POWERLINE ELECTROMAGNETIC ENERGY AND HUMAN HEALTH

Andrew A. Marino, PhD
August 1998

CONTENTS

1. Overview

2. Two Sciences

3. Physics and Powerline Electromagnetic Energy Health Risks

4. Biology and Powerline Electromagnetic Energy Health Risks

5. Epidemiology and Powerline Electromagnetic Energy Health Risks

6. Blue-Ribbon Committees and Powerline Electromagnetic Energy Health Risks

7. Power-Industry Science and Powerline Electromagnetic Energy Health Risks

Notes and References

CHAPTER 1. OVERVIEW

Origins

The question whether powerline electromagnetic energy, the energy field that surrounds the powerlines wires and extends up to several thousand feet from the centerline of a powerline (1), affects human health originated in the 1960s in the United States (2), and some time earlier in the Soviet Union. I first became aware of the question in December 1973, during a conversation with Robert O. Becker, M.D., who was my mentor when I was a graduate student studying physics (1963–68), and my boss for twelve years thereafter.

In December 1973, Dr. Becker told me about a meeting in Washington DC with officials of the U.S. Navy where he learned that powerline electromagnetic energy likely affected human health. He notified the New York Public Service Commission of the public-health risk, and in July 1974 we were both asked by the staff of the Public Service Commission to testify in a Commission licensing hearing involving construction of two 765,000-volt powerlines. We both wrote reports explaining the basis of our view that the powerline electromagnetic energy could affect human health, which the Public Service Commission sent to the power companies in October 1974 (3, 4).The hearing was recessed for a year to allow the companies to recruit expert witnesses. Their reports were distributed in November 1975, at which time Becker and I submitted updated versions of our reports (5, 6).

In 1976 I was cross-examined by the power companies for 10 days, and Dr. Becker was cross-examined for 4 days. The power companies then requested a rebuttal phase of the hearing, and their experts filed additional reports that attacked our reports. By this time Dr. Becker was disgusted with the process, and he withdrew from active participation. I, however, filed a report aimed at rebutting the power-company experts, and was cross-examined for 3 additional days (7).

After the testimony was finished, the lawyers for the power companies and for the Public Service Commission filed legal briefs in an attempt to persuade the PSC Commissioners that powerline electromagnetic energy were not a health risk. The brief of the Public Service Commission staff argued that powerline electromagnetic fields would affect human health, but I thought an even stronger position was warranted. Consequently, representing myself, I submitted a legal brief (8).

The power-company lawyers submitted reply briefs, which prompted me to also submit a reply brief (9). The power-company lawyers then submitted another round of reply briefs, as did I (10).

The Final Decision was issued by the Public Service Commission in June, 1978. In the decision, the Commission acknowledged that powerline electromagnetic energy was a health risk and mandated that specific steps be taken to protect public health.

The decision led to extensive litigation involving the power companies and the Public Service Commission, the upshot of which was denial of permission to build one of the proposed powerlines, the institution of some construction rules to protect the public from exposure to electromagnetic energy from the approved powerline, and the initiation of a research program to assess more precisely whether powerline electromagnetic energy affected human health. Further details regarding the hearing, its subsequent litigation, and the fate of the research project are described elsewhere (11).

I did not anticipate the firestorm of controversy that was birthed by our testimony in New York nor, I think, did Dr. Becker. I was a young Ph.D. in biophysics, and a young lawyer, largely inexperienced in the intricacies of both professions. Dr. Becker had been involved in scientific disputes. He had taken the position that bioelectrical phenomena were important to the understanding of medicine and biology. But any success he might enjoy necessarily came at the expense of the paradigm of solution biochemistry, which was the dominant viewpoint in biomedicine at the time he began his work. Biochemists were therefore prone to attack Dr. Becker. The most vicious of the attacking biochemists were Philip Handler who, at the time, was the president of the National Academy of Sciences, and J. Woodland Hastings, who was the chairman of biology at Harvard.

Dr. Becker had published many novel studies whose results were inconsistent with biochemical orthodoxy and, unsurprisingly, his work was criticized (12). But he was not prepared for the vitriol that developed after he expressed his views concerning powerline electromagnetic energy. As a consequence of the position he took regarding health risks of powerlines, he lost his National Institutes of Health grants, his Veterans Administration grant, and his laboratory, and he was forced to retire at the age of 56.

I too lost my National Institutes of Health grant, and we were both attacked by Handler and Hastings (13). Contracts were awarded by the Department of Energy and the Electric Power Institute to Richard Phillips at the Pacific Northwest Laboratories for the purpose of fabricating and falsifying scientific data that contradicted the results of the animal studies that Dr. Becker and I had performed. Through the intervention of Vice-President Mondale, I obtained copies of Phillips' correspondence in which he described the details of his plans to rig the data he would submit to the Department and the Institute.

Personal Crisis

As the end of Dr. Becker's laboratory approached, the pressure against us mounted steadily and we could no longer do research. We focused on a book we agreed to write on the biological significance of electromagnetic energy. Our interests had already begun to diverge, and the book contract created tension between us. Dr. Becker was the originator of the stressor theory of electromagnetic-energy-induced bioeffects (14). He told me about it in detail in 1974; what he said profoundly affected me and provided a professional focus and direction for my career. If the stressor theory were valid, it would have great importance because it could rationalize the links between the neuroendocrine system and exposure to electromagnetic energy and disease.

After I first met Dr. Becker, in 1964, there was never any doubt about what I would do with my life — research. I realized early that it was necessary for me to first decide whether research that I might do had a reasonable possibility of being relevant to humans — the taxpayers who paid for my research. I did not have a mathematician's outlook on life. I once knew a mathematician who spent his whole career trying to prove an obscure point, that a group of functions was homeomorphic on the unit circle. When I asked him why he devoted his life to such a project, his answer was a paraphrase of the well-known response given by the mountain-climber who was asked why he climbed the mountain. Fine, if that's the way they look at things. For me, if I am going to climb a mountain, then I must have a reasonable expectation of finding something worthwhile at the top. If disease were really mediated by aberrant responses in the neuroendocrine system caused in part by apparently innocuous factors in the environment like powerline electromagnetic energy, proving that fact would be important, and appropriate for my life's work, at least to the extent that the taxpayers would continue to support me financially.

My concept of our book was that it should be focused on Dr. Becker's exciting insight into the possibility that electromagnetic energy was a biological stressor. I wanted to marshal all the available scientific evidence and document the affirmative case. But Dr. Becker saw our situation differently. Although he was proud of his discovery of the stressor effect of electromagnetic energy, he regarded it as one of the lesser of his insights into biology.

Early in his career, before I knew him, he had conducted a stunningly successful series of studies that dealt with electromagnetic energy, particularly effects involving bone and the nervous system. Those early studies led him to three somewhat related theories. First, that bone changes mechanical energy into electrical energy, and thereby regulates its own growth, development, and healing. A key element in this theory was the precise anatomic arrangement between the mineral and protein phase of bone, which he analogized to a PN junction, as described in solid-state physics.

Second, he concluded that the nervous system transmits information in two ways, not in one way as is described in standard neuroscience texts. According to Dr. Becker, in addition to spike-potential propagation, the nervous system is also capable of transmitting information in an analog fashion via the movement of electrons in nerves, roughly akin to the way copper wires carry electrical current.

Third, Dr. Becker believed that the focus of orthopedics on joint replacement using metal and plastic prostheses was entirely misplaced, and that the goal should be to regrow new functional tissue, and not to cut out diseased tissue and replace it with artificial materials. He theorized that mammals, like amphibians, also possessed special cells that could respond to appropriate signals and transform themselves into specialized cells capable of performing whatever biological function was required. For example, growing a new joint. Dr. Becker actually identified the universally adaptable cell in amphibians that was intrinsically capable of sustaining a regenerative response to injury — the nucleated erythrocyte. He theorized that mammals, like amphibians, also possessed such a totipotent cell. Finding the cell and learning how to communicate with it ought to be the goal of our research, he said. We would then know how to grow a new joint and repair a damaged spinal cord.

Dr. Becker's theories presented me with a dilemma. I personally believed the stressor theory of electromagnetic bioeffects was valid, well supported by the experimental evidence, and critically important regarding public health, but that the other theories were problematical. I believed he had gotten sidetracked by the national publicity that resulted from his stressor studies, and had not devoted sufficient time and resources toward establishing the validity of his other theories.

Consequently, for the book, I thought we ought to concentrate on the stressor theory, and present his other work in the best light possible, as permitted by the published evidence. I knew he would be displeased because he viewed his stressor research as the lesser of his accomplishments. The prospect of appearing disloyal to Dr. Becker, to whom I owed so much, was particularly disturbing. Nevertheless, I presented my proposal because I thought it would maximize his credibility in the eyes of scientists who came after us.

I proposed that each of the four theories in the book be presented in a similar organizational pattern in a stepwise process. The theory itself would be stated and the evidence in favor of it produced in our laboratory would be described. Then the evidence published by others that supported the theory would be presented. The next section would contain an analysis of the reports that tended to contradict the theory. The last section would show why these reports could be dismissed or discounted, leading to the overall conclusion that Becker was correct.

