Evaluating nuclear accidents

SIR—Your news columns reported a "scale of severity" to evaluate nuclear accidents, as proposed by the French Industry Minister Alan Madelin[1]. Madelin's scale is intended to allow a layman to appreciate the degrees of risk involved in designing large, risky projects, but has been criticized as giving a false image of objectivity. His six-level scale is defined mainly in terms of the extent of pollution risk, which, in the case of nuclear reactors, corresponds dominantly with the amount of radioactivity released during a potential accident. But a more serious shortcoming in Madelin's scale is that it omits smaller accidents from consideration and thereby reduces the number of datapoints. This makes a statistical approach difficult and minimizes the scale's predictive power.

A year ago, Hsü[2] proposed a multilevel scale based on a magnitude/frequency relationship developed originally to predict the probability of natural catastrophes ranging from simple landslides affecting only a small area to impacts of 10-km-diameter meteorites (or comets) capable of significantly altering the Earth's biota[3]. Hsü also pointed out that if the radioactivity released is used to express the magnitude of an accident, many smaller, non-polluting incidents such as 'operational disturbances' or temporary 'shut-downs' would be excluded from statistical analyses[2]. This would significantly reduce the number of available datapoints. Because the reliability of statistical methods depends on the availability of sufficient observational data, any filter that rejects too many datapoints, including Madelin's scale, is unlikely to be as useful for predictive (and therefore practical) purposes as Hsü's scale which is based on the amount of money lost (or the extent of damage expressed in monetary units) during an incident. As Hsü points out, for the smallest incidents, the financial losses (involving, for example, only the monetary equivalent of the electricity not produced while the plant is shut down) provide a datapoint, although no pollution need occur. For accidents involving minimal radioactive pollution, Hsü suggests that the potential of future occurrences of cancer should also be considered. Loss of lives could be quantified by using insurance premiums or damage payments in law suits.

The big advantage of Hsü's scale over Madelin's is that it makes it possible to add numerous points to our databank to render a statistical assessment of nuclear accident probabilities possible and reliable. As Hsü himself points out[2,3], the success of the method is well-known in the earth sciences and has been employed for some time in predicting the probability of the occurrence (repeat time) of earthquakes. In the light of such a statistical treatment of natural events (including catastrophes), for example, the old debate between uniformitarian and catastrophist schools seems futile unless the magnitude of events considered is specified.

When potential long-term dangers of fossil fuels, such as a catastrophic increase in the CO_2 content of the terrestrial atmosphere (ref. 4, fig. 9-3) are seriously debated and by some considered to be perhaps greater than the risks entailed by nuclear energy[5], it is imperative that a scale such as that of Hsü with a powerful prediction potential to foretell realistically the likelihood of nuclear accidents is employed by all organizations having the relevant data and that the results be submitted to the public. Only then can we discard imaginary fears and over-optimistic hopes and lay the foundations of a realistic energy policy for the future.

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"Artificial" HIV?

SIR—Fukasawa and colleagues (*Nature* **333**, 457–461; 1988) suggest that descendants of the ancestral human immunodeficiency virus/simian immunodeficiency virus (HIV/SIVs) "diverged gradually in concert with the evolution of primates, resulting in the occurrence of species-specific viruses in primates". Although they acknowledge that there may have been some interspecies transmission, the essence of their hypothesis is that there has been species specificity of HIV/SIVs in primates for a long time.

Although this conclusion is consistent with the hypothesis that most primate lentiviruses in existence today are species-specific, and diverged gradually over many millions of years, it does not seem to apply to all of them. If HIV-1 and HIV-2 have diverged during human evolution, why is the nucleotide sequence of HIV-2 much closer to SIV_{MAC} than to HIV-1? Undoubtedly there has been species specificity of SIVs in nature for a long time, but why has SIV_{MAC} never been found in macaques in the wild — only in primate research colonies?

Where are the two lost tribes that kept themselves and their viruses isolated from the rest of mankind for hundreds of thousands of years until the 1960s? An ancient virus of humans which makes people persistently infectious for life, and spreads as rapidly as HIV-1 has spread in Africa, the United States and Europe in the past decade, would inevitably have spread throughout mankind long ago. The proper place for the lost tribe theory would seem to be in a science fiction novel such as Conan Doyle's *The Lost World*, rather than in *Nature*. The hypothesis of Smith and colleagues (*Nature* **333**, 573–575; 1988) that HIV-1 may have evolved by natural selection from its common ancestor with HIV-2 as recently as 40 years ago is much more consistent with experimental and epidemiological evidence.

All molecular biologists seem to take it as axiomatic — at least in public — that HIV-1 must have evolved entirely by natural selection. Why? If it is possible that the pathogenic primate lentiviruses (HIV-1, HIV-2 and SIV_{MAC}) could have evolved naturally from a common ancestor since 1948, it must also be possible for them to have evolved by artificial selection from the same common ancestor starting even more recently.

The fact that it is possible, indeed quite simple, for HIV-1 and HIV-2 to have been created by artificial selection, and the AIDS epidemic started deliberately, does not necessarily mean that this actually happened. But it is a possibility that deserves serious critical analysis by the scientific community — in public.

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Too much secrecy

SIR—A remarkable feature of the present system of anonymous refereeing in scientific publications is that it appears to give the referees power without responsibility. It is unprecedented in any other public human activity to have a reviewing body with whom the protagonist cannot directly dispute in front of his ultimate audience. Contrast this with a typical seminar or conference presentation: any questioner can openly raise his point with the author. The author's response must likewise be open and free from personal innuendos, evasions and irrelevancies. In sport, the umpires must give their impartial verdicts in full view of millions. Travesties are obviously found out. In the judicial process (in civilized countries), a citizen has a right to hear the charges and to refute them openly.

Yet, what do we have at the moment? A system set up, almost as if by design, to propagate COWDUNG (conventional wisdom of the now-dominant group, to quote C.H. Waddington). The deplorable but real tendency of some to hide behind anonymity to write an incompetent, or worse, a motivated report can be inhibited only if the system is changed. Unlike umpiring in sport, there is a