Problems and Priorities in Epidemiologic Research on Human Health Effects Related to Wiring Code and Electric and Magnetic Fields

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Because of a reported excess of cancers among children living near power lines, there is some concern that electric and magnetic fields (EMFs) induced by electric power sources may affect human health, and this possibility has provoked considerable controversy. The scientific question of whether there are such health effects is far from resolved. Building upon a set of detailed reviews of the available evidence, this paper proposes research priorities and places particular emphasis on epidemiologic research. The most pressing need is to verify the validity of the claim that childhood cancer risk is affected by the type of wiring code in the vicinity of the household. More useful work can be done to verify this in the areas in which such studies have already been carried out, and additional studies should be done elsewhere. Methodological investigation of the interrelationships among different measures and proxies for EMF exposure could feed back to influence the type of EMF measures used in epidemiologic studies. Studies of cancer among adults in relation to EMFs in the workplace are needed. Of lower priority are studies of adverse reproductive outcomes in relation to parental EMF exposure and studies of the neurobehavioral impact of chronic EMF exposure. This article also discusses the structural impediments of conducting environmental epidemiology research and argues that bold, large-scale epidemiologic monitoring systems are needed. There is a discussion of the interface between epidemiology and public policy in a topic area as controversial as EMFs.

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Few issues have excited as much public health concern and controversy in the past decade as the alleged harmful health effects of extremely low frequency electric and magnetic fields (EMFs). The controversy does not show any signs of abating. The set of papers in this volume provides not only a review of the scientific evidence concerning the possible human health effects of exposure to EMF, but more importantly, they provide a number of prescriptions for future research in this area (1–6). All of these authors have drawn their own conclusions about future research needs based on the evidence they presented. This paper serves to provide an evaluation of the research priorities across the various areas covered by the authors of the accompanying papers based on the background data they have assembled. Also, this paper will reflect on the reasons for controversy in this area and discuss the implications for both scientific research and public health practice.

By way of introduction, it is useful to summarize briefly, albeit rather simplistically, the current state of knowledge regarding health effects of EMFs (2–7). Based on retrospective case–control studies, associations have been reported between type of electrical wiring configuration in the vicinity of the household (referred to as wiring code) and risk of childhood cancers, notably leukemia and brain cancer. [See (2) for an explanation of wiring code.] Some studies have reported relative risk estimates in the range of 1.5 to 3.0 among subjects classified as very highly exposed, with lower 95% confidence limits near 1.0. Other studies have found no association. While the evidence is not strong, it is suggestive. There is some correlation between type of wiring code and levels of magnetic fields in the home, but the relationships still are poorly understood. While electric and magnetic fields can be measured with relative ease, it is not clear whether contemporary measurements in homes have much relevance to the estimation of past EMF levels. The few attempts to relate contemporary measured fields to cancer risk have produced equivocal or null findings. It is possible that a true association with EMFs has not been detected because the etiologically relevant EMF exposure variable has not been assessed. Based on the epidemiologic studies alone, the statistical evidence is stronger for an association with wire codes than it is for an association with measured fields.

Many studies, most of them based on death certificate notation of the decedent’s occupation, have examined relationships between cancer risk among adults and so-called electrical occupations. The interpretation of these studies is complicated by countervailing biases. On one hand, there may have been biases in reporting results from such surveillance-type studies, namely, positive findings are more likely to be reported than null findings. Also, these studies generally involved posthoc delimitation of certain job titles as exposed (i.e., the investigators usually patched together a grouping of electrical occupations after examining the results for individual job titles). On the other hand, there were biases in the opposite direction, including the questionable validity of the job title and cause of death data and the crudeness of the job title designations as indicators of occupational exposure. Overall, there is a modest degree of consistency among these studies that shows a slight excess of leukemia and brain cancer in such workers.

There has been suggestive evidence of a link between long-term use of certain appliances in the household and childhood leukemia risk. There also is some concordance between the target organs apparently related to domestic wire code in children and to so-called electrical occupations in adults. The most obvious common factor,
if both of these associations were confirmed, would be extremely low frequency electric and/or magnetic fields.