My idea was that if he agreed to my proposal and it turned out that the evidence didn't support one of his theories, we would de-emphasize it in the book. I expected we might discount several of his hypotheses by following this procedure. I hoped I was wrong because nothing would have pleased me more than to write a definitive analysis that defended his views. It would be a small payback for everything he had done for me, and I believed he knew that in analyzing the evidence I would give him, not his critics, the benefit of the doubt. But I also believed his greatness as a scientist would be obscured in the historical perspective if my plan were not followed.

Without a moment's hesitation, Dr. Becker rejected my proposal immediately after he heard it. What he wanted to do was simply describe his theories and the evidence that he produced to support them, as well as other evidence from others that was consistent with his theories. Because I would not have a warrant to search for all the evidence and to probe for the weakness of all of the studies, including those by Dr. Becker, it would be impossible for me to adequately evaluate his theories. Thus, there would be no possibility that we could discover he was wrong. The book would, therefore, contain all four theories, pretty much presented as fact.

I couldn't agree to that, I just couldn't. Ultimately, after painful discussions, we agreed to effectively write two books that would be published between one pair of covers. I would write about the electromagnetic energy stressor hypothesis, and he would write about his three favorite theories. That was what we did. His three theories appeared in the first four chapters of our book (15), each of which designates him as the sole author. My analysis of the energy stressor issue was contained in the subsequent seven chapters, each of which designated me as the sole author.

Sorting Things Out

When I wrote my chapters I saw that the scientific evidence showed man-made environmental electromagnetic energy was a public-health risk. But I also saw many uncertainties and multi-faceted scientific and sociological conflicts regarding that issue. It was going to be necessary to deal with these problems. I was willing to deal with them. I was wanting to deal with them. I felt that I had paid my dues, that I had learned the territory, and that I had something to contribute to the biology of electromagnetic energy.

Our laboratory at the Veterans Administration Hospital in Syracuse, New York was closed after Becker lost his grants and was forced into retirement. At the time, I was a full-time federal employee, GS-14, which paid quite well. Because I had long since passed my federal probationary period for employment, I had a guaranteed salary, but not a guaranteed job. With the laboratory gone, there was simply no need for a GS-14 research biophysicist at the Hospital. According to the Personnel Department, there were only two other jobs for which I was qualified — janitor and hospital director.

I turned down both jobs, but I found a job in Louisiana, which is where my wife and I and our four children moved in 1981.The chairman of the Department of Orthopaedic Surgery at the LSU Medical School in Shreveport, Louisiana, hired me as an Assistant Professor. He was one of the three finest people I ever met.

I had become angry over the question of health risks from powerline electromagnetic energy. I was angry because the power industry had hired scientists specifically to attack me on the basis of lies and fabricated data. I was angry because there were scientists who didn't work for the industry who disagreed with me. I was angry because, as a consequence of telling the truth as I saw it, I lost my grant, my job, and, I thought, my heritage. I grew up in Pennsylvania and New York. I was a Yankee, Italian, Catholic, Ph.D., lawyer, and I never imagined living in a town in Louisiana where even one of these characteristics was a bit strange.

The thing that most made me angry, however, was what I saw as a simple injustice. An unfairness. I never practiced law. Consequently, in many respects, I still harbored the law-school notion that the goal of the law is to facilitate justice among people. It is sometimes difficult for practitioners in the hurly-burly world of courtrooms and clients to remember or even recognize what justice is in particular contexts. I lacked practical experience about the law, but the absence of this experience allowed my notion of justice to persevere.

I constantly receive phone calls from people who are worried about health risks from man-made environmental electromagnetic energy. Someone who read one of Dr. Becker's books, or one of my books, or who saw one of us on *60 Minutes* or read about us in *Reader's Digest* or saw our name quoted in the *National Enquirer* or somewhere else calls me and asks: "I live next to a powerline; is it safe?" My heart goes out to those people because, but for the grace of God, there go I. At least that's what I thought initially. Subsequently, I began to see that they **are** me. Not with regard to health risks, because I know enough about that subject to prevent making the mistake of exposing myself or my family to powerline electromagnetic energy. But the situation regarding the energy has been cloned in our society. There are many examples in which physical factors are present in the environment by virtue of the same process that led to the presence of powerline electromagnetic energy — a presumption of safety. I know the pertinent literature well, but I don't know the literature in myriad other areas. In an important sense, I am as ignorant as the general public because the evidence of risk was hidden, or because I bought the company line that the evidence did not indicate a risk.

What exactly is the injustice regarding powerline energy that I perceived? The power company says that the energy from the powerlines is safe for humans to live in. If they are right, the power companies do not have to spend money to include safety features that would protect against exposure to energy. Under this assumption, there is a trickle-down benefit to homeowners living beside the right-of-way in cases where their electrical service is provided by the same company that owns the powerline, because all of the company's customers, including the resident near the right-of-way, presumably pay less for their electricity. If the power company is wrong, however, their benefit remains the same but the risk-benefit analysis for the resident is shifted enormously in one direction. Some of them will develop diseases that were partly caused by the powerline electromagnetic energy.

Many factors have been implicated as causing cancer in people. But electromagnetic energy was different. It was not the case that the exposed subjects were almost all healthy men who voluntarily chose to work in a profession that resulted in their exposure. It was not like smoking, where mostly adults voluntarily chose to engage in an activity for which the potential link with cancer was known. Instead, it was often the young or old who were unknowingly and involuntarily exposed to electromagnetic energy.

What is the just responsibility of the power industry and its trade associations, particularly the Electric Power Research Institute? I think it is to “lay bare the truth, without ambiguity or reservation” because the power company is in a fiduciary relationship with the people who live near powerlines (16). What occurred, however, was the opposite — a consistent pattern of obfuscation, misrepresentation, mischaracterization, and hiding data by the Electric Power Research Institute and the power companies, motivated, as best I can tell, by simple greed.

The Electric Power Research Institute and the power companies seemed to have limitless resources, and they bought whatever they needed to perfect their position. They entered into contracts with various companies to produce favorable research and other reports. Sometimes the companies were large established research organizations which had pre-existing intricate contractual relations with the power industry that involved many millions of dollars. In other instances the Electric Power Research Institute and the power companies simply created companies whose major asset was a contract for research or analysis regarding powerline electromagnetic energy. The results produced by these contracts that were released to the public never concluded that they had found evidence suggesting that powerline electromagnetic energy might be a health hazard. Thus, the situation was that essentially everyone who *didn’t* work for the power industry or the Electric Power Research Institute found evidence suggesting powerline energy was a health risk, but essentially everyone who *did* work for the power industry or the Electric Power Research Institute failed to find any such evidence.

In the New York powerlines case, the power industry was represented by a disparate group of attorneys headed by a lawyer from Rochester and the Dean of the Albany Law School (11). The industry fared poorly in that dispute, but it learned from its mistakes and entirely shifted its strategy. A nationally integrated strategy was devised that permitted the industry to protect its interests wherever they might be jeopardized, either in court or in the press. The linchpin for the strategy was a lawyer, Tom Watson (17). Through him, power company experts spun trade-association science in court and before various blue-ribbon committees to justify the conclusion that it is acceptable and reasonable to expose the public to powerline electromagnetic energy, even when the residents are completely unaware of the presence of the energy and have never voluntarily consented to be exposed.

I thought the situation was unfair. I wouldn’t want my family exposed to powerline electromagnetic energy based on the present evidence, Watson’s family isn’t exposed to electromagnetic energy and the Board members of the Electric Power Research Institute and the nation’s power companies don’t live beside powerlines, but their spokesmen maintain in every available forum that it is appropriate for you to do so.

Changed Purpose

More and more, in the early 1980s, the things that previously made me angry came to be a source of motivation rather than anger. Some people want to save the whales, some want to fight breast cancer or AIDS. Some people are passionate about abortion, or creation science or saving the redwoods. I have always welcomed this form of passion because I like to see people fight for what they believe. It means they care about society. These people are generally not in it for money or fame, but rather to encourage the ascendency of their ideas. The rest of us are free to accept or reject the reasoning and values of the proponents of the various causes. For me, the task would involve every aspect of the relation between electromagnetic energy and biology — from soup to nuts.

I planned to study the point-of-view of different kinds of scientists in relation to how they approach the powerline electromagnetic issue. The disputes that developed brought me into direct conflict with scientists who seemed to have quite a different view than me regarding how scientific facts should be established. This perception was subsequently reinforced as I progressively came into greater contact with biologists. Their facts generally didn't involve mathematical equations whereas those of the physicists (which was the larger part of my experience at that time) seemed always to involve equations. Were there different ways of establishing scientific truth? If so, which was applicable to assessing powerline health hazards?

