled method for predicting time-current characteristics for h.r.c. fuses", by P.M. McEWAN and R. WILKINS.

5 "The influence of the temperature coefficient (TC) on fuse discrimination", by Y. NAOT.

6 "Deformation and disruption of silver wires", by S. ARAI.

Mr. H.W. Turner

First of all I would like to ask Dr. McEwan for his opinion on the best way of estimating the thermal conductivity of quartz for these purposes. It is well known that tiny amounts of moisture give enormous differences in thermal conductivity. Sand, as used in fuses contains some moisture, and transport of that moisture results in a distortion of the thermal conductivity of the material which must affect the long-time behaviour.

Has Dr. McEwan thought of using his digital techniques in other fields? e.g.: looking at fuses from the other point of view; of attempting to discover the possible variations in time-current characteristics that are theoretically possible by varying the geometry; or possibly using the same techniques to solve the problem of association of fuses with other heat-producing units - cables in series, other thermal devices inside a box and so on - the problem of calculating the temperature rise of a combination of electrical devices either in series or within an enclosure.

On p.48 Prof. Naot gives 'hot' and 'cold' characteristics. In investigations which I have carried out on practical fuses, on the effects of preheating, differences of this magnitude, below 1s, I don't generally find in industrial l.v. fuses. For example in this region an operation of M-effect, if it exists, or a dual-element operation if it exists, is not activated, and that means, therefore, that you are on the region where the element is melting at the constriction. This is at a much higher temperature, and thus the small heating that resulted from the element running at its rated current is insufficient to make appreciable difference to the performance in the short-circuit area. If you calculate the temperature of the constriction, typically 100 - 200°C, and you allow for the reduction in I^2t to produce that small rise in temperature, it makes little difference to the prearcing I^2t of the fuse. Is Prof. Naot considering a plain single element fuse, where the effects of preheating are much greater, because if for example it is a silver wire fuse it is going to melt at 960°C or thereabouts no matter what range of current. But if it has an M-effect then it's going to be around 200°C that that spot is going to melt whereas the element will melt at 960°C in the short-circuit region.

I would be most grateful if Mr. Arai could elucidate further on the arc jets which he shows in his photographs on p.56 and I would be interested in his comments on the final appearance of the X-ray of the fulgurite in the location of these jets.

Dr. R. Wilkins

I have a brief comment on Dr. Barbu's paper, and wish to clarify the differences between numerical methods of solution and analytical methods. In our paper we have concentrated upon numerical methods because these are the only methods which we can use to solve the complicated system of partial differential and ordinary differential equations which represent the prearcing behaviour of an electric fuse. Numerical methods are necessary to represent all the complex geometries and non-linearities involved. However numerical methods do have one disadvantage, and that is that it takes a long time to get results for the very large problem which we get when we represent a fuse. It would be nice if we could have an analytical solution, but this is a mammoth task, and I would like to congratulate Dr. Barbu on his efforts in this direction. Dr. Barbu obtains an analytical expression which is so complicated that he needs a computer to evaluate the expression, but it is important to realise that this is a fundamentally different use of the computer from that described in our paper.

Dr. K. Lerstrup

I found it very interesting that Prof. Naot finds some benefit in a negative temperature-coefficient. However if we try to employ that we should lose the very great benefit we have from the positive temperature-coefficient. Normally if something heats up we have the simple exponential rise of temperature to a steady-state value. But with a positive temperature-coefficient we can get to the point where the increase in resistance just keeps up with the higher cooling and then we have a linear increase in temperature with time. If we go to a little higher current, then we can obtain an exponential runaway of temperature - and this is the great advantage of our fuses.

The ordinary operation of a fuse, just a little below the limiting current will be at a temperature so much lower that it really does not matter very much. The so-called 'hot' curves thus have very little significance in practical work. However, the 'avalanche' effect of the positive temperature-coefficient gives very decisive operation, even if we neglect the M-effect.

For an ordinary silver fuse it only requires a current of about 1.3 times the m.f.c. to get a 'straight-line' increase in temperature. Any current greater than this will produce a positive-exponential effect and give very decisive operation. This is what we live on, and if we lose it I don't believe we will have a fuse any more.

Prof. T. Lipski.

Among many very important papers, for me the most important is that of Mr. Arai. From Fig.5 of his paper, I got the current density at the moment of wire disruption to be 14 kA/mm^2 while from Fig.6 the values were 4.8 kA/mm^2, 10 kA/mm^2 and 12 kA/mm^2. We may conclude that these values of current density are in good agreement with those which we can get from Mr. Nasilowski's paper in the region which corresponds to the striated formations, (not unduloids). Does Mr. Arai agree that this is the striated region? - in my opinion it is.

Could Mr. Arai give us some more detail concerning the model of the striated disintegration mechanism? We are interested to learn whether figures we have obtained in Poland agree with those obtained by Mr. Arai.

My last question to Mr. Arai is the same as that put by Mr. Turner.

Dr. J.G. Leach

I would first of all like to make some comments on the two papers by Dr. McEwan, since my own work is referred to. Concerning the choice of the best numerical method for analysing the heat-flow within a fuse, I didn't spend a vast amount of time deciding which method was the best and quickest, I spent the time producing results, which were useful to me. Mr. Turner has asked whether other uses of the program had been made - this is what I was concerned with, the program was used as a tool to analyse fuses and so to produce better fuses. I have in fact published work on the comparison of a fuse connected to a cable, and the effect of the size of the cable on the current rating of a semi-conductor fuse. It is in these sorts of areas where the numerical method is extremely useful, as well as producing an understanding of how the fuse operates.

Concerning the decoupled method described in the second paper, I would like to confirm that this method is very useful - I have been using it for about four years - for times up to about 10s, depending upon the size of the fuse.

I found Prof. Naot's paper very interesting, even though it is a little controversial. It is very important at conferences like this for something new to be introduced, and this is certainly something new. Having prefaced my comments with those words I now have to stand in the fuse manufacturers position, and I am at odds with Prof. Naot's conclusion that a large number of cases occur where discrimination is unsatisfactory because of preloading. I think that the form of analysis used by Prof. Naot has led him to this conclusion. He began with a rather simplified treatment of the heat flow (which is always necessary if analytical methods are used) and took a very theoretical fuse with no M-effect. This means that at rated current the element temperature can be very high - I worked out that on Prof. Naot's assumptions an element with a melting temperature of 1000°C should be expected normally to run at about 600°C. Obviously this is what leads to the discrimination problem, because if the large fuse is running at 600°C and the small fuse is running cold it takes the small fuse a long time to 'catch up' with the large fuse. In practice fuses do not run with such high element temperatures, and the presence of the M-effect will completely alter the picture as far as discrimination is concerned.

Mr. E. Jacks

I would like to make one or two general remarks. I applaud the motivations behind these papers, which I recognise as being largely academic papers, but I do not entirely appreciate the motivation since the authors have not stated it clearly in the introduction. I hope that in the subsequent discussion the authors will answer the questions: Why have they done this work? What was their motivation? McEwan and Warren say that it would be a good thing to predict time-current characteristics without having to do a lot of expensive testing. Of course I agree with them and I think that they have gone a long way towards achieving that objective. But I think that they have'nt gone the whole way and it should be stated in proper context exactly what they are trying to do. Do they for instance want to calculate the time-current characteristics of existing fuses, or do they wish to assist the development of new designs?

Similarly with the paper by Dr. Barbu. When the specialised academic exercise gets down to such narrow parameters as chosen by Dr. Barbu , he should be able to explain, in the context of fuse technology, just where he is hoping to make a useful contribution. After all, fuses are not produced as an academic exercise, they are produced for a job of work in the world outside.

I agree with Dr. Lerstrup and Dr. Leach concerning the paper of Prof. Naot, in that failures of discrimination are due to external forces which he has not investigated. Nevertheless the ideas that he propounds are stimulating and should be taken seriously. I haven't studied this paper in sufficient detail to be able to comment other than in general terms, so I would return to my original plea - would the authors please, for the benefit of us ordinary fuse engineers, who learned our mathematics so long ago that we have now forgotten them, put these papers into context so that we may know how these papers are useful and what contribution to practical fuse technology is intended.

Mr. R. Oliver

Following on from what Mr. Jacks has said, we have seen in the papers that numerical techniques do yield accurate time-current curves out to about 10s and I would like to ask the authors of the papers on numerical methods what they foresee as regards the analysis of pulsed-loading conditions. This requires the calculation of temperature profiles for pulsed loads which may range from milliseconds to many minutes. Can numerical methods be used for this purpose? What experimental techniques could be used for verifying that the calculated temperatures are in fact the true ones, bearing in mind that these temperatures would be well below the melting point.

Mr. J.W. Gibson

Have the authors verified any of their results experimentally, e.g. by the use of temperature-indicating paints, which nowadays are far more accurate than they used to be?

Mr. J. Feenan

To what extent have the authors of the papers on numerical methods considered the question of mechanical fatigue? We know that on pulsed testing, the effect on the time-current characteristic is distinctly different from that of one-shot testing. This is a problem of interest to many people in this room, and I would ask for the authors' observations on this.

Dr. P.M. McEwan (in reply)

I would like to answer Mr. Jacks' questions first of all, as these are at the root of the work. Apart from the interest in academic studies, the reason for developing numerical methods was that we foresaw that these could be an aid to the fuse designer, because numerical methods permit the possibility of varying the parameters of the fuse and seeing how the fuse performance changes. For instance, one can examine any sort of element shape, and see its influence on the time-current characteristic. Also one can vary filler, length of fuse, etc., so this does give the designer an opportunity to see how the fuse performs before he goes on to the shop floor to get it constructed, and then to have it tested. I think that that is clearly an aid.

I detected, maybe wrongly, that Mr. Jacks thought that we were trying to replace existing testing of fuses, by predicting the time-current performance numerically and saying let's do away with traditional tests. If this be so then it is not the case.

Mr. Turner refers to the variation of the thermal conductivity of the filler with moisture content, compacting and temperature. I agree that the thermal conductivity does vary with changes in these parameters; however, I think that his remarks are particularly important for loose fillers, soils, etc. In the fuse we have a different proposition. We have a compacted filler. There is admittedly some moisture content initially (I think 7% is a typical figure), but after the fuse has been made up, tested, and possibly used, then I doubt whether there is anything like that amount of moisture present.

My view on the measurement of thermal conductivity is that principally we are concerned with the performance of the filler in fuses, so we must derive values of thermal conductivity for the filler actually in the fuse. I have developed a method for obtaining the thermal conductivity of the filler numerically. The method is based upon wires in filler - but I would like to leave this topic to some future discussion.

Another point raised by Mr. Turner was the extent of the use of fuse numerical models. I would say on this point that we do not understand the thermal behaviour of the fuse-diode combination and cannot predict its performance. We also lack coordination experience with fuse/thyristor combinations in regard to thermal performance. Numerical methods offer potential for predicting this thermal behaviour because I envisage that it is not beyond the power of the methods we now have to simulate the thyristor and diode in the same way as the fuse. The two can be connected together numerically, the whole performance simulated, and coordination rules obtained.

Regarding Mr. Oliver's comments on pulsed loading, these programs offer similar potential for such solutions also. We can alter the sorts of input currents to the fuse, and see how it performs. He also asked whether we have any experimental results to justify the predictions of temperature along the element. Mr. Gibson also asked this, and whether temperature-indicating paints had been used. I have not used these paints although I know of them; they have been used for quite a time, but I don't think that they are appropriate for the temperatures which we are concerned with because the notch temperature is really the important value to consider and there are great difficulties in obtaining this accurately using indicating paints.

My view is that the important aim in fuse studies is to predict how the fuse operates. It is interesting to know the temperature values and it is also very nice to have the temperature distribution, but we are principally concerned with predicting how it operates, and this has been our main concern. So I don't judge this as being too important - i.e. to actually know the temperature values within the fuse. The final steady-state temperatures we do wish to know, but this requires a separate analysis. Dr. Leach and myself have such programs.

<u>Prof. Y. Naot</u> (in reply)

Before I answer specific questions, may I give some general comments which may answer many of the questions which have been asked.

My paper is intended first of all to clarify some general concepts, and that is why I considered a fuse with constant cross-section, and used many simplifying assumptions.

In reply to Mr. Turner I think he is right in that, for very high currents (very short times), the difference between hot and cold characteristics becomes less important. The difference is far more important for low overcurrents.

I agree with Dr. Lerstrup that we lose one important advantage of the positive temperature-coefficient. Nevertheless I disagree with him in this way. If we go into the runaway period with 30% more than the rated current, it means that the rated current has to be at least 30% less than the theoretical current which melts the fuse. That will give one very important consequence. The deviation factor of the fuse is smaller, the lower the current, but with long melting times the difference between hot and cold characteristics is much higher and this will give a smaller probability of selective action. So on the one we win something, but on the other hand we lose the possibility of having the two characteristics close together. My intent was not to say that we must throw away positive-coefficient fuses, and use only negative temperature-coefficient fuses. Rather it was to say that besides the positive temperature-coefficient fuses, it would be useful to have also negative-temperature-coefficient fuses. My challenge to physical chemists to produce such a material was more or less an academic challenge, but this does not mean that I do not have my ideas on how to solve this problem. I did not put my ideas in the paper because I do not yet have the experimental results.

Mr. S. Arai (in reply)

The first question was about the disintegration photographs on the right of page 56 (e). I believe that the photographs show jets, as suggested by Mr. Turner.

As regards the relation between the striation of fulgurite and the deformation, the deformation is very fine, (short-pitched) but after arcing has continued for some time the arcs unite and then you can see striations on the fulgurite. The striation pitch is then larger than the initial disintegration.

In reply to Prof. Lipski, I am not sure but I believe that the striations correspond to those of Mr. Nasilowski, and they are due to small disfigurations which merge into one, and then sand striation appears.

In reply to Mr. Jacks, we believe that our experiment is not directly connected with fuse design, but the fuse designer may find some hint from our experiments.

Dr. R. Wilkins (in reply)

I would like to reply to Mr. Feenan's question concerning mechanical fatigue, which I believe is tied up with the issues raised by Mr. Jacks. The answer is that we cannot at present simulate mechanical fatigue, but the techniques developed will enable us to proceed in that direction if this is desired. In order to make reasonable predictions of mechanical fatigue it is essential to know the transient temperature distribution along the fuse-element, during any cyclic loading condition.

The numerical methods which have been developed can be used for a variety of purposes. To put them to practical use requires close collaboration between those who Mr. Jacks calls academics and those involved in fuse manufacture. The object of the work as far as we were concerned was to find out how far we could go in the prediction of temperature distributions with practical fuses. Prediction of the pre-arcing behaviour is also a prerequisite for the simulation of the total performance of a fuse. If you want to simulate a complete interruption you must begin with the prearcing period. We believe that we can now do that fairly accurately, and it seems logical that the next step should be to add a simulation of the arcing process. We would then have at our disposal techniques for simulating the complete operation of a fuse, and this would not seem to me to be a bad thing.

The use to which such a program would be put is a matter which must be decided by discussions between academics and manufacturers.

SESSION 2. Wednesday, 7th April, 14.00 - 15.15.

DISRUPTION AND ARCING PHENOMENA

Session Chairman: Dr. K. Lerstrup
(LK-NES, Denmark).

Paper No.

7 "Overvoltages produced by fuses", by J. PAUKERT.

8 "The calculation of overvoltage characteristics of HRC fuses", by J. HIBNER.

9 "The role of fuse filler in circuit protection", by H.W. TURNER and C. TURNER.

10 "Arcing phenomena in HRC fuses under varying test conditions", by P. ROSEN.

Mr. J.W. Gibson

I would like to ask the authors of the first two papers whether they have any experience of the following phenomenon.

If you have a fuse with short and deep recesses the rate of increase of the arc voltage depends upon the rate of burning back of the element. That rate, as can be shown both experimentally and by calculation, is dependent upon the product of the current density and the sum of the anode and cathode drops, for all the arcs in series. However, when the current density is very high, another phenomenon can come into the picture, i.e. that before arc extinction has been completed, the unmelted parts of the silver are melted, not by burning back, but by the I^2R in the silver itself. This is because the rate of burning back is proportional to current, and the rate of heating is proportional to the square of the current. The result of this is that you are likely to get a sudden increase in the arc voltage. Could the authors of the papers on arc voltage quantify those results?

Concerning the paper by the Turners on fuse filler, what alternative fillers can they suggest? I remember that on one occasion, I went to the library and got a lot of physical tables, to find the ideal fuse filler. I thought it should be one with a high thermal conductivity, because I thought that would abstract heat better, and so increase the arc voltage. In addition it should have a high specific heat, a high latent heat of fusion and a high melting point. Having found such a material I tried it and found that it was no use, possibly because it contained impurities. Can the authors suggest their ideal material? Quartz was used in the beginning because you use sand to put out fires, but are we reasonable in continuing to use it? Crystalline quartz has one disadvantage, that its expansion with temperature occurs in nasty jumps and you may reach a condition where the barrel will burst. That trouble can largely be overcome for steady currents by using the M-effect, but it doesn't follow that the effect does not occur when the fuse operates under heavy fault conditions and transmits heat from the fulgurite to the unfused part of the filler. Do the authors have any comments on this?

As regards Mr. Rosen's work, my question refers to the rather unusual patterns of the places where melting occurs in the transition region. Does the author think that this can be due to fortuitous very small inaccuracies in manufacturing I have seen this effect myself and have only been able to explain it on that supposition.

Can the author also comment upon the fact that when a fuse operates on a small overcurrent, the last element to clear shows arcing at many more restrictions than was evident for the first element to melt. In other words, the fuse then seems to be operating in a current-limiting mode, as can often be seen on an oscillogram, where the final loop of arcing is accompanied by an overvoltage which is of the same order as that obtained at higher currents. Can the author say how this phenomenon is related to the number of elements in parallel?

Mr. W.R. Crooks

Referring to the Turners' paper I would request an expansion of a remark made on page 84, that '....... studying arc behaviour by means of transparent plates does not give a true picture of behaviour in a porcelain barrel.....'. In particular, would they suggest that if allowance is made for the reduced ability to absorb arc energy since the element is now a one-sided one as far as the energy exchange process is concerned, the observed behaviour is representative of the two-sided behaviour of elements in the practical case?

The Chairman in his opening remarks, suggested that the rated voltage of a fuse is the highest that the sales peoples think they can get away with. If I could add to that one the condition that the fuse then meets the type tests as laid down in the appropriate specification it is probably a good place to start.

