Evaluating nuclear accidents

SIR—Your news columns reported a “scale of severity” to evaluate nuclear accidents, as proposed by the French Industry Minister Alan Madelin¹. 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, Hsü² proposed a multi-level 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 biota³. 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 analyses². 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 earth-

quakes. 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 CO₂ 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 energy⁵, 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

powerful conflict of interest inherent in scientific refereeing, as a referee's own work is often the target of an author's article.

We scientists owe it to ourselves and to our financial patrons to be more open and equitable in the critical assessments of non-consensual views and results. Should we perhaps consider a system of completely open refereeing in which the author is allowed to publish his article together with the signed reports of the referees (if the latter are critical) if he so wished? There would still be some people who might publish and risk their reputations despite the openly stated criticism of experts. This can, however, do no harm in the long run as only truly invariant results and concepts would stand up to the relentless scrutiny by many independent researchers. Popperian falsification tests would consign the failures to the scrap heap anyway.

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Ethical bounds

SIR—In a recent issue (*Nature* 334, 560; 1988), members of the Department of Cell Biology of the Max Planck Institute in Göttingen express the opinion that our study on the activation of the human embryonic genome (*Nature* 332, 459–461; 1988) "oversteps the ethical bounds of scientific work". They also propose a moratorium on work on human pre-embryos until ethical boundaries have been established.

In the United Kingdom, ethical boundaries for work on human pre-embryos and a regulating authority to supervise them were established in 1985¹. A proposal for the research work reported by us was submitted to this licensing authority and was approved and funded by the Medical Research Council of Great Britain. These circumstances were acknowledged at the end of our letter.

We have each considered the ethical issues raised by our work carefully over many years, and after discussions with theologians, lawyers and philosophers not only believe this work to be ethical but, in common with many respected scientists in this country², believe that limited and controlled systematic research is needed and should be encouraged.

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2. *Human Embryo Research — Yes or No?* (eds Bock, G. & O'Connor, M.) (Ciba Foundation, London, 1986).

Outlandish claims

SIR—It would be a pity if a journal such as *Nature* could not find time and space for a serious appraisal of such claims as those of Benveniste and his colleagues (*Nature* 333, 816–818; 1988). But, in my opinion, it is absolutely essential that in the first instance outlandish claims should be assessed critically by means of the most scientifically valid and relevant laboratory approaches available in that field at that time.

Let me give you an example, also from the allergy field, of which I have personal experience and which (needless to say) is frequently subject to such claims. A few years ago a reporter from the local paper (*The Birmingham Post*) sought my opinion about a story that he had picked up in Malvern, that hay-fever sufferers there were being cured by eating beeswax capping obtained from a local bee-keeper. It seemed to me just possible that such people were imbibing small amounts of pollen products within the beeswax, and this was leading to their auto-desensitization. But the problem with this explanation was that their hay fever was caused by hypersensitivity to common grass and tree pollens, whereas the bees were collecting flower pollen. Nevertheless, we set up a small pilot study in our laboratory to explore this possibility, involving the monitoring of levels of antibody (sensitizing and protecting) in the allergic individuals' circulation in relation to any change in their clinical status. This preliminary study led to a full-scale 'double-blind' investigation of the similarly claimed efficacy of pollen B tablets (available over the counter from healthfood shops and chemists) in the treatment of hay fever.

In neither study did we find any experimental evidence, based on our current understanding of the mechanism underlying the immunopathology of allergic responses of the hay-fever type, to suggest that such forms of self-treatment were having any beneficial organic effect. But we had at least made the effort to investigate the claims in a rational scientific manner. Similarly, it would be possible to appraise, for example, the extravagant claims of certain clinical ecologists that over 90 per cent of cases of rheumatoid arthritis are attributable to a food allergy, because a whole battery of laboratory methods of assessing the onset and progression of this disease are now readily available.

Turning to the surprising claims of Benveniste and his colleagues, surely here too it is first necessary to assess them by the most reliable and relevant laboratory procedures currently available in this area. This means that, for a start, one would choose the mast cell rather than the basophil as the effector cell in question because mast cells can be obtained in

highly pure form in high yield, and one would therefore expect much less inter-experimental variation. Furthermore, one would measure the release of histamine (or some other mediator), while at the same time measuring the release of a cytoplasmic marker enzyme such as LDH, to ensure that the effect observed was non-cytolytic and therefore really attributable to the immunological triggering of the effector cell and not due to some artefact, rather than continue to struggle with the attempted measurement of the notoriously unreliable degranulation endpoint.

It is because this technique is so unreliable that the so-called new microscopic test for detecting allergies of the hay fever/asthma type based on this principle (reported by Shelley and Juhlin in *Nature* 191, 1056; 1961) never caught on in routine clinical immunology laboratories. Despite this, Benveniste has persisted in developing a more elaborate assay kit which, in the experience of those of us who have had the opportunity to assess it critically, appears to suffer from many of the experimental shortcomings of the attempted accurate quantitation of basophil degranulation referred to in the report that followed your recent visit to Benveniste's laboratory. The explanations for this experimental variation are manifold: basophils are extremely fragile cells, readily degranulated by nonspecific stimuli; they are present as only a minor population of peripheral blood leukocytes; the amount of IgE antibody bound to their Fc receptors differs from individual to individual, and so forth.

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SIR—While I am highly sceptical of the claims made in the article by Benveniste and his colleagues, the work was nonetheless appropriately conducted with seemingly reasonable controls and reported in scientific fashion. There may well have been errors in methods and/or interpretation as regards the effectiveness of their dispersion procedures, but the scientific community has well-tested standard methods for investigating such claims. These methods do not require the use of journalists or magicians and their inclusion on an investigating team is demeaning to the scientific process. It is also highly unusual to include a specialist in 'misconduct in science' when there have been no allegations of such.

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