

TO: Mr. Wade ~~Muller~~ *Moeller*
Chairman of LER Investigation Subcommittee of ACRS

FROM: Sam C. Saunders, Consultant

DATE: April 17, 1980

SUBJECT: Review of NRC Staff's Comments on Appendix E, Statistical Analysis of LER's: A Trial Study, within NUREG-0572

After reading the comments in the letter from R. H. Moore and D. Rubenstein, my reaction is as follows:

I agree with their criticisms. In general, conclusions are not worth reading, unless the assumptions by which they are derived, are clearly stated. The underlying model and probabilistic assumptions (such as independence) are not explicitly stated in section E of NUREG-0572. Ergo.

The only statement I found in this regard was page E-3, line 10 from bottom, "Throughout this study a Poisson distribution of events is assumed." Thus the flaw that Messrs. Moore and Rubenstein have uncovered is all the more incisive, namely, while the distribution of the number of LER's, say N_t , occurring within a period of length $t > 0$ is given by the Poisson probability

$$P[N_t = n] = \frac{e^{-\lambda t} (\lambda t)^n}{n!} \quad n = 0, 1, 2, \dots$$

where λ is the true (expected) rate of occurrence per unit time, the authors of section E of NUREG-0572 did not adjust calendar time to obtain exposure time! This is like having someone say he is using Duhamel's theorem but you find out subsequently he didn't add correctly.

I believe that the altered table in Messrs Moore and Rubenstein's letter places substantial doubt about any conclusion reached in the report, which depends upon an inference derived from the Poisson probability distribution, unless one has access to the NRC Grey Book to verify that the times utilized are the correct ones.

I am not only embarrassed, I am furious with myself for having assumed that such an elementary error could not have been made and that I need not have checked it!

Furthermore upon reading the entire NUREG-0572, my comments are: Despite the criticism in my earlier letter to you about section E of the draft report, for its confusion in the discussion between causality of events and the mathematical description of the stochastic process by which they occur, the report remains virtually unaltered in this regard. For example, some uses of "on the basis of randomness" are:

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p, E-1, para. 2
p, E-3, section (2), line 1-
p, E-8, para. 4, line 4.

(While you may think the language is unimportant, it gives me the same sense of unease as would a chemist using the word "phlogiston" in an explanation of heat.)

Randomness does not cause any variation within the reported rates! In fact, randomness does not cause anything. (Neither does "randomness": p, E-1, para. 2, line 7.) All events have a cause. The cause is not related to the probability law by which wagers as to frequency should be placed. Causes do not appear in the formula of Poisson, nor for the same reason does cause of death appear in actuarial contingency tables. Moreover, if any one cause should be entirely suppressed through an analysis of LER's and subsequent corrective action, only the rate of future production would be diminished. The law would be the same, namely Poisson, if it were so originally.

While it is true that nuclear generating plants having the same intrinsic reliability (rate of LER occurrence) will in the short run, most probably exhibit failure data (i.e. generate LER's) at different observed rates, it is even more probable that failure data from different plants exhibiting different average rates of occurrence for a short time will remain different for all time. The same is true if "statistically significant difference" is used instead of "difference" in the above. (This is an opinion based on the fact that virtually all hospitals have different patient survival rates, and virtually all airports have different safety records, to the chagrin of their operators. Why should nuclear plants be any different?)

In my opinion, the principle error of the report is the assumption that unless reporting rates can be proved to be significantly different, with say 90% certainty, they should be assumed to be equal. While such a procedure lowers the probability of the false alarm (saying there are plant differences when there are not) it raises the probability of neglected early warning (saying there are no differences when there are). I believe it is the latter which is the more serious type of error. On this point, I believe the report is like a "sales pitch" to Congressman Udall, rather than an objective scientific magazine.

Lastly despite these critical remarks about its shortcomings, the collection, reviews and collation of LER's (1976-1978) as performed in NUREG-0572 was, in my judgment, a valuable and worthwhile service not only to the industry but to the nation. After all Appendix E is only about 13 pages long and comprises approximately 1/5 of the total report.

SCS:pw