Reply to "Comments on the Bayesian Method for Estimating Reactor Core Melt Frequency"

The letters by Easterling[1] and Martz[2,3] have convinced us that we are guilty of not following our own advice given in the last paragraph of the introduction to our paper[4]: "It is important to bear in mind that the merit of this study is not so much in the accuracy of the numerical values produced, but in the philosophy behind the method, which forces the analysts to consider in quantitative terms some important aspects of the problem of quantification of judgment and the use of expert opinions." If we had done a sensitivity study and presented several posterior distributions, perhaps we would not have been subjected to this criticism.

Our intent is not to produce a definitive distribution for the frequency of core melts. Rather, we wish to demonstrate how an expert's opinion can be formally handled and what difficulties arise in doing so. Having observed much confusion in the on-going debate about risk assessment concerning what is subjective and what objective, we take the position that coherence is objectivity and we demonstrate that Bayesian methods force the analysts to make explicit their judgment and to show how they comply with coherence. This, we believe, is an essential step, if we are ever to agree on anything that involves rare events. In this respect, at least, we have succeeded. By being explicit and quantitative in the process of deriving our posterior distribution, we have made it possible for Martz and Easterling, as well as other colleagues, to tell us also explicitly and quantitatively where they disagree. In fact, as a result of these comments, we now think that indeed our prior distribution gives more weight to very small values of the frequency than our state of knowledge could justify.

Having stated our understanding of the concept of objectivity, we wonder what Easterling[1] means by "objective accommodation of the critics' opinions." The series of arguments that we have given constitute our effort to be objective. Of course, others may disagree, but now they must be as specific as we are. If they do this, then the debate will become scientific and not emotional and we will be coherent. This is objectivity and nothing else.

Easterling and Martz also claim that we have "inexplicably" reversed the role of the ("objective") data and prior opinion. We restate our position that was given in our previous reply to Martz.[5] Bayes' theorem simply incorporates the information contained in new evidence (represented by the likelihood function) into our state of knowledge (represented by the prior distribution). Both the prior distribution and the likelihood are subjective distributions. Perhaps this point is obscured in the particular application of the paper, but one wonders how Martz and Easterling would treat the Three Mile Island accident to derive an objective likelihood function.

Even if we accept their argument that the data properly belong in the likelihood function (Poisson distribution), the latter is still an expression of a significant subjective decision on our part, namely, that the rate of occurrence of core melts is constant. In light of what we know about plant-to-plant variability and the evolution of the licensing process, it seems to us that, before we reach a meaningful distribution for λ, this assumption (and possibly others) should be carefully scrutinized.

We do not wish to give the wrong impression that we do not like to see the data in the likelihood. We do think that that is where it usually belongs and we have done so in many other applications of Bayes' theorem. What we object to is calling what we have done in the paper incorrect. The disagreement is really on our particular choice of prior distribution.

As we have stated, we now believe that our prior distribution was strong on the low side. The extra check for coherence that we neglected to do was to test each value of the posterior distribution listed in Table II of the paper against what we had stated earlier. Then, the value of the mode (1.8×10^{-5}) would (hopefully) have led us to Easterling's conclusion that "... the meager 0.03/310 data nearly offset the fairly strong assessment that WASH-1400 was most likely off by a factor of 10."

Let us now compare our numbers with those of Easterling and Martz to understand better what all this means. Their posterior distribution is gamma with $\alpha = 2.03$ (or 2.00, which is immaterial) and $\beta = 6977$. We have

	λ_{05}	Mode	λ_{50}	Mean	λ_{95}
Our paper	10^{-5}	1.8×10^{-5}	1.2×10^{-4}	1.7×10^{-4}	5×10^{-4}
Martz and Easterling	5.1×10^{-5}	1.4×10^{-4}	2.4×10^{-4}	2.9×10^{-4}	6.8×10^{-4}

The major difference is in the modal values and we have agreed that ours is an underestimate. Remembering that this is risk analysis, the changes in the other values are not very significant, although the trend is upward, as Martz points out. Of course, others may have different opinions about the accuracy of the results of WASH-1400, in which case they may get different numbers.

Finally, we think that Easterling is unfair to Rasmussen. In his testimony[6] before Congress, Rasmussen quoted only our 95th percentile (among other estimates), which is not very much different from that of Easterling and Martz, and he also stated: "It should be pointed out that the authors had to make some subjective judgments that others doing the same analysis might have made differently, leading to a somewhat different result."

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[1]R. G. EASTERLING, *Nucl. Sci. Eng.*, **75**, 202 (1980).
[2]H. F. MARTZ, Jr., *Nucl. Sci. Eng.*, **72**, 368 (1979).
[3]H. F. MARTZ, Jr., *Nucl. Sci. Eng.*, **74**, 158 (1980).
[4]G. APOSTOLAKIS and A. MOSLEH, *Nucl. Sci. Eng.*, **70**, 135 (1979).
[5]G. APOSTOLAKIS and A. MOSLEH, *Nucl. Sci. Eng.*, **72**, 364 (1979).
[6]Statement of Norman C. Rasmussen before the Subcommittee of Energy and the Environment, House Committee on Interior and Insular Affairs (Feb. 26, 1979).