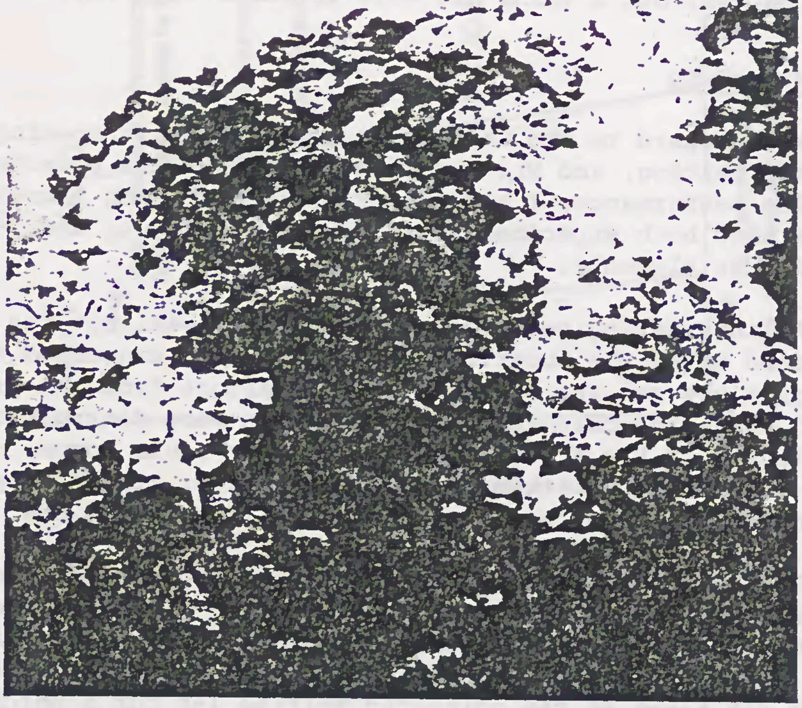
I would like to ask Mr. Rosen to expand upon his reasons for using a rather larger than normal spacing for the restrictions, to prevent the merging of arcs. I would have thought that merging of the arcs is an important aspect of the behaviour, from a practical view-point. To have an element which does not allow the merging of arcs is uneconomical in design. On his conclusion 6.2, I question that the prearcing time is the significant parameter. Surely the behaviour is dependent upon current and voltage, the voltage in turn being dependent upon the properties of the arc path. The prearcing time is a dependent variable, related to the current.

On the film showed by Mr. Rosen, I noticed that at low current levels, on two or three occasions an arc burned on one restriction for perhaps 3 or 4 loops and was then followed by the establishment of 2 or 3 more arcs in a very short time. My question is - why the very large difference in time between the first restriction melting and the subsequent ones?

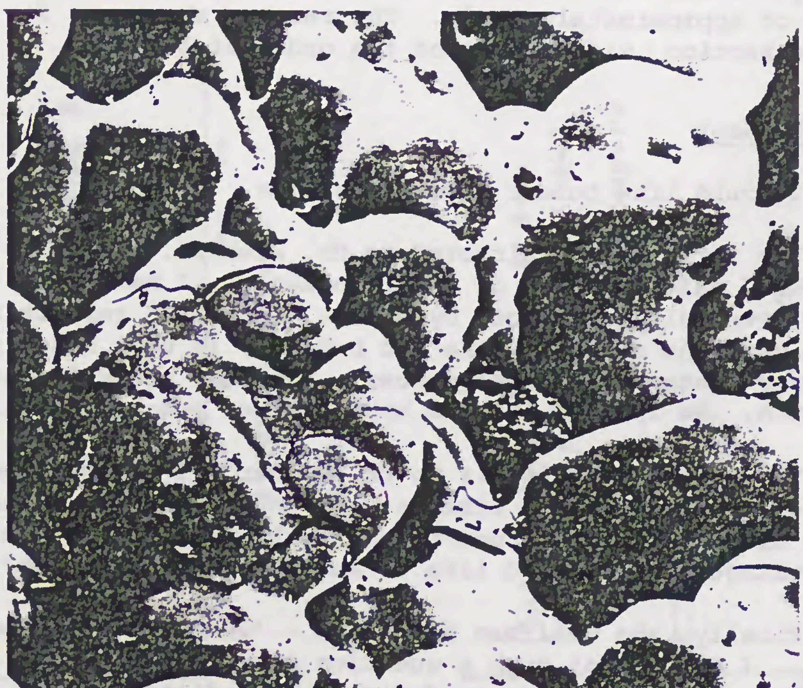
Dr. R. Wilkins

I can't understand Fig.4 in the Turners' paper; what is the time-scale on this figure, which appears to show an oscillating behaviour of fulgurite resistance?

I would like to show some slides which illustrate some of the points made in the paper. These are pictures of fulgurite from a high-voltage fuse, taken on a scanning electron microscope by Dr. J.K. Critchley of Brunel University. The difference in contrast is due to a difference in electrical conductivity, not colour. Non-conducting regions become charged under the microscope and appear bright on the image, so the dark regions are regions where silver has been deposited on the surface of the quartz particles.



Exterior surface, X25.



Interior surface, X100.

Mr. K. Lerstrup

In connection with the pictures shown of the fulgurite I would like to mention the work of Dr. Huhn, who has considered the formation of the fulgurites, the evaporation of the silver, coming out and, more or less, blocking, and starting melting, thereby making an enclosure, in which we get an increase of pressure. Dr. Huhn's name is not mentioned in the reference to the papers, but I think his work is worth studying.

Dr. P.M. McEwan

With regard to the Chairman's remarks in his opening address on adiabatic melting, and Mr. Turner's paper upon the effects of filler on prearcing performance, I have several figures which I would like to show. The figures show both experimental and calculated $I : t_m$ and I^2t results for notched fuse elements.

An interesting point to note from Fig.1 is that the computed points were determined using the numerical model which was discussed this morning. From the figure one can see a significant departure from the experimental results after 0.03s. The calculated results shown were determined with the thermal conductivity of the filler assumed zero, in other words, neglecting the effect of the filler, thus I think sand filler starts to play a significant role in fuse melting for prearcing times exceeding 0.03s.

On the question of adiabatic melting of notched elements it may be shown using a numerical model that adiabatic melting can only occur in very short melting times. Fig.2 shows calculated values for different switching angles from which it can be seen that as prospective currents increase the melting I^2t tends to the adiabatic melting I^2t for a notched element, as one would expect.

Plotting the same results against melting time, (350 is the adiabatic melting I^2t for this element), it can be seen that adiabatic melting occurs in a time of approximately 10^{-4}s. The results shown are for an element with a reduced section : width ratio of the order of 1 : 4.8.

Prof. Y. Naot

I would like to ask three questions.

The first one is directed to Mr. Paukert. It seems to me that you considered only one part of the phenomenon. I have many times observed that by the over-voltage induced by a fuse there is a restriking of a new arc, not only in the region of the arc but also in some other part of the plant with very destructive consequences. This new arc will change your calculation very much. So I would like to know whether you have considered this.

The second question is to Dr. Hibner. I enjoyed reading your paper but I could not find the link between your calculations and the inductance of the circuit. This is one of the most important parameters which induce over-voltages, so I would like to know where is the link?

Finally, the Chairman has asked - 'what is the rated voltage of a fuse?'. I think that such a question cannot be answered in a simple sentence. You first have to define the condition of the circuit which has to be protected. Then you can define the rated voltage as that voltage at which it can interrupt in 99% of cases.

Dr. P.M. McEwan

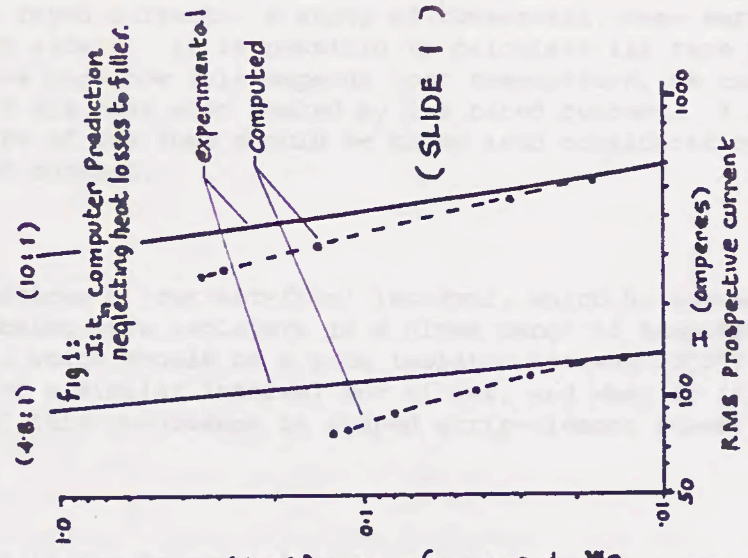


fig 1: I:t_m Computer Prediction neglecting heat losses to filler.
(4.8:1)
(10:1)
Experimental
Computed
(SLIDE 1)
t_m pre-arcing time (secs)
1.0
0.1
.01
50
100
1000
I (amperes)
RMS Prospective current

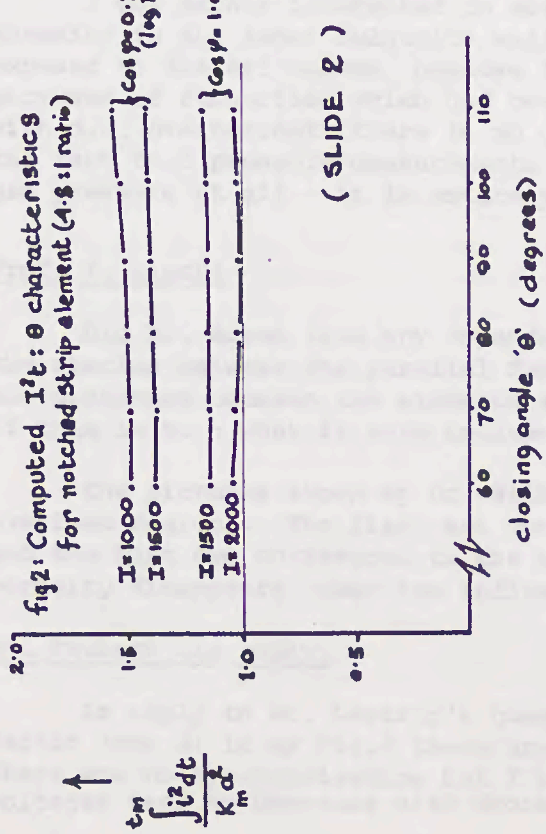


fig2: Computed I²t : θ characteristics for notched strip element (4.8:1 ratio)
I=1000
I=1500
I=1500
I=2000
Cosφ=0.3 (lag)
Cosφ=1.0
(SLIDE 2)
2.0
1.5
1.0
0.5
60
70
80
90
100
110
closing angle 'θ' (degrees)

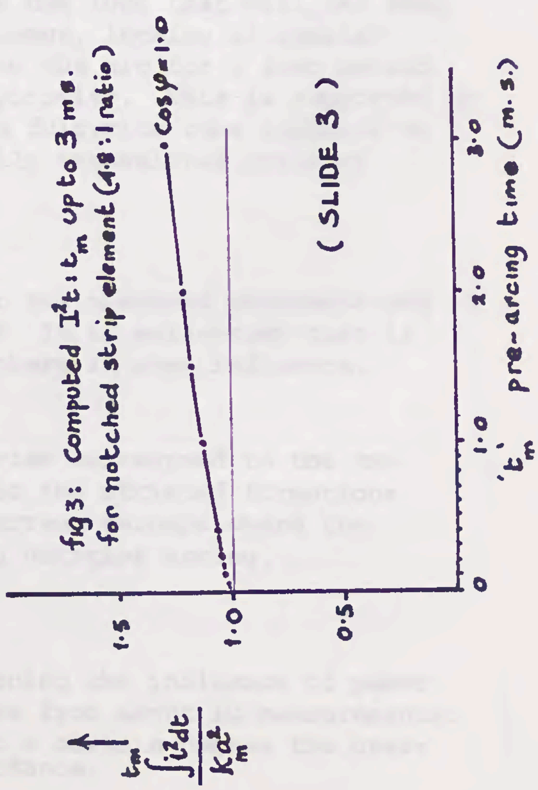


fig3: Computed I²t : t_m up to 3 ms. for notched strip element (4.8:1 ratio)
Cosφ=1.0
(SLIDE 3)
1.5
1.0
0.5
0
1.0
2.0
3.0
t'_m pre-arcing time (m.s.)

[illegible]

In connection with the [illegible] paper of [illegible] I would like to mention the work of Mr. [illegible], who has investigated the [illegible] of the [illegible] [illegible]

[illegible]

[illegible] figures [illegible] experimental and calculated [illegible] results for [illegible]

[illegible] the computed points were determined [illegible] from the experimental results [illegible] thermal conductivity of the [illegible] effects of the [illegible] a significant [illegible]

On the question of [illegible] it may be shown [illegible] only occur in very [illegible] different welding [illegible]

Plotting the same results against [illegible] welding [illegible] it can be seen that [illegible] welding occurs [illegible] a time of [illegible] with a [illegible]

Prof. [illegible]

[illegible] would like to [illegible] questions.

The first [illegible] to Mr. [illegible] considered [illegible] many [illegible] [illegible] the lower part of the [illegible]

The second [illegible] is to Mr. [illegible] [illegible] would like to [illegible]

Finally [illegible]

But I wonder why you did not ask another question - 'What is the rated current of a fuse?' - this is a far more intriguing question. The only thing you know, scientifically speaking (if you look at the rules of IEC it is very simple to define the rated current), but scientifically speaking, all we know is that it must be less than the minimum fusing current. But how much less? I am not able to give you formula, but I am able to suggest a way to determine the rated current. A strip of fuse-metal, when very hot, evaporates very, very slowly. It is possible to calculate its rate of evaporation, and if we know how this depends upon temperature, we can calculate the life of the fuse when heated by its rated current. I suggest that the expected life of the fuse should be taken into consideration when determining the rated current.

Mr. O. Norhølm

Mr. Paukert mentions a 'current-free' interval, which he assumes is caused by the metal vapour being pure isolators in a given range of temperature. He mentions only copper, which should be a pure isolator between 2300°C and 3500°C. Is the author aware of a similar interval for silver, and what is it? Has he ever seen evidence of this phenomenon in shaped strip-element fuses?

Mr. R. Oliver

I am interested in the 'thermite' effect with aluminium mentioned in the Turners' paper. I've heard about it before, but seen nothing quantitative. It sounds a rather dangerous process to me, with respect to aluminium-element fuses.

Concerning the transmission of mechanical pressure by sand, all the evidence I have found on the transmission from the fulgurite (arc) column out to the body wall indicates that the mechanical thrust is transmitted directly via the sand, perhaps with some preset involved, where the grains slide and take up a set position, but thereafter there is a direct relationship between the arc column pressure within the fulgurite tube and the pressure detected at the body wall which can be largely accounted for by the geometry of the fulgurite and the ceramic tube.

I was rather interested to see that in Dr. Wilkins' photographs there was porosity in the inner fulgurite wall. I wonder how long that wall had been exposed to the arc column, because in my experience, looking at similar pictures of fulgurites which had been exposed to the arc for a long period with d.c. overcurrents there is no detectable porosity. This is supported by the fact that pressure measurements outside the fulgurite tube indicate no gas pressure at all - it is entirely mechanically transmitted pressure.

Prof. T. Lipski

Did Mr. Rosen find any dependence between the observed phenomena and the spacing between the parallel fuse elements? It is well-known that if the distances between the elements are small, there is some influence. If this is so - what is this influence?

The pictures shown by Dr. Wilkins in my view correspond to the two overload regions. The first set corresponded to the striated formations and the last two correspond to the small overcurrent perhaps where the porosity disappears under the influence of long duration arcing.

Mr. Paukert (in reply)

In reply to Dr. Lertrup's question concerning the influence of power factor (cos ø) in my Fig.8 these are mean values from about 10 measurements. There was no synchronisation but I hope that to a certain degree the over-voltages tend to decrease with decreasing inductance.

In reply to Mr. Gibson the answer is that I have not made such experiments, to check the speed of burning up of the fuse element. However, in Czechoslovakia current-limiting fuses are used which have the cross-section of the restriction about 10% of the cross-section of the whole, and we have not observed any excessive burning up of the fuse-element.

I think that Prof. Naot meant that if a fuse is correctly constructed, restriking cannot occur. It is known that the filler is influenced to a thickness of about 2 mm, and if the fuse is constructed so that the elements cannot go nearer than 2 mm to the wall, restriking cannot occur.

To the question of Mr. Norhølm, I think that the current-free interval exists with silver too, but I cannot give any exact range. To my knowledge, such phenomena have not yet been observed in fuses.

Prof. T. Lipski (in reply for Dr. Hibner)

The effects of the arc burning back after initial arc ignition does not appear in Dr. Hibner's paper. The method is concerned only with the moment of arc ignition. That is one point, but the main point is the fuses considered by Dr. Hibner have long notches, where arcs are initiated along the whole length. You may have only one strip or one wire, of uniform cross-section. This method is not a universal method; it is not possible to use this method for instance for the notches which we have in semi-conductor fuses.

Coming to Prof. Naot's question concerning the link between the inductance of the circuit and the overvoltage. There is such a link. Generally speaking, this method is valid only when $\varepsilon > 25$ W-s/mm^3 (equation (2) on page 72), i.e. the electro-magnetic energy stored in the circuit should be greater than 25 W-s per cubic millimeter of element. That is the relationship between the magnetic energy and the design. In transformed form, you can see the same value on page 74 for the d.c. characteristic and on page 73 for the a.c. characteristic.

Dr. C. Turner (in reply)

In reply to Mr. Gibson, we would all like to know whether there was another, more suitable filler. The only thing we can say is that up to now people have looked at natural fillers and there is quite a possibility that there would be some synthetic material or mixture of materials that would combine all the properties that Mr. Gibson announced, also including one that is very important, namely, the post-arc conductivity of the material, which should be good. If we had found such a material we would have patented it,but we haven't found it yet.

On Mr. Crooks' question, that a one-sided element could not correctly represent a cylindrical barrel, Mr. Rosen has shown that a flat geometry can give useful information on what a fuse can do. However, you have to take a bit of care, especially if you are talking about low-voltage fuses, because if you do take photographs with a glass plate, the chances are that your glass plate will crack and you will lose all the data as far as the arcing period is concerned. Of course, the prearcing period is all right if you take into account that it's one-sided.

Dr. Wilkins asked about our Fig.4. The test arrangement is shown in Fig.5, and since this is an a.c. measurement the change measured is not a change in resistance but a change in the current in the high resistance circuit, which is simply 50 Hz, so that gives you the time scale.

I found the slides from the scanning electron microscope very interesting. Of course you can see the same kind of striped layers if you take X-rays.

Prof. Naot asked 'what is the rated current of a fuse'. This is a very interesting philosophical question, which has been debated quite seriously in IEC, especially in regard to miniature fuses, which are widely distributed throughout the world and are exchanged over borders between countries because they are built in to equipment. In fact the question is very serious because of the differences in philosophy that there are in rating. The Americans believe that the rated current of a fuse is the current that it can carry for a certain period, while the Europeans believe that a fuse should carry its rated current for ever. There is the whole problem. Of course it's true that it should be able to carry its current at least as long as the equipment that it sits in.