The biological plausibility of significant human health effects due to electric power sources remains controversial. Studies have identified various physical and biological mechanisms that might explain such effects, if real, but these are considered speculative. One of the possible mechanisms proposed, mediated by inhibition of pineal melatonin, would predict the greatest effect of EMFs on hormone-dependent tumors such as breast tumors. Research to test this prediction adequately has not been conducted yet, though there are some hints of excess male breast cancers in some electrical occupations.

Because of the frequently close relationship between carcinogenesis and teratogenesis, it is instructive and prudent to question the reproductive effects of EMFs. While plausible mechanisms can be envisaged, the evidence is still too scant to provide the basis for any inference.

There has been concern that the central nervous system would be particularly susceptible to perturbation. Although there have been several studies purporting to show some effects of EMFs or their correlates on neurobehavioral outcomes, such as suicide, these have, for the most part, been flawed or inadequately reported studies.

Priorities

The listing of priorities is based on the following considerations: What issue is driving the scientific controversy and concern? What is the strength of evidence for different health effects? What types of studies would be needed to evaluate different associations? What methodologic advances would help most in resolving the uncertainties?

In the history of medicine, by far the most important basis for the discovery of true etiologic relations has been, and probably will continue to be, empiric evidence rather than deduction from biologic principles. As Doll (8) illustrated in the area of occupational carcinogenesis, most true associations were discovered first as a result of the chance observation of a cluster of like-exposed cases. If and when a human risk factor has been identified, basic research can be helpful in elucidating its mechanism of action. But, as exemplified by the procedures and experience of the International Agency for Research on Cancer (IARC) Monograph Program for Evaluating Human Carcinogens (9), epidemiologic evidence continues to be the cornerstone of the process for determining whether a given agent causes a given disease among humans.

The driving force in this whole controversy has been the observation initially made by Wertheimer and Lueper in Denver (10), then by Savitz et al. in Denver (11), and by London et al. in London (12) of an association between childhood cancer, notably leukemia and brain cancer, and the type of wiring distribution in the vicinity of the home, referred to as wiring code. A fourth study, in Rhode Island, found no association (13), but it has been criticized as having used methods that biased the results towards the null (14). A fifth study, in Sweden, reported no association for leukemia but a positive association for central nervous system tumors (15). This was based on a simplified method for assessing wiring code. In aggregate, this body of evidence supports the hypothesis of an association between wiring code and childhood tumors. There are four possible interpretations: a) a positive association that reflects a true association between EMFs and cancer has been identified correctly, b) wiring code was confounded by a non-EMF risk factor for childhood cancer that was not adequately controlled, c) there was a bias generated by the study design or data collection method, or d) it was a statistical fluke.

While most attention has focused on the first interpretation, the others also merit consideration. Although there is no documentation currently available on the issue, it is unlikely that wire codes are randomly distributed through a city. There must be many social and geographic correlates of different wiring codes such as spacing of houses, distribution of single versus multifamily units, and age of the housing development. Perhaps the true risk factor is a characteristic of the neighborhood, such as air pollution, population density, or levels of local immunity to infectious agents; possibly it is a characteristic of the home related to its age or building materials, or a characteristic of the family, such as residential mobility. The complex pattern of leukemia risk as a function of crowding and mobility, possibly mediated by infectious agents, that was hypothesized by Kinlen (16,17) in Britain illustrates the kinds of complex and subtle factors that must be considered. While the reports by Savitz et al. and by London et al. made some efforts to take socioeconomic status into account, those attempts were far from comprehensively accounting for the range of possible social and geographic confounders.

Another objection to the Denver and Los Angeles studies that has been raised, and that has not been answered satisfactorily, has to do with the possibility of a biased population control group. Namely, it is alleged that nonresponse, including noneligibility and nonparticipation, may differ among cases and controls and may also be correlated with wire code exposure. For certain parameters, such as use of appliances, the results may also have been biased by differential quality of response from cases and controls.

Finally, the interpretation of the positive findings as a set of statistical flukes cannot be dismissed, because the handful of relative risk estimates has been of borderline statistical significance and have not shown clear dose–response patterns. Even if we could be satisfied that biases were not responsible for those findings, the strength of the accumulated body of evidence (in the sense of a metaanalysis) would not justify concluding that there is an association before additional studies were consistent in demonstrating an association.

