IEEE Committee on Man and Radiation Home

A Paper Commissioned by the IEEE EMBS Committee on Man and Radiation:

Unfounded Fears: The Great Power-Line Cover-Up Exposed

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INTRODUCTION

The 1993 book, *The Great Power-Line Cover-Up*, by Paul Brodeur (Little, Brown) is a reprinting, with minor modifications, of three *New Yorker* articles: "The calamity on Meadow Street," July 9, 1990; "Department of amplification," November 19, 1990; and "The cancer at Slater School," December 7, 1992. A small amount of additional material has been added to bring some of the discussion up to date. In approach the present book is similar to *Currents of Death*, the author's previous book on the topic of adverse health effects from electromagnetic fields. Presenting himself as an investigative reporter, Mr. Brodeur reviews the facts of two main and several smaller cases of purported cancer clusters arising from exposure to high-voltage

power lines from his own point of view, without the solicitation or inclusion of responses by the parties named in the book. These parties had no opportunity for to publicly rebut Mr. Brodeur's accusations, since the *! New Yorker* did not publish letters to the editor at the time. The IEEE Committee on Man and Radiation (COMAR) commissioned a written response to *Currents of Death* [1]; like that one, this article is intended as a combined response, correction and rebuttal. In its preparation, comments were solicited from various scientists, whose names are listed at the end of the document.

Mr. Brodeur oversimplifies the science of health effects of power-frequency magnetic fields (MFs) in order to support his thesis, with errors and misrepresentations throughout his text. The science is complicated and confusing enough to require simplification in a text directed at a popular audience; however, it is poor reporting to oversimplify to the point of misleading the reader on the strength of the evidence, or to imply with selective quotes that the expertise and/or honesty of workers in the field is questionable. One expects of good reporting that all relevant information sources would be sought out and all statements of purported fact would be verified. A reader not familiar with the issues raised in the book would make these assumptions, unaware of factual information provided in this rebuttal.

The following paper is divided into four sections:

1. a general discussion of epidemiology and statistics;
2. a discussion of Mr. Brodeur's coverage of the draft EPA document on the carcinogenicity of power-frequency electric and magnetic fields (EMF), including comments by scientists quoted by Mr. Brodeur;
3. a correction of statements made in the book with regard to the alleged cancer cluster on Meadow Street, Connecticut; and
4. corrections of statements made with regard to the alleged cancer clusters at Montecito Union and Slater Elementary Schools in California.

EPIDEMIOLOGY

In evaluation of epidemiology, it must be stressed that one study, even if very large and well-designed, is never considered conclusive. Any single epidemiologic study consists of the collection of observations, and that very act is subject to error. Some important points will be missed, some unimportant points will be over-stressed, and so on. These are the same kinds of errors made in laboratory experiments. In the laboratory, enough subjects must be tested to ensure adequate statistical power. Alternatively, if the results are controversial or if the study is small, a second or third laboratory should repeat the experiment. When a second, third, or fourth experimenter or epidemiologist finds very similar results, scientists are more confident that the original study is replicable. That is, the inevitable errors and biases are presumably different in the different studies, and since the results are the same, the random errors are presumably not affecting the results. ! When two studies with similar designs find different results, and the

differences cannot easily be explained or rationalized, neither study is accepted as definitive. Thus, although Matanoski's [2, 3, 4] New York telephone study and Feychting and Ahlbom's [5, 6] study of Swedish children are properly acclaimed as well designed, neither is exempt from producing biased results. To refuse to accept the results of either study as "the last word" is not cover-up, but good scientific caution.
Dr David Korn, Professor of Pathology at Stanford University, puts it this way:

> As an observational science, epidemiology is particularly subjected to errors of bias (unintended and unrecognized) and of confounding. At best, it is able to demonstrate associations or correlations that, absent very specific circumstances, do not permit one to say anything definitive about, let alone establish, causality.

In this regard, Mr. Brodeur criticizes Dr. David Savitz's quoted recommendation that "doing nothing [about EMF exposure] may not be irrational" (p. 80): "This seemed an unusually sanguine observation from the author of a study that reported finding nearly twice the expected number of cancer cases among children living near high-current wires..." (p. 80).
Dr. Savitz reiterates:

> My advice was that the current data are sufficiently inconclusive that a rational person might have a variety of responses, ranging from doing nothing to doing relatively easy things to reduce exposure. The citations of `nearly twice the expected number of cancer cases' and `four times the expected rate' are both technically correct, but isolated observations.

No one study stands alone. Particularly with observational studies such as those in epidemiology no researcher can cite his or her own work as conclusive. This principle is clear to professional epidemiologists, t! hough apparently not to Mr. Brodeur. The proper approach is to consider all studies together.

Mr. Brodeur demonstrates a lack of understanding of what is meant when a study finds elevated rates of a combination of different diseases. In one example he states:

> one student at Montecito Union [School]...had been afflicted with testicular cancer but had not been included in the study, because the identified cluster did not include such cancers. Such reasoning seemed arbitrary, in light of the fact that both Wertheimer and Savitz had found that deaths from cancer of all parts of the body were significantly elevated among children living near high-current wires. (p. 63)

A study may find an elevated risk for all cancers, but as Dr. Savitz notes, such associations are "not necessarily applicable to specific sites." Single exposures have been found to cause specific diseases or cancers in most cases, and the Centers for Disease Control guidelines for investigating clusters state: "a variety of diagnoses speak against a common origin" [7]. "Cancer is not one, but many diseases, with different medical characteristics and different causes" [8]. When a study finds elevated numbers of all cancers, one or two cancers may be elevated, perhaps because of random variation, accounting for the finding. The results are generally used as an

indication that more work should be done, and cannot be used to assume that numbers of each different cancer type in the study are elevated.

Another area of misunderstanding is natural clustering. Mr. Brodeur believes that if physicians were presented with a map showing:

> ...a forest of pins [denoting cases of cancer or birth defects]...near the substation, [they] would no doubt have ascribed it to chance statistical variation--the rubric under which members of the nation's medical and scientific community have long chosen to file away (and avoid dealing with) cancer clusters.(p. 41)

If an illness occurs randomly in a population, that does not mean the cases will be evenly spread over the area examined. This is true even if the individual dwellings are spaced evenly over an area (though, of course, the latter is never precisely true). To illustrate the natural clustering of random events, consider scattering seeds over a perfectly smooth surface. If no effort is made to aim them precisely, some will be found close together, while a few will be very far from the others.

