How many nuclear reactor accidents?

SIR—My complaint[1] about Islam and Lindgren's procedure[2] for estimating the probability of a future nuclear accident was that they had ignored the distinction between probability and likelihood, thereby committing a logical error which had led them to estimate the probability with unjustified precision.

Subsequent correspondents have not taken this point. Schwartz[3] integrated the normalized likelihood function in order to obtain what he called a "confidence limit", implicitly adopting a bayesian approach with a uniform prior distribution for r, the rate parameter of the Poisson process.

Fröhner[4] suggested that I had disregarded "the correct prior" when in fact my point was that no prior is correct, and that it is wrong to apply bayesian methods to this problem.

Chow and Oliver[5] stated that I had implicitly used a uniform prior distribution for $P = 1-e^{-rT}$, the probability of one or more accidents in T reactor-years, "which is equally dubious" (that is, as dubious as using a uniform prior for r). But I did no such thing, and once again there has been a failure to distinguish between likelihood and probability. (They went on to consider further information which was not in the specification of the original problem and on which I will not comment here.)

I must emphasize that a graph of a log-likelihood or 'support' function such as I presented is not a probability distribution, cannot be integrated, and does not depend on any bayesian prior distribution. Likelihood analysis has a long history[6], stretching back well before the recent resurgence of bayesianism which is sweeping through physics. The reactor example is an excellent one for clarifying the difference between the two types of analysis.

Fröhner mistook what the argument is about. It is not about how to choose an "uninformative" prior (by using "modern techniques for the assignment of prior probabilities which have a solid foundation in either group theory or information theory"), but about whether it is justifiable to use a probability distribution to represent ignorance at all. Nothing in group theory or information theory addresses this vital point.

R. A. Fisher[7] stated in this connection that "no experimenter would feel he had a warrant for arguing as if he knew that of which in fact he was ignorant." Bayesian risk analysis is contaminated by this logical fallacy. Physicists have been seduced by its extreme elegance and simplicity, those quite proper arbiters of physical theories; but theories of inductive inference are not to be selected on such grounds.

Bayesian risk analysis is but Laplace's Rule of Succession cloaked with the modern trappings of group theory and information theory, but it is no better founded than it was in 1854 when George Boole[8] observed that the appearance of the arbitrary constants which are characteristic of bayesian theory "seems to imply, that definite solution is impossible, and to mark the point where inquiry ought to stop."

Surely the last thing to which we should apply doubtful logic is the recurrence risk of nuclear accidents.

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Schrödinger and *What is Life?*

SIR—In his commentary (*Nature* **326**, 555–558; 1987) on Schrödinger's book, *What is Life?* Max Perutz concluded that "a close study of the book and of the related literature has shown me that what was true in his book was not original, and most of what was original was known not to be true even when it was written". Furthermore, "In retrospect the chief merit of *What is Life?* is its popularization of the Timofeeff, Zimmer and Delbrück paper that would otherwise have remained unknown outside the areas of geneticists and radiation biologists". As someone who spent some years around 1950 working with both Schrödinger and Delbrück, I would like to comment briefly on these strong criticisms, which are quite wrong in my view.

In *What is Life?*, Schrödinger managed in a slim volume to resurrect the ideas of Timofeeff, Zimmer and Delbrück, indicating that genes must be considered as large molecules, to liken these molecules to an aperiodic crystal, to introduce the idea of a genetic code, and to discuss some of the thermodynamic problems posed by these ideas with regard to the stability of living systems. Not all the ideas may have been original, or all strictly correct (after all Schrödinger was a physicist discussing biological problems) but what the book did was to formulate these ideas in such a way that they gave a sense of excitement about the future perspectives of biology to established biologists as well as to non-biologists, students and laymen. This is the chief merit of the book and the reason why it has become one of the few classic volumes in popular science.

This ability to conjure up vivid imagery, and Perutz's failure to appreciate its merit, is reflected in that part of his commentary which deals with Schrödinger's introduction of the "famous hypothesis that the gene is a linear one-dimensional crystal, but lacking a periodic repeat: an aperiodic crystal". Perutz goes on to wonder why Schrödinger did not adhere to Delbrück's much better formulation of "a polymeric entity that arises by the repetition of identical atomic structures". The reason seems clear. Schrödinger's presentation was aimed at a wide audience and created a picture that even today manages to stimulate new readers. Delbrück was writing a scientific paper, and although his version was possibly more correct it was also eminently forgettable.

Where Schrödinger admittedly did go wrong was in the arguments that led him to postulate that "Living matter, while not eluding the laws of physics as established to date, is likely to involve other laws of physics hitherto unknown which, however, once they have been revealed, will form as integral a part of this science as the former". It is important, however, to realize that this conclusion was not based, as implied by Perutz, on the supposedly unpredictably erratic behaviour of a small molecule such as a gene, but on the problem of biological order, by which in Schrödinger's words, "a single group of atoms existing only in one copy produces orderly events, marvellously tuned in with each other and with the environment according to most subtle laws". Schrödinger's contribution to the debate on order in biological systems has been discussed by Jacob (*The Logic of Living Systems*, Ch. 5, Allen Lane, 1974), and more recently in a perceptive review by Yoxen (*Hist. Sci.* **17**, 17–52; 1979).

The idea that new physical laws might be needed in order to explain the behaviour of living systems was not unique to Schrödinger and persisted for a long time after the publication of *What is Life?* In 1958 I was working at the Massachusetts Institute of Technology, and Bohr, who was then 73, came and gave a special series of lectures ranging over physics and philosophy. As part of the festivities, Delbrück (who was viewed with varying degrees of adulation, affection and awe by scores of scientists who had been at the California Institute of Technology) was asked to deliver a lecture dealing with Bohr's influence on biology. In this lecture Delbrück (who was a disciple of Bohr) first reviewed the then current research of Benzer on the fine-structure analysis of the rII genes of phage T4, and went on to argue that as the analysis de-