Although the investigators used wiring code as a proxy for EMFs and subsequent work has shown that wiring code is correlated with EMFs, the attempts to relate childhood cancer to EMFs directly have not succeeded yet. This is not to say that the available evidence disproves an association with EMFs, but it does not support such an association even to the modest degree that it supports an association with wiring codes. The physical and biological mechanisms that have been postulated to explain the alleged harmful effects of EMFs on humans are largely speculative. The two legitimate reasons for according this issue a high level of attention are the epidemiologic evidence linking wiring code to childhood cancer and the fact that EMF exposure is so pervasive. Were it not for the epidemiologic observations of an association between childhood cancer and wiring code, the issue of EMF and health would merit little more scientific attention than the potential harmful effects of many other common physical and chemical exposures. It might be argued that its apparent association with wiring code has served to open the issue of EMF exposure and cancer risk and that it is now appropriate to study more credible measures of EMF exposure. However, it is not opportune to put all the research eggs in the EMF basket, and it would be more cost-effective to give some priority to establishing the validity of the Wertheimer–Lueper observation. In part, this judgment is based on the fact that the Wertheimer–Lueper observation is a fairly straightforward hypothesis to evaluate; conversely, the investigation of one or another of the measured EMF metrics would be
much more expensive and time-consuming to evaluate, and any null finding in respect to a given measure of EMF exposure will be unconvincing, because it will be argued inevitably that the wrong metric of EMFs was studied. If the Wertheimer–Leeper observation were confirmed, it would reinforce the high priority of research in this general area and it would suggest two lines of research, the effects of EMFs on cancer risk and the significance of other correlates of wiring codes as possible risk factors. The combination of positive findings on the Wertheimer–Leeper observation and null results on attempts to correlate measured EMFs with childhood cancer would provide an extremely important lead in searching for the etiology of childhood cancer. If the association with wiring codes is not confirmed, then the general priority level for research in this area would be lowered, and the issue of exploring non-EMF correlates of wiring codes would be eliminated.

Thus, if the problem is formulated as the search for the etiology of childhood cancer rather than the search for the health effects of EMF exposure, the top priority is to determine whether there is an association between childhood cancer, notably leukemia and brain cancer, and wiring code. Of slightly lower priority is the closely related question of whether other, more direct measures of EMFs are associated with cancer risk. In evaluating these related issues, there are several facets that require specification, including the types of cancer on which to focus, whether to assess an effect in children or in adults, and how to measure the exposure variable. Finally, the last priority would be to evaluate the effects of EMFs on other health outcomes. Methodologic developments would be needed at several steps.

Revisit the Available Case–Control Data Sets on Childhood Cancer and Wire Codes

It is important to try to address the confounding and selection bias issues in Denver and Los Angeles more thoroughly than previous studies. It should be possible to ascertain that the observed findings are not due to uncontrolled confounding by the types of neighborhood characteristics, dwelling characteristics, and family and/or social characteristics mentioned above. There may be relevant data already available to the investigators of the Denver and Los Angeles studies. If not, it would be desirable to conduct some additional data collection in these areas to detect and to deal with potential confounders. Ideally, this could involve visits to the homes of study subjects already interviewed. If this is not feasible, it could involve examination of the social and geographic correlates of wiring codes in representative samples of households in these cities (i.e., to characterize the exposure–confounder arm of the conventional confounding triangle).

In the studies of Savitz et al. (11) and of London et al. (12), random digit dialing (RDD) was used to ascertain eligible controls. It would be informative to compare, on a sample basis, the kinds of households elicited by an RDD procedure as compared with those elicited by different procedures and then to estimate the prevalence of different wiring codes in these cities. Because of the possibly idiosyncratic nature of telephone coverage and social behavior, it would be preferable to carry out these methodologic studies in Denver and Los Angeles rather than trying to transfer inferences from another locale.

Studies to Replicate Cancer Found in Children

Even if the supplementary studies recommended above confirmed the associations with wire codes initially reported, these findings would have to be replicated elsewhere to provide some assurance that they were not statistical flukes or products of uncontrolled bias. Fortunately, wiring code is a relatively easy exposure variable to assess and it does not require access to households. It might be possible for investigators who have previously carried out leukemia or brain cancer case–control studies to piggyback a new evaluation of wire codes onto their previous studies.