Alternatively, consider rolling a die repeatedly and keeping track of how frequently a given number comes up. Assuming random rolls, you would not expect to roll one of each of the six numbers before you roll a repeat. If you should roll the die 600 times, you would expect to roll the same number several times in a row, and statistical methods can be used to predict how many such "runs" you can expect and how long they may be. You would still expect to have very close to the same number of throws of each face at the end; thus the "runs" would not be significant. This is what is meant by random clustering, and it accounts for the fact that clusters of cancer usually have no single discernible cause. Again, to answer "we don't know" to the question of why any apparent cluster is present is not an attempt to hide the truth, but rather to tell it. Most often, health officials honestly cannot tell if there is any cause of the apparent cluster other than chance.

Unless the number of cases is very large, an apparent cluster can rarely be distinguished from a pure chance occurrence. Thus epidemiologists check for statistical significance of data, usually at the 95% level. They use statistical tools to help distinguish chance occurrences (like the "runs" of numbers on the dice throws above) from non-random increases, i.e. those due to an external cause. If pure chance cannot be excluded with at least 95% certainty, as is very frequently the case in EMF studies, the result is usually called not significant. The observation may not mean a thing outside the specific population studied. Most often the statistical information available is expressed as an odds ratio (OR) and confidence interval (CI). The OR is the estimate of an exposed person's risk of the disease in question relative to an unexposed person's risk of the same disease. The CI is the range of ORs within which the true OR is 95% likely to lie, and when the CI includes 1.0! (no difference in risk), the OR is commonly defined as not statistically significant.

Mr. Brodeur's discussion of a study of childhood leukemia in Los Angeles County [9] (pp. 141-142) illustrates his failure to understand statistical significance. This study found a statistically significant increase in risk of childhood leukemia for children living close to power lines (OR =

2.15, CI = 1.08-4.26). However, it did not find such an increase for children living in the 10% of homes with highest measured 24-hour mean magnetic fields (over 0.27 μT: OR = 1.48, CI = 0.66-3.29). Mr. Brodeur notes, "the 50% increased risk of leukemia they observed in the highest exposure category--children in whose bedrooms magnetic fields of two and two-thirds milligauss or above were recorded--was not considered to be statistically significant", as though this is an opinion. It is, however, a statement with a particular mathematical definition. The numbers of cases and controls in each category limit the certainty of the results, so that it cannot be said with 95% certainty! that the association seen is not a pure chance occurrence. In fact, it is within a 95% probability that the association is really inverse and residence in such high fields (compared to the rest of the population) actually protects against cancer. In practical terms, such a conclusion indicates there were too few cases and controls included to tell whether measured fields are associated with childhood leukemia or not. The fact that proximity to power lines, but not measured MFs themselves, was found to be statistically significantly associated with disease is one of the conflicts or inconsistencies that makes scientists unsure whether magnetic fields are a cause or promoter of cancer. Perhaps some other factor that is present near power lines, rather than magnetic fields, is a problem; or perhaps sufficiently large, well-designed studies have not yet been done. Again, no one study stands alone.

It should be noted that an epidemiologic association, even if very clear and unambiguous, does not automatically imply that the environmental condition (power-frequency magnetic fields, in this case) has caused the disease in question. Many epidemiologists use a clear set of rules to determine if an association is causal [10, 11]. These include:

1. a strong association (one source suggests risk ratios greater than 3.0 [11]);
2. involvement of a very specific disease (one type of leukemia, for example);
3. consistency with other studies and with available data from laboratory work, cancer incidence trends and other sources; and
4. biologic plausibility based on our present knowledge of biology and physics.

At present, the biologic plausibility and consistency elements are missing in "EMF science", specificity is debatable, and, when risk ratios found are above 1.0, they are weak, typically in the range of 1.5-2.5.

REVIEW OF EPA DRAFT REPORT ON EMF

Woven through his reporting of perceived cancer clusters in California, Connecticut and North Carolina, Mr. Brodeur details his version of the release, review and eventual withdrawal of the Environmental Protection Agency's 1990 draft report on the carcinogenicity of power-frequency fields [12]. The scientists involved are very selectively quoted, and their own versions of events differ from those of Mr. Brodeur. The latter's assessment of the report is summed up on page 112:

> ...the summary and conclusions section of the report contained a persuasive indictment of power-line magnetic fields as a cancer-producing agent.

Later he writes:

> However, in apparent deference to the wishes of White House science and policy advisors, [the report authors] went on to say that they did not consider it appropriate to classify electromagnetic fields as a cancer-producing agent, because the basic nature of interaction between the fields and the biological processes leading to cancer was not understood(p. 113).

The completed report was to be reviewed by the EPA's Scientific Advisory Board (SAB), but according to Mr. Brodeur:

> William Reilly, the administrator of the EPA, had asked D. Allan Bromley, a former professor of physics at Yale University, who was the director of the White House Office of Science and Technology Policy [OSTP] and the chief science advisor to President Bush, to arrange for additional review of the report, to be carried out by the Committee on Interagency Radiation Research and Policy Coordination (CIRRPC), which is an offshoot of the Office of Science and Technology Policy. In August, Bromley not only arranged for the review, but he also sent Reilly a list of 14 people whom he recommended for inclusion on the EPA Scientific Advisory Board's subcommittee [on EMF], and whom he described as being `very knowledgeable' about the biological effects on power-line electromagnetic fields. (p. 115-116)

Mr. Brodeur goes on to describe the list thus:

> Coming from the nation's highest-ranking science-policy official, Bromley's list was disingenuous at best and insensitive to conflict of interest at worst. Of the 14 experts he recommended for Reilly's consideration, four were paid consultants of the electric utility industry, who had testified in behalf of the industry in court cases involving the health hazard posed by power-line electromagnetic fields; three others had published articles suggesting that further research on the biological effects [of power-frequency fields] be suspended; two worked for the Naval Aerospace Medical Research Laboratory, an outfit that during the 1970s had conducted human experiments showing that extra low frequency fields could have significant effects upon behavior; two had been financed by the Air Force, which had been trying to suppress information about the adverse health effects of low-level electromagnetic radiation since the early 1960s; and one worked for the General Electr! ic Company, a firm that had denied the possibility of adverse health effects from such radiation for an equal period of time. The remaining two--[Dr] Robert K. Adair, a professor of physics at Yale University and a former colleague of Bromley, and his wife, Eleanor R. Adair, who is a fellow of the John B. Pierce Foundation Laboratory at Yale's Center for Research in Health and the Environment--had stated publicly on a number of occasions that the nationwide

> concern about power-line magnetic fields was nothing more than mass hysteria. (p 116-117)

All of these characterizations are either inaccurate or wrong. Knowing the others on the list Dr. Robert Adair states: "Brodeur's criticisms of the people on Bromley's list are no more than unsupported innuendo, gross exaggeration, and serious misstatement. The people listed were highly competent and probably more free of conflicts of interest and bias than the committee members chosen by the EPA."