In reply to Mr. Oliver, the thermite effect is simply a chemical reaction between the aluminium and SiO_2 which gives aluminium oxide and an exothermic reaction.

Mr. P. Rosen (in reply)

I would like to answer Mr. Jacks' general question - 'what is it all for?' As a design engineer I have very little time to exercise my intellectual curiosity, things have to be for a purpose. What I have been trying to do is to build up eventually a complete picture of how commutation works between parallel elements, so that in trying to optimise, particularly high-voltage fuses, in terms of the right number of elements, the right number of notches and the spaces between them, I will have some more of the answers.

Mr. Gibson asked about the 'patterning' effect, i.e. that you generally saw some of the arcs igniting before others. This is because I used a pattern of notches along the element, some long, and some short. Of course the long notches always started to arc first.

Mr. Crooks asked why I had deliberately kept the reduced sections far apart. This was because I was trying particularly to study commutation, and I realised that if I had the notches the normal distance apart, and merging did occur it would rather foul up the thing I was looking for. The next phase of the work which I hope to do is to bring the notches to the correct sort of distance and study how merging affects the commutation effects which we have seen. The central notch seemed to arc for longer than the others because on the low overcurrent tests we had a spot of M-effect there, so the arc began to burn there before the other places broke in the elements.

With regard to the question about randomness and manufacturing tolerance on elements, no attempt was made to try and match the elements in these tests, they were normal production fuse-elements with a tolerance of about 5% on resistance. We did about 60 tests and hoped to randomize the results that we got. As we were trying to see that happened in production fuses rather than in academic test-boxes I thought that this was the right sort of thing to do.

One other point about the position of the elements. We did initially have them buried beneath the surface of the cell, with about 2 - 3 mm of sand between the face of the element and the glass. We then tried moving them right up to the face of the glass and found no perceptible difference. The fulgurite barrel, instead of being around the element simply dropped,as it were, below the face of the element - we still got the same size of fulgurite barrel, but this time buried into the sand.

As regards Prof. Lipski's question concerning the distance between the elements - they were mounted about one inch apart. I have not tried to find what happens if you move them closer together.

SESSION 3. Wednesday, 7th April, 15.45 - 17.00

ARCING PHENOMENA

Session Chairman: Mr. H.W. Turner
(Electrical Research Association).

Paper No.

11 "The behaviour of d.c. overcurrent arcs in fuses", by R. OLIVER.

12 "Pressure in enclosed fuses", by M.R. BARRAULT.

13 "Spectroscopic observations of arcs in current - limiting fuse through sand", by T. CHIKATA, Y.UEDA, Y. MURAI and T. MIYAMOTO.

14 "Chain of arcs as determining factor in electrical explosion of wires", by J. NASILOWSKI.

15 "Optical observations of ultra high pressure sodium arc in the permanent power fuse", by H. SASAO, T. MORI, Y. UEDA and T. MIYAMOTO.

Dr. L. Vermij

A small question to the paper of Mr. Chikata et al. From Fig.6 of the paper we can see that temperatures have been obtained experimentally which range from 20,000^{o}K to 30,000^{o}K roughly, and these are seen a fraction of a millisecond after the start of arcing. Since this is so close to the beginning of arcing, have the authors some indication of the temperature at which the evaporation of the metal has taken place? I have brought forward this question because as you know, the temperature of evaporation is estimated by several authors in the order of magnitude of 7,000^{o}K - 10,000^{o}K. There is a large difference between these evaporation temperatures and the temperatures shown in Fig.6, a fraction of a millisecond afterwards, - a difference of 10 - 20,000 degrees. What is the reason for this large difference - is there no local thermodynamic equilibrium? One of your assumptions is that there is local thermodynamic equilibrium. Therefore I ask the question 'what is the evaporation temperature'?

In connection with the discussion earlier, there is no evaporation at normal operating temperature. Evaporation occurs at temperatures of at least 7,000^{o}K. At 7,000^{o}K the metal vapour is partly ionised.

My second question is on the paper by Mr. Sasao et al. On page 136 you see '...... under the assumption that the plasma is an ideal gas of the temperature 2,000 - 5,000^{o}K.... ', and they conclude that the pressure should be less than 10^4 atmospheres. What is the value of such an assumption when the temperature is far beyond the critical temperature of such a plasma?

Dr. C. Turner

On page 104 Mr. Oliver states: 'It has been suggested that the behaviour of the fuse arc may be modelled by assuming that the mass of electrode metal eroded is directly proportional to the electrical charge passing through the arc column'. We have proved over and over again that this is not the case. In fact, looking at arcs on contacts, which are very similar to arcs in a fuse, we have shown very clearly that there is a relation between the current and erosion which is proportional to a power of the current, and directly proportional to the arcing time. The power of the current is at least 1.6. So it isn't Coulombs in the arc that are mainly responsible for the erosion.

The other thing is the sudden change in the behaviour of the arc, where you get sudden drops in arc voltage. Again, in our studies on arcs on contacts, we have shown by high-speed photography quite clearly that if you have an arc between two points you often get the arc bowing out, and then the arc will short-circuit this bowing-out point, so that you get a change because of the shortening of the arc distances. In the same way, if you have an element in a fuse you can have a shorter path somewhere which is bridged by the arc which always tries to find the shortest distance, and then you get a lowering of the arc voltage.

Prof. T. Lipski

I would like to give some further comments concerning the question raised by Dr. Turner. It is very interesting to note that in Mr. Oliver's equation the last member is not so good - the 'Coulombs' member. I agree with the author's remarks that in fact it is not so. In Poland Mr. Ossowicki found that in the overload region there appears an additional member, which has the form which is proportional to the I^2t value of the current. But we don't know yet the physical reason for the appearance of this additional member.

Dr. K. Lerstrup

I would like to follow up this question of the burning away of the metal. It is quite clear that if we take a constant anode and cathode drop, we will have a release of energy which is proportional to the current. However the erosion is somewhat higher. We also have the arc energy in the plasma, which is given off by the plasma itself; and a good deal of that is transmitted by radiation. There is the possibility that we have an additional energy released at the metal surface by radiation from the arc plasma. This may explain some of the developments, although in many practical cases it is sufficient to figure it proportional to the current.

Mr. H.W. Turner

Dr. Lerstrup has omitted one very important point. That is the concentration of current in the spot at the base of the arc, where the current density is considerably greater than elsewhere; where you have a super-critical region beneath the spot and consequently heating in the metal which is proportional to a power 1.6 of the arc current which is responsible for causing this jet of metal to appear from the surface.

Dr. D.R. Aubrey

On the same topic, of erosion, although I am not a physicist, and knowing nothing of the erosion which takes place in fuses, Dr. Turner has referred to the erosion which takes place with contacts. I have experience of large-scale short-circuit tests on some half-a-dozen bulk-oil circuit breakers. This resulted in many hundreds of oscillograms which were not perused in pedantic detail but averages were used where possible, and the erosion appeared to be a function of I to the power x times the time, where x would vary between 1.1 and almost verging on 2 with one circuit breaker, the others being in the region 1.5 - 1.6. But I can say that one of them appeared to be an outstanding circuit breaker and the value of x there was close to 1.1, however for another outstanding circuit breaker the value was almost 2. I am prone to a half-hearted conclusion that it might be with fuses that the nearer 1 that value can be got, whatever the physics, it might just be that that is a criterion on which to judge a fuse.

Mr. R. Oliver (in reply)

Dr. Turner, Prof. Lipski and Prof. Lerstrup all raised the question of the charge model. I don't know whether I have created a misunderstanding - I am not 'selling' the charge model. I would draw your attention to the top of page 105 where I say 'Experimental results indicate that the charge-controlled model is not entirely satisfactory in describing the fuse arc under d.c. overcurrent conditions' - so we agree?

I have in fact demonstrated that in my case, under d.c. overcurrent conditions, that the charge model does not work. The questioners have evidently found that under different conditions, the charge model is similarly invalid. In fact Prof. Lipski mentions Dr. Ossowicki's work, which is in a similar area to mine, and of course I know that a term has been introduced into the equation, which is a function of $\int i^2 dt$. This produces answers which fit, but I think that Prof. Lipski and Dr. Ossowicki would admit that there is no validity in terms of the physics, as yet, to justify the inclusion of that term, so for the moment it can only be described as a way of fitting the experimental results.

In reply to Dr. Turner, rapid changes in arc voltage were observed. Dr. Turner indicates that bowing of the arc had been found: I have called this 'lateral movement'. I think this is open to discussion. There is undoubtedly evidence on the wide elements where this phenomenon occurs, and not on the narrow elements, that the anode and cathode spots do in fact move laterally across the strip as can be observed from an examination of the strip erosion pattern after extinction.

I think that it is interesting to note the somewhat anomalous behaviour in final arc lengths, which can be demonstrated by looking at Figs.5,6 and 7. Figs. 5 and 6 follow a rising characteristic, the increase in width from Fig.5 to Fig.6 results in a somewhat shorter final length. In Fig.7 we have this unusual behaviour, where the curve turns over with increased thickness. This is a rather interesting anomaly. I have a suggestion, although I can't prove this, that the reason why it turns over is because we are looking at the largest cross-sectional area of strip investigated and therefore the element which probably had the highest axial heat loss. What we may be observing is the increasing significance of axial losses from the ends of the arc, producing an increase in the field in what I have generally termed in the body of the paper as the second phase of arcing.

Referring briefly to this second phase of arcing, it would appear that the behaviour of the arc can be generally segregated into two parts. The higher-current sector, which is the early part, where the axial field is maintained at a fairly constant level (this was investigated by the crowbar system), the fields obtained being shown in Fig.9. However, as the current falls, and Fig.10 shows, if you move towards zero on the current-axis the electric field declines, particularly below 80A. Whatever the reason for this it explains why extended arc lengths are found under d.c. conditions, and why the d.c. overcurrent arc is probably the most difficult condition for an h.r.c. cartridge fuse to interrupt.

Dr. M.R. Barrault

In the equation at the bottom of page 110, the 2 should be multiplying w rather than squaring it.

I want to say a few words about the interaction between some of the things we have just been discussing and the pressure in the fuse. I would like to indicate a scheme (Fig.) whereby we can relate the pressure which is within the arc to pressure which we measure at the wall of the cartridge and with the power and arc processes. We can imagine that we are dissipating some power within the arc, which is leading to an increase in the space available for the arc to burn in. This is because we consider the melting of the sand when we start out, by having a solid element embedded in some sand. We convert this into a void which is constrained within a molten silica tube, and this process takes in the space which was originally present between the unmelted grains of sand. In addition to this we have a certain 'set', which is introduced into the problem, because the sand is not in perfect contact with the element at the start of arcing. There is therefore some small area which I call a_o which is immediately incorporated in the lumen size occupied by the arc once the arcing process has developed.

We also have to take account of the final destination of the silver element. This will take up some space, and so will subtract from whatever space is available for the arc to burn in.

In order to build up a model, (we have just seen a model for the creation of an arc space), we can imagine that the amount of molten material will be proportional to the power dissipated. That is a simple first approximation which may not be correct since some energy may be radiated away. That closes this part of the process (Fig.) - we have this part under control, I think.

We must then somehow join these two parts in order to work out what the pressure is going to do, and this can only be done through an arc model. On the next figure (Fig.) I have illustrated a few of the sorts of arc models which we may use to try and solve this very difficult problem. There are basically three types of arc models which one can consider as applicable to the fuse domain.

We could imagine that we have radial conduction cooling. This may in fact occur under certain conditions, and if this occurs then the radial heat flux from the arc must be matched up with the current of the arc, and the final result is that one obtains this sort of arc equation, which relates the local electric field to the current and to the dimensions of the arc. This I have taken to be somewhat elongated, and depends upon the initial proportions. Most fuse arcs have electric fields which vary quite slowly with current, and this is certainly so in the overcurrent domain which Mr. Oliver and myself have been looking at. Under these conditions the second half of this equation is almost a constant, and we find that the electric field depends inversely upon smallest section of the arc.

The second model is a black-body cooling model. Here we can imagine that the arc is being cooled purely from the surface at a rate determined by Planck's Law. In that case we get a different form of scaling for the electric field. We see that the electric field in this case varies as the square root of the size of the arc. I think that that is quite an interesting fact.

The third case corresponds to cooling through what I would call optically thin radiation, and I think that this case is the most likely one. We can think of the arc as having a central zone with an almost constant temperature. From that central zone radiation is being lost (it may be re-absorbed at the boundary but finally it will be re-emitted) and the result is that we will have volume loss rather than an area loss, and in that case the theory works in this way: here I have used certain relations to show that we will have to introduce a pressure term in this. The pressure term does not apply in the other two cases, because in one case Planck's Law does not require a pressure to be present and in the case of the conduction cooling, there is not very much variation of either the electrical conductivity, except for very high densities, or the resistivity, upon pressure. In this last case then, the electric field is more or less constant, and depends only upon $p^{\frac{1}{2}}$.

One of these three schemes, or all three of them, have to be used in order to close our system of equations.

The last problem connected with pressure is one of measurement. The pressure which we measure at the cartridge wall is not the pressure which is generated within the arcing column, and this is because most unfortunately, or happily in some other way, the sand is not just a fluid, but it made up of a large number of little beans resting upon each other so that the transmission of pressure through this sand takes place in a non-fluid manner. In the paper I have indicated one sort of relation which may be used. That relation implies that there is a constant ratio between the amount of shear and the amount of pressure which is applied to the sand. Clearly, under these conditions, one would expect to find hysteresis in the behaviour of the pressure. When the arc pressure goes up at the centre, you would expect the pressure at the cartridge wall to increase at some reduced rate, then when the arc pressure decreases, you would expect the pressure at the wall to remain constant for some time until the ratio of shear to pressure has been reversed sufficiently, so that the sand grains can start slipping again and transmit this change. Experimentally however, it is found that the pressure changes within the column are very easily and rapidly conveyed to the tube wall, albeit at reduced values.

I would conclude this short talk by showing some results taken with different bore fuses (Fig.), which illustrate quite well that, taking for instance the 90 mm bore cartridge, when the power dissipated within the fuse changes, so we can find the pressure at the body wall changing also in synchronism. This is quite contrary to what a simple theory of transmission of pressure from the arc column to the wall would indicate. Understanding the transmission of pressure provides us with an initial diagnostic means on the arc and it is also extremely important in understanding the performance of the arc, because if we look here at these three different body walls we have quite different behaviours for the amount of energy dissipated and of course finally for the voltage behaviour as a function of time. The whole performance of the fuse is affected by the body wall. The only way that the body wall can affect the performance of identical elements for the same current is either a change in the lumen size available for the arc, or a change in atmospheric pressure between the grains of sand. However, measurements which we have made show that there is no significant increase in the pressure of the atmosphere between the grains of sand and that the pressure measured at the body wall is in fact being transmitted through the sand and not through the atmosphere, and therefore the conclusion is that it is the change in the lumen size which is a result of the change of the bore, which is in fact controlling the performance under these conditions.

Prof. Hirose (in reply for Chikata et al and Sasao et al)

I would first like to make some corrections. On page 134, '..... during Ta + τ ....." should read '..... during Tp + τ ......'. In reply to Dr. Vermij's first question, Dr. Miyamoto believes that the fuse metal will be heated up to 2000 - 3000°K, and the surrounding sand granules would begin to vaporise at this point, because as the oscillograph on page 119 shows the intensity of Si II and Si III reaches a high value in a very short time after arc initiation, so he believes perhaps that sand is always? vapour?.

* Some informal discussion took place here regarding Dr. Vermij's question concerning the 'ideal gas' assumption, and it was agreed to refer this question back to the authors. *

Mr. C.B. Wheeler

I would like to present some equations which give a result bearing upon the rate of growth of arc length, as to why it depends upon a power of the current between 1 and 2.

For a radial-conduction cooled cylindrical arc,

$$\sigma E^2 = \text{div } (K \text{ grad } T)$$

assuming that the gas is fully ionised, $\sigma \alpha T^{3/2}$ and $K \alpha T^{5/2}$.

Solving we obtain: (J. Phys.D., 3, 1374-1380, 1970)

$$E \alpha R^{-7/5} I^{2/5}$$ where R is the arc radius.

Hence the power/unit length:

$$EI \alpha I^{1.4}$$

Mr. H.W. Turner

What we were talking about here is not a column effect but an electrode effect. Erosion is taking place within the little cone of energy within the electrode itself and you could postulate this, as something going on just below the surface of the contact and causing an erosion rate to a power of the current. So you are referring now to the compressed plasma within the base of the arc.

Mr. C.B. Wheeler

I am suggesting that the bulk of the heat loss is radial, and that some small fraction of it is transmitted longitudinally, giving the same power dependence at the electrode. I would add that this relation has been verified experimentally for capillary discharges as opposed to discharges in a fulgurite. (J. Phys. D., 4, 400-406, 1971).

Dr. J. Nasilowski (communicated)

In my paper there should be an arrow inserted in Fig.9, from the phrase 'disintegration of the fuse-wire into segments' towards the phrase 'cut-off current'. Also Fig.10 was missing from my original paper. This is reproduced below.

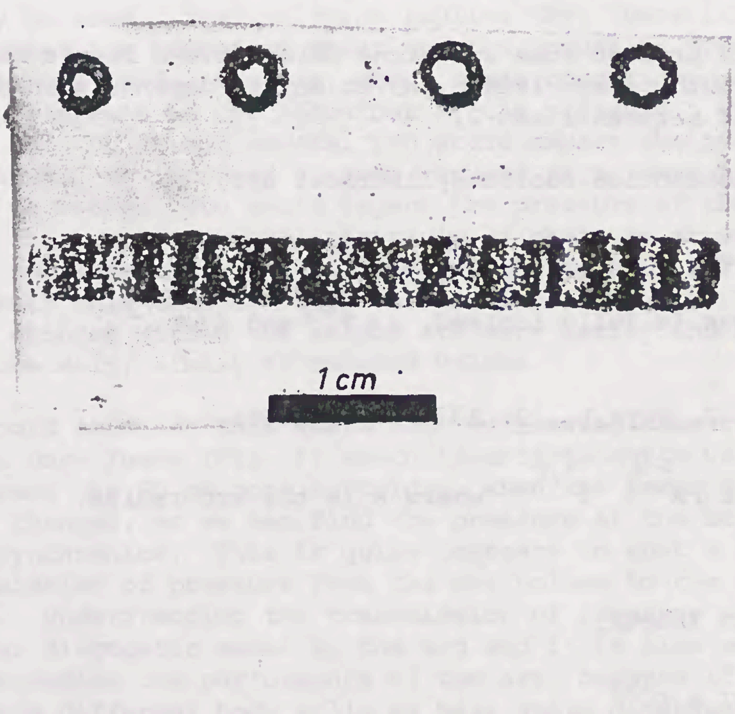


Fig.10. A fulgurite produced from d = 0.75 mm. Cu wire in quartz-sand. U_o = 400V d.c. L = 1.5 mH; $I_{cut\text{-}off}$ = 2300 A. Dark strips are post-arc zones which divide the fulgurite into individual rings.

