judges noted that "both Hubbard and Benner correctly predicted the first six strands," missing only the final secondary structural element. Despite this error, three (out of 196) possible folds were chosen to represent the beta sandwich of this protein (*3*); one of them was correct. This sounds "close" to us.

Predictions today are not simply contest entries; they are good enough to be applied to solve real biochemical problems. Progress has come in part through the recognition that the protein folding problem is a special example of a much older problem in organic chemistry, conformational analysis. Through this has come the realization that organic chemical approaches have something to contribute to protein folding. *Science* readers should therefore be encouraged to apply prediction tools to their own research problems.

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2. T. DeFay and F. E. Cohen, *Proteins* **23**, 431 (1995).
3. S. A. Bennder, D. L. Gerloff, G. Chelvanayagam, *ibid.*, p. 446.

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EMF Report: Is There Consensus?

Although the National Research Council's (NRC's) new report (*1*) on electromagnetic fields (EMFs) (J. Kaiser, News & Comment, p. 910) makes interesting scientific reading, it falls short as a balanced and informative public communication tool. Like its predecessor committees, the NRC panel chose not to make a prominent display of the true state of scientific uncertainty in their ranks; instead, it issued a carefully worded conclusion that "no conclusive and consistent evidence shows that exposures to residential electric and magnetic fields produce cancer." This presentation is regrettable for two reasons.

First, rational people may choose to take action to eliminate or reduce risk even when the probability that the risk is real is less than that associated with "conclusive and consistent evidence." The NRC report provides the public with no judgments about how likely or unlikely it is that EMFs really cause cancer. All we can glean from the conclusions is that the panel thinks that there is something less than perhaps a 90% chance that the EMF hazard is real. A more neutral approach, and one that addresses the public's information needs more effectively, would have been for the committee simply to report the range of members' subjective judgments of the probability that EMF exposure is truly hazardous.

Second, those readers unfamiliar with the long-standing scientific uncertainty over EMF health effects may miss the nuances of the NRC committee's conclusions and come to the mistaken belief that scientists have concluded with certainty that EMFs pose no health hazard. Indeed, this inference was the gist of many news stories that followed the release of the report. We might ask how different those news reports would have looked had the committee reported the complementary and equally true conclusion that "no conclusive evidence shows that EMFs are safe."

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Low-Level Radiation

It is hard to see what good taking another look at low-level radiation risk would do (ScienceScope, 27 Sept., p. 1787). Five previous reports have created nothing but controversy (*1*). In each report, one faction identified 0.1 Gray as the lower limit of acute and cancerous effects (*1*), only 100 times below the lethal dose and unchanged in 30 years of intense research. And each time a second faction insisted on exaggerating scientific uncertainty and creating artificial risks in order to "save lives." Hard numbers were usually buried under an inch of paper, but the frightening speculation appeared on page 1.

Not mentioned in those five reports is the important fact that radon has been safely regulated by a reasonable standard predating the Environmental Protection Agency (EPA) standard. That standard, still in use, corresponds to a cumulative dose only five times lower than the 0.1-Gray threshold (*2*). Yet surveys with exceptional statistical power show that this standard is safe (*3*).

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References and Notes

1. *BEIR V: Health Effects of Exposure to Low Levels of Ionizing Radiation* (National Academy Press, Washington, DC, 1990).
2. Calculated from the 4 pico-Curies per liter action level for radon ["Technical support document for the 1992 Citizen's Guide to Radon" (EPA 400-R-92-011, Environmental Protection Agency, Washington, DC, 1992)]. The lowest carcinogenic dose in miners with an odds ratio greater than 3 is 226 working level months (WLM) [J. Sevc, *Health Phys.* **54**, 27 (1988)]. Conversion factor: 10 pico-Curies per liter per year = 2 WLM.
3. B. L. Cohen, *Health Phys.* **68**, 157 (1995).

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Women Alcoholics at Bellevue, 1918–1919

Data published in *Science*'s pages in a 1936 article about historical trends in alcoholism admissions at Bellevue Hospital in New York City are probably mistaken. The questionable data occur in a paper authored by alcoholism, vitamin, and cholesterol researcher Norman Jolliffe (1901–1961) ["The alcoholic admissions to Bellevue Hospital" **83**, 306 (1936)].

Jolliffe's paper reported a generally downward trend in the proportion of female (to male) Bellevue alcoholism admissions from 1902 to 1933—the latter, national prohibition's final year. The trend was punctuated however by a sudden spike in 1918 and 1919, when the proportion of female admissions virtually doubled to 41.8% and 39.5%, respectively. Jolliffe offered two guesses for the occurrence. First, it might have been "due in part to an increase of social drinking occasioned by entertaining soldiers embarking for and returning from overseas." Second, the unhappiness caused by the war-time absence of men turned more women to drink. Jolliffe cleverly deduced that the absence of men, and not worry about men's safety in combat, explained the rise, incidentally, by noting that female admissions were almost as large in 1919 as in 1918, even though hostilities had ceased by the latter year.

In 1990, I exchanged correspondence with the late Mark Keller, longtime editor of the *Journal of Studies on Alcohol*, who worked as Jolliffe's editorial and research assistant in the 1930s. Keller noted that a mixup had occurred in the collection of data for Jolliffe's Bellevue admissions paper. He explained that both of Jolliffe's hypotheses for the female admissions spike were moot because