Scaling Factor Input

[redacted]

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Summary:

I don't see a need for any additional scaling factor at all. I think this is stretching. If the need for this correction is based on arguments I made during meeting, then they are being taken out of context here. Generally, I would stick with C=1.0.

Basis:

- We do see that exposure mode is important to duration with the longer damage times implying longer durations. However, by and large the risk important scenarios are going to be flame/plume scenarios which tend to yield the shorter times. At most, one might argue a c-factor adjustment specific to hot gas layer damage scenarios only, but I don't see the practical utility of that type of approach because…
- The likelihood of fires sufficient in both intensity and duration to cause hot gas layer damage to cables is very small. These should not be dominant in the risk analysis. We have never seen this in practice to my knowledge. A special treatment for such a speculative case does not seem warranted.
- A floor has already been set for the likelihood of infinite duration signals (0.1% (ac) or 3% (dc)). This is critical to my position. These numbers actually strike me as a bit high, but I am not going to argue that here. The fact that the approach forces consideration of signals that never clear, and at relatively high probabilities means that, in my opinion, there is no need to further bias the data.
- As I understand the approach, the 'c' factor is not intended for case-specific application, but rather, is intended to generically compensate for some perception that real-plant conditions call for an adjustment to the duration curve in comparison to test conditions. I disagree given the pooling results in particular.
- The pooling results eliminated flame zone for ac and all of the radiant heating tests for dc, and these tests account for most of the very short duration signals. This pushed the duration curves towards conservatism (longer times) already. This has created a bit of a mis-match between the data used in probability and that used in duration given that the short duration faults were included when we considered the probability values. Addition of a 'c' factor would only make that mis-match worse.
- For the pooled data set of 106 values, the longest duration spurious actuation signal lasted 7.6 minutes. Given a very simple-minded view of statistics, that would tend to say the 0.01 (1-in-100) probability value should fall somewhere out in the 10 minute range for the data. By comparison, taking the extreme value of c=10 as plotted, the curve suggests a probability for

spurious operations lasting 10 minutes or greater is roughly 0.4 – a factor of at least 40 over what the data indicate. That is, frankly, absurd. There is no experimental basis for this result.

- Even looking at the c=2 curve, the 10min duration looks to be roughly 0.05 which is an increase by a factor of 5 compared to my simple minded expectations. I cannot find any justification for even that degree of adjustment given an already strong data set.

Bottom Line:

I don't think it is defensible to say on one hand that we have a strong set of test data that passed poolability tests and on the other hand imply that we have so little confidence in that data that we are going to apply a conservative correction factor to it as a generic practice. Either we trust the data or we don't. Personally, I trust the data pretty well.

Further, in my opinion, the pooling results already added a degree of conservatism such that the straight analysis of the pooled data adequately reflects plume scenarios that I expect to see as the most important contributors for fire risk scenarios. Application of additional corrections seems inappropriate and arbitrary.

The 'c' factor approach strikes me as inappropriate manipulation of the data that cannot be justified. I certainly would not support the suggested numbers of 3.5-7.8 developed based on the "anchoring" analysis approach. My own anchoring is based on looking at the probability numbers predicted for 10 minute signals. Even a relatively low correction of c=2 yields results that belie the data. I don't see how the correction factors could ever be justified or adequately explained.