SESSION 4. Thursday, 8th April, 11.00 - 12.15

APPLICATIONS & DEVELOPMENTS 1

Session Chairman: Mr. J.W. Gibson
(Brush Power Equipment)

Paper No.

17 "Current-limiting capability and energy dissipation of high-voltage fuses", by L. VERMIJ and H.C.W. GUNDLACH.

18 "Design and application of expulsion fuses in 123 kV networks", by S. GRUDZIECKI, A. WISNIEWSKI and B. KAPRZAK.

19 "Self-rehealing performance of the p.p.f. for a control centre", by Y. WADA, S. HAMANO, T. MORI, T. MIYAMOTO, T. INOUE, I. ISHII and T. SHIRASAWA.

20 "Mathematical analysis of breaking performance of current-limiting fuses", by A. HIROSE.

21 "The role of the semi-enclosed fuse in circuit protection", by P. MORRELL.

Mr. H.W. Turner

What is the maximum number of operations a p.p.f. can make, and how does this depend upon prospective current? What is the influence of filler on the work described by Dr. Vermij and Prof. Hirose?

Does Mr.Morrell foresee any developments in semienclosed fuses to retain their convenience yet improve their performance?.

Mr. P. Rosen

It is apparent from the papers presented on the p.p.f. that beryllia is used as one of the component parts. Beryllia because of its thermal properties also has great attractions for the h.r.c. fuse designer, but considerations of its extreme toxicity under certain conditions tends to preclude its use. I should like to ask the authors of the paper on the p.p.f. how they justify the use of beryllia in their device, from a health hazard point of view.

My second question is directed to Dr. Vermij and Mr. Gundlach. They say that you cannot have a current limiting high voltage fuse of compact dimensions, moderate watts loss and high current rating. Yet such fuses are in fact widely available commercially. The point I wish to make is that in a well designed fuse, the provision of reduced sections on the fuse elements to ensure current limitation need only add 30% or less to the watts dissipation of the fuse at its rated current. So where is the problem?

Mr. E. Jacks

I would like to ask Prof. Hirose whether his model adequately represents all the fortuitous conditions which can occur in the system and whether he is satisfied that his calculations do really give what we call the worst-case condition so that when a fuse is tested to it it can be considered safe under all circumstances in service.

Dr. P.M. McEwan

I would like to make a comment on Dr. Hirose's paper. The analysis on the prearcing performance is directly related to Meyer's equation, which implies that the analysis is sound for wires and possibly also for long notched elements. For modern fuses containing notched elements Dr. Wilkins and myself have found that the heat conducted away from the notch is considerable, and in these conditions a different approach would have to be adopted. We have in fact put forward a different approach on this.

Mr. W.R. Crooks

I should like to ask two questions relating to the paper on expulsion fuses.

Would the authors like to comment on the effect upon the arc voltage of different materials in the fusible element?

Secondly, I would ask whether there is in fact any value in producing a rather high value of arc voltage, since this increases the arc power, without, I think, changing the conditions too much at the current zero.

Mr. R. Oliver

A very brief question to Prof. Hirose, with reference to Fig.5, which he derives from his model. I find it rather interesting that in fact the arc energy increases with falling current to a peak and then drops away rather rapidly.

I would have expected in fact, perhaps some fall, but quite a rapid turn-up at the lower current, i.e. the low overcurrent region. I wonder if perhaps it is the type of fuse he is looking at, or if he feels this is maybe a defect in the model he is using.

Prof. Y. Naot

I would like to make a brief comment on Mr. Feenan's review paper. He showed some slides showing that the rated current of a motor protection fuse depended upon the starting current of the motor. On has to define first what we wish to protect. If the case is to protect the motor only against short-circuits, leaving the overload protection to other devices, which may also be thermal devices, then the problem is very easy. But if we wish to protect with a fuse against short-circuit and overload, it becomes a very complicated problem, because the rated current of the fuse in every case exceeds very much the rated current of the motor.

It is possible to demonstrate that if we have a fuse with a long enough time constant, of the order of 15 - 60s, it will be possible to protect the motor with a fuse having the same rated current as the motor itself.

But I know that the tendency of the fuse manufacturer is not to have too many types. I don't think this is right. In my opinion a fuse is a device which has to be coordinated with the protected circuit, and we cannot include every case with only two or three types of fuse. I think that the IEC should consider this.

Mr. Feenan described a multiple fuse, consisting of a parallel connection of many fuses. In Israel, a hot country where conditions are not favourable we have had bad experience with parallel-connected fuses. After a given period of time of service, the contact resistances start to increase unevenly and the current partition amongst the parallel fuses changes. Has Mr. Feenan experienced this problem in cooler countries?

Dr. D.R. Aubrey

I would like to compliment Mr. Grudziecki and Mr. Wisniewski on a fine piece of work, and simply ask them if they could describe for us in some detail the actual fuse element. A humble point in relation to the rest of the paper, but it would be interesting. Mr. Gibson poses the question as to whether the protective configuration described in the Polish paper would be of help in the British Area Board set-ups. Basically expulsion fuses are used in the greatest quantity at 11 kV on rural overhead networks, and their protective scheme is quite the opposite of what we want. In many instances, say 50%, it is not certain that the fault behind a fuse is a permanent fault, it is frequently a transient fault caused by some flashover caused by the vagaries of the weather, typically lighting and clashing conductors, and therefore it is necessary to trip the circuit-breaker first, before any fuse operation occurs.

I could demonstrate this on the first slide, which shows the type of characteristics we require from an expulsion fuse on the 11 kV overhead network. I show there two characteristics, of a 30A and a 60A fuse of a standard type and the black curve is one that we felt was needed. At the low current end, the I^2t let-through at 10s is something in the order of 16 times less than the I^2t at 0.1 seconds. Now this of course cannot be achieved with a piece of wire. We had to go to extreme measures to get this degree of non-linearity, and the second slide shows the adaptions we made, in cooperation with G.E.C. The bulk of that is the standard unit, but there is an additional contact plus a transformer. The fuselink itself has three tails .... (detailed description of two slides follows ......).

Mr. S. Grudziecki (in reply)

I wish to make some comments on the work described in the paper; they are connected with the questions. The 123 kV fuse short-circuiting switch is one of several types studied at Gdansk Polytechnic over twenty years. They include different types of lightning arresters, fuses, reclosers, protective apparatus, switches and circuit breakers.

Investigations in this field led to observations which have been taken into account in the design of apparatus described in the paper.

As we know, in gas-expulsion apparatus, a special type of insulating material produces gases under the influence of an electric arc. Its extinction is achieved by volume cooling. According to one theory, the so-called expansion effect from the gas-evolving material which depends upon the rate of decrease of the gas pressure during the passage through current zero, makes this extinction possible. Another theory takes into account a stream of gases accumulated from the special container before the current zero.

In reality both phenomena exist and the extinction condition is given by the relation on page 162. Depending upon the type of the apparatus the share of the components on the left side of this relation is different.

As we observed in the right type of extinction chambers, the inside diameter is very important. ..... (detailed description of slides follows ....).

Dr. L. Vermij (in reply)

Mr. Turner asked whether the filler has any influence upon the characteristics of the fuse which we describe. Yes, it has definitely. This is shown by the following equation for the factor G which we used in our paper, and which represents the heat flux from the wire to the surroundings for a cylindrical model (long wire).

$$G = \frac{1}{2\pi\lambda_f} \ln \frac{r_2}{r_1} \; *$$

where λ_f is the thermal conductivity of the filler. So when you change λ_f or r_2 then you have a different value of G. But this is a very simplified model. The problem is much more complicated because you have a medium which is inhomogeneous, with grains and moisture and so on. Because this model is so simplified, we prefer to work with a value of G which is determined experimentally. Then you find that G is not constant, either as a function of the current, or as a function of the wire, the filler and so on. As also mentioned by Dr. Lerstrup, I think that the work of Dr. Huhn is very important in this respect.

Previously a question was asked concerning what was an 'ideal' filler. Huhn points out that a much more important factor may be the form of the grains and the distribution of the grain size in order to give an opportunity for plasma jets to build. If Huhn's suggestion that plasma jets are a very important factor in the heat dissipation to the surroundings is true then you can forget this factor G - (during the arcing period).

* r_1 is the radius of the wire and r_2 is the radius at which the temperature of the surroundings will be found.

I refer you also to the experiments of Prof. Salge from Braunschweig who made experiments on wires in water. Then you see a very different factor G.

Mr. Rosen has asked a question about the constrictions and he said that only 10% of the length of a high-voltage fuse gives only 30% more energy dissipation so what is the problem? In fact the work was initiated by a question we got from the Americans - to develop a high-voltage fuse which should be built-in, which should have a large nominal current, and which should also be current-limiting, up to 300 kA, for a 400A fuse. (That is an impossibility with normal wire).

So you have these two requirements. Because the fuse is to be built-in the energy dissipation should not exceed 50-60W. The other requirement is that it should be current-limiting. Then you have to design fuses with necks, with short parts in it, and that increases the power dissipation. That was the reason we studied this problem in depth, to compute theoretically what can be expected. Then you arrive at the conclusion that it is hardly possible to build a high-voltage fuse with a high nominal current and a low energy dissipation and with current-limiting capabilities which exceed the normally required current-limiting capabilities.

I have some remarks about the p.p.f. We have computed some parameters of sodium, in our paper; this was not connected with the p.p.f. but it may be important.

We also are very busy in the field of supercritical states of a metal, sodium amongst others, and we have also some experience with the p.p.f. In our experience the maximum number of operations is three, no more. And the third time you have quite different characteristics, compared with the first time you break a current. The Chairman mentioned the problem of the enlargement of the diameter of the hole - that is a real problem, yes, we have seen it, and also we have had very nice explosions with it.

There was a question about the reliability. In my opinion you may not ask this question now, because it is a research phase. It is not yet a product, but it is promising.

Prof. A. Hirose (in reply)

First of all I would like to express my sincere thanks to all those gentlemen who have submitted their comments on my paper. On page 182, in equation (1) K is the constant prearcing Joule integral; for silver it is:

(equation written on board)

n is the number of parallel elements. S is the constricted cross-sectional area.

$I_{T/2}$ is then $\sqrt{K/(T/2)}$

In some cases this is not the same as the current on the time-current characteristic, but my paper is based upon this assumption. Hence equation (5) is established. This is my answer to Dr. McEwan. Diagram (1) on page 184 was published in Japan about 6 years ago, and the upper half of this diagram is very useful for determining the making angle when making I_1 tests. The upper half of this diagram is being introduced into our revised Japanese recommendations for high-voltage fuses as an Appendix. Diagram (2) on page 185 is also being introduced as an Appendix.

This is useful for determining the required sensitivity of current-mesuring instruments. On the oscillograph the value of i_o should be as high as possible, without being off-scale. For this purpose diagram (2) is useful. Diagrams (3) and (4) are also used in Japan.

Mr. Turner asked what influence the filler would have on equation (4) (page 188). This is a very rough equation, and the constants V_o and r depend rather largely on current and voltage. So I think that equation (14) is not a strict equation. However in the past we have used only V_o, not r, and so the representation in equation (14) is one step forward, and not more. Fig.2 and Fig.3 are of technical interest, they may be the hobby of a teacher. But nevertheless they teach us something. For example, the hump in Fig.3(b) will rise if V_o or r are decreased, meaning that the arc energy increases. Fig.5 shows that the I_2 current is about 3. But the I_2 current (maximum arc energy current) moves to the right when V_o and r are decreased. What does this mean? If low-voltage fuses are tested at increasing voltage the summit moves to the right and the arc energy increases. This is because V_o and r are normalised with respect to the test voltage and $I_{T/2}$.

So if you test a fuse with a higher test voltage, the normalised r decreases. This is qualitative, not quantitative, but I believe that it teaches us something about the behaviour of a current-limiting fuselink. This would be my reply to Mr. Jacks, and Mr. Oliver.

On the p.p.f. I would say that nothing can endure the heat of an arc for a long time. The sun's temperature is comparable with the arc temperature, and on the sun everything evaporates or dissolves.

It was confirmed that the questions on the p.p.f. should be referred back to the authors. Ed.

<u>Mr. P. Morrell</u> (in reply)

Mr. Turner asked whether there are likely to be any improvements to semi-enclosed fuses in the future. I can only reply that continual research and development is being carried out, mainly in respect of the possible replacement of the asbestos, due to health hazards in the manufacture, and who knows what may result from this work. However, if circuit conditions do require any improvement in breaking capacity, all semi-enclosed fuses of our manufacture, and I believe that this also applies to other manufacturers, can easily be replaced by h.r.c.

SESSION 5. Thursday, 8th April, 14.00 - 15.15

APPLICATIONS & DEVELOPMENT 2

Session Chairman: Mr. P.G. Newbery
(Brush Fusegear Ltd.)

Paper No.

22 "Overcurrent protection of cables by fuses", by S.B. TONIOLO, G. CANTARELLA, and G. FARINA.

23 "The protection of industrial capacitor banks by current-limiting fuses", by M.J. SMART and B. WADCOCK.

24 "Protection by fuses of mechanical switching devices", by S.B. TONIOLO, G. CANTARELLA and G. FARINA.

25 "Calculation of the course of the current and voltage of a current-limiting fuse", by M. DOLEGOWSKI.

Dr. K. Lerstrup

I would like to say a few words about the cable loading. There is a lot of discussion on this at the moment, but to my mind much of this discussion is actually superfluous, because the discussion centres around finding a thermal device (a fuse) to give thermal protection to the cable, and trying to fit close to what the cable can stand. In by far the most cases it is not necessary. Really we cannot afford to load the cable that heavily, because we are just paying for kilowatt hours instead of paying for kilocopper. It actually turns out that if you have a use time of about 2,000 hours a year as is usual for most industrial installations, then you cannot afford to carry more than $2A/mm^2$. That is quite far from what most small cables can carry thermally, and this question of fuse protection of cables is in particular of interest to the small cables. When you come to the large cables you can usually afford a divided protection between short-circuit and overload, on another thermal device for instance, and therefore this whole discussion in TC 64 is more academic than of practical value, once you consider the economic conditions too. Now these remarks go very well with the fusing of capacitors, because to have advantage of e.g. power-factor correction capacitors, we are normally running for 2,000 - 3,000 hours/year. When you are in that region, and you are keeping between 1.5 and $2 A/mm^2$ on the cables, even on the somewhat heavier cables needed for capacitors, there is no difficulty in finding a suitable fuse for that. In particular, as you consider that the capacitors do not have any starting current to cope with. There is perhaps an inrush current, but after that there is no more problem.

Turning now to the other paper of Toniolo et al, I would say that this is not said as a criticism of the paper, but only the mention of some additional problems, because this matter Toniolo and I have been discussing for about twenty years now and none of us have come to the true solution, but I will just point to some difficulties. He is coming to the conclusion that you should look at the time when the fuse lets so much current through that you will just lift off your contacts. That certainly is the most dangerous position for most contacts. However, the interesting thing is that if you take a larger fuse, then you will have no welding. It so happens that if you can get a current peak that just barely lifts off the contact you will just have a little short arc, on that spot where it has lifted off. You get two soft spots, and you will have the contact back in place before these spots solidify and then the contact will stick.

If you take a fuse of a couple of numbers higher current rating, you will have the contacts thrown off much further, the arc will be magnetically blown away and will burn in other places, the two original touching spots will have solidified before the contacts come back to rest, and you will have no welding.

So there is far more to this problem than appears here on the surface of the paper. There is simply no answer to it, except the one which is now embodied in the IEC specification for contactors, which simply says that welding of contactors is something you must consider a possibility, irrespective. And that is just about the only solution we have, which again leads to the conclusion that one should never rely on a contactor in matters of safety.

Mr. J.W. Gibson

Concerning the paper by Toniolo et al, it seems to me that in the old days, the fuse manufacturer would claim that his fuse was a good one because it did not deteriorate. Should he now alter his claim to say that it does deteriorate, but at just the right rate to match the cable?

The paper by Smart and Wadcock contains a great deal of useful information for the fuse manufacturer who is called upon to supply capacitor fuses. Sometimes to a fuse manufacturer capacitor bank design seems very abstruse, but it is rather well elucidated in this paper. There is one point on fuse selection which is not covered by the paper. Assuming that one unit of a number in parallel fails, you would expect that normally the fuse of that unit would be blown by the discharge current from the other units. But I was told a long time ago, by a colleague of the authors that you can't always rely on that, because small boys have air-guns, and the bullet may arrive when there is zero voltage, and then one has to rely on the power frequency follow current, which it is desirable should be cleared rather quickly, and I understand that the capacitor manufacturer knows roughly how long this unit will survive if it is faulty before the current is removed and before the can will explode.

My other point is that, in selecting a capacitor unit fuse, the principle is the same as for selecting any other fuse. That is that the best current rating is that which will withstand any normal transients without blowing the fuse, but without too large a margin. It seems to me that in this case the fuse has to be chosen essentially of a current rating to withstand the switching inrush transient, and there seems to be rather a lack of information on that point. I think that often the capacitor manufacturer does not know very well the system on which it is being installed, but he probably knows the fault level of the system which means he knows the L of the system. He has built the capacitor, so he knows what the C is, so from that you can calculate the natural frequency and peak value of the inrush. But this does not give any direct help in choosing the fuse. You choose the fuse on the basis of I^2t, and for that you also need to know the decrement of the inrush current. In the IEC, we were trying, with help and advice from the capacitor manufacturers, to write a specification for capacitor fuses, and test requirements were laid down and included a statement that 'the natural frequency of the test shall be so-and-so.....', and gave values of the decrement. The fuse manufacturer didn't know much about that, and had to take the word of the capacitor people. I am wondering whether the capacitor manufacturer could, when he wants fuses, give the value of the decrement, based upon what has recently been agreed in IEC.

Another point is, again on the choice of fuse with respect to I^2t, can the capacitor manufacturer also give some idea of how much the I^2t is increased when one bank is switched into another? The fuse manufacturer generally knows that that is rather a vicious condition; the frequency is much higher of course but he would like some guidance as to just how much allowance he should make for it. As with most cases of fuse selection, there is a lot of guesswork (based of course on previous experience) but the more the fuse manufacturer's hand can be held, by people who understand capacitors, the better.

Dr. K. Lerstrup

Mr. Gibson prompted me to speak again. The capacitor manufacturer knows a good deal more - because when you put a capacitor in for some reason, you want to switch it on and off without disturbing anybody. You don't want the voltage to change by more than about 4% when you switch it on. That means that you know right away that you will have a power follow-up which is 25 times the rated current of the capacitor.

Dr. C. Turner

I would like to make a few short remarks on the paper by Toniolo et al, on the protection of switching devices. I don't quite agree with Dr. Lerstrup's remarks just now because, if you have a modern contactor material, and it arcs, it won't weld. If it doesn't arc, it might weld. So the most dangerous condition is not when it separates, but just before it separates.