After noting that, "to their credit, Reilly and Donald Barnes, the director of [the SAB], did not select any of the people whom Bromley had suggested" (p. 117), Mr. Brodeur accuses Dr. Bromley of delaying the release of the revised draft:

> On November 26th, [1990], Bromley was briefed at the White House by [Erich] Bretthauer [EPA assistant administrator for research and development], [William] Farland [director of the EPA Office of Health and Environmental Assessment], and Robert E. McGaughy, the project manager and chief author of the report. Bromley then told the three men that the report would alarm the public and asked them to delay its release until it could be further evaluated. (p. 119)

Dr. Bromley's opinions and recollection of events differ substantially from Mr. Brodeur's reporting of them.
Dr. Bromley writes:

> In midsummer of 1990 Bill Reilly and I did have several discussions about the whole EMF question, and I learned that a group in EPA chaired by Robert E. McGaughy [was] in the process of pulling together a review of all the published literature in the field. I asked Bill at the time to be sure that whenever they were finished with the review, that I be briefed on what they had concluded."

He adds that he had heard rumors suggesting McGaughy felt EMF was a hazard, and knew that his long-time colleagues Robert and Eleanor Adair were less convinced than was McGaughy, "but I had not myself spent any significant time looking at the original publications or worrying about the issue itself." Rather than arranging for additional review at Dr. Reilly's request, Dr. Bromley says, "I, in fact suggested to Reilly that it would be appropriate to ask Alvin Young, who was the long-time chair of [CIRRPC] to undertake such a study." He adds that it took some time to convince Dr. Young to do this, as the latter was not convinced the existing science was sufficient, and that:

> because I was concerned about the possible misinterpretation of any formal contact between OSTP and this committee I, for example, insisted that we not do what was standard in all FCCSET [Federal Coordinating Council for Science, Engineering and Technology] activities, namely, have an OSTP/FCCSET representative meeting regularly with the study group. I also insisted that Alvin Young work with Reilly in pulling together the membership of his committee

> which was established under the aegis of the Oak Ridge Associated Universities and that my FCCSET staff remain uninvolved.

Regarding the list of names for the SAB subcommittee, Dr. Bromley writes, "Bill Reilly asked me simply as a personal favor to provide him with a list... and I did so, without any particular thought of possible conflicts of interest, and, of course, in most cases I simply had no knowledge of the connections that Brodeur suggests as part of my alleged sinister effort to pack the committee."

The briefing on November 26th, 1990 did indeed take place, but Dr. Bromley's reaction was not what Mr. Brodeur recorded:
Rather:

> My examination of the report itself indicated that it was a sound scholarly piece of work that claimed neither more nor less than the research itself warranted. What did become clear was that much of the research was not of very high quality and that in many cases adequate control studies were simply missing... The Executive Summary suggested much less uncertainty than did the report itself and, in particular, the vugraphs that McGaughy proposed to use were far more definite than even the Executive Summary in suggesting that the correlation between EMF and the incidence of childhood leukemia was too great to be attributed to chance. As the senior scientist in the Bush Administration, I considered it my responsibility to make certain that statements issued by our Administration on the basis of alleged science did not convey more certainty than the underlying scientific research and analyses warranted. For that reason I insisted that McGaughy not use the particula! r vugraph that was under question, but at the same time I indicated... that I had no concern whatever with the report itself which was fair and represented the underlying science in completely appropriate fashion.

In response to Mr. Brodeur's claim that he asked that the report's release be delayed Dr. Bromley states:

> This is flatly wrong since I had no complaint whatever concerning the report, requested no change in the Executive Summary and only insisted that the vugraphs to be used in the release presentation be consistent with the report. I did say that the entirely unwarranted conclusion on the vugraph would unnecessarily alarm the public and believe deeply this to be the case. My reason for insisting not on the delay of the report but rather the elimination of this totally unwarranted statement was purely on the grounds that within the Bush Administration we believed that scientific statements should be based on science and where uncertainties existed they should be transmitted with the data and the supposed conclusions.

Mr. Brodeur repeatedly makes his claim that Dr. Bromley was attempting a cover-up, by selectively quoting Dr. Bromley again:

> Bromley was again quoted in Time as saying that the EPA's finding of a positive association between exposure to electromagnetic fields and childhood cancer was `unnecessarily frightening millions of parents".(page 123)

Bromley replies:

> Of course, I was concerned about unnecessarily alarming the public, but the reason for this was simply that McGaughy's vugraph was not at all supported by any of the evidence then available, or for that matter since available, nor was it supported by his own report. Here as in so much of his writing Brodeur chooses to use quotations out of context and in incomplete form to bolster his totally unwarranted assumptions.

In other words, the EPA *did not* find a positive association between exposure to power-frequency fields and cancer. Dr. Bromley adds that he is still in full agreement with the conclusions of the CIRRPC report, released in June 1992, that:

> there is no convincing evidence in the published literature to support the contention that exposures to extremely low frequency, electric and magnetic fields...are demonstrable health hazards [10].

Mr. Brodeur goes on to describe testimony on the draft report before the SAB. He writes:

> In preparation for the hearings Crowell and Moring [a Washington law firm that represented many utility companies] had set up an organization called the Utility Health Sciences Group--a coalition of major utility companies that claimed to be interested in promoting research on electromagnetic fields--and the group had arranged for four prominent scientists to come to Washington to give testimony before the SAB subcommittee which would discount the association between electromagnetic fields and cancer. (pp. 124-5)

These four were Dr. David Korn, Dr. Mark Mandelkern, Dr. Edward Gelmann and Dr. Dimitrios Trichopoulos. In his own defense Dr. David Korn, then chair of the National Cancer Advisory Board and familiar with the EMF issue through periodic reports to that body from the Director and other staff scientists of the Division of Cancer Etiology of the National Cancer Institute, states:

> When I agreed to participate in the SAB hearing, I made it very clear to Crowell and Moring that I would present testimony based upon my own views on this controversy, and those views would not be influenced by Crowell and Moring in any way whatever. Although I did provide copies of my testimony to Crowell and Moring shortly in advance of the SAB hearing in which I participated, Crowell and Moring made no changes or suggestions of changes of any kind to me. In other words, the testimony I gave was mine and mine alone.

