LETTERS TO THE EDITOR

COMMENTS ON "WASH-1400: A COMPARISON OF EXPERIENCE AND PREDICTION"

The paper of Lellouche[1] raises an important question: What can be said about the accuracy of the WASH-1400 estimate of the frequency of core melt, given that no core melts have occurred in light water reactors to date? However, the claims in the Abstract of the paper that the WASH-1400 core melt probability cannot be an underestimation, and that the increase in WASH-1400 uncertainty can be no greater than a factor of 4, are not borne out by the test.

One of the problems lies in the interpretation of the WASH-1400 result, and in particular the interpretation of the uncertainty. The uncertainties on the parameters used to calculate the core melt frequency are of two different types: first, there is a statistical uncertainty on a parameter believed to have a unique but unknown value, and second, there is an uncertainty that characterizes a real physical variability in component characteristics from plant to plant or component to component, etc. Since both types are present in the WASH-1400 data base, it is correct to regard the distribution on core melt frequency as, at least in part, characterizing the real variation in core melt frequency between plants. However, in his analysis, Lellouche makes the assumption that all plants have an identical core melt frequency. Consequently, a comparison with WASH-1400, particularly regarding uncertainties, is inappropriate.

Moreover, an extrapolation of present experience of no core melts to the year 2000 does little but show that that assumption is consistent, subject to the reservations expressed above, with the estimates of WASH-1400. It does not, however, cast any light on the validity of that estimate. In the light of these two observations on two major elements of Lellouche's analysis, it is difficult to support his claims concerning the accuracy of the WASH-1400 analysis.

While statistics has an important role in risk assessment, imprecision in its use is confusing and can detract from the real value of the assessments.

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REFERENCE

1. G. S. LELLOUCHE, "WASH-1400: A Comparison of Experience and Prediction," *Nucl. Technol.*, **53**, 231 (1981).

REPLY TO "COMMENTS ON 'WASH-1400: A COMPARISON OF EXPERIENCE AND PREDICTION' "

In reply to Parry's comments[1] on my paper[2]: It is quite likely that the set of nuclear power plants does not form a statistical class which is truly independent and identically distributed. It is also true that the WASH-1400 study allowed for uncertainty in the failure rates thus producing bounds on the calculated median value. Finally, it is correct that the uncertainties used in WASH-1400 account in part for both limited data and plant-to-plant (or at least component-to-component) differences. The interpretation of the WASH-1400 results is therefore difficult, at best.

I do not know to what "test" Parry[1] refers, but in science there is a philosophical view that adheres to the dictum of Occums Razor (choose the simplest approach that works). In the absence of actuarial information on WASH-1400 types of core melt (indeed with a total statistical population of <300 plants), the only reasonable assumption concerning the experiential data is to assume the plants to be independent and identically distributed and to make the same assumption *vis-à-vis* the WASH-1400 calculation. Is it therefore coincidence that such an assumption at both the 50 and 95% levels leads to unit margin at about the same time? Possibly. Are the plants independent and identically distributed? Probably not, but they probably are not as diverse as Parry feels. It is recognized that complex systems often exhibit quasi-constant failure rates (exponential) even though the separate components have greatly disparate failure rates. It does not seem unreasonable (but unprovable) that the various plant-to-plant disparate failure rates for components average out (due to maintenance perhaps) to more nearly the same core melt rate than would be expected *a priori*. My little calculation appears to imply so.

Finally, I would address Parry's comment concerning the so-called accuracy of WASH-1400. I do not find any reference in my paper to WASH-1400 being accurate. I referred to the conservative nature of the WASH-1400 calculation only. I personally believe WASH-1400 to be quite conservative *vis-à-vis total* core melt frequency and that was what I was examining. I would also point out that we have some newer results, which have been submitted to the Editor, that increase the 95% margin from ~4 to ~9 (under special statistical assumptions). However, this leads to about 16 total core melts to make *that* estimate true. It is true that statistics can be abused; I don't believe I have done so.

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REFERENCES

1. GARETH W. PARRY, "Comments on 'WASH-1400: A Comparison of Experience and Predictions,'" *Nucl. Technol.*, **55**, 728 (1981).

