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LETTERS

EMF and Cancer

Richard Stone's article "Polarized debate: EMFs and cancer" (News & Comment, 11 Dec., p. 1724) provides fair coverage of the controversy concerning the possible health effects and, in particular, the carcinogenic potential of extremely low frequency electric and magnetic fields (ELF-EMF). The controversy has been enhanced by the simultaneous release of two Swedish reports (*1*, *2*) suggesting that there is a causal association and a critical review of the total existing evidence by an Oak Ridge Associated Universities (ORAU) panel (*3*). Because the two Swedish studies were made public when the ORAU report was already in the printing process, we consider it necessary to indicate that, in our opinion, the evidence presented in these studies is not sufficiently compelling to alter the conclusions of the ORAU report.

The two recent studies (*1*, *2*) and the earlier published study by Tomenius (*4*) represent the three major research projects on EMF and cancer undertaken in Sweden. The study by Tomenius (*4*) examined residential exposure in relation to childhood tumors (mainly brain tumors and leukemia); the study by Feychting and Ahlbom (*1*) examined residential exposure in relation to brain tumors and leukemia among both children and adults; and the study by Floderus *et al.* (*2*) focused on occupational exposure in relation to leukemia and brain tumors in adults.

For none of these cancer sites or types were the results of the two studies that examined them similar; indeed, the findings of the two relevant studies pointed to opposite directions. For childhood leukemia, an inverse association is evident in the study by Tomenius (*4*), but a positive association was presented in the study by Feychting and Ahlbom (*1*); for childhood brain tumors a positive association was found by Tomenius (*4*), but an inverse association is discernible in the results of Feychting and Ahlbom (*1*) when their preferred exposure metric (calculated magnetic fields) is considered; for adult brain tumors, an increased risk was found by Floderus *et al.* (*2*), but a declining trend with increasing exposure may be noted in the results of Feychting and Ahlbom (*1*); for adult lymphatic leukemia, the risk increases with exposure in the study by Floderus *et al.* (*2*) but appears to decrease with exposure in the investigation by Feychting and Ahlbom (*1*); and for adult myeloid leukemia, there is a positive association in the study by Feychting and Ahlbom (*1*), whereas an inverse trend is evident in the study by Floderus *et al.* (*2*).

Feychting and Ahlbom (*1*) examined the association between calculated (through a computer program) or actually measured magnetic fields and childhood leukemia, childhood brain tumors, other childhood tumors, adult myeloid leukemia, adult lymphatic leukemia, and adult brain tumors. No positive association was found for any of the studied malignancies, among either children or adults, when actually measured magnetic fields were considered. However, a positive association with calculated magnetic fields was reported for childhood leukemia (which is usually lymphatic) and for adult myeloid leukemia.

Time is an important variable for growth-enhancing factors, and it is generally accurately measured. Therefore, it should be expected that relative risk for leukemia contrasting "long" and "short" durations of exposure to magnetic fields would be generally high if the exposure to these fields were conducive to the development of this disease. Data comparing children "who have lived at least 50% of their life before diagnosis in the house near a power line" with children who have not, and adults "who have lived 10 years or more at the home near a power line" with adults who have not, generated nonsignificant associations, with relative risk estimates of 1.26 for childhood leukemia ($P > 0.30$) and 0.95 for adult myeloid leukemia [our calculations, based on data in tables 2.2, 4.12, and 5.10 of the report by Feychting and Ahlbom (*1*)].

The study by Feychting and Ahlbom (*1*) covered a period of 26 years and a population "close to a half a million" living within 300 meters from 220- and 400-kilovolt power lines in Sweden. During this period, 39 cases of childhood leukemia and 129 cases of adult myeloid leukemia were identified. The corresponding incidence rates can be calculated and compared with those of the total Swedish population (which was largely unexposed, according to the authors' operational criterion) to assess the external validity of the reported findings. Accurate rates for the population studied by Feychting and Ahlbom cannot be estimated by outside reviewers from the data provided, but on the basis of crude approximate calculations it appears that the studied

population had *lower* leukemia rates than those of the Swedish population as a whole. It is also of some interest that for childhood tumors other than leukemia and brain cancer there is a suggestive (although statistically not significant) inverse association with calculated magnetic fields.

The study by Floderus *et al.* (*2*) is a large, population-based occupational study and reports positive associations of actual measurements of magnetic fields (in the jobs held longest during the 10-year period before diagnosis) with chronic lymphocytic leukemia and brain tumors. There are, however, several concerns: (i) In an occupational case-control study evaluating a widely publicized hypothesis, information bias is always an important problem. The likelihood of this bias does not appear to have been adequately addressed. (ii) Response rates (around 66%) were low, and cases and controls were apparently not treated in the same way throughout the study, even though this is mandatory in a case-control study (for example, dead cases were included but dead controls were excluded; and information on cases and controls was not simultaneously collected). (iii) Although occupational exposure history is complex, it appears to have been ascertained through a single open-ended question, and no residential exposures were taken into account.

We have never stated that a causal association between EMF and cancer is impossible or inconceivable; we have indicated that the evidence for such an association is empirically weak and biologically implausible. We have not proposed that research concerning the health effects of EMF be discontinued; in fact, we have indicated areas of some scientific interest that warrant consideration for future research. However, given the decreasing resources available for basic health and science research, we believe that in a broader perspective there are currently more serious health needs that should be given higher priority.