Although the evaluation of the childhood cancer and wire code associations is a sufficient motivation for additional case–control studies by itself, if feasible, it would be important to use that opportunity to evaluate again the role of measured EMFs and appliance use. Because the nature of the risk factor is completely unknown, it would be prudent to include an exposure assessment protocol in as many different types of exposure metrics as possible, including various functions of spot measurements in subjects’ places of residence, work, or school and continuous monitoring of personal exposure. It also would be useful to explore alternative exposure metrics such as the resonance model and exposure to transient fields, though methods for so doing in an epidemiologic study are not apparent (2).

Any new studies undertaken to tackle this issue ideally should be carried out in areas that have had relatively stable populations to minimize complication of the retrospective exposure assessment because of immigration, and in areas that contain an adequate proportion of high wire code homes. Also, they should involve larger numbers of study subjects than previous studies. Because of the rarity of childhood cancers and the desire to subdivide them by histological subtypes, it may be necessary to resort to multicenter studies. A population-based, case–control approach with data collected on social and geographic characteristics of neighborhoods, dwellings, and households (so that these factors can be incorporated into analyses) would be the design of choice.

The issue of control group selection is important not only in this area of EMF exposure and cancer but also in any case–control studies. Thus, a brief digression is in order. While convention holds that a set of population controls selected from the free-living general population represents the optimal choice in a population-based case–control study, the practical aspects of implementing this strategy may render it decidedly less attractive than alternatives. Obtaining a sampling frame is not straightforward, since in North America, at least, there are few, if any, continuously updated population registers. Random digit dialing has become a popular method for control selection, but there is little understanding of the biases that might ensue from noncoverage due to having no telephone service, being unavailable to answer an initial call, or being unwilling to respond honestly to the most elementary eligibility questions. Once subjects are eligible, their willingness to participate may differ between cases and controls, and once they are willing to participate, the quality of their participation may differ. Differential losses at any stage can result in bias, as can differential quality of response. Alternative methods of population control selection (e.g., birth certificates, immunization rosters, school lists, utility company lists, address directories) also may have problems with differential losses (selection bias) and differential quality information (information bias). A properly chosen disease control group (e.g., selected from the same hospitals as the cases and among diseases having similar referral patterns) may minimize selection bias and avoid information bias. Because it is impossible to be certain of the relative pros and cons of different potential control groups within a given study, it is prudent and efficient to use two control groups, one a so-called population control.
group and the other a so-called hospital control group. This design is shunned sometimes for fear that it might produce conflicting, and therefore difficult to interpret, results. However, this argument is flawed; its logical conclusion is that there should not be more than one study on any issue because of the possibility of conflicting results. The use of multiple control groups can be seen as a way of carrying out multiple (albeit not independent) studies at a small additional cost. In any case, the verdict on these hypotheses regarding wire code or EMFs will not be based on any single study but on the body of studies, some already complete, some now in progress, and some perhaps coming later. It would be a pity if all of these studies used basically the same control group strategy and thus were open to the same set of criticisms. If an association were found using different types of control groups, this would be the most powerful way to disarm critics.

In this section on studies of childhood cancer and domestic exposure, case-control rather than cohort-type studies are recommended. It is unlikely that lists to constitute a retrospective cohort of exposed children for follow-up are available anywhere. The possibility of establishing a prospective cohort of children, with baseline information on exposure to wire codes and possibly other exposure variables, and follow-up through morbidity or mortality registers is a daunting prospect when the outcome of interest is as rare as childhood cancer. Furthermore, for a variable with long-term stability such as wiring code, it is likely that a properly conducted case-control study would provide results equivalent to those of a properly conducted cohort study. For a variable that is very unstable over time, as some of the EMF metrics may very well be, a measure taken at the outset of a prospective cohort study may be no more meaningful than a measure taken in a retrospective case-control study.

Exposure-Related Methodology

The development of general epidemiologic methodology, including insights into design, fieldwork methods, or analysis, may not be specific to this particular problem, but it would benefit research in this area. Of specific relevance to research in this area are methodologic studies focusing on the measurement and meaning of the exposure variables. There are many sources of EMF exposure and many approaches to measuring it. Among the most prominent approaches to measuring nonoccupational exposure are spot measurements in the home; personal, portable dosimeters; and wiring code. The relationships among these and their stability over time are crucial to planning and interpreting epidemiologic studies but are understood poorly.