I began a study of the cellular biology of how stimuli in the environment are detected by the body (18). Both in my own research, and in the research of others, I planned to learn where and how the body transduced electromagnetic energy. Although the question was important, it was not the seminal question. The question of *how* the body detected electromagnetic energy would not be ripe until the fact that the body *could* detect the energy was first proven. Industry experts confounded the issues of detection and mechanism and argued that absence of knowledge regarding mechanism of detection of powerline electromagnetic energy was evidence that no energy-induced biological effects existed. To me that view was illogical, and the Siren song of mechanism was best avoided until experimental biological studies demonstrated beyond reasonable doubt that biological effects of electromagnetic energy actually existed.

I also planned to study how alterations in the neuroendocrine system could lead to disease. Dr. Becker never restricted his concern about the health effects of electromagnetic energy to cancer. He thought it might have a role in all human diseases, even AIDS. He was mocked for this suggestion, but that response only intensified my desire to pursue inquiry into the effector systems in the body whose alteration by electromagnetic energy could be linked to disease. Early in this quest I settled on the immune system as a likely target for electromagnetic energy in relationship to inducing disease. No other possibility even comes close to being able to explain the range of empirical data that has been adduced regarding the biological effects of electromagnetic energy. If the efficiency of the immune system were reduced by the energy, then it is easy to see that the probability of disease (19) would be increased.

I planned to study epidemiology. That gray science does not permit deductions nor provide explanations like physics, and it is methodologically incapable of demonstrating cause-effect relationships, as biology can. Nevertheless, epidemiological studies strongly influenced perceptions regarding powerline health risks, and it would be necessary to be able to distinguish a good electromagnetic energy epidemiological study from a bad one.

As I saw the fundamental issue, the question whether powerline electromagnetic energy was a health risk was only partly a scientific question. Even unlimited research funding given to the brightest scientists with the highest degree of integrity would never lead to an answer based only on scientific considerations. If the question were, for example, whether under a particular set of conditions a particular form of electromagnetic energy applied to a given strain of rats would produce a statistically significant change in a particular dependent variable, *that* information could be obtained with enough money and the right investigators. But the question of electromagnetic-energy-induced health risks was not that kind of question. Its resolution would involve the use of scientific data, but scientific data alone was not enough. There was a need to focus on the process by which, as a society, we make decisions regarding matters that involve scientific data; in other words, the decision depended ultimately on values.

Finally, I would study and document the strategy of the Electric Power Research Institute and the power industry generally as it went about the business of defending its interests. It was not that I had a historian's interest or that I merely wanted to chronicle their activities. And I didn't really intend to offer interpretations and characterizations to try to prove that they were bad actors. What I was mostly interested in was encapsulating their activities for the purposes of posing the question *Is this what we want, as a society?* Given the importance of electricity in daily life, the economic aspects of the industry, the various stake-holders in the dispute, is the present system for resolving the dispute what we want, or not?

My epiphany regarding electromagnetic energy occurred after I arrived in Shreveport. It didn't occur instantly, but rather slowly, like the coming of spring in the South which develops imperceptibly and then, one day, is simply there. One day I realized that my real goal was not to prove that I was right and Electric Power Research Institute was wrong. Rather, it was to find the truth about the relation between environmental electromagnetic energy and human disease, regardless of who might be hurt or displeased (20).

The ultimate issue would be whether electromagnetic energy affects human health. If the answer was yes, *why* was it yes? If the answer was no, *why* was it no? I started my career by studying how electromagnetic fields could be used to treat diseases. Maybe they could be used to regenerate missing or diseased organs and tissues, as Dr. Becker believed so passionately. It was clear, however, that there was a problem. The Food and Drug Administration said (in 1979) that electromagnetic energy, when carefully and precisely administered by a physician under controlled circumstances, could be used to treat specific bone diseases. But the Electric Power Research Institute said that essentially the same kind of electromagnetic energy, when administered involuntarily in a completely uncontrolled fashion, even for a lifetime, had no effect whatever on human health. Somebody was wrong.

No matter what answer lay at the end of the inquiry, knowing the answer would be a public benefit. If powerlines were safe, the homeowner could turn his attention to other areas and worry about other things. There are a lot of elephant traps in life, but at least powerline electromagnetic energy would not be one of them. On the other hand, if powerline electromagnetic energy were a health risk, then people affected by them needed to know about it. The information needed to be presented in an honest and forthright fashion, "without ambiguity or reservation."

Congressional Interest

While I was attempting to understand the electromagnetic-energy health-risk dispute, a remarkable thing happened. In the 1970s, when the issue first surfaced, most scientists, and I think essentially all laymen, had no conscious understanding or awareness of what electromagnetic energy is (21). By the 1990s, almost everybody had heard that powerlines give off something that might be bad for your health.

Throughout the 1980s pressure continued to build on Congress to do something about the potential problem of powerline electromagnetic energy. It took a long time for the pressure to develop. I think the chief reason was that there was a kind of basic unfairness on both sides of the dispute, and for a long time these two conditions balanced out one another. The proponents of the powerline-electromagnetic-energy-is-safe view had all the money on their side. They completely controlled the targeted research and the public spin involving powerline electromagnetic energy. Research that had the potential to yield results that implied powerlines caused health risks was not funded, and opinions that powerline electromagnetic energy was a health risk were infrequently voiced in high government or industry councils. The research that was funded by industry was usually irrelevant. The industry viewpoint was extremely over-represented on every blue-ribbon committee tasked to evaluate the health risks. Unsurprisingly, their conclusions invariably were broadly reassuring to the public and supportive of the power industry.

On the other hand, it was distressingly easy for a print or visual media journalist to do a powerlines-cause-cancer story that distorted or misrepresented the nature of the risk and that overemphasized the reliability of the evidence that was discussed in the story. I do not mean to say that all industry-supported research was without value or that most media reports were not accurate. My point is that the money factor cut in one direction and the publicity factor cut in the opposite direction, and that consequently the electromagnetic energy issue only simmered in the ’80s.

A prominent aspect of the Congressional interest in the powerline electromagnetic energy issue was the distrust that developed regarding whether the industry would honestly evaluate the health risks of powerlines (22). An indication that the problem was serious for the industry was the position taken by their representatives during Congressional hearings which eventually created the law that set up the federal program to evaluate the health implications of powerline electromagnetic energy (23). In related congressional hearings, high-level officials from the power industry strongly urged Congress to enact legislation aimed at determining whether powerline electromagnetic energy affected human health. This was the first step by the Electric Power Research Institute to co-opt the process and insure that it ultimately reached a conclusion that powerline electromagnetic energy posed no health risk whatsoever.

The law called for research to determine whether powerline electromagnetic energy “affects human health” and required the issue be addressed directly by Dr. Kenneth Olden, the Director of the National Institute of Environmental Health Sciences in a report to Congress.

Whether because of ignorance or design remains to assessed by future historians, the entire adjudicatory process was rigged from the beginning, because the only possible answer was “no.” That answer was guaranteed because the standard of proof was “beyond a reasonable doubt,” and such a program was impossible, given the intrinsic limitations of the methods of experimental biology. The clever and strictly self-interested leaders of the power industry transformed what was a genuine congressional interest into a Trojan Horse.

The Director’s report to Congress is due in November, 1998. In response to the question “Does powerline electromagnetic energy affect human health?,” Dr. Olden will effectively say “I can't tell for sure” (24).The underlying reasons this will occur go deep into the nature of biomedical science and its relationship to society. Those reasons are the subject of this report.

Why Continue?

Public and Congressional interest in the powerline electromagnetic-energy issue may have crested and started to diminish. It has been argued that the inquiry should be abandoned in favor of consideration of other issues. But if the electromagnetic energy issue dies following the

Director's report in November, 1998, then the insights into the nature of science and its relationship to society that can be gleaned from an analysis of the issue will be lost. The reason that this loss would be serious is that the underlying problems that gave rise to the electromagnetic-energy dispute are structural. Hence they will persevere and be re-fought in other contexts, again requiring the expenditure of hundreds of millions of dollars in public money, and the occurrence of avoidable levels of disease.

I think, therefore, that the common good would best be served if the issues were considered in detail and evaluated on their merits. It seems to me that the time has come for us to establish a set of rules by which it can be determined objectively, without resort to idiosyncratic judgments of ad hoc experts, whether or not environmental factor X affects human health. Then, and only then, could a disinterested judge ascertain the correct answer in the context of the available scientific evidence in the particular case X = powerlines electromagnetic energy. A further set of rules is needed to determine what it means to say that factor X "caused" a disease in a particular individual.

The electromagnetic energy dispute can be dispassionately analyzed to show that rules are needed, and that in their absence, there can occur only intentional neglect or interminable controversy. The former is unjust because it amounts to involuntary human experimentation and the latter is needlessly wasteful and corrosive.

Tom Watson and the Rules of the Contest

My view is that powerline electromagnetic energy does affect human health. Tom Watson defends the opposite conclusion on behalf of his clients. I have seen him and his experts make many different arguments. I think he has neither a single valid scientific argument, nor the majority of the evidence on any legal point pertinent to the electromagnetic-energy health-risk issue. Despite this, he usually wins.

How can Watson consistently win before various tribunals when he is wrong? Watson has won, at least up until now, because he is a consummate professional at organizing information created for the purpose of defending the power industry, and at orchestrating that information in an effective manner. Considered purely as Theater or as a law-school-evidence-class example of how to marshal evidence in support of a client's position, he is the best I have ever seen. This, roughly, is what he does.