It is of course dependent upon your switching device, and in fact if you plot your weld force, for different I^2t against your contact force; the relation is as shown in Fig.1.

At one end, if your contact force is small, it will blow off and it won't weld. In the middle it will weld strongly and further on when the contact force is very large the contact area will be so large that it won't weld. However, if you have a larger I^2t it moves and the trouble is you can't really decide beforehand where you will be. You know what your I^2t is, but you don't know the point-on-wave and other things. But there is another complication, and that is that a modern contactor does not just have two contacts, but a pair of contacts. (Fig.2).

Now that has many more degrees of freedom and the trouble is that instead of having a nice simple curve you have got something like Fig.3. And now you don't know where you are because the curves move in a 3-dimensional way, and it gets very complicated. We are giving a paper on this in Tokyo in August.

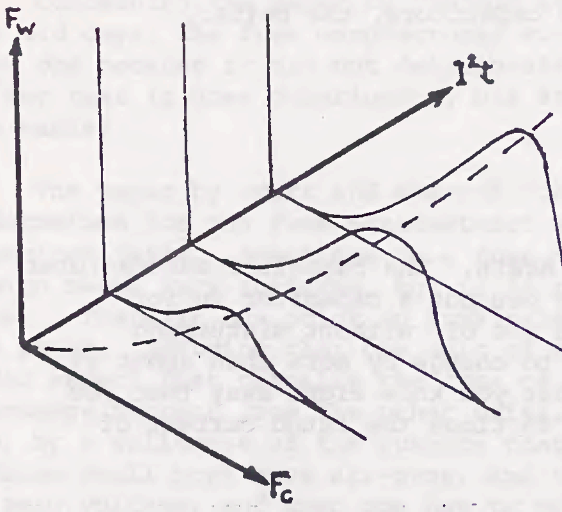


Fig.1. Relation between contact force, weld force and I^2t.

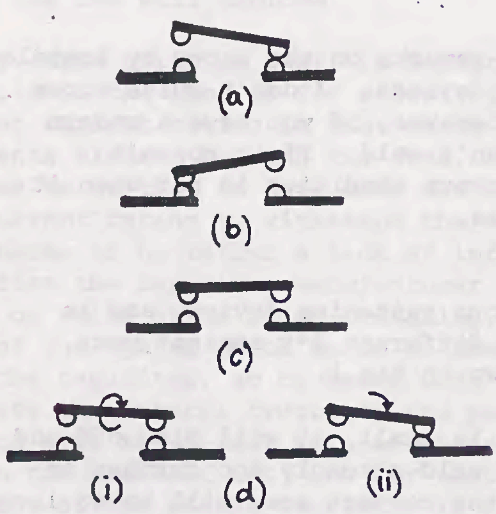


Fig.2. Effects of throw-off on double-break contacts.

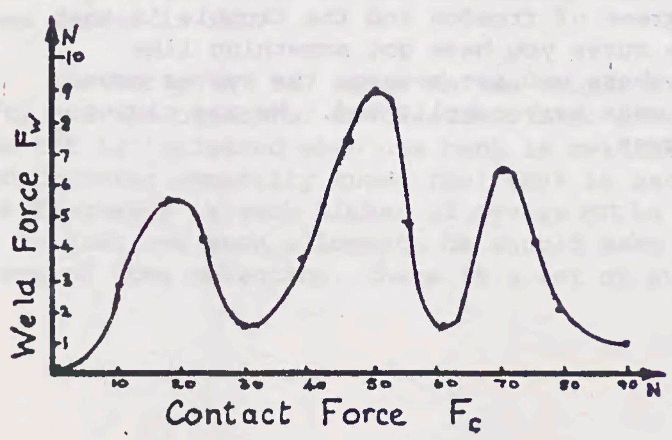


Fig.3. Contact force-weld force, double-break (I^2t = 72000 A^2s).

Dr. J.G. Leach

I was very interested to read the paper by Dolegowski. I am not so much speaking for myself but for my colleagues, Dr. Wright and Dr. Beaumont, who have done some work on analysing the arc in a fuse, with which I have been moderately involved. We have been trying to do it from a totally theoretical point of view, looking at the arc from an energy balance and looking at the rates of burnback and so on, with the minimum amount of experimental work, on the principle that if you only do a model based upon experimental work, then as you move outside the range of types of fuses that you have done the experimental work on, your equations become suspect. Most previous attempts to do a mathematical model of arcing have always taken a lot of assumption and simplications, and we were very impressed with Dolegowski's paper in that a vast amount of work has gone into looking at almost everything that can be considered during the course of the arcing. I think that in general this is the way analysis of arcing has to go; it is not sufficient to make sweeping assumptions like a rectangular arc voltage and so on. It's very important to either get down to fundamentals to find out what is really going on in the arc, or if you can't do that, you have got to try absolutely everything to find out what is influencing the arc.

Mr. H.W. Turner

I would like to pose a question on the paper on overcurrent protection of cables. The fusing current, and the non-fusing current of the fuselink are measured under standard conditions, with standard cables.

The protection that we are looking for here is at the maximum current rating that you can get with a given cable. Under those circumstances the fusing current is going to be lower than the value determined in the standard condition. Consequently, it would imply that a lower value than the authors have stated could perhaps be used. Therefore the fusing factor could be higher when measured under the standard conditions than it turns out to be when it is used with these smaller cables where the heat from the cables causes the fuse to melt at a lower current than it would in the standard test condition.

I agree however that if this 1.45 factor is adopted then there will be a further degree of safety added because of the methods of measurement used in the standard tests.

Mr. E. Jacks

I have not had the opportunity to study the last paper, but looking at the title prompts one or two questions in my mind which have also arisen during the presentation of other papers in this conference. One of the most fundamental problems facing the fuse designer is how to test his product in such a way that the test represents what he calls the 'worst-case' condition. I would have thought that at a gathering of this sort

the evaluation of the 'worst-case' condition would have been more in evidence. In the paper I can see no evidence that the fortuitous behaviour of the system has been taken into account. Many of the mathematical models are constructed on a number of simplified assumptions. Rectangular arc voltages have been mentioned - (I think that they probably exist, somewhere) and beautifully sinusoidal system conditions. Of course in service you don't get that sort of thing at all. You get two arcs in series, you get the arc in the fault and the arc in the fuse and it is the interplay between these two arcs and the way the energy is shared between them which in many cases can give rise to the maximum dynamic stress conditions in the device. These variations again do not follow any nicely uniform pattern. They vary very fortuitously, some very rapidly with very great rates of change, and it is always a question when one is, as a manufacturer, spending huge sums of money in testing fuses, whether he can really say to the user that the fuse has been tested to represent the worst case condition and can be confident that no condition can arise in service which will cause the fuse to fail.

I have not received any more evidence at this conference than I have received at other conferences of this kind on this particular topic, and I would plead for considerations of system behaviour to be looked at more closely.

The same applies of course in the case of cable protection. Everybody knows that you can't protect anything until you know its essential withstand. We still don't know the essential withstand of cables under all the fortuitous circumstances in service. I think we know a lot more now than we used to do, but it's not very long ago since it was almost impossible to obtain from any cable manufacturer any idea of the short-time rating of his cables expressed in a simple time-current characteristic which could be related to the time-current characteristic of a fuse. You could get one or two points but what happened between these points nobody knew. Of course there are complications due to the diversity and load-factors in service, but I suspect that the main reason that this information has not been forthcoming is because really speaking, the cable manufacturer has very little commercial incentive for producing this information. After all, his cables work all right, they carry the current and as Dr. Lerstrup has said, very often if you can protect whatever is at the end of the cable the cable can look after itself. It's also quite true that in service a lot of the theoretical hazards that are supposed to affect cables never actually occur. I can quote cases where cables have been subjected to very severe tests and then subjected for analysis, and no significant change of state was discovered in them. Similar cables which had been standing on the shelf still on the drum, not carrying current have been similarly analysed and a greater change of state had been discovered in these. So the problem is not simple. I would suggest that more of these fortuitous circumstances are taken into account in papers such as have occurred in this session.

Mr. L. Vermij

I would like to comment on the remarks of Mr. Jacks.

When we have a lot of circles, small ones, large ones, and not-so-large ones, then if we want to know what is the circumference of these circles, then you can take a measuring tape and you can measure the circumference of each circle, and you can write under each circle what its circumference is. Another way is, when the radius 'R' is given for each circle, to calculate it using $2\pi R$.

In our subject, in fuses, arcs, and so on, what we are doing is to determine the circumference of each circle by measuring it, because we do not know the formula to come from R to the circumference. Mr. Jacks asks for the 'worst-case' condition. To give an answer you should know exactly how and under what conditions it operates, and what physical processes are involved. So we ask for the formula 2π times R. We do not know yet that formula and therefore research is necessary, to find an answer to your question, 'what is the worst-case condition for fuses etc. .... ?'

In the paper by Mr. Dolegowski, who tries also to find an answer to this question, (what is the formula connecting the circumference of a circle with the radius?), he gives an answer from experiments, from what he measured. Other people gave an answer from theory about a theoretical model, but we know that we don't have the physical parameters to solve these models. As long as we do not have a good insight into these physical parameters to solve this problem theoretically, the only method is the method as given by Mr. Dolegowski. Then you can reach some result, and that is a step on the way, a step on the way to find the formula $2\pi R$. When we have that formula $2\pi R$, then we can determine the circumference of the circle by computing it from R. Not now.

Prof. L. Lipski (on behalf of Mr. Dolegowski).

Mr. Dolegowski gave me permission to make some remarks concerning his work. As Dr. Vermij said just before, he tried only to get more detailed information concerning the arc behaviour in the inductive a.c. circuit, which is described by Kirchhoff's Law, from the point of view of laboratory investigations. That is the main idea of Mr. Dolegowski. He takes the a.c. network into account and therefore the solution has very close connection with the actual conditions appearing in an a.c. circuit.

The Chairman raised the question of the range of applicability of Mr. Dolegowski's method. This method is valid of course only in the case of short-circuit, and in the case with deep and short notches. It is not the same case as that considered by Dr. Hibner. It is a quite different case. It is interesting to note that in equation (6) the electrode voltage drop has two members. One is independent of current, while the second is dependent upon the current. (On page 219, in equation (2) U_N should be U_B).

Prof. Y. Naot

I would like to ask the authors of the paper on capacitor-bank protection why they did not consider one phenomenon which is important. The inrush current of a capacitor bank is extremely short in time because the time constant is generally some nanoseconds, but it can be extremely high in magnitude. I had many times the occasion to observe that the electrodynamic forces acting on the fuses produced some distortion of the contacts, starting a trouble which may become serious some weeks or months later. It seems to be that in choosing a capacitor fuse, not only the thermal properties should be taken into consideration, but also its fitness to withstand shocks.

Dr. R. Wilkins

I would like to make a few remarks in support of Dr. Vermij's view. The approach should I think be to try and represent a fuse, a circuit, and maybe a device as well at some later stage,by some sort of model, and it is only necessary to have a model which is sufficiently accurate for the particular phenomenon you are interested in. Development of such a model is a very difficult process, but that is no reason for not attempting it.

There are various degrees of sophistication. Mr. Jacks has mentioned in particular the use of a rectangular arc voltage. A recent paper of mine gave many characteristics and interactions between a fuse and a circuit based upon the assumption of a rectangular arc voltage. If you interpret that value of arc voltage as, say, the average arc voltage during the arcing time, the answers you get do have some value. A slightly more sophisticated approach was presented this morning, by Prof. Hirose; this seems to be definitely a better arc model which gives transients under short-circuit conditions which look very similar to those which can be seen on oscillograms. This is a step forward.

But in order to use these models you have to have experimental data, and I support also Dr. Leach's view, that the paper by Dolegowski represents an enormous amount of work, gives experimental data which is of great value. Some of the modelling techniques should also prove useful.

As regards the ideas about the 'worst-case', it depends which particular thing you are talking about. If you are talking about Test 2 current, when you are testing for maximum arc energy dissipated within the fuse, the studies I have made, and also presented by Prof. Hirose this morning, indicate quite clearly that critical current depends upon the arc voltage of the fuse. These are the things which can be illuminated by the use of models, even though they be very simple ones.

Another 'worst-case' may be discussed in the next session - the maximum I^2t let-through by a fuse will depend upon the point-on-wave at which the fault occurs. So when we talk about 'worst-cases' we must make sure that we know which worst case we are talking about.

Mr. B. Wadcock (in reply)

I would like to take the point Mr. Gibson raised regarding damping in capacitor switching circuits. Ideally of course the prearcing I^2t of the fuse should be related to the I^2t of the capacitor inrush current, and we realise that this I^2t value is affected by the damping in the circuit. Unfortunately, in the size of capacitor bank that we are considering in this paper these factors are very difficult to obtain.

We can generally obtain them on large capacitor banks, but on small banks of this nature, which are essentially off-the-shelf designs, the resistance, or damping in the circuit is very difficult for us to obtain. We really rely entirely upon the capacitor user to provide this information for us. If they can do this we can then of course tell the fuse manufacturer.

I think also Dr. Lerstrup mentioned a factor of twenty-five times. In other words, the inrush current of the capacitor, for an isolated capacitor bank, be approximately 25 times the normal capacitor current. This is a rule-of-thumb measure that we are quite well aware of. In actual fact, on multistep banks this figure is substantially increased. To reduce the figure to some value more reasonable from the fuse point of view, we have to introduce some inductance between the sections of the capacitors.

Mr. M.J. Smart (in reply)

One thing I should clarify with regard to our paper is that it is related particularly to small banks, for industrial purposes, and a significant factor of these is that the capacitor is designed so that on capacitor failure, the fusing current is largely power-frequency. There are very few capacitor units connected in parallel, so that there is very little inrush current from the healthy capacitors into the failed capacitor.

The application of current-limiting fuses to bigger banks where there is a very large inrush current into the failed capacitor from the parallel-connected healthy capacitors makes fuse operation and application much more difficult. The inrush current into the failed capacitor in these circumstances, largely the discharge frequency current, can be up to 10 kHz. Perhaps some of the fuse experts would care to give thought to the operation of fuses under these high-frequency conditions. It is an area where we could do with a little more knowledge.

In extreme cases, we can find that because the frequency of the current is so high, and fuse operation can take place in less than one-quarter of a cycle of the high-frequency current, which at 10 kHz is less than 25 µs, the fuse can be damaged due to thermal shock effect on the fuse barrel.

Another thought here is whether there is in theory any limit to the numbers of capacitors you can connect in parallel, from a fusing point of view. This is something we are very sure about and we like to keep our banks down to about 60 kJ level of available energy.

The Chairman asked whether the motor-circuit fuse would be a useful fuse for capacitors. This may be so, because obviously it is designed to withstand mechanical strain of motor starting. I must say that when we have applied fuses in accordance with the rules laid down in our paper, we have no evidence of problems from current shock on switching, and perhaps this answers Prof. Naot's question about thermal shock on inrush currents.

I think that one of the snags of the motor-protection fuse is probably expense, because a motor needs three fuses, while a capacitor bank needs one fuse per unit, and we are talking about a lot of fuses, in some cases.

Prof. Lerstrup raised a question about relating fuse, cable and capacitor ratings. I think the problem arises here where the fuse is on the feeding end of the cable and has to protect the cable and the capacitor. The fuse has got to be oversized to withstand the capacitor inrush current, and something like 200% is typical for a high-voltage capacitor bank. If that same fuse has also to protect the cable, then the cable must likewise be oversized, by something of the same order. This is where the problem can occur.

Mr. Gibson mentioned 50 Hz fusing and bursting curves. It is true that it is difficult to get a current-limiting fuse to discriminate with our unit-bursting curves, all the way throughout its length. In fact if we take the time-current characteristic of the capacitor units, we normally take the 10% probability curve because unit bursting is not a precise thing, it's a question of probabilities. We find that when we apply a fuse in accordance with the parameters laid down in the paper, that it crosses the curve something like that. So that we only get correct discrimination down in this area. But with banks arranged as we have stated in the paper, the fusing current on capacitor failure is very large, because it is only limited by the system impedance, so that we can ensure that operation is in this region, and we do get correct discrimination.

* Figure drawn on blackboard. *

Mr. G. Farina (in reply)

With reference to the first paper, I would only remark that the fuse is not able to carry the current I_{nf} indefinitely, for a time exceeding the conventional time. The temperature rise of a fuse although less than that necessary to reach the melting temperature, is high enough to cause accelerated ageing of the fuse element. Consequently the permanence at such high temperature causes the operating characteristic to drift to the left, eventually bringing the fuse to melt with the conventional non-fusing current.

These tests have been carried out in our laboratory. The tests were carried out on about 20 samples of fuses of 50A rated current. The fusing time was scattered from 1 hour to about 5 hours. On this basis the current-carrying capacity of the cable to be protected against overload, may be exceeded to a reasonable extent by the conventional fusing current of the fuse itself, the characteristic of which is subject to a drift to the left when high temperatures are maintained. This criterion does not apply for protective devices operating on different principles, e.g. a circuit breaker.

For the other paper, with reference to the question of Dr. Lerstrup, if we use a fuse exceeding the rated current of the contactor, welding or heavy erosion of the contacts will very probably occur. The high rated current of the fuse causes a high energy between the contacts, and when contacts come back together, although the current has been terminated, the contacts are still at a high temperature, and welding can occur. This was confirmed by many years of tests, on fuse-contactor coordination in Italy. We have verified that the worst condition occurs at a current slightly exceeding the current that equals the contact force, because in this way the contact comes back to touch together suddenly, and the current is not terminated. If in these conditions welding occurs, we must reduce the rated current of the fuse, to avoid risk of welding.

SESSION 6. Thursday, 8th April, 15.45 - 17.00

SEMICONDUCTOR PROTECTION

Session Chairman: Mr. E. Jacks
(G.E.C. Fusegear Ltd.)

Paper No.

26 "I^2t values of real and ideal semiconductor fuses", by T. LIPSKI.

27 "The dimensioning of fuses for the protection of diodes and thyristors", by K. LERSTRUP.

28 "Advances in the protection of semiconductors by fuses", by J.G. LEACH.

29 "Liquid-filled fuses for the protection of thyristors", by Y.A. PASTORS.

30 "The desirable short-circuit parameters of semiconductor fuses", by J. CZUCHA.

31 "Cyclic loading of fuses for the protection of semiconductors", by G. STEVENSON.

Dr. R. Wilkins

I would like to raise a question for comment by Prof. Lipski and Dr. Leach.

Dr. McEwan pointed out this morning that where you have a fuse-element with a deep notch in it then the prearcing I^2t is certainly not constant. He has made this point and Dr. Leach has also emphasised this.

There are instances in the protection of power converters when we do not want the fuse to operate, i.e. we wish it to discriminate correctly with another protective device. What is the point of using I^2t values for discrimination when we know that what really matters is whether or not the element will actually reach melting point.