Given the current available epidemiologic literature, the top priority is to investigate the significance of wire codes. This should include some general description of the historic evolution and current urban geography of wire codes. Empirical studies should be undertaken to correlate spot measurements of electric and magnetic fields in houses with wire code. Various exposure metrics should be examined to determine which are best correlated with wire code.

Temporal stability of spot measurements, both short-term and long-term, requires further documentation. Ideally, there should be a representative panel of households monitored over a long period, perhaps 5 years, with spot measurements, personal dosimetry, and wire code data collected periodically. The relative importance of at home versus away from home EMF exposure should be evaluated in a general population, as should relative contribution of appliances in the home to the overall burden of domestic EMF exposure. Such panel studies should be conducted in at least two geographically separate locales so that an estimate of the generalizability of these interrelationships may be determined.

Occupational Studies of Cancer Occurrence

Despite the problems with the occupational studies alluded to above, the relatively consistent evidence of slightly increased risk of brain tumors and leukemia in so-called electrical occupations deserves further evaluation. A possible advantage of occupational studies over residential studies is that they may provide clearer exposure differentials. However, there needs to be better characterization of the exposure factor than the job title. This could be accomplished by using some sort of mechanism that measures exposure levels either by means of a job-exposure matrix or by measurement protocols. Studies should be devised to obtain exposure information not only on EMF but also on occupational and nonoccupational exposures that may confound the association between EMF and cancer. Useful studies could be carried out in the context of cohort studies of workers known to be highly exposed (some major studies of utility workers are already in progress) or in the context of population-based case-control studies. While most interest should be in leukemia and brain tumors, there is sufficient uncertainty about other cancers to justify examining many cancer sites, especially skin melanoma, lymphoid tissue, breast, and prostate.

Databases for Attributing Occupational Exposure to EMF

It is possible for experts in hygiene to make useful estimates of past occupational exposure to chemicals (18). It has also been shown that experts can make useful estimates of EMF exposure ranking in a cohort of utility workers (19). But the validity of such expert judgments depends on the availability of some exposure measurements in various occupations. There is very little information available on the relative levels of EMF exposure in different occupations. Most measurements have been made among utility workers. There is only a scattering of data on other occupations (20), and no indication of the relative stability of occupational levels of EMFs over long time periods. Surveys should be carried out to document EMF levels in representative samples of many occupational groups. The development and availability of data bases on relative EMF exposure levels in different occupations and on the temporal stability of such levels would aid in the interpretation of past occupational studies and in the conduct of new ones.

Animal Carcinogenicity Studies

Although uncertainty about the ability to extrapolate evidence of carcinogenicity across species still exists, it is widely accepted that evidence of carcinogenicity in one species increases the plausibility of carcinogenicity in another. Thus, some evidence concerning the animal carcinogenic potential of these exposure variables would benefit the planning and interpretation of epidemiologic studies in this area. The wiring code variable, which may represent a complex of sociological as well as physical parameters in epidemiological studies, has no meaningful equivalent in animal experiments. If an animal model of EMF carcinogenesis can be developed, it would help greatly in elucidating which exposure metrics might be useful to assess in epidemiologic studies. Animal experiments to investigate various exposure metrics should be set up. The most interesting end points would be leukemia and brain tumors. If there is any further support for the hypothesis that melatonin levels are affected by EMFs, then mammary tumors also would be worth examining. Different types of experimental protocols would be needed to evaluate different possible mechanisms of action (e.g., complete carcinogen versus promoter).
National Survey of Domestic Exposure and Ecological Studies