He presents evidence showing that calculations indicate that powerline electromagnetic energy is safe. If the calculations are not persuasive he shows that there are no mechanisms of interaction between electromagnetic energy and biological tissue. If that line of argument is breached he argues that the animal studies are unreliable or inconsistent. If that strategy fails he urges that effects found in animals cannot necessary be imputed to human beings. If he loses this argument he claims that the epidemiological studies show no consistent pattern and have serious methodological flaws, and thus that there is no evidence that actual harm to human beings has occurred from powerline electromagnetic energy.

He says that the only acceptable evidence that a human being got cancer from exposure to powerline electromagnetic energy is an uncontroverted series of animal experiments in which only 60-Hz electromagnetic fields were applied to animals with the result that the animals subsequently developed cancer via a specific and established series of mechanistic steps involving the proven activity of particular oncogenes and their protein products. In addition he demands the existence of epidemiological data from studies in which subjects were exposed to powerline electromagnetic energy and no other potential risk factor for cancer. The studies must involve only a single histological subtype of cancer exhibited by the patient. All data must meet the scientific standards of certitude, 5% or better.

Watson likes to hire experts from famous institutions like Yale, Cornell, the National Cancer Institute, and Roswell Park. He maintains a separation between the investigators who do research on behalf of the power industry, and experts who testify for him in court. Consequently, because the investigators are not offered as expert witnesses, Watson's opponents cannot dig into the contractual details between the power industry and the investigators that resulted in the data relied on by Watson.

Probably the single most important reason that Watson has done so well thus far is not that he is an able lawyer or has an unlimited budget. Mostly his success is a result of the continuity of his work on powerline electromagnetic energy. Since the 1970s, he has acquired an enormous data bank of scientific reports, testimonies, and other pertinent documents. Watson knows the scientific jargon regarding electromagnetic energy and he understands how differently different kinds of scientists look at the same issues. He skillfully exploits these differences. In contrast, Watson's opponents in particular disputes are invariably new to the issue of electromagnetic-energy bioeffects. The difference between knowing the territory and not knowing the territory is the difference between winning and losing.

Well…what Watson urges as the standard of evidence needed to conclude that powerline electromagnetic energy affects human health or that powerline electromagnetic energy caused cancer in a particular case could be the rules if that is what we want. I do not think that most people want them to be the rules, but I could be wrong. This is really the heart of the issue regarding whether powerline electromagnetic energy affects human health. *What are the rules for answering the question?*

Ultimate Goals

The dispute involving health risks from the electromagnetic energy produced by powerlines has generally been styled as one involving only a scientific issue that should be decided by scientists, all of whom are idealized as using the same methods and models and assumptions. It seems to me that Congress essentially adopted that viewpoint when it told Dr. Olden to decide the issue. The fact that any answer to the question posed would be heavily value-laden, and that non-representative blue-ribbon committees are intrinsically invalid tools for making public policy were not appreciated by anybody in 1992. But, today, I think that these facts can be seen (24).

I want to show that the question whether powerline electromagnetic energy affects human health is not an abstract scientific question capable of resolution via a self-extracting procedure. Rather, it is a mixed question of science and sociology whose resolution must be based partly on scientific knowledge and partly on values, and pursued within a determined procedural framework where pivotal terms are defined and the rules for deciding are established. It is a question like: Are nuclear plants safe? Is cisplatin effective for treating cancer? Do the preservatives in bread have any side-effects? Do insecticides adversely affect the ecosystem? Such questions cannot be answered with laboratory and epidemiological data alone.

Resolution of a mixed question of science and sociology requires that the available evidence be compared against a standard, it requires a set of rules, and it requires a disinterested judge. But whose values? And whose judgment? The powerline-energy question must be distinguished from those where values play no significant role and where who should decide the issue is clear. For example: How much fuel is needed to send a spaceship of mass m to the moon in time t? How much current will flow in a particular circuit when it is energized with a given voltage? What is the melting point of iron? Does release of freon into the atmosphere cause a hole in the ozone layer? Is cold fusion real?

The Electric Power Research Institute and the power industry claim that the values which necessarily enter into the resolution of whether powerline energy affects human health ought to be the values of scientists, particularly the scientists that they hire. I think that is wrong. The values incorporated into the decision ought to be those of society, not those of scientists who work for industry.

These issues may be difficult to appreciate because they require a new look at science and its relationship to society. This may be troublesome. But I will show that this relationship must be rethought and then defined before it is possible to answer the question *Does powerline electromagnetic energy affect human health?* I suspect powerline electromagnetic energy is not the only problem that forces us to look more closely at exactly what science is, and who and what it serves.

To accomplish my goals, I wrote this report as a series of separate Sections, starting with the most basic issues involved in the powerline electromagnetic energy dispute, and then progressing toward the more concrete issues that animate the controversy. I am aiming to be understood by both scientists and laymen. This objective presented a difficulty because the kind of detail needed to persuade both groups sometimes differed. In most instances where the inclusion of additional detail would have buttressed my point but at the expense of clarity and succinctness, I chose to foster clarity in my presentation. My thinking was that if the only objection to my analysis was the absence of detailed proof, then I could supply it later. Even so, I tried to provide the supporting evidence or citations in those instances where I thought they were important to sustain or explicate my point.

CHAPTER 2. TWO SCIENCES

Introduction

Following the end of the Second World War, Herman Schwan, a German physicist, became a professor at the University of Pennsylvania, Department of Biomedical Engineering, and remained there until his retirement. Schwan's area of expertise was the biological effects of electromagnetic fields, and he played an important role in a 1960s government program aimed at determining a safe level of exposure to microwave radiation for servicemen. Schwan's approach was based on a series of calculations and assumptions, and in the legal dispute he applied them to powerlines and concluded that powerline electromagnetic energy would not affect human health (11).

Schwan was cross-examined for 2 days in April, 1976 regarding his opinion about powerlines, and he fared poorly. As I watched, I tried to put my finger on exactly why he was unable to sustain his opinion. On the surface it appeared that Schwan's mistake was to equate the absence of a known mechanism of interaction between electromagnetic energy and tissue with the idea of the absence of a health risk. But I knew that the problem must go far deeper. Somehow, it was related to his attitude toward science, which was so different than mine. I saw the possibility that electromagnetic energy could cause biological effects as exciting, a previously unanticipated and unexplored idea that might have profound implications. I therefore viewed the handful of reports that existed in 1976 which supported this idea as tiny flowers growing in the garden of science. Schwan, however, saw the reports as weeds.

In the succeeding years individual physicists and groups of physicists offered the opinion that, essentially, effects of powerline electromagnetic energy on human health were impossible (25). But their arguments were no different from those of Schwan. It dawned on me that Schwan and those who think like him were not just offering poorly thought-out opinions. Rather, within the frame of reference of what science was to them, these physicists considered themselves to be correct and it was hard to imagine anything that could make them change their minds. Schwan, for example, reacted to his cross-examination not by conceding that he could not sustain his position, but rather by becoming angry at the cross-examiner. At one point he glared at the attorney and said that he was a "very poor physicist." Schwan really believed he was right and that he could convince a room full of good physicists that he was right because they would understand how he thinks.

Many professional physicists, including even Nobel Prize winners, believe that their approach to the study of the natural world is pertinent to and can be used to address the issue whether powerline electromagnetic energy affects human health. Somehow, I thought as I watched Schwan in April of 1976, this is not the case. He was being a good physicist on the witness stand. If all the physicists in the country were asked to vote, I think they would have backed him and simply equated being a good physicist with being a good scientist. Perhaps the problem was not Schwan's way of thinking, but the relevance of his way of thinking to the issue of powerline electromagnetic energy health risks.

I begin mulling over how scientists think, and how they decide what is or is not a scientific fact. It's easy to see that specific questions like *Does powerline electromagnetic energy affect human health?* are meaningless unless one specifies how the scientific facts to be used in answering the question will be obtained. Why? Because if Dr. A requires that scientific facts be obtained in a particular way, and Dr. B requires that they be obtained in some other way, then Drs. A and B can never agree. The other guy’s data is simply junk science.

If I am correct that in an important sense physicist’s opinions about whether powerline electromagnetic energy affect human health don’t matter because the way physicists think is inapplicable to the issue, then I should be able to prove this contention by an analysis not connected directly with the electromagnetic energy issue. That is exactly my goal in this section and in the next section. First, I will show here that there are in use in science today two different reasoning processes for deciding what constitutes scientific knowledge — those of physics and biology. In the next section I will show why the physical approach has little to offer towards resolution of the powerline health-risk question.

Scientific Methods

There have been many philosophical analyses of science by philosophers and scientists (26). Generally, the aim in these studies was to identify what the authors considered to be the basic features of scientific practice, which was done by selectively choosing special cases for analysis. By choosing special cases, differing conceptions of scientific practice could be described. The purpose here, in contrast, is to establish how science is done today, without limitation to specially chosen individual cases, and in the absence of idiosyncratic ideas regarding how it ought to be done. Consequently, I employed representative sampling to facilitate identification of the rules and procedures of scientific reasoning that are used to establish a putative fact as scientific knowledge (27).