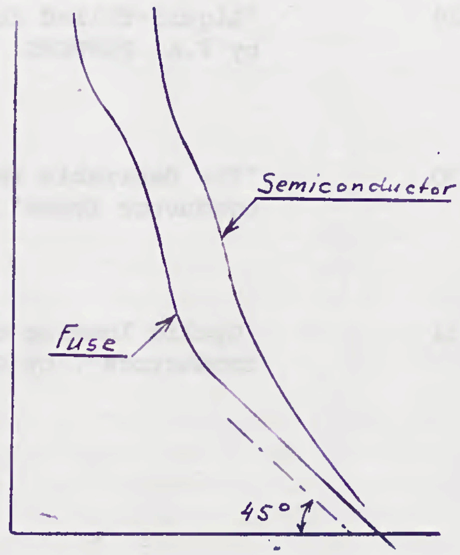
Power converter fault current waveforms can vary quite widely, and the I^2t values vary widely, as Dr. Leach has shown. So numerical methods must be used to predict the transient temperature at the hottest point on the fuse-element.

Since the prearcing I^2t values are so variable, what is the point in using them?

Dr. Lerstrup

I have one question to Dr. Pastors. Why has he not tested the most obvious liquid of all - water? It will give us hydrogen for interruption and incidentally is readily available in most places.

Apart from that I would like to turn to the I^3t. Normally we will have curves like this, coming down to the 45° line. This is typical of having two different time constants, that of the fuse element and that of the fuse body. If we are dealing with semiconductor devices, we have very short times, we have some very thin layers which have to transfer heat from the places where the losses take place. So the semiconductor device probably has something like this, giving something that, on the whole, may approach a higher power than 2. Not necessarily 3, but higher than 2.



The next thing is that this is not at all an improbable thing within fuses. In fact we have sitting here Mr. Nørholm, who to my great academic irritation has designed a fuse element that did not behave the way my theory wanted it, namely to follow I^2t, but actually followed a steeper motion. And therefore actually in its melting characteristic comes closer to I^3t. However, this is no good if you have a fuse with too small an arc voltage so that for higher current you have a greater I^2t during arcing, but if you combine this with sufficient arc voltage to be sure that your actual operating current also falls down along the same line, we may get that fuse which lifts up a little higher here, and allows better utilisation of the semi-conductor, and a closer approach. So, even if they should start talking about $I^{2.5}t$ or I^3t, I think we are still up to it and we are capable of delivering the goods.

Mr. R. Oliver

I would like to ask Dr. Lerstrup a question which I think he will be very keen to answer. I asked a question yesterday about the exothermic reaction with aluminium. I actually used a rather important word which perhaps wasn't heard; I said "quantitative" information on the exothermic reaction and whether such information is available and whether it has been studied. I knew that an exothermic reaction took place, but I would certainly be interested in what Prof. Lerstrup has to say as to how he contains this reaction and absorbs the energy released. A further point on aluminium is that as an element material I think that serious consideration must be given to its low-current arcing behaviour, which I have found is distinctly inferior to silver. It can exhibit, under low-current arcing conditions, phenomena where the second phase of arcing referred to in my paper is even worse with aluminium, to the extent that the increase in length during the arcing period is equalled in effect by the fall in the axial field with the consequence that the arc burns with constant arc drop. I would be interested in any comments Dr. Lerstrup might have in that direction.

The other question, to Dr. Leach, I was interested in his temperature predictions under various conditions, shown in several figures. The one which interested me was Fig.13, where his resolution axis runs from 200 to 220°C, so the curves which are shown are obviously very accurately resolved from a temperature point of view. I would be interested to learn if Dr. Leach has established the effects of manufacturing tolerances on the predicted temperature bands, under the same conditions.

Prof. Y. Naot

I would like to make some general remarks. Let us try to see the problem in a different way.

I don't think that the I^2t, or I^nt is relevant in this case, because I^2t is an integral measure. This is like saying that the total number of calories developed in the fuse should not exceed a given value. I think the problem looks somewhat different.

Let's look at the circuit. We have a fuse, then we have a semi-conductor device, then we have a load.

Now a semiconductor device and an arc have typically a negative 'temperature' coefficient. If you look at the two main equations I gave in my paper yesterday, and you suppose two bodies having the same time-constant, T, you see that inherently the negative temperature coefficient brings to much shorter times of temperature rise than the positive one, for the simple reason that by positive temperature coefficient the heating process starts slowly and increases thereafter, and by negative coefficient it is exactly the reverse. The heating process starts strongly and decreases thereafter. So I think the problem is that which one of these two elements reaches the dangerous temperature first. We want the fuse to reach the melting point first. What we are looking for, since the fuse has a positive temperature coefficient, is a fuse with a very short time constant. It is not a matter of how many calories we are dispersing in it; it must be very very quick in order to be first to reach its dangerous temperature. I am sorry, I shall go the way of the famous Roman Cicero, who said: 'Ceterum censius Cartago delendam esse'. If you would have an n.t.c. fuse the problem would be solved.

Dr. P.M. McEwan

Speaking specifically about this problem I find I am in full agreement with Dr. Leach's recommendations, and I would like to make a point on the previous speaker's remarks. For correct fuse performance in this case we would also have to consider the arcing performance surely, and it would not just be a case of the fuse reaching melting temperature. I would also add that the time constant of silver is the shortest that I have come across, and I doubt if we will find a better element material. In the slide I showed yesterday I think it was evident that adiabatic conditions existed for about 10^{-4} seconds, which indicates the time constant of heat flow from one section of the element to another is extremely short.

Looking at this particular arrangement here, my view is that we do in fact have to model both devices and pass the same current through both since they are connected in series, and see how they perform, but this would involve modelling the arcing period also, which takes us back to this morning's sessions.

Mr. J.W. Gibson

I just want to ask Dr. Leach a question arising from remarks that have been made about the very short time constant. 10^{-4} seconds has just been mentioned, but if we look at Dr. Leach's Fig.7, the curve for symmetrical current at 10^{-2} s almost has a characteristic of $I^{10}t$ or $I^{20}t$. Does that ever finally reach the adiabatic condition? It still seems concave to the left. Does it finally turn to the right and reach the constant I^2t condition?

Mr. J. Feenan

Picking up a point that Dr. Wilkins made. When he queried whether prearcing I^2t was essential or necessary, I would like to enlarge on that and ask the authors their views. I think that the answer to his question is that it is the best we have for the moment. It is a reference point from which to assess what the fuse will withstand, but we are, as you will have gathered from the papers, a long way from the final solution.

There are in Dr. Leach's paper methods of calculating the prearcing I^2t, methods of calculating the state of the element under certain pulse conditions, and there are in Mr. Stevenson's paper methods of trying to determine the same information by empirical methods.

One of the factors which has been mentioned during this conference, but which I don't think is mentioned in any detail in the papers is the effect of mechanical fatigue, and I wonder whether any of the authors could comment on this particular point. I think an attempt has been made using empirical methods using prearcing I^2t as a reference point.

On the comment that I^3t might be a good idea, having been involved with a number of people in this conference in trying to formulate IEC 269 part 4 on semiconductor fuses, and in the application guide, we did of course get some measure of agreement that I^2t was not a bad basis on which to commence work.

It took us some time to produce the document, (although not as long as part 2 and part 3 - so we are improving), but I think that a consideration of a different form of presentation of data is dependent to some extent upon the available information on devices. This has already been mentioned, and one of the tasks which is still left to the working group concerned, (I personally feel that it is an impossible task) is to produce simple rule-of-thumb methods for selecting fuses, even for the most complicated equipment arrangements. Information from the fuse viewpoint whilst not perfect, is available, but not from the device viewpoint.

I would like some observations from the authors in this session on those particular points.

Dr. K. Lerstrup

In my many discussions with Mr. Feenan he has never given me such a beautiful opening as he just did. He always insists that we should give the I^2t characteristics on a separate sheet of paper, because that ties down to the I^2. If instead he had stayed with the original idea of giving the characteristic curves of time and current, he would know then that a 45^o slope represents the second power (I^2), because we have a scale ratio of twice as long per decade for current than for time, and therefore a deviation from this direction will show an approach to a higher power, or a lower power. Therefore if we went back to the original and did not speak about any separate curve, we could present the facts to the users in such a way that with a little thinking (of course that is necessary) they would be able to use them.

Prof. T. Lipski (in reply)

Generally speaking I may say that my paper is only a very small attempt at the very large problem of semiconductor fuse selection. To characterise my paper, I would say that I only try to show how far we are from heaven. And that is I think the main point in my answer. From this point of view I will try to answer the more detailed questions.

First of all the question of the I^2t value as a constant value. In my paper it is not necessary that this value be taken as a constant, because the main results, shown in Fig.4 and in Fig.5 are calculated on the basis of the 99% for diodes and thyristors. These two devices are shown here by conventional means, and taking into account these 99% lines, or 50% lines which one can get from the marks given on the figures, and which correspond to actual state which we have at the moment for semiconductor devices in production now, I only used idealised semiconductor fuses to show you how far the total I^2t lines should be for ideal semiconductor fuses.

On page 235 some dependences are shown. Especially interesting is the third relationship which gives the ratio of the prearcing I^2t to the total I^2t, independent of the I^2t value of the shoulders. I take into account only the present state in semiconductor devices, and I calculate on this basis the values for ideal fuses.

Of course I agree with Dr. Wilkins and with Dr. McEwan that this influence is very great in some time region, but in this paper it is not necessary.

Of course this answer is in close connection with the problem of discrimination. It is right that in the case of discrimination between two fuses in series the influence of cooling by the shoulders must be considered. This may be affected by the fact that the current in the two fuses may not be the same.

Once more I would repeat that my main intention was to show how far we are from heaven.

Dr. K. Lerstrup (in reply)

It appears that all I have is the question about the aluminium.

In reply to Mr. Oliver, there has been nothing said about using aluminium for fuses that operate on a sneaking overload. It is useable only for short-circuit protection on the high overcurrents. That is what it is for, and that is what semiconductor fuses are for. If we take a long, slow, heating up of it, all we get out of it is a nice fire bomb.

Now we come back to the question of the chemical reaction. The chemical books will tell exactly how many calories you will get out of every gram of aluminium that you melt and react, but I believe this was not the question asked, but 'what will it do in conjunction with the amount of energy released in the fuse otherwise, and how do you get rid of it? Fortunately, when you have very short arcs you have a greater amount of the voltage drop very close to the surface of the metal. That means that the bigger part of the arc energy goes into melting more metal and to be conducted away from the rest of the metal provided there is plenty, and this is one of the most important things, you must have plenty of metal, to cool. That is, very deep indentations, and what 'very deep' is, is something you had better ask the toolmaker about, because the technique of making successful fuses in that direction depends upon how deep he can make the cuts.

Now, we have this additional energy released by the chemical reaction. That too, fortunately is released near that surface of molten metal where the foot-points of the arc are. In other words, the chemical reaction and the electrical reaction work in the same direction, namely to elongate the arc and to give some higher pressure around the small arc. It means in practice that you get - and shall I give a figure? - 25% higher average arc voltage than you would get with silver under the same conditions. Now when you have a higher arc voltage then you will have a quicker decrease of the current and you will have a shorter arcing period, and this is where you gain. I can tell you, just to give exact figures, that we have compared a silver and an aluminium fuse, as closely alike as you can make them, as to the melting I^2t and so on, and found that the total integral plus the chemical reaction of the aluminium fuse was less than the electrically released energy in the silver fuse. This is not always the case, but the fact that it is possible to get so far that you actually have less energy shows that this is not such a far-fetched idea at all. So even if you have to pay the price of having the chemical release as well as the electrical release, you still may have a smaller total release of energy. I hope that these clear figures will satisfy Mr. Oliver.

Dr. J.G. Leach (in reply)

First of all Mr. Oliver's question concerning the y-axis resolution. The reason it is so fine of course is that this particular test was to determine the effect of current waveform on the generation of heat within a fuse. It seemed obvious that very high form-factor currents, for example, would lead to higher notch temperatures and consequently a higher rate of energy production. The question is whether we should derate our fuses if it's on half-wave rectified current compared with the sinusoidal current it's customary to test with. The model was set to take steps of a cycle, very similar to the other model, but this was with the rated current of the fuse rather than an overload and so time-steps of the order of 1 ms were taken. The results showed that it doesn't make much difference to the energy; manufacturing tolerances, cables etc. have a greater effect upon the current rating than the waveform, at least for up to 120° conduction.

I used my mathematical model for doing some studies on the effect of manufacturing tolerances, though I must admit in this particular case I haven't; however, I have done curves like Fig.13 for different notch shapes, and as you can see, since the fluctuation is only of the order of 20°C or so, a different notch shape doesn't make more than say 50% difference. So with manufacturing tolerances, even if they made 10% differences, we are only talking in the order of half a degree. In fact in that connection I would draw your attention to one of the earlier papers by Dr. Barbu, which didn't receive much discussion, mainly because I think he was considering a very complex mathematical model of a fuse neglecting radial heat loss. One of his conclusions was that at rated current the axial temperature gradients were fairly small, and this is borne out by my Fig.13. When we get near to minimum fusing current, or higher currents than that, this situation changes and this is shown in some of my other figures, where very large fluctuations of the temperature of the restriction do occur.

Another point raised on various occasions was the shape of time-current curves. I should emphasise that all the time-current curves that I have drawn are drawn for real time. Because I live in a real world, unlike possibly one or two analysts, I like to use real time and not virtual time. And this goes along with device manufacturers, who always use real time. I have never seen I^2t values quoted for anything other than real time.

Mr. Feenan talked about the effect of mechanical fatigue. It is mechanical fatigue that I was mainly concerned about with my suggestion for using curves of prearcing I^2t against prearcing time to decide how close we can take a fuse to its time-current curve. This is something that I am always getting asked, in one form or another. Obviously if one fuse is meant to discriminate with another fuse, the question is, O.K. well if one curve is there and the next curve is next to it, they will discriminate, but what will it do to the fuse that is being discriminated with? Will it cause mechanical fatigue? So I thought it was a good idea to throw out a few simple rules - perhaps not so simple because no-one has commented on them, and I have presented these basically as a basis for discussion. Perhaps you would all like to go away and try using them and let me know in a few years time. The idea is that if we limit the element temperature during what is in effect an overload caused by the fuse having to sustain a current before a breaker trips out for example, the situation where you don't want the fuse to operate, it's a problem then to ensure that the fuse won't be mechanically fatigued.

So what I propose is that if you limit the element temperature during this pulse of current then the chances of mechanical fatigue are considerably reduced. Because obviously the higher the element temperature gets, the more stress is put on the restrictions and the more chance of mechanical fatigue occurring. So these percentages I have quoted are as to how close one can go to the prearcing I^2t curve.

I would point out that I use a prearcing I^2t curve rather than a time-current curve for times of less than a second or so, because time-current curves in this region are very susceptible to the wave-shape, far more susceptible than the prearcing I^2t against time curve. This gives a curve which is a one-stage better approximation to what is going on. So I would be very interested in anyone's comments on this, at some future date. As to this principle of how close you can get to this minimum prearcing curve consistently, I have based recommendations on either a few operations, the sort of fault you expect the fuse to see only a few times during its life; I have said 10, perhaps it's 20, my rules are pretty conservative I believe, or frequent, 1000 times/year or perhaps 2000 times/year.

I think I said yesterday all I want to say about negative-temperature coefficient fuses.

The only other comment I have got is on Mr. Stevenson's paper, as he is to speak next. I thought that this was a different approach to mine, in that he is looking at repetitive cycles, for which I just give a rule-of-thumb, because I haven't got round to doing the sort of tests he did, so I am grateful for him for doing the tests for me. I will obviously be looking at it too and it seems to me a very good idea, to produce curves of number of cycles against pulses to give more information to the user. I am all for anything that gives extra information to the user.

<u>Mr. G. Stevenson</u> (in reply)

My paper was primarily concerned with cyclic loading, or fatiguing of fuses. I would just like to make it quite clear that as far as we are concerned, this fatigue problem is mainly a function of fuse design, and there are many fuse designs on the market, not particularly in the U.K. but from other countries where the basis of rating is different, such that cyclic loading or the pulsed loading of fuses causes mechanical fatigue. In the U.K. the general practice has been to use fast-type fuses, rather than ultra fast-type fuses which means that the current densities in these fuses are a lot smaller, so the element itself is less susceptible to mechanical fatigue. The other point I would like to make is regarding Dr. Wilkins when he talked about the importance of element temperature, and not allowing the element to melt. I think that from a discrimination viewpoint, that is not the whole story because as Dr. Leach points out, it's not just a question of preventing the fuse from melting, but it is a question of how many times you want to go there. If you want to go there, say, 10 or 20 times in a fuse lifetime, well then of course you can go very close to the time-current characteristic ordinate for that particular time, but if you want to do it a lot of times then you must bring in quite a large safety margin to ensure that there is no mechanical fatigue.

There was one query raised concerning the overload on page 277, where I mention that you can go to 85% of the time-current characteristic from a discrimination viewpoint. In this particular section I had in mind where the equipment is only liable to say 100 or less faults in its lifetime. For another equipment, such as a chopper, a traction application, where you can get motor flashover quite frequently across the commutator, and the fuse has to withstand this value while you bring out a circuit-breaker, further back so the train can continue to its destination, well that's where the 70% factor comes in. So the first section there is just concerned with overloads which only happen 100 times or less in the lifetime of the equipment.

What I think we ought to start thinking in terms of, from a particular application viewpoint is how frequently we want to go to the time-current characteristic, and then by the use of the curves that I propose you can determine how long the fuse is likely to last.

The other thing regarding mechanical fatigue is that one can get round this problem by kinking the element, as was mentioned by Mr. Feenan early this morning, and this technique has been employed successfully. It may be that in the future, for very large fuses and very fast fuses, that in fact there is an upper limit whereby this mechanical fatigue becomes the major factor in fuse design. At the moment this is not the case; I think that it is mainly on the basis of rating primarily American fuses and others where this desire for information on the withstand value for a fuse, this is where the emphasis has come from rather than from the U.K.

Dr. Y. Pastors (communicated reply)

The use of water in the liquid-filled fuse link appears difficult in view of the following factors.

When water is in a continuous contact with various metals and plastic materials, its electric conduction is increasing. In addition to that, continuous heating causes water to liberate hydrogen. The latter, forming a gaseous obstruction, disturbs the process of vapour condensation in the fuse link.

Experiments with water, however, are highly useful for finding out the role played by the liquid phase in a heterogeneous filler, as water has a high critical boiling heat flux.

SESSION 7.   Friday, 9th April,   11.00 - 12.15.

TESTS AND STANDARDS 1

Session Chairman: Mr. J. Feenan
(GEC Fusegear Ltd.)

Paper No.

33   "Optimum conditions for testing fuses at maximum prearcing energy" by C.B. WHEELER.

34   "Breaking capacity tests for miniature fuses" by G. CANTARELLA, G. FARINA and S.B. TONIOLO.

