References

1. Brouwer FP. Re: "Case-control study of childhood cancer and exposure to 60-Hz magnetic fields." Am J Epidemiol 1995; 141:375–6.
2. Wertheimer N, Leeper E. Electrical wiring configurations and childhood cancer. Am J Epidemiol 1979;109:273–84.
3. Savitz DA. Case-control study of childhood cancer and residential exposure to electric and magnetic fields. Supplement to final report to the New York State Powerlines Project, contract no. 218217. Albany, NY: New York State Powerlines Project, 1988.

Nancy Wertheimer
1330 5th Street
Boulder, CO 80302

Ed Leeper
Monitor Industries
6112 Fourmile Canyon Drive
Boulder, CO 80302

RE: "MAGNETIC FIELDS AND CANCER IN CHILDREN RESIDING NEAR SWEDISH HIGH-VOLTAGE POWER LINES"

When epidemiologic studies such as that of Feychting and Ahlbom (1) have only a small number of cases, it is particularly important to describe clearly whether the fundamental statistical criteria for validity are satisfied. If one decides to calculate the probability of an event only after the event has occurred, the answer is a tautology. This has been described elegantly by Feynman (see reference 2), who asked about the probability of finding a car with a particular license plate, which car he had just seen! If an epidemiologist decides to discuss the significance of a particular set of cancers only after the events are known to him, he is making this same error. We therefore call it the "Feynman trap" (3).

Three preceding studies (4–6) showed an association with proximity to power lines (wire codes) but no association with contemporaneously measured (spot) fields. If Feychting and Ahlbom (1) studied these particular associations (even though the Swedish study involved 50-Hz not 60-Hz magnetic fields), it can be presumed that the Feynman trap does not apply. It also seems probable that the decision to compare with calculated historical fields was made in advance, but we are not told whether this was the case. The way the results are presented gives us the impression that the "cutoff" points for field strength in table 5 were chosen after the data were seen. If so, the authors partially fell into the Feynman trap and the statistical significance, already marginal, is lost. It would be nice to know whether this was what happened.

The barely significant association of table 5 is between calculated historical fields and leukemia for *all* homes whether or not a field measurement was made. Close examination suggests that the association vanishes if made only for these homes where there was a field measurement and consequently is larger if restricted to the homes without such a measurement. This is a peculiar internal inconsistency. We have requested the original data to verify this, but deduce it as follows.

Table 5 shows that seven cases were found in those of the 695 homes with calculated historical fields above 0.3 μTesla (μT). By proportionality, we would expect about four cases ($= 7 \times 433/695$) in the 433 homes where a measurement was made. If the calculated historical fields were above 0.3 μT, a comparison of figures 1 and 2 shows that, on average, calculated contemporaneous fields (presumably because of increased electricity use) are above 0.5 μT and figure 1 shows that the measured (spot) fields would on average be above 0.55 μT. Table 9 shows that four cases were indeed seen but with spot fields above 0.2 μT. In this wider range, we would have expected more. This suggests that there is an unusually large number of cases, leading to a larger risk ratio, among the 262 ($= 695-433$) homes for which there were no spot measurements.

This leads us to speculate that the failure to make a spot measurement might itself be associated with an increase in leukemia.

Although this post hoc discussion is statistically invalid, we examine it further to find suggestions for further studies. Table 6 suggests that the cases were not in Stockholm. This suggests that there is a possible confounding factor in homes outside Stockholm (where it might be harder to measure fields) such as an increase in chemical exposure at a farm as compared with a city.

This study raises interesting questions, but until the statistical issue is resolved, and some internal consistency obtained, they are hypotheses for further study rather than conclusive results in themselves.