To characterize contemporary scientific thinking employed in experiments routinely performed in universities, government laboratories, and corporate facilities, and published in peer-reviewed journals, I randomly chose Issue No. 5248 of the journal *Science* (January 26, 1996). The Issue contained 12 reports that could be analyzed to ascertain the thinking that was employed by the investigators in arriving at a judgment that new knowledge had been found. The reports are summarized in Table 1. Four additional reports were not considered because they involved measurement or other activities (invention and discovery) that did not utilize formal reasoning (28).

Table 1. The two kinds of scientific reasoning employed in *Science* Issue 5248 were explanations based on the application of a covering law (Reports 1, 2, 6, 7, 9), and proof of cause-effect relationships (Reports 8, 11–16). Because Report No. 9 contained both kinds of reasoning, its classification in this Table is arbitrary. Note that, whether or not consciously, the editor of *Science* grouped the Reports on the basis of the kind of reasoning employed, as evidenced by their order of appearance in the Journal (Report No.). CES, cell expression system. Reports No. 3–5 and 10 involved invention or discovery, but did not utilize formal reasoning processes. They were therefore not considered further. The lines in the last column provide a brief summary of the individual reports. The reports are numbered in the order of their appearance in the Journal.

Report No.	Model	Covering Law	Phenomenon Explained
1	1600 atoms 128 polymer chains	Physical theory	Energy dissipation
2	55–256 atoms	Physical theory	Structure and stability of liquids
6	Phosphorus coupling with C, O_2, and Fe	Heuristic rate equations	Stabilization of atmospheric oxygen during the phanerozoic
7	Any non-specific immune process	Heuristic rate equations	Clearance of HIV from the blood
9	Structure of selected proteases; CES	Heuristic parsimony algorithm	Serine protease diversity
8	KD cells	Decreased cyclin-E/ CDK2 activity	Loss of anchorage
		Increased CDK inhibitors decreased phosphorylation of Thr_{160}	Decreased cyclin-E/CK2 activity
11	A31.C1 cells	Osteopontin	Activation of CD44 receptors
12	10 human subjects	Vigilance	Increased brain flow
13	CES	Mutant enzyme and Cu2+ chelation	Altered catalysis
	CES	Cu2+ chelation	Altered cell growth
14	CES	High density lipoprotein	Activation of SR-B1 receptor
15	CES	Products of ALG-2, ALG-3	Apoptosis
16	5 barn owls	Ligation of NMDA receptor	Auditory learning

Scientific Reasoning

A common feature of the reports summarized in Table 1 was the use of a model to facilitate reasoning. The model was either a physical system that was manipulated in the laboratory, or a conceptual simplification of a real system such as a particular arrangement of a small number of atoms. Use of a model was fundamental and essential in all cases of scientific reasoning.

Two kinds of studies could be distinguished. In one kind, the goal was to provide an explanation of a phenomenon in terms of mathematical equations (*covering laws*), which were regarded by the authors as governing the phenomenon of interest, and which were afforded a prominent role in accounting for specific changes in the model system. A force, explicitly or implicitly contained in the covering laws, was regarded as the necessary and sufficient cause of change in the model and, ultimately, of the phenomenon to be explained. No other factor or condition was needed to explain the changes. Thus, in the cover-law studies, a deductive form of reasoning was employed to rationalize particular observations, namely those for which the model used was deemed appropriate.

In the other kind of study, the goal was to prove that a particular factor was a *but-for cause* of a particular observation. In Table 1 Report No. 8, for example, the authors employed KD cells and demonstrated particular cause-effect relationships involving decreased cyclin-E/CDK2 activity and loss of anchorage. Similarly, in Report No. 11, A31.C1 cells were used to demonstrate that osteopontin21 activated CD44. In the cause-effect studies, no attempt was made to explain the results in the sense of showing that the relationship between the postulated cause and the observed effect was a necessary consequence of a general mathematical principle.

The authors of the cause-effect studies extended their results beyond the particular biological objects that they manipulated in their own laboratories by means of abduction, which is an inferential reasoning process distinct from induction and deduction (29). In these studies, it was either argued or assumed that the relationships observed were not specific to the respective laboratories, but rather would be found by others in appropriate replications of the studies (30).

The term most frequently employed to describe the link between the study actually conducted and the larger conclusion advanced by the investigators was “suggests,” but many other euphemisms were used (Table 2). For example, if it were true that decreased cyclin-E/CDK2 activity generally led to loss of anchorage, then the results observed in the KD cells (the study actually conducted) could be viewed as a deductive consequence of that general principle. On the other hand, on the basis of the data, it would not be true to say (and the authors did not do so) that the results proved that loss of anchorage observed in KD cells was due to decreased cyclin-E/CDK2 activity, because the authors did not exclude all other possible explanations. The study only suggested that this is the case. Thus, no logical inconsistency would be entailed were it the case that investigators in a different laboratory failed to find the reported cause-effect relationship.

Table 2. Euphemisms for *suggests* used in *Science*, Issue 5248.

…indicate…
…may have been instrumental….
…not unreasonable…
…results in…
…may be one of the mechanisms…
…consistent with…
…provide direct evidence for…
…is the most likely…
…is involved in…
…raised the possibility…
…believed that…
….may underlie…
…provide insight into…
…support a determining role…
…orchestrated…
…does not readily account for…
…showed…
…confirmed the role of…

Moreover, it could be the case that the reported link between decreased enzyme activity and loss of anchorage occurs only for KD cells and not for other types of cells. It seems clear from the report that the authors viewed KD cells merely as a convenient model within which to study a model-independent phenomenon. I expect that the editors of *Science* regarded the observed cause-effect relationship as likely to be model-independent because KD cells have no particular significance in themselves, but served merely as a convenient tool for demonstrating a basic biological phenomenon. But nothing in the study precludes future investigators in other laboratories using non-KD cells from observing that decreased cyclin-E/CDK2 activity does not lead to loss of anchorage of the cells.

These considerations make it clear that whatever generality may appropriately be inferred using the KD model, the basis of the validity of the generalization is the following abductive argument: were it the case that it was generally true in nature that decreased cyclin-E/CDK2 activity causes loss of anchorage in cells, then the data and relationships observed in the present study could be explained deductively.

Each of the other cause-effect studies in Table 1 similarly relied on abductive reasoning as a means of generalizing the results beyond their individual laboratories.

The authors of the covering-law studies, in contrast, proved their point (assuming the validity of the law). For example, consider the report dealing with rupturing of adhesive bonds formed by short-chain molecules. A model was adopted that involved 2 walls containing 800 atoms each, coupled by stiff springs on a face-centered-cubic lattice; the space between the walls was occupied by 128 polymer chains that each contained 16 molecules of a given mass. Equations based on physical theory (electromagnetism and energy conservation), assumed forces (introduced in the guise of potentials), and numerical values of particular parameters in the equations were regarded as jointly controlling the process of rupturing of bonds between the polymers. In simulation, the walls were maintained at different temperatures and then separated from one another at different velocities, and it was shown that energy dissipation occurred by means of viscous forces at high temperature, but by particular structural rearrangements of the polymer chains at lower temperatures. The results obtained were absolutely certain, and would be obtained by any knowledgeable investigator who employed the same model and made the same assumptions. The molecular sequence of events in the model could be explained in the sense that it could be deduced from a covering law as the result of a particular cause (the force) via particular temperature-dependent mechanisms. Further, the results obtained necessarily apply to an important class of real systems, namely those systems for which the model was a true and accurate representation. The point is that, given the model and the assumptions, no conclusion other than that stated by the authors was possible.

Thought-Styles

On the basis of the evidence provided by the representative sample of *Science* reports described here, it can be seen that there are two fundamentally different approaches to doing science in the 1990s — two distinct scientific thought-styles. In the physical thought-style, the goal is to explain an observation by showing that it is compelled by basic physical laws or at least by phenomenological equations. In this thought-style, a scientific fact is a deduction from a relevant covering law made in the context of particular assumptions. The concept of causality does not occupy a central position in the physical thought-style because the necessary and sufficient cause of the observation to be explained — a force — is known in advance of the explanation.

In contrast, in the biological thought-style, the goal is to establish a scientific fact. In this thought-style, a scientific fact is a but-for cause of an observation established using orthodox measurement methods and appropriate statistical techniques. In the biological thought-style, covering laws are not employed and linkage with covering laws, even in principle, is not required as a precondition for accepting observations as valid. Scientific facts are generalizations that admit of exceptions.

The analysis of the reports in Issue 5248 of *Science* leading to the conclusion that two distinct thought-styles were utilized to produce scientific facts applies equally well to all subsequently published issues of *Science* that I have considered. That is, I can show that each report in any issue of *Science* that involves formal reasoning can be classified into one (or a combination) of the thought-styles described here. It can permissibly be concluded, therefore that there presently exist two distinct valid methods for producing scientific knowledge. Consequently, the scientific facts of the physicist and the biologist are fundamentally different objects (31). This analysis makes clear — I think for the first time — that there presently are two distinct pathways by which observations can rise to the level of scientific fact.