Little information is available on how the exposure variables, wiring code, or the various EMF metrics may vary from region to region and from city to city. Such information would, at the very least, be useful in the selection of appropriate sites for carrying out the case-control studies mentioned above. But even further, such information may be useful in assessing the feasibility of, and eventually the implementation of, ecological studies. Although the limitations of ecologic studies are not to be minimized (21,22), these limitations should not prohibit the use of such studies where they may be useful. The opportunity for distortion in ecologic studies is minimized when the intercommunity variation in the exposure variable is relatively large compared to the intracommunity variation. Wiring characteristics and/or electric and magnetic fields in buildings may be factors that vary substantially from county to county and from city to city. If so, these would be beneficial variables to include in a geographic correlation study of childhood cancer risk, notably leukemia and brain cancer. Because mortality rates are readily accessible for all causes, many types of cancer and even noncancer death rates can be assessed. Also, it would be desirable to use incidence rates, but this would limit the outcomes and the possible geographic areas to those covered by tumor registries. Such a study would relate some aggregate measure of exposure to wire codes and/or EMFs to rates of any type of cancer or any other health outcome. In fact, for a couple of reasons, it would probably be more successful in detecting an association with a childhood tumor than with an adult tumor. First, the induction period probably would be shorter for childhood tumors; thus, the current aggregate exposure index would be more etiologically relevant for childhood tumors. Second, the opportunity for confounding by other factors is probably greater for adult tumors than for childhood tumors because the web of causation is probably more complex and drawn out in time, which would make it more likely to vary from place to place.

Because information on wiring characteristics and fields is not readily available at the aggregate (e.g., city, or county, or state) level, it would require some effort to carry out representative field surveys in selected areas. It would be desirable to collect information on potential confounding factors among the aggregate units. Some would be available from sources such as the census bureau. Some confounder data, notably social characteristics of the families residing in different homes, could be collected in conjunction with the field surveys of wiring codes and EMFs. The collection of such data would allow estimation of exposure-confounder associations at both the individual and aggregate levels.

Surveys of as few as 30 to 100 representative households per area may be enough to address the issue of interarea versus intraarea variability in wiring code distribution. If the interarea variability in wiring code is large compared to the intraarea variability, then an ecologic study with as few as 10 to 20 ecologic units may provide useful results. Such a study could be quite easy and not too expensive to mount. For relatively low marginal cost, it also would be possible to evaluate some simple measured EMF metrics.

Neurobehavioral Effects

The biologic plausibility for neurobehavioral effects is somewhat higher now than it is for other disease outcomes (5). However, despite a substantial amount of literature, the evidence for such effects is too flimsy, and the biologic rationale is insufficiently compelling to justify giving high priority to research in this area. Nevertheless, methodologically sound studies should be encouraged in this area, and the findings should be monitored. For short-term effects, it should be possible to generate fairly clear indications of whether there is an effect.

Reproductive Effects

The evidence concerning reproductive effects is inconclusive and inconsistent. Because of the public’s concerns and because of the possible link between carcinogenicity and teratogenicity, it would be justifiable to pursue studies in this area. Because the induction period between exposure and disease may be shorter than the period for carcinogenic effects, it may be easier to relate exposure to reproductive effects than to cancer, if there are such effects. The disadvantage of studying reproductive outcomes as opposed to cancer is the notorious difficulty of ascertaining outcomes. The reproductive effects may be nonspecific; thus, it may be appropriate to focus on such things as birth weight, congenital malformations as a class, and perhaps even sperm quality. As the impetus for such studies would come from the analogy between carcinogenicity and teratogenicity, rather than from evidence about reproductive effects of EMFs, many of the arguments used to prioritize cancer studies would prevail here as well. Outcomes should be studied in relation to domestic wire code, measured EMFs, electrically related occupations, and appliance use.

Adult Cancer and Nonoccupational Exposure

There is weak evidence of a differential cancer risk in adults from residential wire codes. Additional case-control studies could be carried out along the lines of the childhood cancer studies. It would be worthwhile to examine risks of leukemia, brain cancer, and following Stevens’ (4) conjecture, cancers of hormone-dependent tissue. However, such studies would be difficult to carry out properly since the etiologically relevant exposure period might be many decades before disease onset (e.g., in puberty for breast cancer), and it would be desirable to obtain at least wire code information on all homes inhabited since childhood—a daunting prospect. If an ecologic study in which information on wire codes and EMFs are collected from different areas is carried out and if the interarea variation is large, then it would be interesting to submit adult cancer rates to an ecologic correlation analysis. As indicated above, however, the interarea variation in other risk factors for adult cancers might be quite significant and would confound the observed associations. It appears that the investigation of adult cancers in relation to EMF exposure would be more effectively conducted in occupational studies, not nonoccupational studies, because there may be a greater likelihood of estimating past relative exposure levels.