35   "The influence of standards on the design of l.v. fuselinks" by P.G. NEWBERY.

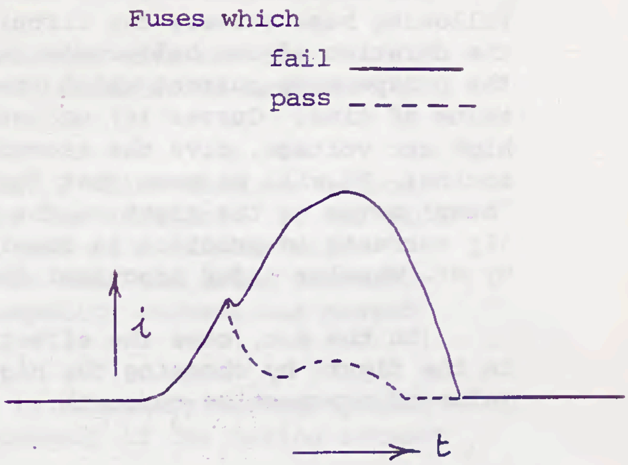
Mr. H.W. Turner

I would like to comment on Mr. Farina's paper, which I think is an interesting theoretical and practical study of this subject of miniature fuse testing, and as a summary, I would say that what he says is undoubtedly true for good fuselinks, but I would suggest that it is not suitable to be introduced at this moment. One point is that the suggested angle of arcing that should be sought is 80° - 90°. With the tolerance on making switches, this would undoubtedly mean that the 90° arcing would be extended to a value beyond 90°, on a falling voltage. I would submit that this would be unsatisfactory.

Accepting the evidence presented here, one would say that one could perhaps reduce the number of fuses tested by selecting certain current ratings from a homogeneous series and testing them solely at this value. However this would not result in a reduction in cost, because in order to do this it would be necessary to take an oscillogram of every shot and to measure the arcing angle and adjust the making angle of the subsequent shot to make sure that it lay between 80° and 90°, and this will increase the cost of testing tremendously. It may mean that you might (on the basis of this evidence) reduce the number of fuses tested, but the cost of testing for example, a fuselink to B.S.88 on the breaking capacity test, (which involves this type of arc instant measurement) is over 100 times the cost of making a breaking capacity test to IEC 127, where you use a test switch set at 30° ± 10° and test all the fuses at that same setting, which is a considerable economy. This is permitted because, with miniature fuses (unlike other fuses), all the ratings in a homogeneous series are tested. Because of the nature of these little fuses, it is essential to do this, and consequently there is much more general control over miniature fuses. In fact it is possible to test 48 fuses in all the different tests, for power loss, millivolt drop, dimensions, breaking capacity time-current characteristics, etc. for less than one-fifth of the cost of making a breaking capacity test on one rating of a B.S.88 fuse. Now, even allowing for the higher power involved, this should be considered when evaluating cost, because the cost would not, as is suggested, be reduced by this procedure.

However, would it mean that the devices were tested more satisfactorily? Again I would submit that this is not the case because with the miniature fuse the value of 1500A is far in excess of what is ever experienced on the sub-circuits at the end of a piece of flex that these little fuses are subjected to. There are special uses where they might get up to prospective currents of this kind, but the manufacturer of the device which uses them for that special use should introduce some form of resistive protection, which increases the power factor to a higher value than the 0.7 at which these are tested. So, in that case also the specified test is more severe than normal practice. And then again I would turn to the actual evidence, which I do not dispute at all in the way in which it has been presented here, but I would ask Mr. Farina if these are tests on actual manufacturer's good fuselinks. I would also ask him how many failures there were in the tests which he carried out, because my experience of testing these fuselinks over about 15 years is as follows. The ones that pass the test follow the forms that we see in these curves, Fig.2, for example (page 314), with a very rapid reduction in the current immediately after cut-off, and Fig.6 (page 316) which shows the kind with the beginnings of a "camel's hump". However, the ones which fail usually fail by a more extreme case than this, namely, something like the sketch shown.

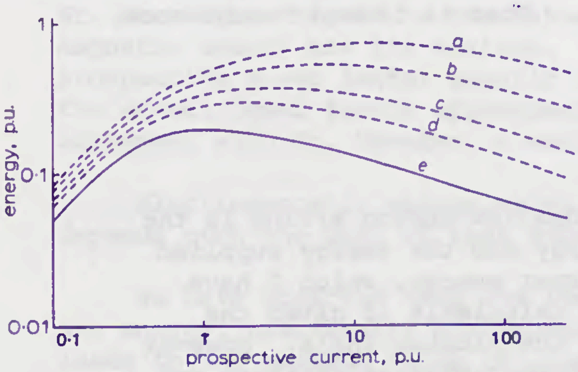
A year or so ago I tested a number of internationally obtained fuselinks in helping to set these breaking capacity conditions, and I found that there were some that would fail at 30° making angle, but when tried at later point-on-wave, including 90°, arcing commenced on a falling voltage and cleared, and the fuses did not explode. Thus I repeat my question to Mr. Farina, asking whether he did this with a set of fuselinks, not only the best ones from his best Italian manufacturers, but also with some bad ones with filler deficiency, and other inferior characteristics.



Dr. R. Wilkins

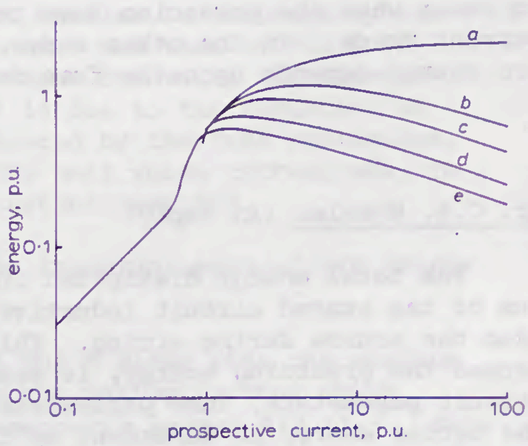
The stored inductive energy at the start of arcing, discussed by Mr. Wheeler, is only one component of the total energy dissipated in the fuselink during arcing. The energy supplied from the source during the arcing period must be added to the stored inductive energy to obtain the total.

Using a rectangular arc voltage waveshape, these characteristics have been computed and published elsewhere, (Proc. IEE. 122, 1975, pp.1289-1294) and are shown in the figures, for both d.c. and a.c. circuit conditions.



D.C. arcing energy characteristics

a $V_a = 1{\cdot}1$ *d* $V_a = 2{\cdot}0$
b $V_a = 1{\cdot}2$ *e* $V_a = \infty$ (inductive energy curve)
c $V_a = 1{\cdot}5$



A.C. arcing energy characteristics (cos $\phi = 0{\cdot}3$; maximum values shown)

a $V_a = 1{\cdot}5$ *d* $V_a = 5{\cdot}0$
b $V_a = 2{\cdot}0$ *e* $V_a = \infty$ (inductive energy curve)
c $V_a = 2{\cdot}5$

The characteristics are shown in normalised form using the following base values; the circuit time-constant (L/R) for d.c. and the duration of one half-cycle for a.c. The base value of current is the prospective current which produces element melting after the base-value of time. Curves (e) on both diagrams, corresponding to an infinitely high arc voltage, give the stored inductive energy at the start of arcing. It will be seen that for more realistic arc voltages the 'hump' moves to the right on the curves, and so the critical current (I_2 current) in practice is considerably higher than that calculated by Mr. Wheeler - for practical fuses usually about 3 - 4 times.

(In the a.c. case the effect of point-on-wave has been eliminated in the figure by choosing the highest values of arc energy for each value of prospective current).

Prof. T. Lipski

It is well-known that the fuse-resistance (R_F) of miniature fuses may considerably reduce the prospective current during the prearcing time.

High-breaking capacity (2500A) quick-acting fuses, particularly for smaller rated currents, may have $R_F > R_1$ or even $R_F >> R_1$. The reduction of the current trace in such cases leads to the alteration of the instantaneous value of the current at arc beginning. Then arise questions concerning:

(i) the calculation errors of the function in Fig.3 in comparison with the measured values in Fig.4,

(ii) the influence of R_F on the energy equation (1),

(iii) the influence of R_F on the making instant giving maximum arc energy.

It seems to me that the first part of the conclusions is limited to cases when the prearcing fuse resistance does not reduce the prospective current trace. In the other cases, the making instant giving maximum arc energy depends upon the fuse design. That is indeed troublesome.

Mr. C.B. Wheeler (in reply)

The total energy dissipated in a fuselink during arcing is the sum of the stored circuit inductive energy and the energy supplied from the source during arcing. This former energy, which I have termed the prearcing energy, is exactly calculable if given the circuit parameters, fuse parameters and the closing angle. However the latter energy is dependent on the current-voltage relation for the arc which, in practice, is found to be a very variable quantity. Dr. Wilkins' calculations are for an assumed rectangular arc voltage waveform.

Mr. G. Farina (in reply)

The test current of 1500A is in agreement with the IEC Publication 127 in which this current was indicated as maximum breaking capacity for miniature fuses.

With regard to the cost of the tests, I think that our method is not more expensive than that stated by the standard. In our paper we have observed that in most cases the test at a current below breaking capacity, but sufficiently higher than the rated current, can be omitted if the test at breaking capacity current was passed successfully.

Certainly oscillographic recording is necessary to verify that making instant is near 90°, as it is necessary if the making instant is 30°, as stated by IEC 127.

In our tests we have tested only four types of fuses of many rated currents. Only one type does not pass successfully the tests and corresponds to the type of Fig.6, in which piercing of the external surfaces of the end caps occurred. In this case the maximum of arc energy occurs with making instant near 0°.

In reply to Prof. Lipski I would observe that expression (1) is referred to the arc period and the fuse resistance cannot have importance at this time.

The fuse resistance is very important for the prearcing time and it modifies substantially the prospective current. We have taken into account this resistance to calculate the peak current at arc beginning. A certain error can appear from Fig.3 and 4, due to the fact that we have considered for the fuse resistance a constant value instead of variable value as a function of temperature, during the prearcing time.

The fuse resistance is very important to determine the making instant giving maximum arc energy.

With reference to the paper of Mr. Wheeler, a discrepancy can be noted between our diagram of Fig.3 and 4 and that of Fig.5 of Mr. Wheeler as regard to the making instant for which the electromagnetic energy has its maximum. This is due to the variation of prospective power factor greatly influenced by the fuse resistance; the actual power factor approximates the unit which corresponds, in agreement with Mr. Wheeler, a making instant near 90°.

Electromagnetic energy which is an important part of arc energy depends upon the peak of test current.

We have observed that the instant which gives rise the maximum arc energy, practically corresponds to the making instant which leads to the maximum value of electro-magnetic energy $1/2\ LI_o^2$. This condition for all the tests carried out occurs near 90° and is confirmed by experimental results.

Mr. P.G. Newbery (in reply)

In view of the recent revision of IEC 269 and the subsequent revision of national standards to meet these requirements it is unlikely that further fundamental changes will take place for perhaps a decade. In the meantime the fuse manufacturer and user will be assimilating the present specification.

The only aspect which could perhaps influence a quicker evolution of a new specification would be the publication of the International Wiring Regulations. In practice, however, the fuselink does give adequate protection to wiring installations and, therefore, even this problem should be able to be resolved within the framework of the present specifications.

The dimensional standardisation of fuselinks is a difficult technical and commercial problem. It is, however, hoped that the British practice of having discrete dimensions for different categories of fuselinks will prevail, i.e. Domestic, industrial and semiconductor. In addition if more than one standard voltage is common then again for safety reasons discrete dimensional systems should be used. This is particularly applicable in the fast developing field of semi-conductor fuses.

During this conference little mention has been made of the miniature circuit breaker or moulded case circuit breaker, a growing competitor of the fuse. We are all aware of the limitations of these devices particularly regarding breaking capacity which is evident in their associated type test requirements. There may be some merit in the future of standardising breaking capacity type test requirements for all overcurrent protective devices. As indicated in my paper the type test requirements for fuses are so severe that many national testing houses are unable to fully test all fuses, this has hopefully been noted by such bodies.

On the subject of national testing authorities there may be considerable pressure for approvals marks to be extended for industrial products. It is, however, advocated that the well proven system of independent short circuit certification coupled with manufacturers' self certification of other type tests should be adopted in the fuse industry.

Finally I would like to comment on the discussions relating to the practical use of theoretical investigations. My own company has sponsored research at University level for some ten years and I can only reiterate the comments given in my paper that such investigations although very fundamental have given great assistance in our developments.

SESSION 8. Friday, 9th April, 14.00 - 15.15

TESTS AND STANDARDS 2

Session Chairman: Dr. R. Wilkins
(Liverpool Polytechnic)

Paper No.

36 "The standardisation of HV current-limiting fuses", by J.W. GIBSON.

37 "The role of ASTA as the U.K. certification body", by J.G.P. ANDERSON, R.E. BLAKE, B.S. CHALLENGER, R.H. GALLAND and D.G. MEE.

38 "The quality aspects of HV current-limiting fuse protection", by W.R. CROOKS.

Dr. L. Vermij

Regarding the paper of Mr. Crooks, I have the impression that this is the only paper which goes into the question of how to manufacture a good fuse. It is a general experience that the design of apparatus can be very good, but that the apparatus delivered to a customer can be bad. We have the problem of the quality control during the production process, which is not a simple one. Unfortunately there is no test which makes a good fuse from a bad one, so quality assurance is extremely important, and especially for the manufacture of fuses.

There are many many problems associated with quality assurance and there exists at present a lot of literature on it and also much investigation, especially in Japanese industry. There is much literature on the quality assurance of products where the product is less important, I think, than it is for a fuse. Maybe this, the last paper of this conference should be the first subject for consideration at the next conference. My impression is that when, as a customer, I want to buy a fuse from a manufacturer, which can guarantee the quality of the fuse on the basis of an adequate quality assurance, then I have the problem of deciding 'what is an adequate quality?' This is a serious problem for the user of fuses.

In Mr.Crooks' paper there is I think at least one omission in the field of quality assurance, that is with respect to the inspection of tools, which from my experience is a very important factor with respect to quality. I would like Mr. Crooks' comments on this.

Could Mr. Crooks also tell us something about the ageing effect with motor fuses, particularly when M-effect is used, due to the diffusion of the low melting-point alloy into the silver carrier. This diffusion is stimulated by the fact that the low melting-point alloy can melt under the influence of high current pulses, which do not disrupt the element. This changes the characteristic of the fuse, and I suppose especially for the case of motors, where you have heavy inrush currents. Is this a problem?

Mr. M.J. Smart

I would like to refer first of all to Mr. Gibson's paper where, towards the end, he refers to capacitor testing, and particularly paragraph 6.2, where he refers to the voltage for the breaking test. He suggests that for the current-limiting fuse it should be at two times the rated voltage, because of the overswing which occurs on capacitor switching. He doesn't mention what the factor should be for expulsion fuses, non-current-limiting fuses, and I would suggest there that the voltage should be maximum system voltage because the expulsion fuse normally arcs until the energising transient has disappeared, so that the voltage which is relevant is the maximum system voltage.

He also mentions that it is proposed that the frequency and the decrement of the inrush transient should be proportional to those for system conditions and we had some argument about this yesterday -

what are system conditions? It was even suggested that we could base on a constant factor of 25 times for the maximum frequency and inrush current level. Damping is dependent upon system damping which is a very variable thing as I think we all know. It is also suggested that frequency of inrush varies as system voltage; well I think we would like to dispute that. I can think of many applications for instance on weak systems where capacitors are installed for voltage regulation reasons, they tend to put fairly large banks on a fairly weak system, causing a fairly low frequency of inrush transient. On the other hand in this country we can get relatively small banks on quite high fault levels, for tariff reasons for instance, where the transient currents are of very high frequency. So perhaps Mr. Gibson could elucidate further on this question of frequency being proportional to rated voltage.

Prof. T. Lipski

I would like to comment upon the problem of ageing of fuses with M-effect, and the suggestion to eliminate this kind of fuse. Some old investigations made in Poland showed very clearly that in the case of this kind of fuse, in the M-effect region we have two curves limiting the ageing region, as shown in Fig.1. Curve 1 is the one-shot time-current characteristic and curve 2 is the non-deterioration time-current characteristic. The difference Δt_1 between these two characteristics depends of course on the values of overload current in this region. In our case the difference was about 50% of t_1 on average.

Our colleagues from Hazemeyer found more or less the same value. Recently Siemens very clearly showed in an official explanation for users of this kind of fuse that on average in their case this factor is 35%. If you take into account, by designing the low-voltage network not characteristic (1) but this non-deterioration time-current characteristic (2) of course in a circuit on the end of which we have a motor, which has a starting current not higher than the points lying on the non-deterioration time-current characteristic, and you use these time-lag fuses, selected on the basis of this characteristic, and then you compare with the same low-voltage network, with a normal quick-acting fuse, without M-effect, then generally speaking there is no difference between the two networks. The cross-sections of the cables are more or less the same; the electrodynamic stresses are less; selectivity - generally no change, in some respects it's better. The problem is power loss, which is a little bit higher. In our calculation we took into account a 10m length of circuit to the motor, and the next example was 100m. Another problem of replacement of time-lag fuses with M-effect by quick-acting fuses without M-effect is a higher temperature at the point of installation.

Nevertheless, we found that from the point of view of the network these two solutions are very similar.

On the other hand you should know that if you have a time lag characteristic in a low-voltage network, then operation of the fuse may occur, say, after 500 starts for a motor. (Of course only in one phase. Then you will have two-phase running and possible failure of the motor).

We concluded in Poland that we should go in the direction of eliminating this type of fuse from our systems. Besides that, we tried to improve our quick-acting fuses to get better time-current characteristics from this point.

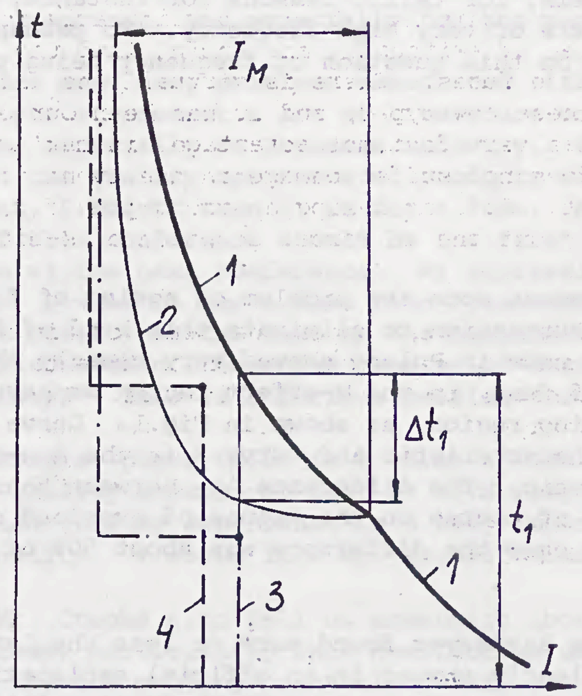


Fig.1. Time-current characteristics of a time-lag fuse with an M-effect.

