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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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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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

had used cigarettes or marijuana even once was also unaffected.

The net effects of the program on smoking were unreported, but are readily calculable from the tables. Overall, the program had no net effect on the prevalence of smoking [daily smoking at 15 months was 8.2% (experimental group 1), 9.4% (experimental group 2), and 8.9% (control group)]. The increase in quit rates involves a decrease in smoking from only one or two cigarettes in the past year to none.

Given the strength of the experimental design and procedures, we think the harmful effects of the program on smokers should not be glossed over and euphemistically referred to as "boomerang effects." Interestingly, many of the "smokers" would have been classified as "experimenters" by other researchers, and the "experimenters" would have been classified as "triers" (for example, in the Waterloo Prevention Trial, which defined experimenters as those who had smoked two or more cigarettes and those who smoke less than weekly, and triers as those who had smoked one cigarette) (*1*). We wonder what the results of Ellickson and Bell would have looked like if they had used more conventional categories of smokers.

Sample attrition in the program was 40%. While the authors reassure us that the change in sample composition averaged "only about five percentage points," this translates to a loss of 33% of marijuana users and 11% of cigarette users. The use of percentage points to minimize the differences in sample composition contrasts with the use of percentages based on very small percentage points to emphasize program effects (a 60% reduction in marijuana use based on a difference of 2.3 percentage points). Although attrition rates did not vary across experimental conditions, the loss of high-risk users probably understates the negative effect of the program on this group. Without this bias in sample composition, the net effect of the program might well have been negative.

Ellickson and Bell conclude "that the social influence model of prevention, as implemented in Project ALERT, works." In fact, it worked only to delay the onset of more frequent cigarette smoking among those who had tried smoking only once or twice, and to delay the onset of first use of marijuana among cigarette and marijuana nonusers. And it had damaging effects on cigarette smokers.

Promoting programs that help some students but harm others and have no effect on others is questionable, both ethically and economically. It is impractical to target an intervention only to those who have used cigarettes one to two times in the past year. Furthermore, it is those users who start earliest, continue to use substances, and use them heavily who will account for the greatest health costs and damage. Even if one grants that these programs are marginally effective, they are not likely to be cost-effective compared with other approaches, such as enforcing bans on cigarette sales to minors and raising taxes on cigarettes and alcohol (*2*). In summary, we continue to doubt the value of widespread deployment of social influence programs to reduce smoking, drinking, and drug use in the schools (*3*).

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Response: Ferrence and Kozlowski present an alternative interpretation of our results, emphasizing the program's negative impact on confirmed smokers (17% of the sample). We think our results document substantial benefits for Project ALERT's primary target groups and that those benefits outweigh the boomerang effect. Rather than throw out an approach that helps most students, we suggested ways to eliminate or counter the negative impact (*1*).

Project ALERT was designed to help keep nonusers from becoming involved with drugs and both nonusers and experimenters from making the transition to user. It was not designed to reverse the trajectory of committed users—a goal that is more suited to treatment or cessation programs. The early smokers, in whom Project ALERT triggered a rebellious response, clearly needed more than a prevention message. The early marijuana users, on the other hand, responded positively to a prevention approach (*2*).

Contrary to Ferrence and Kozlowski's assertion, the program produced consistently favorable trends for marijuana across all three risk groups. That held true under both individual and school-level analyses. Moreover, reductions in regular marijuana use by high-risk, early users showed up during grade 7; after 15 months, the results still favored the program but were statistically significant only in the school-level analysis.

It is also important to note that the cigarette experimenters, despite having smoked "only once or twice," were four times more likely than the nonsmokers to become regular smokers by the eighth grade. Hence, reducing weekly and daily smoking by these high-risk adolescents and helping many of them to quit altogether are not trivial outcomes. Nor is it correct to state that the results for early smokers would have looked worse had attrition been lower. The results could just as easily have improved. As for net effects, 19 of 24 overall comparisons for cigarettes favored the program, although only one was statistically significant.

We share Ferrence and Kozlowski's concern that claims for the social influence model not be exaggerated. However, it is just as important to recognize its potential contribution to a complex problem. School decision-makers do not have the luxury of relying on other institutions to do the job or of waiting for an, as yet, unrealized ideal. They must develop a drug prevention plan or risk the loss of federal education funds.

Although the social influence model is by no means a panacea, it is the best school-based approach we have to date and the only one that has demonstrated results (*3, 4*). Moreover, our findings counter three common criticisms of prevention programs: that if such programs work, they do so only in middle class, suburban, largely white environments; that they only help the children who need them least; and that they only prevent trivial levels of use. Further, the curriculum produced results after only eleven 50-minute lessons spread over 2 years. It thus takes very little time away from other school activities.

Non-school strategies such as enforcing bans on cigarette sales to minors or raising taxes on cigarettes and alcohol should complement, not replace, school-based programs. While neither economic nor legal sanctions have stopped the spread of crack or other drugs, both may contribute to a social climate that inhibits use. Indeed, as the erosion of Project ALERT's alcohol gains suggests, the social influence model will be most effective when, as is the case with cigarettes and marijuana, broader societal attitudes and mores reinforce the message. No single approach can solve the problem alone.

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