General Comments

The above ranking is rather arbitrary. I would consider the top six items to be of high priority and the bottom four to be of low priority. There are studies in progress that correspond to several of the themes listed above. For instance, there are multicenter case-control studies of childhood leukemia in the United States, in Canada, and in New Zealand; there are cohort studies of utility workers in the United States, in Canada, and in France; there are long-term animal carcinogenicity studies in the United States and in Canada, and so on. As they become known, results of these studies may alter significantly our view of the research directions to be fostered. In the above listing, I have included epidemiologic, measurement, or toxicologic research whose results might have a direct impact on epidemiology. Research on basic biological effects of EMFs will continue undoubtedly at its own rhythm, and
the findings may influence the conduct of epidemiologic research in this area.

Problems in the Science and Public Health Aspects of EMF Research

Unique Aspects of EMF Epidemiology

In most respects, the problems of research design, delimitation of disease outcome categories, confounding, and defining appropriate comparison groups are similar in EMF studies to what they would be in other environmental epidemiology studies. However, while many exposure variables are difficult to measure and do not lend themselves to a self-evident exposure metric, EMFs pose particular challenges in this regard. First of all, it is uncertain if the etiologically relevant exposure variable is electric field, magnetic field, or some other correlate of wire code. Furthermore, there is no such thing as a truly unexposed group. Finally, if effective exposure depends on theories of resonance or on exposure to transients, then the notion of monotonous dose–response, which serves with chemical exposures as an additional means of juxtaposing exposed and unexposed, may not be operative. Many exposure circumstances in environmental epidemiology are very difficult to characterize or measure (e.g., air pollution, toxic waste sites), and many are so ubiquitous that it is difficult to identify a truly unexposed group (e.g., ultraviolet light, motor vehicle exhaust). But the peculiar hypothesized models of dose–response (e.g., the resonance model or the transients model) may be unique to the EMF issue. The rest of this paper does not concern specifically issues related to EMFs or wiring code, but rather, it concerns the social and scientific contexts in which the issue is being addressed.

Structural Impediments to Environmental Epidemiology

EMFs from power sources have been part of the urban environment for most of this century, and over a decade has passed since the initial Wertheimer–Leeper report. The available epidemiologic evidence on this issue is still very thin. Our species will continue to live in an environment full of potential health hazards. The hypothetical matrix of all exposures by all diseases is virtually limitless. Our capacity to evaluate each possible association is very limited. These limitations affect our ability to detect and evaluate the effects of any environmental agent, including EMF. What measures, if any, can be taken to enhance the capacity of epidemiology to provide more rapidly better quality evidence for a larger part of the hypothetical matrix?

The number of epidemiologists, and particularly environmental epidemiologists, who cope with the multitude of potential environment–disease associations is small. This can be improved by increasing the training and job opportunities in environmental epidemiology. Although this would help, within the bounds of feasibility, it is unlikely to make a significant dent in the problem. Pouring more money into EMF-related research might help, but it would detract from other equally worthy issues. We need to develop and implement more efficient methods for studying environmental disease associations. The use of experimental in vivo and in vitro procedures for testing environmental agents is not capable of serving as an effective proxy for human evidence. Epidemiologic evidence is still essential.

The traditional biomedical research paradigm of studying a single hypothesis at a time does not serve well when addressing a problem of this magnitude. More efforts should be devoted to large-scale data collection endeavors. Population-based disease registries, such as tumor registries, represent one essential element of a useful intelligence system. Systems for routine registration of exposure are less common but also would be of use. The ideal might be a system that goes beyond the traditional passive disease registry system to something approaching an ongoing case–control study (18,23). As cases are ascertained in the system, a data collection procedure can be implemented to obtain different kinds of information such as occupational history, residential history, and dietary habits. As time goes on, the accumulated data can be analyzed to examine the relationships between the diseases covered by the registry and the various exposure variables routinely collected. A permanent infrastructure to run such a system would be an extremely cost-effective tool that could be mobilized to generate hypotheses in periodic analyses or to test hypotheses suggested by other evidence. The institution of such systems, which should combine the breath of traditional vital statistics functions with the depth of so-called analytical epidemiology projects, would greatly increase our capacity to confront rationally the hypothetical matrix of all environmental exposures by all diseases. The main impediments to such development are in the anachronistic attitudes toward research that doesn't fit into the narrow hypothesis-testing mold of conventional biomedical research and in the structural difficulties of funding such endeavors.