Mr. Brodeur dismisses Korn's testimony by noting that "he admitted he had not read any of the key papers on the cellular and animal effects of electromagnetic fields" (p. 125). Korn responds:

> In preparing for my appearance at the SAB hearing, I spent a considerable amount of time carefully reviewing in detail the review draft report [in question]. That voluminous report contained extensive references to and reproductions of the available data under the various chapters and subheadings, and indeed, to my knowledge, it represented an exhaustive summary of the published literature relating to the problem at issue... The EPA had taken pains to assemble the strongest evidence available in support of the carcinogenicity hypothesis, and I am convinced that by proceeding as I did, I had an excellent exposure to the best of the data in support of this hypothesis. Those data were internally inconsistent, entirely based on epidemiological associations that were far from robust statistically, and simply not conclusive. Accordingly, Mr. Brodeur's representation of the facts of my familiarity with the issues (p. 125) is highly misleading.

While noting that:

> Mr. Brodeur accurately captured in quotation (p. 125) my key conclusion that `the case for the potential carcinogenicity of power frequency electromagnetic fields is not convincing,' and that the evidence to date was `vastly insufficient to support any kind of sound decision making with respect to new cancer regulatory policy,'...[w]hat Mr. Brodeur carefully neglected to cite was the strong opinion that I expressed in the hearing regarding the urgent need for additional carefully controlled, high-quality research on this issue.

Korn concludes:

> I continue to believe that the data remain at best suggestive, far from convincing, and well short of the threshold required to support regulatory action. Contradictory findings between even the best of the most recently reported studies underscore the continuing circumstantiality of the evidence and the lack of a sound biological mechanism that could explain the epidemiological associations that are described. Thus I continue to believe that additional high-quality research is needed and that the support of such research would represent an appropriate response by the Federal government to this matter of major public controversy.

Dr. Dimitrios Trichopoulos testified at the SAB hearing regarding worldwide trends in cancer incidence. Mr. Brodeur reports:

> Dr. Trichopoulos...suggested that if the proposed association between exposure to electromagnetic fields and cancer were true, the increasing electrification of the nation over the years should have resulted in an `epidemic' of childhood leukemia" (p. 126).

In response, Dr. Trichopoulos cites a World Health Organization's report, published in 1993 [13], that adult leukemia incidence is stable or rising very slightly world-wide. This does not correlate with the rapidly increasing use of electricity. In the same document the risk of childhood leukemia is described as stable over time in the U.S. Also, since much weight has been placed on studies of leukemia incidence in Sweden, it is worth noting that no discernible change in leukemia incidence in adults or children is apparent in Swedish records from 1960 to 1991 [14]. Dr. Trichopoulos observes that Sweden has the most reliable health records of any country. Published health statistics, not utility opinions, support his claim that leukemia incidence is not increasing at a rate anywhere near comparable to the increase in electricity usage.

The draft EPA report on the carcinogenicity of power-frequency EMF was withdrawn in 1991. This action was the result not of a concerted effort by the government and utilities to cover up evidence of hazard, but rather of the reasoned and dispassionate review of the evidence and the report itself by numerous respected scientists. Their reviews are in the public record though Mr. Brodeur chooses not to mention them [10, 15], and their conclusions uniform as quoted above.

SCIENTISTS BIASED BY FUNDING SOURCES?

Evincing mistrust of all scientists who disagree with his point of view, Mr. Brodeur singles out Dr. M. Granger Morgan, an electrical engineer who has been involved with matters of public policy on EMF since 1982. Mr. Brodeur remarks, "Over the years...[Morgan's] attempts to maintain equilibrium while traversing the tightrope of the electromagnetic field controversy had been revealing" (pp. 177-8). Through quotes of Morgan's writings and testimony, Mr. Brodeur tries to suggest that Morgan's views on "EMF" have changed over time, and that those changes are due to change in his funding source, from the National Science Foundation (NSF) and the Department of Energy (DOE) to the Electric Power Research Institute (EPRI). Morgan replies, "Mr. Brodeur does not accurately describe those changes [in my views]. The sources of my funding have not been a factor in those changes."

Mr. Brodeur describes Morgan's statements in a May 1986 article in *Science*:

> Morgan claimed that several years of expensive research into the question of whether electromagnetic fields posed a health hazard had produced ambiguous or `decidedly inconclusive' results that had, nonetheless, alarmed the public and encouraged litigation to be brought against the nation's utilities. (p. 178)

This is contrasted with a 1988 quote "in which [Morgan and others from Carnegie Mellon] declared that `the results from the epidemiological studies are grounds for concern'" (p. 178). But, according to Morgan:

> The thrust of my argument [in the *Science* editorial] was, in fact, precisely the opposite: that recent federal budget cuts were a mistake and we most definitely should not stop research given the current state of knowledge...[The *Science* editorial] was heavily edited to fit space constraints and so might be

> misinterpreted in a hasty reading. But, there is no way to similarly misread the piece I published fifteen months earlier in *IEEE Spectrum* in February of 1985. This piece, which Mr. Brodeur chooses *not* to cite, makes it very clear that I believed then, as I do now, that the evidence should be taken seriously and that there was `an obvious need for research.' A boxed section on `stopping rules' also makes it clear that I was not proposing that research on 60 Hz fields be stopped, as Mr. Brodeur's wording strongly implies. Thus, the flip-flop in views which Mr. Brodeur reports between 1986 and 1988, is the result of some ar! tfully manipulated quotes. It may be the way he wishes things had been, but it is not what happened.

Mr. Brodeur repeatedly casts aspersions on the views of persons with whom he disagrees by pointing out their reliance upon utility support, including EPRI. Here, Mr. Brodeur contrasts what he sees as two different opinions espoused by Dr. Morgan in documents funded from different sources. First, a contract from the Office of Technology Assessment of the US Congress:

> Morgan and two coauthors...went on to describe a `prudent avoidance' strategy, declaring that `by avoidance we mean taking steps to keep people out of fields, both by re-routing facilities and by redesigning electrical systems and appliances,' and that `by prudence we mean undertaking only those avoidance activities which carry modest costs' (p. 179).

Second, a brochure funded by NSF and EPRI:

> In it Morgan defined his concept of prudent avoidance in a manner that was considerably more palatable to the power companies... `If you are buying a new home it might be prudent to consider the location of distribution and transmission lines as one of the many things you consider,' he wrote. (p. 179)

After detailing other presumed reversals in Dr. Morgan's views, Mr. Brodeur ends with:

> Considering the contradictory nature of Morgan's assessments of the power-line hazard over the years, it seems ironical but not surprising that in the summer of 1991 he should join his colleagues on the SAB's subcommittee in telling the EPA that its report on the potential carcinogenicity of electromagnetic fields required `logical reorganization and complete rewriting with particular attention to careful and precise use of language.' (p. 181-182)

Dr. Morgan denies any cover-up due to funding sources or any other pressure:

> EPRI has been the largest and most consistent supporter of 60 Hz fields research. Virtually every serious researcher in the field has at one time or another received support from EPRI. While their funds come from power companies, by and large, they have done a pretty good job of avoiding motivational bias in the work they

> have supported. A number of the key positive health studies have been EPRI funded.