I will show how failure to distinguish between the thought-styles and to identify the applicable thought-style accounts, in part, for the present controversy regarding whether powerline electromagnetic energy affects human health.

Summary

The methods of physics and biology are different, and they produce scientific facts in different ways. This means that the question *Does powerline electromagnetic energy affect human health?* must be considered from two different perspectives.

CHAPTER 3. PHYSICS AND POWERLINE ELECTROMAGNETIC ENERGY HEALTH RISKS

Schwan and the Linear Model

Historically, Herman Schwan was the first physicist who sought to explain powerline electromagnetic energy bioeffects on the basis of the laws of physics. His laboratory research on the topic was carried out while he worked in a Nazi laboratory during the Second World War. After the war he was brought to the to the U.S. under a "person of interest" citizen program by the U.S. Navy, which hired him directly for several years, and then indirectly for many years by paying his salary while he worked as a professor at the University of Pennsylvania in Philadelphia. His office was less than five miles from where I grew up, and I first learned of his work in 1963, when I was a first-year graduate student studying physics at the University.

Schwan's analysis led to the conclusion that powerline electromagnetic energy did not affect human health, and his work still constitutes the most lucid explanation of the application of the physical thought-style to the issue of powerline electromagnetic energy health risks (32). It is the cornerstone and the substance of every subsequent opinion in which the physical thought-style was employed to rationalize the same conclusion (33).

Schwan assumed a model for the interaction between electromagnetic energy and biological tissue, and then applied the basic physical laws that govern electricity (Maxwell's equations) to assess whether any biological effects would be predicted or expected. The assumption of the linear model specified how Maxwell's equations should be used to make predictions.

Schwan reasoned that if powerline electromagnetic energy caused biological effects, then two things had to occur. First, the powerline fields needed to penetrate into the exposed subject and reach the place in the body where the presence of the fields could be detected. For Schwan, these possible locations were the body fluids (interstitial fluid and blood), and the membranes of nerve cells. Second — this is where the assumption of a linear model entered explicitly — the magnitude of the fields that penetrated into the body had to satisfy a numerical significance criterion, defined by the ratio of the strength of the electromagnetic energy produced by the powerline at the putative locus of interaction to the strength of the electromagnetic energy that was already present in the fluids or membranes. Schwan pegged this relationship at from1/100 to 1/10, and used it as a threshold for deciding whether the powerline electromagnetic energy could cause a bioeffect. Below the threshold, the powerline electromagnetic energy was regarded as insignificant.

The basic idea in Schwan's approach was that any possible cause-effect relationships would be explained on the basis of electrical forces. Prior to the penetration of powerline electromagnetic energy to the putative interaction locus, there were already fields naturally present that were exerting forces on ions and other electrical charges present at that location in the body. The motion of these ions and charges, as reflected in their chemical activity, was completely determined by the presence of the forces. A change in activity caused by powerline electromagnetic energy could occur only if the powerline electromagnetic energy forces were 1–10% of the pre-existing forces.

To apply the model, Schwan calculated the strength of the powerline fields that would actually penetrate into the exposed subject. Because calculations based on biological reality are impossible, Schwan made simplifying assumptions regarding the shape and electrical properties of human tissue. He usually assumed that humans had a spherical or cylindrical shape, and were composed of only one tissue having the electrical properties of salt solution. The results showed that very small fields were expected inside the human model. Next, Schwan estimated the strength of the fields already present in the body and argued that they were very large, at least in the immediate vicinity of electrical charges. He concluded that powerline electromagnetic energy would not affect human health because it was essentially impossible for something very small to affect something very large.

To drive home this point, Schwan made a third assumption: he assumed that there were only two physical processes that could be affected by powerline electromagnetic energy that penetrated the body (34). One possibility was that the orderly pattern of electrical activity that occurs in excitable tissues such as the heart or nerves could be interfered with by the electromagnetic energy induced by powerlines. The second possibility was that, in principle, the powerline electromagnetic energy fields that penetrated the body could affect the motion of ions and charges, resulting in the generation of heat. The utility of this third assumption was that it permitted Schwan to inject into his analysis two cases where the linear model of energy-tissue interaction did apply, and could be used successfully to explain the data. The successful application of the linear model to explain two types of data was cited as evidence to support a claim of universality for the model.

Schwan's key assumption was that of the linear interaction model. Using it, he calculated the magnitude of powerline electromagnetic energy that would be unsafe, and it turned out to be impossibly high. Any attempt to create unsafe powerline electromagnetic energy would result in the breakdown of the air surrounding the powerline, thereby preventing achievement of the air field necessary to produce an internal field that would be a health risk. Schwan had two good reasons for assuming a linear model. First, it is the simplest way of modeling nature's response to physical stimuli. Although biological organisms are hugely complex and appear to carry out their activities in complicated ways, most physicists subscribe to the metaphysical principle that nature follows the simplest efficacious pathway, and hence that models of nature should be as simple as possible. This notion, first explicitly identified with Occam, a 14th-century logician, requires that the simplest sufficient model be adopted and regarded as the best representation of reality, if it fits the data.

Second, microwave energy was the form of electromagnetic energy Schwan initially studied in Germany before and during the war, and he proved it was capable of cooking tissue and interfering with heart rhythms. The linear model explained both effects, encouraging Schwan to abuse it. He ceased regarding the linear model as simply a tool, and advanced it as something akin to a law of physics. For Schwan and those who adopted his arguments, the fact that the electromagnetic energy biological data could not be explained with reference to a linear model was evidence that the data was defective, rather than evidence that the model was inapplicable (34). When new data appeared, Schwan ignored it or mercilessly attacked it (35).

Schwan's analysis of electromagnetic energy health risks was arguably reasonable in the 1950s but demonstrably incomplete in the 1970s. When used to conclude that powerline electromagnetic energy is safe, it is unreasonable because the number of studies whose results do not fit the linear model is vast and increasing exponentially. It is now the task of physicists to revise their assumptions and propose new models for use in understanding the interaction of electromagnetic energy and biological tissue. In the meantime, to resolve the question whether powerline electromagnetic energy affects human health, it will be necessary to evaluate the biological literature to assess what scientific and public-health conclusions follow from that literature.

Nonlinear Interaction Models

At the present stage of development of physical theory, the model that best explains electromagnetic-energy-induced bioeffects is unknown. I would like to make it clear, however, that some effects could someday be satisfactorily explained by an appropriate physical model. I will do this by showing that a nonlinear model of interaction is compatible with the laws of physics.

We have seen that the essence of a linear model is the proportionality between cause and effect. How do nonlinear models avoid such an enforced proportionality, and the inexorable conclusion to which it leads in the context of electromagnetic energy bioeffects? How is it possible to retain Maxwell's equations and yet reach different conclusions simply by changing the model?

Consider the patterns exhibited by a set of 6 identical lava lamps (Figure 1). Although the lamps were identical in size, shape, weight, and chemical composition, after they were turned on for a few minutes, the pattern of the lava was different in different lamps. No matter how many times the experiment was repeated, no matter what efforts were expended to ensure that there were absolutely no differences in the conditions that could affect the lava pattern, it was always the case that the lamps differed from one another and differed from how they appeared in all previous replicates of the experiment.

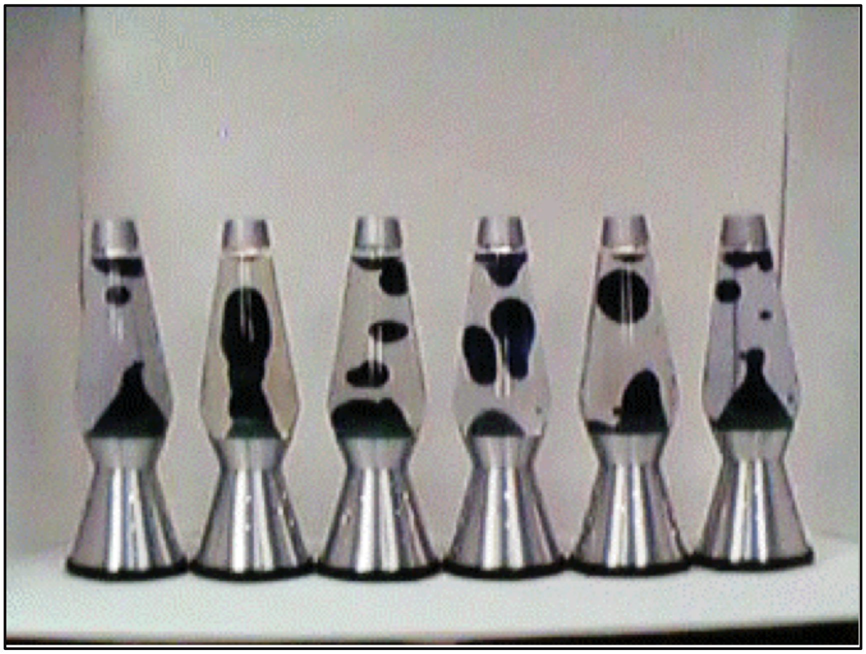

Figure 1. Variability exhibited by identical lava lamps. The lamps were all the same model and were operated under identical conditions insofar as that was possible. Nevertheless, a consistent pattern of lava flow in the different lamps never occurred despite many attempts to produce it. It can be concluded, therefore, that small differences in conditions between the lamps (too small to discern) were capable of dramatically affecting the future behavior of the lamps.