Epidemiology, Controversy, and Public Policy

Since the 1960s, there has been a growing consciousness of the potential harm that environmental pollution can cause. It has become widely accepted that extrinsic factors (as opposed to genetic factors or pure chance) play a role in many, if not most, cases of disease. It has also become clear that the benefits of modern technology have been accompanied by significant degradation and pollution of the environment. In this context, it has been easy for the public and for scientists to entertain, if not readily accept, the hypothesis that pollutant X causes disease Y. The initial reports of carcinogenic effects of EMFs were greeted with skepticism by much of the scientific community. But there was sufficient scientific interest to foster concern in nonscientific circles. In such a context, it was natural for epidemiology to be called upon to carry out the studies to resolve the issue. Like other sciences, epidemiology operates iteratively between hypotheses and empirical evidence. Generally speaking, as valid scientific data accumulate, the underlying truths are elucidated, and at least a consensus about which hypotheses are untenable may develop among informed scientists. But there is no law of nature that determines how much data on a given issue are needed before a consensus develops. Unlike other sciences, epidemiology is frequently drawn into a public policy terrain with little tolerance for uncertainty or equivocation. Issues such as EMFs and human health force epidemiologists to draw inferences concerning causal relations before the data are developed enough to support conclusions. Furthermore, the issue may be so enmeshed in adversary or ideological interests that it becomes difficult to address it in the ideal scientific context of objective disinterest. Scientists are not unwilling victims of this process. Research careers and funding opportunities can be enhanced greatly by engaging in controversial and high-profile research.

The rules of the game differ when scientists move onto this adjoining turf. It is not necessarily the best science that prevails. Suboptimal science, whether motivated by altruistic or base motives, can thrive in such a context. Epidemiology is more susceptible to misuse than other disciplines, because it can be carried out by persons who have little or no specialized training in the area. Because of the
invisible and ostensibly mysterious nature of EMF, research in this area may be susceptible particularly to the social pressures that may detract from methodological rigor. It is impossible to establish hard and fast rules to regulate epidemiologic or scientific competence. Certainly, science must make room for mavericks who are often at the origin of important developments. However, even if mainstream science is sometimes slow to accept new ideas, it is supple enough that valid ideas would not be shunned indefinitely.

As with many environmental health issues, the scientific evidence concerning effects of EMF is weak. This may be because the relative risk is too low to be readily detected by the epidemiologic methods used or because there really is no effect. For public health purposes, this is a crucial distinction, because even a relative risk that is low by epidemiologic standards can produce a nontrivial burden of disease if it is due to a prevalent exposure. Perhaps this is the case with EMFs. What type of public policy should be recommended in such a situation? Should the standards for evaluating causality be relaxed in the case of a potentially significant pathogen? Should we be prepared to conclude that there is an association when the evidence is weak or even contradictory? No. This would not serve science, and in the long run, it would not serve public health. The temptation to cry wolf on the grounds of prudence will begin discrediting epidemiology and science in general. We may have seen some of this already. Is a scientifically conservative attitude tantamount to licensing pollution until the ephemeral scientific certainty is achieved? No. Public health decisions are made with a different set of rules than scientific ones. Scientific evidence is one of the parameters that enters the equation, but there are also issues of social values, economic costs, political will, technological alternatives or fixes, and so on.

It is entirely defensible and even desirable to contemplate a policy of prudent avoidance of substances for which there is weak evidence of health effects. Within reasonable economic constraints, we should try to minimize pollution by any substance, irrespective of known toxic effects, because what we know of toxic effects of environmental agents is only the tip of an iceberg. However, we must resist the temptation to disguise a tough public policy decision in a cloak of ostensibly scientific rigor and precision, whether this abuse of science is in defense of vested corporate interests or the preservation of public health.

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