He notes that Mr. Brodeur fails to mention publications in which Dr. Morgan has strongly urged utilities to adopt prudent avoidance and has stated that "the general public wants significant levels of risk abatement."
Dr. Morgan also notes:

> Unlike Mr. Brodeur, I have never testified in any power-line citing controversy or similar adversarial dispute, and have never realized any income from such a dispute. And, unlike Mr. Brodeur, none of my income comes from selling books on this topic, so I have no financial stake in sensationalizing it.

One important development in the field that Mr. Brodeur fails to mention is the passage of the Energy Act of 1992, which sets up an ambitious research program funded half by tax revenues and half by electric power interests, including utilities and appliance manufacturers. Indeed, the utility industry sought to have the program set up. The utilities raised their portion of the funds soon after the bill passed, because the public wanted unbiased research done on the health effects of power-frequency fields. Far from a conspiracy to cover up health risks power-frequency fields, this project is evidence of good faith on the part of both utilities and the federal government to get answers to the public's questions.

SPECIFIC CASES

The book concentrates mainly on 3 perceived cancer clusters: one near a substation in Guilford, Connecticut; one in Montecito, California; and one in Fresno, California, the latter two involving schools near power lines. Several errors of fact in regard to these three cases should be corrected, and some comments regarding the sciences of electric and magnetic field bioeffects and epidemiology are pertinent.

Guilford, Connecticut

The discussion of the Meadow Street substation in Guilford, Connecticut, was originally published in the *New Yorker* as two articles: "Calamity on Meadow Street," July 9, 1990 and "Department of Amplification," November 19, 1990. Northeast Utilities (NU) owns the distribution substation on Meadow Street through its subsidiary, Connecticut Light and Power. In response to Mr. Brodeur's articles, NU and the Connecticut Department of Health Services wrote letters to the *New Yorker* to correct errors of fact and clarify their statements and actions. The *New Yorker* did not at that time publish letters to the editor, and the errors were not subsequently corrected in *The Great Power-Line Cover-Up*. They are therefore discussed below.

Reporting Errors

In the description of the electric distribution system in Guilford, Mr. Brodeur states that the substation in the past had "several 115 kV transmission lines" feeding it (p. 19). This has never been the case. The substation is a 27 kV/13.8 kV distribution (low-voltage) station. He claims "measurements taken at various places near the peripheral fence of the facility...showed magnetic fields ranging from 20 to several hundred milligauss [2 to 100 µT]" (p. 17) and that "strong magnetic fields can often be measured within a hundred feet of a distribution substation" (p. 18). Extensive measurements made by Enertech, a consulting firm and manufacturer of magnetic field measuring equipment, did not exceed 0.65 µT at the road 20 feet from the fence and directly beneath exiting power lines. After measuring dozens of distribution substations, this writer can strongly affirm the claim of NU that at the fence magnetic fields could not reach "several hundred" milligauss. T! wo µT is a reasonable number; 10 µT is extremely unlikely. Certainly magnetic fields near transmission substations can reach into the tens of µTs, but such facilities are rarely sited near residential districts.

Mr. Brodeur repeatedly refers to "high-current distribution lines" or "high current feeder lines." While the lines around the Guilford substation are built to handle high currents, utility information shows that most have not carried heavy power loads as far back as 1970. The only lines carrying high currents (over 200 A) are two 13.8 kV distribution lines that exit the substation underground and switch to overhead lines some 100 ft north of the substation. They are configured in a compact arrangement that significantly reduces magnetic fields nearby. Currents on the other lines leaving the station are in the range of 20 A, and do not produce magnetic fields at ground level greater than 0.7 µT. NU did inform Mr. Brodeur of these facts in letters to the *New Yorker* dated July 12 and November 8, 1990. However, the text of the book has not been changed from the original in the *New Yorker* to include these corrections.

Minor matters of fact are presented by Mr. Brodeur in an apparent attempt to indicate at the least NU's unhelpfulness in relation to the Meadow Street residents.
He tells the reader:

> On the morning of January 29th, yellow trucks bearing the logo of Northeast Utilities were to be seen everywhere in Guilford. Three trucks... were parked on Meadow Street... farther along the street a yellow van belonging to Northeast Utilities and bearing the sign `infrared survey' was parked beneath the wires" (p 15).

Later he says:

> ...on February 5th town officials were planning to meet with officials of Connecticut Light and Power and discuss the health problems that had been reported. The meeting... was soon canceled, however, and not rescheduled. Over the six weeks following [publication of a newspaper article on Meadow Street] the utility's yellow trucks and vans kept showing up at the Meadow Street

> substation with such frequency that some residents of the street began to jot down their license numbers... They concluded that the company was engaged in an effort to reduce the amount of power being handled by the substation. (p. 18)

He also reports that the substation had at various times "exploded" (pp. 12, 27-28, 39). On all of these points at the time of original publication in the *New Yorker* and again when the second article was published, NU tried to correct Mr. Brodeur's misinformation. None of his original statements have been changed in the book, even though utility officials repeatedly informed him that:

1. utility workers were in the area for regular maintenance according to a schedule established weeks earlier;
2. no infrared survey van belonging to NU was in the area at the stated time;
3. the meeting between Connecticut Light and Power and Guilford officials did indeed take place, at the originally scheduled time so far as NU people are aware, and was covered by the press;
4. the substation transformers had never "blown up" or otherwise malfunctioned since they were installed in the early 1970's.

It is possible residents thought fuses blowing were really transformers exploding; fuses are designed to blow when large power surges such as those caused by lightning strikes or downed power lines threaten to overload the transformers. They are noisy and throw off rather spectacular showers of sparks when they blow, but certainly do not release any gases or other substances harmful to human health, as one might possibly expect of oil-filled transformers if they were opened or broken in some way.

Being alerted to and upset about the substation and the power lines, residents could reasonably be expected to notice more power company vehicles whether or not there really were more in the area. What they were doing and why could easily have been determined by checking with the utility, and in fact the utility did inform the *New Yorker* without being asked after the first article was published. Their information was never used.

Errors in Scientific Understanding

Mr. Brodeur associates television picture interference with power lines, assuming it is an indication of very strong fields or possibly of high-frequency fields. These, he implies, might be somehow more damaging to human health. Strong static or extremely low- frequency (ELF) magnetic fields near a television set or computer monitor may produce the "drastic warping and blurring" cited by Mr. Brodeur on page 23. "Drastic warping" would be caused by a very strong inhomogeneous field; power lines outside homes create very uniform fields at distances greater than 10 feet and thus would not warp pictures. Research on jitter of computer monitors [16, 17] shows that power-frequency fields above a threshold between 1 and 5 μT produce noticeable blurring (rapid jitter, or movement of the entire picture) of computer monitor screens. This is a work-related problem in some offices that is seldom seen in homes; distribution lines are usually

to! o far away to create such strong fields. Television screens are less sensitive to 60-Hz fields; 10 µT is a reasonable threshold for noticeable blurring.