References

1. Feychting M, Ahlbom A. Magnetic fields and cancer in children residing near Swedish high-voltage power lines. Am J Epidemiol 1993;138:467–81.
2. Goodstein DL. Richard P Feynman, teacher. Phys Today 1989(Feb):70–5.
3. Shihab-Eldin A, Shlyakhter A, Wilson R. Is there a large risk of radiation? A critical review of pessimistic claims. Environ Internat 1992;18:117–51.
4. Wertheimer N, Leeper E. Electrical wiring configurations and childhood cancer. Am J Epidemiol 1979;109:273–84.
5. Savitz DA, Wachtel H, Barnes FA, et al. Case-control study of childhood cancer and exposure to 60-Hz magnetic fields. Am J Epidemiol 1988;128:21–38.
6. London SJ, Thomas DC, Bowman JD, et al. Exposure to residential electric and magnetic fields and risk of childhood leukemia. Am J Epidemiol 1991;134:923–37.

Richard Wilson
Alexander Shlyakhter
Department of Physics
Harvard University
Cambridge, MA 02138

THE AUTHORS REPLY

Wilson and Shlyakhter (1) raise two issues with regard to our paper on magnetic fields and cancer in children (2). First, they are concerned that what they refer to as the "Feynman trap" may be applicable to our report; if so, they argue, the statistical significance would not be at the levels reported. However, we have not referred to statistical significance anywhere in our paper, so this would only be a

problem for those readers who try to interpret the location of our confidence interval boundaries strictly in terms of presence or absence of statistical significance. Furthermore, if we understand the Feynman trap correctly, the general issue is that whenever data are selected for reporting depending on outcome, the usual probabilistic models do not apply. Therefore, the argument goes, a posteriori analyses should not be reported, at least not without an appropriate label. However, it can be questioned whether the usual probability models are applicable to observational epidemiologic data under any circumstances and certainly the strict probability statements can generally not be expected to hold (3). Furthermore, investigators must take every opportunity to test their findings and try to have them refuted by further analyses. Thus, the standard procedure in data analysis in epidemiology is to perform thorough extensive analyses determined both prior to the study and as a follow-up of the previous analyses in an attempt to fully evaluate consistencies and inconsistencies in the pattern of results.

Second, Wilson and Shlyakhter note a difference in the relative risk estimate across subjects with and without contemporaneous measurement, even though this was not reported in our paper. We have made the appropriate analyses and in the stratum of subjects with measurements, the relative risk associated with >0.2 μT is estimated at 1.9 (95 percent confidence interval (CI) 0.5–5.6) and, in the stratum of those without measurements, the corresponding estimate is 5.7 (95 percent CI 1.1–23.1). These estimates are based on four and three exposed cases, respectively. We cannot explain the difference between these two stratum-specific point estimates, but we do caution against far-reaching speculations in an instance where the underlying absolute numbers are as small as these. This is, of course, not affected by the fact that only one of the two confidence intervals include the null value. In our opinion, chance is a likely explanation for the disparity.

In their discussion, Wilson and Shlyakhter read our table 6 as indicating that the excess risk is restricted to those living outside Stockholm and then they speculate that other exposures that are more common in the countryside, such as pesticides, could explain this by acting as confounders. We don't agree with the way Wilson and Shlyakhter read the table; in fact, the relative risks in Stockholm and outside Stockholm are 2.1 (95 percent CI 0.4–7.1) and 3.8 (95 percent CI 1.0–12.5), respectively. Again, we warn against speculations based on analyses of subgroups with small numbers and in particular when based solely on the presence or absence of statistical significance. Furthermore, even if an association were seen only in rural areas, the suggested confounders would still not be good candidates since the entire analysis would be restricted to this segment of the population and this restriction would apply equally to cases and controls.

References

1. Wilson R, Shlyakhter A. Re: "Magnetic fields and cancer in children residing near Swedish high voltage power lines." (Letter). Am J Epidemiol 1995;141:378.
2. Feychting M, Ahlbom A. Magnetic fields and cancer in children residing near Swedish high voltage power lines. Am J Epidemiol 1993;138:467–81.
3. Greenland S. Randomization, statistics, and causal inference. Epidemiology 1990;1:421–9.

Maria Feychting
Anders Ahlbom
Institute of Environmental Medicine
Karolinska Institute
S-104 01 Stockholm, Sweden

Editor's note: Drs. Savitz, Wertheimer, and London were also asked if they wished to respond to the letter by Drs. Wilson and Shlyakhter, but they chose not to do so.