This example shows that unavoidably small differences in initial conditions can cause gross differences in the behavior of, for all practical purposes, identical physical systems. Put another way, the lava lamps could detect uncontrollably small differences between one another in ambient conditions and, in response, exhibit different behaviors. It was always possible to write an equation that described a particular observed pattern. It was never possible to write an equation that predicted a pattern that would be observed.

The laws of physics, in particular the laws of mechanics and thermodynamics, govern the motion of the lava, just as Maxwell's equations govern any possible effects of powerline electromagnetic energy on exposed subjects. But a linear model cannot be employed in conjunction with the laws of physics to explain the motion of the lava, and it would be absurd to argue that, as a consequence, the appearance of differences in the flow between different lamps is an illusion or artifact. The fact is, the lava flow differs in apparently identical lamps despite all attempts to assure identical behavior. If there is an intention to describe the flow, an appropriate nonlinear interaction model must be used. The seminal property of the required model is precisely that there is no proportionality between the input and the output of the system.

If a simple physical system such as a lava lamp can exhibit complex behavior and sensitivity to initial conditions, then it should be obvious that living systems, which are vastly more complex, may similarly be capable of detecting small changes in environmental conditions.

How small a difference in initial conditions might be capable of causing an effect? Consider the Lorenz system, a set of nonlinear equations that govern the behavior of weather in the atmosphere. Initial conditions that must be specified in this model include the temperature, humidity, and pressure. Any particular set of initial conditions corresponds to a predicted pattern of change. Differences in initial conditions lead to unpredictability (deterministic chaos) even though the behavior is completely determined. To understand how chaos can result, suppose a description of the weather at a certain time is used, and the subsequent change in relative humidity is calculated. If the calculation is repeated exactly except for a change in the initial temperature of 0.000001° C, after a short time the system evolves along a totally different path (Figure 2).

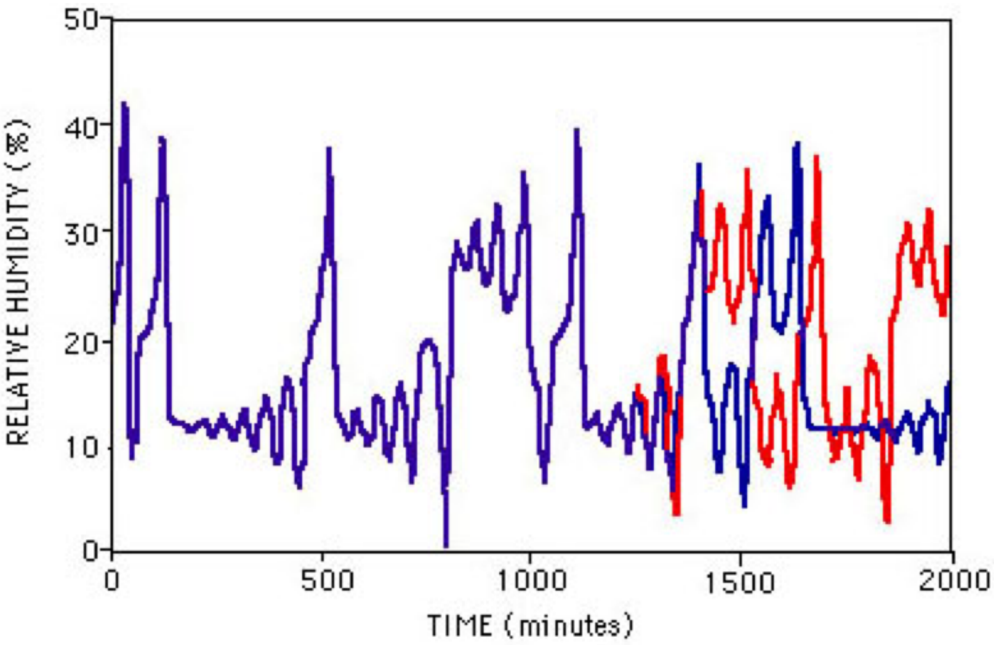

Figure 2. Unpredictability in a deterministic model of the weather. The blue line depicts the relative humidity predicted by the laws of physics using a nonlinear model for a given set of conditions. The red line shows the humidity under exactly the same conditions except that the initial temperature was increased by 0.000001°C. The change had no effect on the prediction for about 1300 minutes. Thereafter, the two cases differed markedly, showing that the system could respond to and modify its behavior as a result of changes that would be entirely insignificant under the assumption of a linear model.

The example of the lava lamp shows that, even though the linear interaction model does not explain electromagnetic-energy-induced bioeffects, a nonlinear model could rationalize the existence of such effects in the sense that one could understand how their occurrence would be consistent with the laws of physics.

Physicists have not determined what nonlinear model could be used to explain electromagnetic-energy-induced bioeffects or predict the time scale associated with their occurrence. But this is a practical limitation on the physics thought-style, not a theoretical limitation; it is possible, in principle, that the particular nonlinear interaction models may be discovered for some types of electromagnetic-energy-induced bioeffects.

The analysis presented here does not prove that electromagnetic energy bioeffects are nonlinear. It shows only that such effects could exist and be compatible with the laws of physics and the hypothetical-deductive method of physics. Thus, the laws of physics are entirely consistent with the claim that powerline electromagnetic energy could be a health risk.

Physics and Complexity

There is nothing novel in the conclusion that the laws of physics are powerless to predict or preclude some phenomena. The structure of normal joint cartilage is the result of a balance between synthesis and destruction of extracellular matrix proteins. If disruption occurs in regulation of the proteases that regulate the process, the result is osteoarthritis. The laws of physics neither predict nor explain how this process occurs, and it does not appear there is any reasonable likelihood that they will do so soon. Ultraviolet light, radon gas, tobacco smoke, and asbestos each can cause cancer but, again, the laws of physics neither predict nor explain the relationships. Following a fracture, the local cellular cytokine environment is altered, resulting in cellular proliferation and the formation of osteoblasts that synthesize new bone. Neither the appearance of the osteoblasts nor their disappearance following injury repair are predicted or explained by the laws of physics. These and myriad other examples plainly show that the laws of physics don't explain everything. Indeed, they explain almost nothing about complex systems such as biological organisms. The inability to predict or preclude powerline electromagnetic energy bioeffects in the physics thought-style is a direct consequence of the complexity of biological organisms, in particular, their nonlinearity.

The ability to predict the future and to neglect small differences is possible only in the context of closed linear systems. That is, systems that can be modeled linearly only if they do not exchange energy with their surroundings. In that case the laws of physics can explain and predict. The operation of automobiles, space ships, atomic bombs, and powerlines are all achievements of twentieth century physics. But earthquakes, volcano eruptions, the weather, the activity in lava lamps, and the behavior of living things cannot be predicted because these systems exchange energy with their environment and are governed by nonlinear empirical laws. These systems do not violate the laws of physics as would, for example, a perpetual motion machine, or a spaceship that could travel faster than the speed of light. It is simply that we do not know how to apply the laws of physics to them (36).

Theoretical Limit of the Physics Thought-Style

Some effort is presently being devoted to identifying the particular nonlinear model applicable to powerline electromagnetic energy, and the day may come when it is possible to satisfactorily explain or even predict some electromagnetic-energy-induced bioeffects. Even if that occurs, however, it will still be impossible to resolve certain kinds of crucially important questions concerning the health risks of powerline electromagnetic energy within the physics thought-style.

Physics deals with empirical mathematical laws in the context of particular conditions of observation. The empirical law for a particular case is an amalgam of one or more of the laws of physics and one or more auxiliary hypotheses and models that are necessary to tailor the basic laws to the particular case. The empirical law is then said to "explain" the observations. The observations affect prediction in two ways. First, they help to define the particular auxiliary hypotheses that are needed. Second, they establish the starting point and general frame of reference of the applicable empirical law (the initial conditions and the boundary conditions).

Physics is geared toward the activity of prediction because that evidences explanation, which is what makes physics powerful and useful for the development of technology. But physics has its limitations. It is useless for proving what caused a specific event, especially so a biological event. In particular, if X is a stimulus, Y is a response, and Z is a particular subject, propositions of the form X caused Y in Z are meaningless within the physics thought-style because postdiction is impossible unless all conditions are known, and it is generally the case that all the pertinent conditions that existed in the past are not known (37).

Summary

Whether or not powerline electromagnetic energy affects human health cannot be ascertained within the physics thought-style. This fact does not imply that powerline electromagnetic energy is not a health hazard. Rather, it indicates only that the question cannot be answered if one chooses to think solely like a physicist. In other words, physics does not predict or preclude that powerline electromagnetic energy affects human health. Although the risk question remains open within the physics thought-style, there is another way to establish scientific facts — the biological thought-style. It is possible, therefore, that the question could be answered affirmatively within that thought-style.