Higher-frequency electromagnetic fields are generated by corona on power lines and by sparking caused by minor flaws in insulators or unwanted matter on lines (tree branches, leaves, squirrels, etc.) These fields are in the same frequency range as AM radio but far too weak to be a health hazard: standards for safe exposure to electromagnetic fields of these frequencies are agreed upon internationally [18]. The interference they produce on a television set is a pattern of discontinuous horizontal lines or bands, not warping. Interestingly, Mr. Brodeur notes that the person complaining of television interference "arranged to have cable television installed in his home," which presumably cured the problem. If the interference were caused by power-line magnetic field this action would not have helped, since the field would affect the electron beam in the television tube, not the incoming signal to the antenna.

Finally on pages 85-86 Mr. Brodeur states that "the residents of Meadow Street were living so close to substations and high-current wires that they were continuously exposed within their own homes to electromagnetic fields of occupational levels." The results of extensive measurements of magnetic field exposures among workers in "electrical" occupations in Los Angeles show that time-averaged exposures lie between 0.16 and 2.36 µT depending on the specific occupation, while non-electrical workers experience a mean exposure of 0.17 µT [19]. The mean across all electrical occupations was found to be about 1 µT. A substation is much like a large household electric appliance: power-frequency fields are very strong near the transformers, switches and busses within the station, but drop off rapidly with distance. Enertech's measurements on Meadow Street are in agreement with measurements near many other substations [20]: peak mag! netic fields 20 feet from a substation's fence rarely exceed 0.8 µT, and for distribution substations such as that on Meadow Street, 0.2 µT, away from incoming lines. The homes on Meadow Street closest to the substation are across the road, 40 feet from the substation fence. Enertech's measurements in November of 1992 show the 60 Hz magnetic field to be less than 0.2 µT along the road in front of these houses. The residents of Meadow Street are exposed to fields at or below the time-averaged strength non-electrical workers experienced in the Los Angeles study.

Montecito Union Elementary School

In the second case described by Mr. Brodeur, an apparent cluster of childhood leukemia cases consists of students at Montecito Union Elementary School in California. A substation and power line are located near this school. In response to letters from the California Department of Health Services (CDHS), some changes in claims of measured magnetic fields and of cancer rates in the school were made before the *New Yorker* articles were re-published in Mr. Brodeur's book. Other errors pointed out in these letters were not corrected.

Reporting Errors

With respect to measured magnetic fields around the school, Mr. Brodeur states:

> On Sunday, September 24, [1989]--a time of the week when power demand is invariably lower than on weekdays... staff members of [CDHS], who had never made electromagnetic-field measurements before, used borrowed equipment to measure the strength of the magnetic fields [in the vicinity of the school, power line and substation].. .Even on Sunday...a level of 12 mG [1.2 µT] was found under the power line opposite the substation, and one of four magnetic field readings taken on the kindergarten patio was nearly 2 mG [0.2 µT] -- a level just below that shown in 3 different epidemiological studies to be associated with twice the expected incidence of cancer among children. (pp. 57-58)

In response, Dr. Lynn Goldman of CDHS wrote in a letter to the New Yorker in November of 1990:

> Utility engineers from Southern California Edison estimate that weekend current use in large residential areas such as Montecito would not be much different than weekday use. In fact, the Sunday measurements taken outside of classrooms in the September, 1989 survey were at most only a few-tenths of a milligauss lower (equivalent to the contribution of the classrooms' ceiling-mounted fluorescent lights) than the weekday measurements taken in the center of the same classrooms in the February-March 1990 survey [21].

Magnetic field data collected over a week in homes near power lines serving several residential areas show *peak* field magnitudes are more often than not the same on weekends as weekdays, and the peak occurs at about the same time in late afternoon regardless of the day of the week [22]. Mr. Brodeur emphasizes a measurement of "nearly 2 mG" [0.2 µT], but Most epidemiologic studies find no increased rates of cancer for children whose calculated average annual magnetic field exposure is less than 0.2 µT.

Later Mr. Brodeur mentions other measurements taken in March 1990 by Enertech:

> The results showed levels of between four and six mG on the kindergarten patio and along its fence; a level of almost 7 mG [0.7 µT] on the benches beneath the feeder line; a level of 17 mG [1.7 µT] in the corner of a classroom on the southeast side of the school; and levels of between 600 and 1000 mG [60-100 µT] next to the transformer in the parking lot (p. 67).

Dr. Goldman wrote further in her letter to the *New Yorker*:

> Not only did [Mr. Brodeur] confuse the location of these measurements, but [his] description of these measurements as `between 4 and 6 mG' [0.4-0.6 µT] is inaccurate and misleading. The spot measurements of 4.3, 5.0, 5.2 and 0.9 mG [0.1-0.5 µT] were recorded on the kindergarten playground, not the... patio. In the May draft report we reported that the 4.3 mG [0.4 µT] reading was recorded on a

> heater/air conditioner unit, and that most of the playground, with the exception of the northern edge and the heater, had magnetic fields below 2 mG [0.2 µT].

In a previous letter to the *New Yorker*, dated August 1990, Dr. Kreutzer and Mr. Schlag, also with CDHS, wrote:

> Levels of between 200 and 1000 mG [20-100 µT] were found directly on the transformer in the parking lot, not next to it. The levels fell [to] between 2 and 11 mG [0.2-1.1 µT] just four feet away.

In describing the CDHS report Mr. Brodeur writes:

> [The CDHS authors] went on to say that the levels near the power lines along the north side of the school, which had been found to include some distribution wires buried in an alley along the kindergarten patio, were in the 5-30 mG [0.5-3 µT] range, and they described these fields as `similar to what one is exposed to when near a common electrical household appliance such as a TV or a radio.' This, however, had little, if any relevance to the situation at Montecito Union, for the simple reason that one would be exposed to five milligauss [0.5 µT] from a television or radio only if one sat within a few inches of it, and to thirty milligauss [3 µT] only if one pressed one's face to certain locations on its side (pp. 68-69).

The range of magnetic field directly under the lines (0.5-3 µT) is correct; however, 25 feet from the lines fields are measured below 0.25 µT. The objection to comparing the field strengths to those from electric appliances is unfounded; children do not spend extended periods directly under power lines any more than they do directly in front of electric stoves or dishwashers, appliances with comparable mean magnetic fields at a distance of six inches [23]. The EPA finds a typical color television produces a magnetic field of 0.7 µT within a foot from its screen.