I_m - M-effect region,

1 - one shot characteristic,
2 - non-deterioration characteristic,
3 - substitutional motor start current in the non-deterioration region,
4 - substitutional motor start current in the ageing region.

Mr. P. Rosen

I would like to compliment Mr. Crooks on an excellent and very informative paper, and my comments are not criticisms but perhaps expansions of a couple of points.

One is in defence of M-effect, particularly for h.v. fuses. We have heard at this conference quite a lot against the use of M-effect for low-voltage fuses but my own feelings, and I am sure those of Mr. Crooks, are that for h.v. fuses it is very difficult to make full use of all the advantages of an h.v. fuse if you don't have M-effect. The practical advantage tremendously outweigh the theoretical disadvantages that have been advanced against the use of M-effect. If we take first the distribution fuse, this is normally switched on once and left for many days or weeks, and we have done many deterioration tests at our Liverpool GEC factory to see what happens to M-effect when the fuse is used correctly, under continuous conditions, and we have detected no sign whatsoever of migration of the alloy point into the silver. On motor-circuit fuses it is true of course, as has been said, that if you pulse a fuse at too great a value, you will eventually start the migration process off, but I think that most manufacturers of h.v. motor-circuit fuses now publish data which show the highest current and largest run-up time that can be used and so on, and this should always be based upon doing exhaustive tests to see what happens to a motor circuit fuse to make sure there is no deterioration when it is used in accordance with the manufacturer's specified data.

The advantages of the M-effect of course are that the body temperature is kept down and this is becoming particularly important now where cast resin types of switchgear are coming into use, into which you have to put these fuses. There is virtually no air circulation in some of these new types, and it has been our experience that the M-effect fuse has very distinct advantages in making sure that the switch itself does not suffer damage in the event of a low overcurrent type of fault. So I am making a strong plea for, not doing away with M-effect, but the universal adoption of M-effect for h.v. fuses.

In paragraph 2.4 of Mr. Crooks' paper, he talks of minimum melting time, and mentions very briefly the fact that on continental equipment you may not have instantaneous trip-all-phase feature. It is my experience that the advantages of the use of instantaneous trip-all-phase features to give complete protection are very poorly understood. We fuse designers know about this but almost invariably I find that when talking to other people about this it is not well understood. Would Mr. Crooks in his reply explain briefly how this operates for the benefit of others.

Mr. E. Jacks

I was very pleased that Mr. Gibson has been brave enough to point out the dangers of overstandardisation. One gets the impression once people get enthusiastic about standardisation, that standardisation becomes an end in itself, and I think Mr. Gibson's warning on this is timely. I know how much time he puts in on standardisation work, and I know that he believes, as I'm sure we all do, in standardisation where it is useful. In fuses there are many areas which can be standardised to good effect, but he is quite right in my view in warning us against any move towards standardisation which will denigrate the versatility of fuses. This is one of the main advantages in fuses. They are wonderfully versatile, and standardisation can rob us of this versatility if we take it too far.

I also welcome the ASTA paper. There is one aspect which I'd rather hoped they might have mentioned, which is I think peculiar to fuses rather than to other devices. That is, since we have to do type tests for fuses and since we have legal obligations, or at least contract obligations for the performance of fuses in service, it is necessary to be able to identify fuses in service with the ones which were type-tested by approvals authorities such as ASTA. This means that ASTA and other approvals authorities have to go to a great deal of trouble to identify the items they have tested. They dismantle them, they take physical measurements of all the components in great detail.

They do this so that if at any time in the future, there is a need to identify a fuse in service with one which was type-tested and carries a certificate, this can be done to satisfy any legal or contractual arguments which arise. Since we are dealing with safety the legal and contractural aspects which manufacturers and users have to face together,these matters are very important. So I would hope that the authors would expand their next paper to include this.

I would also like to join in on the M-effect argument. M-effect has been a bogey for at least the last fifty years, and there are just as many arguments in favour of it as there are against it. My own view is that it can be a very excellent servant but it can be equally a bad master. Our job is to make it into a good servant, and if we do that it can have very obvious advantages and I would go further than Mr. Rosen who advocates it for h.v. fuses and say that it is far too useful a tool to neglect even for low-voltage fuses. When we talk about deterioration of the element we must not forget that other components in the fuse may also deteriorate, and if you get to the point where a fuse is being misused and something has got to give, it is a lot better that that failure should take place in a controlled manner, so that you get failure to safety. Now M-effect does allow you to do this over quite a big area, and if the geometry of the fuse is carefully looked into, the correct metallurgical decisions made regarding the choice of alloy, and so on, then it is my view that M-effect can be extremely useful. It is also useful in the more active function of the fuse, in arc control - deciding where the arc starts and so on, under certain circumstances. This is a very big subject and I would just content myself by coming down on the side of the people who favour M-effect, providing that it is used properly.

Mr. S. Norton

We have said quite a lot about ageing, but one or two of us will be aware that it's not so much ageing, in the case of the motor fuse, it is fatigue of the actual element, mechanical breakage of the element, independent of the M-effect. I mention this for two reasons, because it brings in the point of the standardisation of curves, and I am in agreement with Mr. Gibson and others that standardisation can be restrictive, but I would like to raise this point, that considering the time-current curve of a fuse,

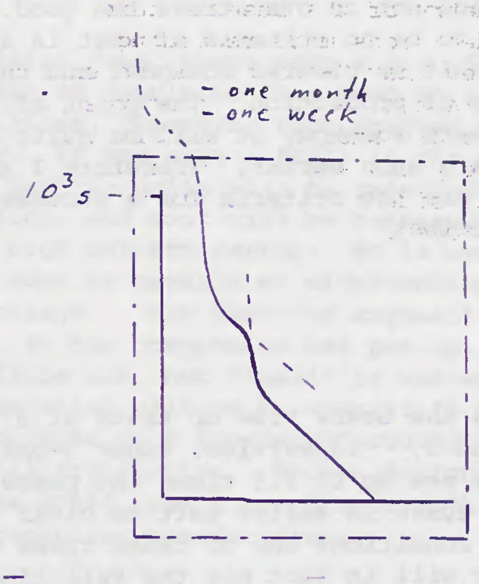
* Diagram drawn on

blackboard *

in the early days, distribution fuses were used for motor protection. We were in trouble because of mechanical fatigue. In the interim period we moved this curve over here. When we solved the problem, by a new element configuration which allowed expansion and contraction, we then found that you could move this back here. Basically what I'm getting at is that the bottom part of the curve is an electrical part of the curve, the top part is a mechanical thing, so we end up with something like that. This is fine, just to talk about it, but what I'm saying is that you've changed the overall characteristic of that fuse employing that element. That is, it's not only the time-current characteristic, it is also the mechanical characteristic of the fuse so that you end up with a somewhat steeper characteristic which is required for a motor fuse. This is just a general thought. What I'm getting at really is that if you had a nice steep curve time-current-wise, it still might not be suitable for motor protection if its element were not designed to withstand this pulsing characteristic.

Dr. K. Lerstrup

I just want to join the anti-M people by pointing to another end of the curve. If we take our regular curve here; if we go up in this region we find something else. The metal may creep over the surface



of the silver and then get into the reduced sections, and we have something like an extension of the curve, like that. So it is not only the short pulses that Prof. Lipski spoke about, it is also the accumulated time at rated current that can produce a bad ageing effect in the long run. But the biggest trouble comes back to what Mr. Gibson says, namely, we have standardised curves, and when you have standardised curves as we have, on rather narrow bands, then we are forced to use the M-effect in order to make it at all. So we simply couldn't get away from it, so therefore I would suggest how to get away from it on high-voltage fuses, where it is quite important to get away from it.

It is a Patent that already has run out, but if you put a striker on a high-voltage fuse, a string with a spring holding it, and you break it in the middle before you put it in and then join it together again with a low melting point solder, you can get something that at a certain temperature will trip out your switch. In other words, it's equivalent to what you do on the true motor back-up fuse, where you have the sand-filled fuse for the short-circuit protection, and you have the contactor or switch, with a kind of thermal relay to break it, and here we can put it in instead. But the important thing is that we should retain, or should we say regain the freedom of making fuses of proper characteristics, giving us a possibility of making progress in the art of making fuses and not be bound by too strict standardisation of what we produce, neither in dimensions, nor in the curves. And I should like to mention just one thing in that connection - whom of you has seen houses travel from one country to another? So why should we necessarily have a standardisation of domestic fuses? Each country is sufficiently great to support the industry supplying one type of locally standardised fuse for domestic use.

Dr. D.R. Aubrey

Short-circuit tests are used as a justification for years of production, and on test it is therefore agreeable if the clearances achieved are good ones. Mr. Turner tells us that tests are used to sort out the good from the bad, and not to overstress the good. It simply puzzles me that there seems to be no criteria of what is a successful test, other than that the fault is cleared somehow, and this seems to be a strange basis for years of production. The point of view seems to be that on test if it clears somehow, it will be quite satisfactory because service conditions are much easier. Therefore I ask Mr. Gibson or the ASTA gentlemen, what are the criteria for a successful test, and are those criteria sufficient?

Mr. B.M. Pryor

I would like to express the users view on tests at 87% of rated voltage for test duties 1 and 2. In service, under 3-phase conditions the first fuse to clear will see up to 1.5 times the phase voltage, then after that we will have two fuses in series left to clear the circuit. In many cases under 3-phase conditions one of those fuses will not blow, and the second fuse to clear will in fact see the full line voltage across it. Now I would like to ask what justification there is for tests at 87% of the rated voltage and whether any tests have actually been done under 3-phase conditions with a fuse designed specifically on the basis of the test at 87% of the rated voltage.

Mr. H.W. Turner

Just before the authors reply, I just want to take up one point Dr. Aubrey raised. I didn't say that the fuses would pass anyhow. The point I raised concerned the testing of miniature fuses, of the higher breaking capacity type. The bad ones tested at the earlier closing angle exploded in the test, while similar ones passed when tested at a later point-on-wave, more severe for good ones. What was really wanted was a test that picked out the bad ones rather than putting a slightly higher arc energy through a good one.

The criterion with these particular fuselinks is that they should be completely intact at the end, and other requirements in the standard which virtually come to similar requirements to ASTA criteria for passing high-voltage fuses.

Prof. T. Lipski

In East Germany, in the standard for low-voltage fuses they have 3-phase short-circuit tests. This means that the conditions are not so severe as we have in IEC and many other countries.

Mr. J.W. Gibson (in reply)

Mr. Smart refers to the test voltage for the breaking discharge test. The new standard will suggest that for current-limiting fuses it it twice times, subject to some small tolerance, because in capacitor work tolerances are looked at rather differently, but in principle it's twice.

For the expulsion fuse, there were a lot of talk about that. It was pointed out that an expulsion fuse when on a discharge test can have two boundary conditions, depending upon whether the element is high or low current rating. If you have a fault in the unit, most of the energy in the paralleled healthy units will be released in the fuse if it is of low current rating, and most will be released in the faulty unit itself if the fuse is of high current rating. So it was said that both the unit and the fuse must be capable of withstanding the energy associated with twice peak voltage. But then the argument came about that the can wouldn't stand it, so the compromise was put up, and discussed for a long time that it should be 1.6, but finally it was whittled down to 1.2, which is the figure which will go in when it is printed. I think it's a pity, if only because as a fuse manufacturer I wouldn't find the figure of 1.6 at all restrictive. Modern designs of high-strength tubes for expulsion fuses would permit compliance with 1.6 and we feel it might be of use to the capacitor manufacturer.

Why was the frequency of the breaking discharge test chosen to be proportional to the rated voltage? Well, the capacitor people who advised us said that if you have a given can, of a given kVAr, and you double the voltage, that means that the rated current is halved, and so the capacitance is one-quarter. Since the inductance of the connections is relatively constant, the frequency is then inversely proportional to the square root of C, and so will be doubled. That seemed to have general approval, we had no adverse comments upon that proposal from the committees of any country.

I agree generally with Mr. Norton's comments.

I was pleased that Mr. Jacks and Dr. Lerstrup both caution against over-enthusiasm on standardising dimensions.

On Dr. Aubrey's point regarding the criteria of failure, I feel that it is not necessary to specify these very elaborately in testing a current-limiting fuse or any other fuse. A fuse is a GO/NO GO device - it either works properly or goes off with a tremendous bang, but on the other hand there are criteria laid down in the IEC specification. They must not emit flame or powder and the components other than the fuse-link should be in the original state and it should be possible to remove the fuse-link in one piece after operation.

At one time in the British Standard we had a stipulation about the insulation resistance of the blown fuse, but that has been withdrawn as being unnecessary.

Mr. Pryor asked why 87%, and have any tests been made under 3-phase conditions to prove that 87% is sufficient. I think that Dr. Lerstrup mentioned this morning that this is rather a grey area. When the 87% was first proposed in IEC, we were told by the proposers that it was only a coincidence that it was $\sqrt{3/2}$, it was nothing to do with the 87% voltage on the first phase to clear because that doesn't apply to a device like a current-limiting fuse that doesn't clear at a natural current zero, but the argument was that although two fuses only may blow on a 3-phase fault, if one or other of them is having some difficulty the arcing will be prolonged and that will bring in the third one, which will then assist. That's what we were told by the proponents of it, who seemed to be in the majority. In the U.K. we were not very much in favour of it, although now it's there we are rather in the hands of users. If users agree to 87% we think it's a good thing, they get a standard fuse in accordance with IEC and BS and so on, but if they want 100% well then the customer is always right and we give them that. We have not had any adverse effect through using the 87% and on the technical side, some test results were produced in IEC many years ago, seeming to show that you needn't use 100%, but they weren't very complete. There is another point, that on the maximum arc energy test, if that were applied to a 3-phase group, it would be difficult to imagine that one fuse would remain unblown. It's only on a heavier current that that could happen.

Prof. Lipski's comment that 3-phase tests are used in East Germany (for low-voltage fuses) is very surprising. This strikes me as an enormous easement. I know that on the high-voltage side, in discussing it with the West German delegates to IEC about a year ago they told us that they weren't very happy about keeping the single-phase test. We told then that we used to have 3-phase tests with one-phase solidly linked in our specifications but they were nothing but a nuisance. No one could ever interpret them.

Mr. J.G.P. Anderson (in reply)

I shall respond to your kind treatment of the ASTA paper by being equally brief, in dealing with I think only two points which emerged.

Firstly, it is interesting to see and to experience Mr. Jack's change of view, of putting himself in the attitude of the customer by asking himself 'how does he know that he is getting what has in fact been certified?'. This of course is a very important matter and is dealt with only in one paragraph in the paper, but nevertheless it is covered in great detail in the new STL guide which is being prepared for high-voltage fuse testing which should emerge hopefully early next year. In the meanwhile, pending the issue of the STL guide for fuses, ASTA also has a publication dealing with this in great detail.

Referring now to Dr. Aubrey's comment on what are the criteria for successful test, it depends upon one's point of view, whether one is being creative or destructive, optimistic or pessimistic. As you know, standards, no matter whether IEC or British, tend to define the verifications required for problem areas, either in performance, design or some other characteristic, of the product being prescribed. Strangely enough - I don't know whether Dr. Aubrey has looked at it from this point of view - having prescribed what verification testing it necessary for a product. all standards then describe the criteria for failure, not the criteria for success. So if he applies a negative point of view I would suggest that he has the basic criteria for success.

Mr. W.R. Crooks (in reply)

To be the last may be the least, but it is to have the last word. So far, I count the votes on M-effect - three for, two against. I declare the motion carried.

First of all I thank Mr. Rosen for having answered most of the questions on my paper, but there are one or two points I will take up.

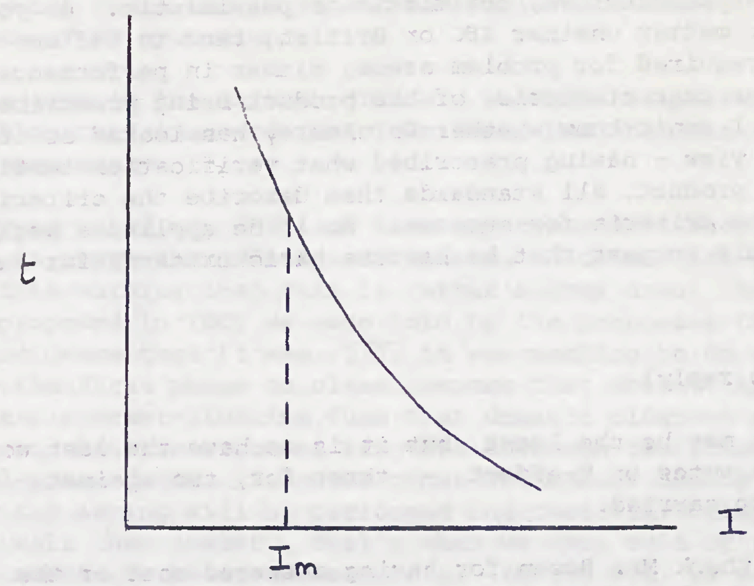
Dr. Vermij's point about inspection of tools is of course important, and this is covered in the programme, but also it is covered by the inspection of material made by those tools, on the normal acceptance basis. His remarks about M-effect can be coupled with those of other speakers and I would like to look once again at Prof. Lipski's diagram.

If we draw his curves again, this one he said was for $n = \infty$. Who is interested in $n = \infty$? The only occasion that you will some anywhere near to that is for a motor-circuit and we are taking of the motor-starting region here. Then we choose the fuse accordingly. I would suggest that he should also have drawn another curve, for $n = \infty$ for the pure element material, because it would not be the same as for $n = 1$, as we have shown very well with our experience with motor fuses, where the failure will eventually occur by mechanical means.

On Dr. Lerstrup's remark about M-effect, the spread of the M-effect alloy under more steady-state conditions, all I would say to that is that there are means to prevent it.

I have yet to see evidence of M-effect being a problem in the service life of high-voltage fuses (I speak only of high-voltage fuses), but on the other hand, without M-effect we are certain that the problems of very high temperature rise will occur. Also, equally severe, the very real possibility that a porcelain tube will not withstand the associated temperature.

Finally, Mr. Rosen has asked me to expand a little on the combination of fuses which have striker pins and their function with trip-all-phase switchgear.



We have a minimum breaking current I_m. We know that if the fuse should melt at a current less than I_m the fuse may fail. So it is arranged that the striker pin shall operate the mechanism of the switch to provide 3-phase tripping of the switch, and the current then being within the range of the switch can be successfully cleared. It is necessary that the fuse must be capable of withstanding the effects of arcing at the particular current (less then I_m) for a period greater than the tripping and clearing time of the associated switch. Having said that, I must also say that the experience with switchgear not having trip-all-phase features is also very good, provided that fuses have a good performance.