ORAU Panel on Health Effects of Low-Frequency Electric and Magnetic Fields*

References

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A panel established by ORAU at the request of the U.S. government has reviewed the literature on EMF and cancer and has concluded that epidemiologic findings about EMF and cancer are inconclusive and inconsistent and that there is no presented plausible mechanism (*1*). The panel also concludes that further research in this area should not receive high priority.

The ORAU panel states that the new Swedish results do not warrant a change in the panel's previous conclusions. The panel concludes that our data (*2*) provide weak and inconsistent evidence for an association between EMF and childhood leukemia and say that the inconsistency is derived from the fact that the association is present when EMF is assessed with calculations but absent when it is assessed with measurement. A similar apparent inconsistency was seen also in two previous studies and has been an issue for discussion ever since these studies were presented (*3*, *4*). Our calculated fields are annual averages, in most analyses referring to the year closest in time to diagnosis, while our measured fields refer to a 20-minute period, in some cases as many as 26 years after diagnosis. These two EMF assessments show a poor correlation, so it is not surprising that the results of the epidemiologic analyses are dependent on which of them is used. We have also shown that the EMF calculations are able to predict the fields at the time of the measurements, and we have access to power line information so that we can also calculate historical fields. This seems to indicate that, in fact, the EMF calculations are better predictors of past fields than the actual measurements and that the apparent inconsistency between results based on calculations and results based on measurements might be explained by this.

(*Continued on page 16*)

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The panel further speculates, on the basis of some "crude approximate calculations," that the leukemia incidence in the power line corridor is actually lower than in the Swedish population as a whole.

One cannot do reasonable approximations of cancer incidence on the basis of the data available in our report; but we have performed the necessary calculations based on the original data, and for all childhood cancers together we observed 142 cases as compared with an expected number of 138 based on the number of person years in the power line corridor population and incidence rates from the Swedish Cancer Registry.

We are aware of nine studies, including our own and a Danish study that so far has been presented only orally (*2–10*), with information about leukemia in children and exposure to EMF. The ORAU panel makes several comparisons between our study and the study by Tomenius (*5*). However, although the study by Tomenius was not published until 1986, it was actually conducted shortly after the original study by Wertheimer and Leeper (*6*) and shares some problems with other early studies in this field. Tomenius used as an exposure measure the presence of a 220-kilovolt line within 200 meters and alternatively a short-term EMF reading outside the front door of the building, often an apartment building. We have now clear indications that neither of these provides a valid EMF assessment in the home. The 200-meter range is much too wide to be meaningful, and a short-term reading outside the front door is only a reasonable predictor of indoor EMF if there is a dominant external EMF source, such as a nearby high-voltage line. Although this does not explain all aspects of the results, it might explain the absence of an association for leukemia in children and it is sufficient to give the study minimal weight when combining results across studies.

Two other studies should also be given minimal weight when evaluating the childhood leukemia studies. One has been criticized for having a control selection bias with the potential for explaining the lack of an association (*7, 11*), and another does not have a sufficient number of subjects with considerable exposure to provide meaningful information in this context (*8*). Thus, we are left with six studies of childhood leukemia and EMF.

We believe that we have good reasons to prefer exposure assessments based on the presence of nearby lines and other installations over short-term readings. If the corresponding results are used, one obtains a fairly clear consistency across studies, with all six relative risk estimates in the range of 1.5 and 3.0 and with only two of the studies having 95% confidence intervals with lower bounds below unity (*9, 10*).

It would indeed be appealing if all available evidence—from residential and occupational studies, from children and adults, and from studies on different types of cancer—fit together in an intelligible pattern, but this does not appear to be the case. However, in our view, the evidence on leukemia in children is actually fairly consistent, and inconsistent results from studies on other types of cancer or on adults should not detract from this (*12*).

We agree with the ORAU panel that there is no known mechanism by which EMF might play a role in cancer development. However, this may be viewed from two directions. One could conclude that the lack of a known mechanism makes the link between EMF and cancer a priori unlikely and, therefore, that further research should not be given high priority. Alternatively, one could conclude that, if this link exists, there appears to be an as yet unknown mechanism through which EMF interacts with human cells. It could then follow that research should be pursued in order to explore this potentially important knowledge.

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Drug Abuse Research

I would like to congratulate *Science* on the 18 December article "Pot, heroin unlock new areas for neuroscience" by Marcia Barinaga (Research News, p. 1882) and point out that the National Institute on Drug Abuse (NIDA) has funded pioneering neuroscience research for 20 years, including that done by most of the grantees identified in the article. We also provided support to the National Institute of Mental Health laboratory that was cited. In fact, NIDA supports 88% of all drug abuse research conducted in the United States.

To be precise, of course, it is the American taxpayer who supports research about drug abuse and other health problems. The American taxpayer needs to know that knowledge gained through research is helping to address some of our more wrenching health and social problems.

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Corrections and Clarifications

The references in Philip H. Abelson's editorial of 5 February, "Science, technology, and national goals" (p. 743), were incorrect. The first reference should have read, "H. T. Shapiro, *The Bridge* **22** (no. 3), 14 (1992)." The second reference should have read, "R. E. Gomory, *ibid.* (no. 2), p. 18."