Errors in Scientific Understanding

On pp. 63-64 Mr. Brodeur calculates a cancer incidence rate at the school based on five cases of leukemia or lymphoma and one case of testicular cancer diagnosed over a period of 8 years among children attending Montecito Union.
Dr. Goldman wrote the *New Yorker*:

> In clarification, five of the seven Montecito children with leukemia or lymphoma attended the school. Of those five, two attended the school for a very brief duration, leaving only three with plausible cases for school exposures as a possible cause. Not only is there a problem with school attendance history, but also, on review of the measurements, we conclude it does not appear that the time-averaged magnetic field exposure of a child attending that school would be dramatically different from that of other children across the United States.

The testicular cancer case cannot be lumped with the other cancers. Dr. Goldman explained: "when studies find elevated rates of `all cancers', `other cancers' and `other tumors' this does not mean that rates of every type of cancer were increased."

On page 81, Mr. Brodeur claims the magnetic fields in the school were "approximately half as strong as those associated with a seven-fold increase of leukemia among telephone company cable splicers." His reference to Matanoski et al. [2], is completely irrelevant to the question of whether the measured field strength (0.01-0.22) next to buildings [24]) is harmful. The values of measured magnetic fields are well within the range of measured fields for "non-line workers" [2] (and thus used as the presumed healthy group to which the cable splicers were compared), and also in the center of the range of fields called "typical residential" by the EPA [25]. A field strength "half as strong" as the 0.43 µT mean exposure of cable splicers has not been linked with adverse health effects in any published epidemiological study.

Dr. Kreutzer and Mr. Schlag explained CDHS's position on the Montecito cancer cluster in their letter to the *New Yorker*:

> Investigators concluded that the transformer was most likely not the cause of the cluster for several reasons. First, not all cases in the cluster... went to the school, and only one attended... classes in rooms closest to the transformer. Second, childrens' exposure to the transformer fields would have been episodic and brief since the higher levels occurred where children would briefly pass but not dwell... Third, there was no evidence that the field strength from the transformer had changed coincidentally with the emergence of this cluster... Finally, the epidemiologic studies... did not suggest a strong enough association to account for [the observed] increase in cancer.

Slater School, Fresno, CA

The third cancer cluster Mr. Brodeur discusses is at Slater Elementary School in Fresno, CA. This school has four sections, called Pods, spaced around a central area; Pods A and B are on the side of the school nearest the road and the transmission lines. Mr. Brodeur introduces this case through a reporter for a Fresno newspaper:

> half a dozen women who taught at [Slater]... were interviewed in the teacher's lounge by Amy Alexander, a staff writer for the Fresno Bee, who wanted to know if they were concerned about the presence of a pair of high-voltage transmission lines that ran past the school (p. 105).

Noting that the school sits "only a hundred feet or so" from the power lines, Mr. Brodeur goes on to describe the interview:

> [The women] were quick to inform [Ms. Alexander] that an unusually large number of teachers and teacher's aides at Slater had developed cancer in recent years." (p. 106).

Mr. Brodeur's thesis, as he describes the Slater situation, is fourfold: 1) there is a cancer cluster among teachers and staff at Slater School, specifically in Pods A and B; 2) the school is "close" to power lines; 3) the magnetic fields in the school are "high"; and 4) CDHS, and specifically Dr. Raymond Neutra, were unhelpful and even obstructive in their dealings with the problem, as perceived by the Slater School staff and parents.

Alleged Cancer Cluster

The number of cancer diagnoses confirmed among teachers and staff at the Slater School from 1972 through 1992 increases through Mr. Brodeur's book, from 8 on p. 189 to 11 or possibly 14 on p. 276:

> Neutra and Glazer... had confirmed eleven cases among past and present teachers and employees. The eleven confirmed cases included 3 breast cancers, 2 uterine cancers, 2 ovarian cancers, 2 melanomas, the brain cancer that had killed Katie Alexander, and the colon cancer that had killed Curtis Hurd.

The CDHS report on the alleged cluster [26] lists 13 confirmed cases from 1973 to 1992. Mr. Brodeur goes to great lengths to make these numbers sound high; in reality, "probability theory would suggest that 25 out of any 1000 schools would show this kind of excess by chance alone, and California has approximately 8000 schools" [26]. The total of seven confirmed cancers among children who had attended the school during the same time period is considerably less than the expected number of 27. Thus in number of cancer cases, at least, Slater School is not unique among California schools, whether or not it is in proximity to power lines.

Wire Code and Distance to Lines

The Fresno Bee article motivated the school principal to ask that the electric utility, Pacific Gas and Electric (PG&E;), take magnetic field measurements around the school. Mr. Brodeur summarizes the utility's report:

> PG&E;'s facilities near the Slater Elementary School included a 230-kV double-circuit transmission line and a 115-kV double circuit transmission line, and that the latter was located `approximately 100 feet from the nearest classrooms'.(p. 132)

This sounds ominous, since distance to power lines has been used as a measure of magnetic field exposure, and associated with increased risk of cancer in some epidemiological studies [9, 27, 28, 29].
The Neutra-Glazer report states [26]:

> Living within [zero] to 50 or 51 to 129 feet of a transmission line was considered, in these studies, to be a `very high' or `ordinary high' exposure respectively... [However,] parts of some classrooms in Pod A but none of Pod B or the central area fall within 129 feet of the line. Pod B is more than 150 feet and the central area is between 175 and 240 feet from the lines. Thus, the majority of the [cancer]

> cases worked in classroom areas situated at distances not associated with excess childhood cancer risk.

Mr. Brodeur does review this argument:

> ...faced with the problem of having to explain why so many cancers had occurred among people working on the south side of Slater School, Neutra, who had previously declared the wiring code didn't apply to California, decided it was relevant, after all. (p. 224)

Dr. Neutra states:

> I don't recall ever having declared such a thing. We [at CDHS] have used wire codes in our own studies and those we have funded.

After presenting the distance facts above, Mr. Brodeur argues:

> if Neutra had considered the magnetic-field levels that had been measured in several of the epidemiological studies in which the wiring-configuration categories where employed as a surrogate for exposure, he might not have been so quick to make this assertion. For example, [researchers in one study [28]] had measured an average magnetic field of only 2.5 mG [0.25 µT] in a sampling of very-high-current homes and of only 1 mG [0.1 µT] in ordinary-high-current homes. (p. 225)

In making this statement Mr. Brodeur is misusing the data of the cited study, in addition to suggesting that two experienced health professionals did not know their own business. The authors of the cited study examined only wire codes, not measured MF strengths, in relation to cancer incidence in adults. It is very difficult to find a scientifically adequate study that reports positive associations of magnetic field levels with leukemia and brain cancer in adults, let alone the diverse cancers found in adults at Slater School. In presenting the specific wire codes within which the school fell, Drs. Neutra and Glazer were dealing only with data from studies indicating that high wire codes are associated with high cancer rates.

