Letters

Gravity-Wave Astronomy

I was interested in M. Mitchell Waldrop's article on the Laser Interferometer Gravitational Wave Observatory (LIGO) (Research News, 7 Sept., p. 1106). Although direct detection of gravitational waves would be a fundamental advance, the article discusses a number of issues that raise doubts about the wisdom of going ahead with such a costly project quite so early. First, the personnel of the project consist of a single worker in residence (R. W. P. Drever), one consultant from the Massachusetts Institute of Technology (R. Weiss), and two or three nonspecialists. That's a heavy concentration of responsibility.

Second, the article states that Drever has "already" achieved a sensitivity of 10^{-14} centimeters over 40 meters. Three years ago at Caltech, Drever gave a report outlining the numerous technical problems that they were struggling with at that time; it's not as if they had bread-boarded some components on a desk top and gotten such sensitivity. It would be interesting to know the basis for the "confidence" that this already remarkable sensitivity to displacement can be improved upon by another factor of 100, even without increasing the scale of the apparatus by another (but different) factor of 100. Practical laboratory experience usually suggests quite the opposite.

Third, the literature consists largely of reports in unrefereed conference proceedings that broadly outline the general concepts (*1*). Waldrop's article alone has significantly increased the available public information on LIGO.

Fourth, the astronomical community at large seems to have been left out of the loop. Surely the National Science Foundation (NSF) has conducted some sort of reviews, but what questions were asked? Would the observations be interesting? Can it actually work? The obvious danger is that something has been overlooked, which is increasingly likely if only a handful of people has been consulted.

One might think that, being a special request to Congress, this is simply money that otherwise would buy another B-1 bomber, so what is the risk? But once built, LIGO will be an NSF "observatory." And if it doesn't work, additional money will be needed to try to fix it (the taxpayers having already spent $192 million, if not more).

Surely a prudent approach to funding large projects is to expose them in a timely fashion to open discussion, analysis, and criticism by the same scientific community that will, by default, end up supporting them. If LIGO is indeed feasible, the astrophysical community needs to know that, to share in the anticipation and excitement, and to give serious renewed thought to the likely sources to be detected.

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REFERENCES

1 J. Hough *et al. Vistas Astron.* **30**, 109 (1987); G. P. Newton *et al. Proc. 4th Marcel Gressman Meet. Gen. Relativ.* (1986), p. 599.

Electromagnetic Fields and Cancer

The News & Comment article "Is there an EMF-cancer connection?" by Robert Pool (News & Comment, 7 Sept., p. 1096) presents a fairly balanced account of the epidemiological research into possible health hazards of electromagnetic fields (EMFs). Several statements, however, must be questioned.

To say that David "Savitz essentially replicated the Wertheimer-Leeper results" (suggesting a correlation between childhood cancer and EMF exposure from power lines) and that "his thoroughness gave the results greater weight" is misleading. With respect to possible confounding exposure factors, Savitz and Feingold (*1*), using the same data set from the EMF study, found that the incidence of childhood cancer was associated with traffic density; increased risks for total number of cancers and leukemias were related to increased traffic densities. The odds ratios for these associations were greater than those reported earlier by Savitz *et al.* for EMFs and cancer (*2*). [In the original Wertheimer-Leeper 1979 study (*3*), "cases were found to generally live closer to high traffic routes."] One potential consequence of high traffic density is a high level of benzene, which, as pointed out later in Pool's article, is known to cause cancer (in particular, leukemia). Interestingly, one of the authors of the study by Savitz *et al.* has been quoted as saying (*4*): "It is very noisy data. It's noisier than anything I've ever had a part in publishing, and it's quoted more than anything I've ever published."

Concerning the various epidemiological studies, Pool's article states that "as a group they have a rough consistency that is harder to ignore." This "consistency" is indeed rough, as shown by the fact that the descriptions of exposure levels in these studies are clearly inconsistent. In addition, relative risks on the order of 1.2 to 2.0 are extremely difficult to interpret because of the potential confounding of many unrelated factors (*5*). Thus, it seems invalid to characterize the risk ratio of 1.2 which Savitz *et al.* found with meta-analysis as an "unmistakable effect."

In Pool's 5 October article "Flying blind: The making of EMF policy" (News & Comment, p. 23), a policy analyst is quoted as saying, "If EMFs do pose a risk, the persuasive evidence could emerge rather quickly—within 5 to 8 years." This prediction seems illogical when one examines the current state of research in this area. At this time, the variability and complicated nature of EMF characteristics do not allow researchers to even *design* definitive studies of EMF health effects. As R. A. Cartwright has stated (*6*), "The criticisms of surrogate measures mean that no proposed study will ever directly address the issue."

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REFERENCES

1. D. A. Savitz and L. Feingold, *Scand. J. Work Environ. Health* **15**, 360 (1989).
2. D. A. Savitz *et al.*, *Am. J. Epidemiol.* **128**, 21 (1988).
3. N. Wertheimer and E. Leeper, *ibid.* **109**, 273 (1979).
4. K. Fitzgerald, *IEEE Spectrum* **27** (no. 8), 27 (1990).
5. R. Doll, *Int. J. Epidemiol.* **14**, 22 (1985).
6. R. A. Cartwright, *Br. J. Cancer* **60**, 649 (1989).

*These views and opinions are those of the author and do not necessarily state or reflect those of the U.S. government.

Drug Abuse Prevention Programs

P. L. Ellickson and R. M. Bell, authors of the article "Drug prevention in junior high: A multi-site longitudinal test" (16 Mar. 1990, p. 1299), are encouraged by results of the drug prevention program for junior high school students, Project ALERT. However, it seems to us that the benefits of the program are small and the net effects do not justify deployment.

At 15 months the program reduced cigarette smoking only among experimenters (those who had had just one or two cigarettes in the past year and none in the past month) and reduced marijuana use among nonusers (those who had never tried cigarettes or marijuana). Alcohol users (those who had had three or more drinks in the past year or any drinks in the past month) and cigarette nonusers were unaffected, and cigarette users were actually harmed by the program. Marijuana use among those who