Letters

Radon Hazard

Richard A. Kerr reports (News & Comment, 23 Sept., p. 1594) on the Environmental Protection Agency's (EPA's) announcement of their latest radon survey in an article entitled "Radon survey seen as misleading by some scientists."

Both Lee Thomas, Administrator, EPA, and I emphasized at our press conference and in subsequent discussions with the media that EPA was reporting screening tests done usually in basements during the winter when the readings were likely to be highest. We both emphasized that before individual risks could be estimated and any appropriate action taken, additional tests in the living areas of homes were necessary. While the screening tests cannot estimate individual risk, I do not believe they are "nearly useless." With screening tests that are quick and inexpensive (72 hours and about $10), individuals can determine whether or not their homes have a potential problem. If not, as is the case with the majority, the concern can be dismissed. If the screening test is positive, then more definitive tests of longer duration that cost somewhat more (3 months to 1 year and $30 to $75) in the living area are necessary.

In addition to emphasizing the screening nature of the tests to the media, I further emphasized the synergistic relation between smoking and indoor radon risk. I urged people not to smoke and recommended that, if their home had radon in it, they prohibit smoking inside their home. This was generally picked up by the news reports, but there was no reference to it in Kerr's article.

Using data we have for humans and animals—epidemiologic studies of more than 40,000 miners, including both smokers and nonsmokers, with what comes close to providing dose-response relationships, and applying sound scientific judgment (not just rote mathematics), can estimate, although not precisely determine, the risk from radon. In most other environmental areas we have nothing close to the quality and quantity of data that exist for radon. If we cannot use these data to recommend that individuals take preventive measures—lowering the radon in their homes where it is elevated, not smoking, and prohibiting smoking in their home—where are we in estimating other environmental risks for preventive action?

In my opinion, to equate the risk from radon to that of death from falls and fires in the home at a 0.4% lifetime risk appears to trivialize the radon risk. In fact, annually nearly 6000 people die from falls in the home and 5000 die from home fires. The number of deaths from falls and fires is not inconsequential and, just like deaths from indoor radon exposure, they are preventable and not inevitable.

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Response: I did not state that radon screening tests are "nearly useless." I did report that "such screening surveys are nearly useless in determining the prevalence of radon health hazards." Screening surveys have their purposes, as is made clear in the article and in Houk's letter; but determining the magnitude of the national radon problem is not one of them. Unfortunately, perhaps despite Houk's best intentions, the media took the survey results as new proof of the pervasive threat of radon. Clarifying this misconception was the sole point of my article.

—RICHARD A. KERR

Methods and Molecules

Roger Lewin (Research News, 23 Sept., p. 1598) is to be applauded for calling the DNA controversy to the attention of the scientific community, but he does not make clear the position of phylogenetic systematists (cladists). Cladists hold that taxa should be united by synapomorphies, shared derived traits, rather than by raw (average, general, overall) similarities. A synapomorphy could just as well be molecular as morphological, and many molecular systematists follow a cladist approach. Sibley and his co-workers have routinely used UPGMA, a phenetic clustering method, to produce trees from their data. Cladists would object to UPGMA even if it were used on morphological information.

The choice of grouping method can easily affect results. While Sibley and Ahlquist concluded (*1*) that their data definitively established the placement of humans with chimps, one of us (J.S.F.) found (*2*) that grouping humans with gorillas gave better fit to those data. The difference in goodness of fit may not be significant, but then those data do not settle the placement of humans.

In their numerous publications in bird phylogeny, Sibley and Ahlquist give no indication of having investigated whether other groupings might fit their data better than those produced by UPGMA, nor do they provide the information necessary for others to do so. How well their conclusions are justified will not be known until their data are more widely available.

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2. J. S. Farris, *Cladistics* **1**, 67 (1985).

Psychiatric Diagnosis

In their article "The expert witness in psychology and psychiatry" (1 July, p. 31), David Faust and Jay Ziskin discuss the many problems involved in having psychologists and psychiatrists present "expert testimony" in court. We take exception to several statements in their article and in their subsequent response (Letters, 2 Sept., p. 1143) to correspondence about recent efforts to improve the classification of mental disorders and the reliability with which psychiatric diagnoses can be made. Referring to the recent revisions of the American Psychiatric Association's Diagnostic and Statistical Manual of Mental Disorders (DSM-III in 1980 and DSM-III-R in 1987), they say that "This process of revision little resembles the refinement of categories or cumulative gains common to advanced scientific fields." Unfortunately, this remark is not followed by a discussion of what they believe is necessary before revisions in a general purpose classification system in medicine is appropriate.

The reader is not told that, with the advancement of DSM-III, organized psychiatry, for the first time, faced up to the long-standing problem of diagnostic unreliability and devoted significant resources to a process that would improve diagnostic agreement. Despite the critique of methodologic flaws in the DSM-III field trials, which have been addressed elsewhere (*1*), no one can reasonably deny that diagnostic reliability in psychiatry has been significantly improved by the innovation of specified diagnostic criteria in DSM-III and DSM-III-R.

Diagnostic unreliability in psychiatry continues to be a problem, but the reader should realize that the rest of medicine also has problems with reliability. For example, Feinstein (*2*) has noted that the reliability for abnormal opthalmologic findings is poor