2. G. S. LELLOUCHE, "WASH-1400: A Comparison of Experience and Predictions," *Nucl. Technol.*, **53**, 231 (1981).

FURTHER INFORMATION ON "WASH-1400: A COMPARISON OF EXPERIENCE AND PREDICTION"

In a recent paper,[1] we attempted to evaluate the effect of future reactor experience on predictions of core melt probability. The approach taken was to assume that the current bounding values obtained from the chi-square tables

$$\lambda_{\text{true}} < \lambda^*(\alpha) = \frac{\chi_2^{2\,1-\alpha}}{2T(1980)} \ , \quad \Pr[\lambda_{\text{true}} < \lambda^*(\alpha)] = \alpha$$

would be valid for all time. Using this result, the uncertainty in the WASH-1400 estimates for $\lambda^*(\alpha)$ could be shown to be at most a factor of 3.88.

Further work[2] shows that this conclusion is reasonable for $\alpha \lesssim 0.75$ but not for $\alpha = 0.95$. The new results (both numerical and analytical) can be made clear in an example. After T reactor years of experience, $\exp(-\lambda T)$ is the probability of an event having occurred. For the sake of exposition, suppose $\lambda T = 1$ implies the event occurs. If no events occur up to T_0, then $\lambda_0^* = \chi_2^2/2T_0$. Accepting λ_0^* as the failure rate, then an event should occur by $T = T_0(1 + 2/\chi_2^2)$, which yields a new estimate for $\lambda^* = \chi_4^2/2T_0(1 + 2/\chi_2^2)$. By induction, the time to r events is

$$T_0 + S_r = T_0 \prod_{i=0}^{r-1} \left(1 + \frac{2}{\chi_{2i+2}^2}\right)$$

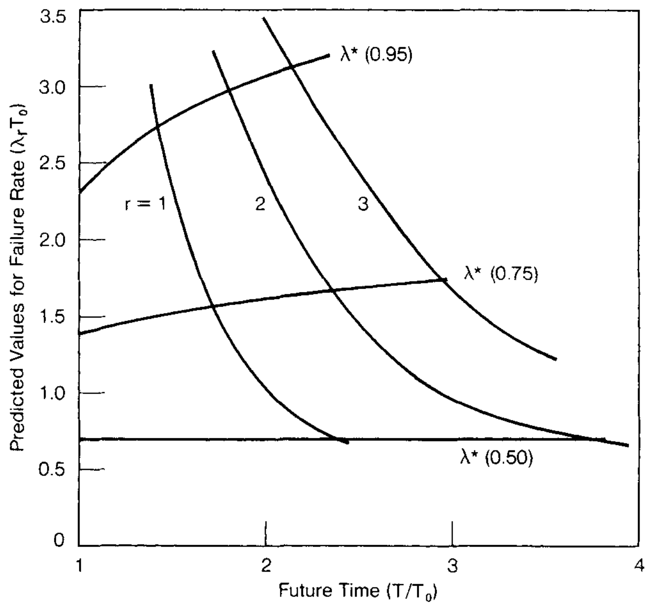

Fig. 1. Predicted values of failure rate estimates.

and the failure rate estimate at the end of the interval is

$$\lambda_r^* = \frac{\chi_{2r+2}}{2T_0 \prod_{i=0}^{r-1} \left(1 + \frac{2}{\chi_{2i+2}^2}\right)} \ .$$

Inherent in this is that both λ_r^* and S_r are functions of α, the time to r failures being much greater for low values of α than for high values. The relation between r and T and the prediction of $\lambda^*(\alpha)$ is shown in Fig. 1. A very interesting result is that at T_0 we have the estimate

$$\Pr[\lambda_{\text{true}} < \lambda_0^*(\alpha)] = \alpha \ ,$$

but using $\lambda_r^*(\alpha)$ as the estimate for the following time interval indicates that $\lambda_r^*(\alpha) > \lambda_{r-1}^*(\alpha)$ for $\alpha \gtrsim 0.75$. This disrupts the probability estimate. This has interesting implications concerning the very conservative nature of this type of extrapolation.

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REFERENCES

1. G. S. LELLOUCHE, "WASH 1400: A Comparison of Experience and Prediction," *Nucl. Technol.*, **53**, 231 (1981).

2. Anon, "Analysis of Extrapolated Failure Rates," EPRI NP-1892-SR, Electric Power Research Institute (June 1981).