Measured Magnetic Fields

Magnetic field strengths were surveyed several times at the Slater School. Mr. Brodeur paraphrases the utility report cited above:

> It went on to say that between three and four o'clock on the afternoon of Dec. 17, 1990, a magnetic-field level of 7.6 mG [0.76 µT] had been measured directly beneath the 115 kV line on Emerson Avenue; a level of 5.2 mG [0.52 µT] had been found at the curb on the school side of the avenue; a level of 4.3 mG [0.43 µT] had been found by a fence separating the school grounds from the sidewalk by the avenue; and levels of 1-2 mG [0.1-0.2 µT] had been measured at nine locations immediately outside Pods A and B. (p. 132)

Because electricity demand in the area is highest in summer, other readings were taken in June and July by and/or at the request of teachers at the school. Mr. Brodeur writes (p. 165):

> ...on July 5th -- a hot day in Fresno, when air-conditioning use was heavy -- [a] first-grade teacher... had measured magnetic-field levels of between 3.5 and 4 mG [0.3-0.4 µT] in an adjacent classroom... 4 mG [0.4 µT] is almost equal to the average daily exposure levels of the 4,500 New York Telephone Company cable splicers in whom Matanoski and her colleagues at Johns Hopkins had found the incidence of leukemia to be seven times higher than expected, and cancer of many other types to be elevated. At no time did Neutra... express any concern to [the teacher reporting the measurements] over the magnetic field levels she had measured, or inform her that they were approximately the same as levels that had already been associated with a significantly increased cancer rate in children.

The cable splicer comment is again in reference to Matanoski et al. [2]; it is unclear to which study or studies the comment on childhood cancer refers, but no study has associated average fields less than 0.2 µT with cancer. Mr. Brodeur is equating a peak measurement in one situation with an average level in another. If the peak fields in the Slater School were no greater than 0.4-0.5 µT, the average fields are considerably less, since peaks last only a few minutes out of a day.

As Mr. Brodeur again implies that Dr. Neutra did not know his job, it is appropriate to let Dr. Neutra speak for himself. In their report of the alleged cancer cluster Drs. Neutra and Glazer state [26]:

> ...measurements on the hot days of August and September 1991... read an average of 1.05 [0.1 µT] and a ten-minute peak of 2.01 [0.2 µT]. The teachers reported peak fields between 2 and 4 mG [0.2-0.4 µT] in the room in about half the days which they monitored in July 1991. Midroom measurements in Pods A or B by several other observers during the last half of 1991 were around 1 mG [0.1 µT]

They add that a review of data on electric currents on the power lines and computer-generated estimates of maximum and minimum fields in the school building:

> suggested that the fields in the middle of the nearest room of Pod A would have peak fields below 2 mG 70% of days and in the closest room of Pod B, 95% of days.

This is in agreement with measurements listed in the CDHS report as well as all measurements quoted by Mr. Brodeur. The authors of the well-known "Swedish study" [5, 6] used the same method of estimating MFs from electric current data to determine exposure.
[Further from the report by Drs. Neutra and Glazer [26]:

> Thus the majority of the campus where cases have been reported is not within a distance (129 ft of a transmission line) or does not have a long-term annual average magnetic field (2.5 mG [0.25 µT]) which has been associated with

> childhood cancer in the epidemiologic studies which showed a positive link between magnetic fields and childhood cancer

The lawsuit brought by relatives of teachers at Slater School who had been diagnosed with cancer has been dropped. A California Court of Appeals ruled that such cases should be heard by the California Public Utilities Commission, which cannot award damages, so the plaintiffs agreed to drop the case in exchange for Pacific Gas and Electric's paying its own legal expenses [30].

This section is best concluded with the words of Dr. Richard Kreutzer and Mr. Robert Schlag of CDHS, from their initial letter to Mr. Brodeur at the New Yorker following publication of his first article on the Montecito Union School. They wrote:

> Your misstatements and innuendos have been issued without offering the affected parties [residents, county and state health department staff and nationally prominent scientists] an opportunity to respond. We urge you to promote your cause with higher journalistic standards.

CONCLUSION

Whatever Mr. Brodeur's motivation for writing *The Great Power-Line Cover-Up*, it is apparent he has failed to thoroughly research his topic. The book reveals a lack of understanding of epidemiology, although the author's thesis is strongly dependent on epidemiological data. Stated facts about the transmission of electricity, both specific to utilities and in general, are missing or in error. Historical facts that would seem to be pertinent are also missing. For example:
- the reviews of the draft EPA report on EMF carcinogenicity are in the public record, available for anyone's perusal;
- the U.S. Government, far from attempting a cover-up, passed a large research program to attempt to determine the carcinogenicity of magnetic fields; and
- the utility industry vigorously lobbied in support of this research program and almost immediately raised its contribution to the total funding.

Mr. Brodeur does not mention any of numerous reviews of research on health effects of ELF magnetic fields done by experts worldwide; other than those previously cited [10, 13] some of the most important are listed at the end of this paper.

In dealing with specific individuals -- professional scientists and public health officials -- Mr. Brodeur failed to contact them or give them a chance to clarify their statements, motivations or actions. In several places in the text he implies that well-respected scientists have been corrupted by their funding sources and/or are not expert in their fields, yet he never contacted any of them before publishing his work. In at least one case (the Montecito Union School) he failed to take advantage of scientific input freely offered. In dealing with the reported cancer clusters Mr. Brodeur has been selective in his sources of information, opting to describe the actions of health officials and utility employees second-hand, through the eyes of residents. It would have been appropriate for him to attempt to cover both sides of the story; if in fact his case is sound such neutrality should serve only to strengthen the presentation. Yet in the cases of Meadow Street

and Monteci! to Union School, he chose to ignore factual information provided by the utility in one case and the state health department in the other.

It has been stated, even by some of Mr. Brodeur's strongest critics, that there is a valid place in society for investigative reporting. Such reporting can help keep both government and industry attentive to health and safety issues. However, biased reporting that provides no opportunity for discussion or rebuttal creates fear and paranoia rather than constructive dialogue or correction. Such one-sided presentations as Mr. Brodeur makes in his book do not truly serve the public in revealing the actual possibility of hazard. Rather they mislead and inflame, wasting resources and producing only controversy.

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