CHAPTER 4. BIOLOGY AND POWERLINE ELECTROMAGNETIC ENERGY HEALTH RISKS

Introduction

There are two scientific methods for establishing scientific facts. In principle, therefore, there are two ways in which scientific facts could be established that bear on the question whether powerline electromagnetic energy affects human health. The method of physics does not result in facts that materially support either side of the issue. Here, I again consider the question of harm from powerline electromagnetic energy, but in the context of the more general thought-style of biology.

Many disparate views regarding whether powerline electromagnetic energy affects human health have been expressed in editorials, informational pamphlets, government reports, journal articles, and books. The opinions differed even though the investigators who performed electromagnetic energy bioeffects studies professed common goals for their experiments, and even those who offered the opinions evaluated the same laboratory data.

Why do divergent opinions abound regarding the public-health significance of the electromagnetic energy biological studies? My first goal is to show that differences in the hypotheses, norms, and theories of both the laboratory investigators and the expert reviewers caused the split in opinion. Different scientists did not reason the same way, and it is therefore not surprising that they reached different conclusions.

Because differences in biological reasoning lead to opposite conclusions regarding whether powerline electromagnetic energy affects human health, it is necessary to choose how the issue ought to be decided. My second goal is to explain why this decision rests only partially with scientists. It is the right of the public to decide some pertinent issues as, for example, the level of certainty to be used when evaluating the scientific evidence for the purpose of making policy decisions that affect public health.

The Biological Evidence

Biological evidence about the effects of powerline electromagnetic energy can come only from studies in which animals or human subjects were exposed to electromagnetic fields and then observed to determine the consequences of the exposure (38). We expect that if it is true that powerline electromagnetic energy can affect human health, then some kind of a consistent pattern of changes will be observed in such studies. We recognize that the mechanisms may be obscure or even completely unknown, but we require, at a minimum, the existence of some reproducible or reliable phenomena that can serve as the basis of an inference that powerline electromagnetic energy can affect human health.

The reported electromagnetic energy bioeffects studies, however, appear to be highly problematical for at least two reasons. First, there are instances in which investigators failed to find an effect due to electromagnetic-energy exposure. For example, a group of investigators tested the hypothesis that exposure of lambs to powerline electromagnetic energy would alter melatonin patterns and thereby cause a delay in the onset of puberty.

They reported no effect on the time of onset and argued that the results were evidence against the theory that electromagnetic energy affects melatonin. But there are many possible reasons why no effects were seen by the investigators besides the reason they favored — the absence of an ontological basis. The investigators could have been dishonest and/or incompetent, as examples, or perhaps they were just unlucky and looked in the wrong place (39). Although logically absurd, it is a fact that all electromagnetic energy studies are viewed by some experts as dubious largely on the basis of comparisons between negative and positive studies in which a particular parameter was measured using different experimental designs.

A second reason for uncertainty regarding the implications of the electromagnetic energy bioeffects studies is that there appear to be inconsistencies involving similar experimental designs within virtually every line of electromagnetic energy biological research. A pattern has emerged during the last 25 years in which a report of an electromagnetic energy bioeffect in a particular animal model observed under particular conditions was followed by a second report by another group of investigators who performed a similar study but could not confirm the original results. The pattern has been repeated many times. Calcium adsorbed on brain tissue was reported released at different rates depending on the presence or absence of weak electromagnetic energy (40), but others were unable to reproduce this effect (41). Electromagnetic energy affected skeletal growth in chicks (42), but the same model system did not yield positive effects in the hands of other investigators (43). Sometimes electromagnetic energy affected growth rate of animals (44), but not in other cases (45). Electromagnetic energy altered transcription (46) or not (47) in seemingly identical experiments performed by different investigators. Electromagnetic energy was or was not associated with cancer (48), affected or did not affect melatonin levels in the blood (49), and did or did not induce a stress reaction (50), modify behavior (51) or affect cell growth *in vitro* (52), again depending on who conducted and evaluated the experiment. Even small children can see the obvious pattern — independent experts find effects but experts financially bonded to the power industry as employees, consultants or contractors find no effects. Doubts and uncertainties are intentionally manufactured by the power industry, just as they were manufactured by the tobacco industry.

The species of inconsistency that occurs when results from different experimental designs are compared is not important for the simple reason that no skill whatever is required to design and perform a study that finds nothing. I will deal with this issue in a later section on trade-association science.

Here I address the serious kind of inconsistency that apparently occurred when a group of investigators used an experimental design similar to that of an initial group but failed to find the same results. If the reality were that the exposed subjects did not detect the presence of the electromagnetic energy, then the reports that failed to find a biological effect due to electromagnetic energy exposure would reflect the objective state of nature (53). In that event, the positive reports would be artifacts, errors, or statistical fluctuations. It is crucial, therefore, to determine whether the results of the intra-experimental-design studies were actually inconsistent.

Possible Bases of Apparent Inconsistency

Early in the evolution of the dispute regarding whether powerline electromagnetic energy affects human health, some literature dealing with the issue was pregnant with the notion that essentially all positive reports were somehow due to poor experimental procedures on the part of the investigators. The criticism initially appeared as a series of accusations against Soviet scientists made by experts economically bonded to various industries involved in the generation of electromagnetic energy normally found in the environment. Their criticism spread to American and European investigators who reported electromagnetic energy effects. Ultimately, however, as the electromagnetic energy health-risk dispute developed it became broadly obvious that this explanation was baseless and inaccurate.

A second possible explanation for the apparent inconsistencies was that they resulted from statistical fluctuations. In this view, a few studies that looked positive were to be expected on the basis of statistical fluctuations alone. A difficulty with this argument was that each of the electromagnetic energy studies was independent in the statistical sense, and each was protected at the 5% level against the statistical error of declaring an effect when none actually existed. Consequently, assuming statistical fluctuations were important, there was no reason to conclude that it was the statistical fluctuations associated with the positive studies that were misleading, rather than the statistical fluctuations associated with the negative studies. But even if the statistical-fluctuations argument was a good one, it applied only where a few kinds of electromagnetic energy studies were performed. The argument failed to explain why putative statistical fluctuations occur in the context of every experimental design in which a positive effect was reported.

A third potential basis for intra-experimental-design inconsistency was biological variability. The proponents of this view pointed to circadian rhythms, genetic differences between individuals, microenvironmental factors, and the complexity of the neuroregulatory and immunoregulatory systems of the body, and argued that interactions among these myriad variables, not the consequences of electromagnetic energy, produced the claimed differences between exposed and control animals. But this explanation cannot be correct because it too is improbable. If it were true that the many interacting variables caused inferential errors in the biological studies, then the overwhelmingly likely direction of the error would have been towards failing to recognize true effects, rather than towards failing to correctly accept results as negative. Thus the argument is premised correctly (biological variability), but the conclusion is wrong.

Another explanation is that the appearance of inconsistency arose because of differences in purpose or plan among the investigators who performed the electromagnetic energy studies, as reflected in their hypotheses, norms, and theories. To understand how, in principle, such differences could account for the appearance of inconsistency between studies that were intended by the investigators to be similar to each other, consider (hypothetical, for now) studies dealing with the effects of powerline electromagnetic energy on the growth rate of animals. Let W stand for the average value of the weight of a group of animals in a study and V stand for the variance in the weight. The subscripts E and C will be used to designate the experimental and control groups, respectively.

The purpose or plan of an investigator is reflected in his hypothesis. Possible study hypotheses include:

- Hypothesis No. 1: W_E is greater than W_C
- Hypothesis No. 2: W_E is less than W_C
- Hypothesis No. 3: W_E is not equal to W_C
- Hypothesis No. 4: V_E is greater than V_C
- Hypothesis No. 5: V_E is less than V_C

- Hypothesis No. 6: V_E is not equal to V_C
- Hypothesis No. 7: W_E and V_E are not equal to W_C and V_C

Suppose results supporting Hypothesis No. 1 and Hypothesis No. 2 were observed in two different studies. It has been argued that the studies were inconsistent, and in a sense the argument is correct because identical results were not observed in different experiments. But in another sense the results *were consistent* because both studies agreed that electromagnetic energy affected body weight — they differed only with regard to the direction of the change that was observed. Thus, the results are consistent or inconsistent depending upon one's attitude regarding the meaning of *consistent*.

Now consider an inconsistency in average body weight between positive and negative reports (an "effect" or "no effect") when the experimental design of the two studies is deemed to be identical (see Project Henhouse below). In this case, the reports are inconsistent regarding the implications that electromagnetic energy exposure affected the average weight of the animals. The implication of the positive report would be that the electromagnetic energy was somehow detected by the bodies of the exposed electromagnetic energy animals, resulting in a change in the average body weight. The implication of the negative report would be that detection did not occur because, if it had occurred, the results would not have been negative.

In this case, there is a possible state of nature in which the implications of both studies would be consistent.

Suppose in the study that was apparently negative on the basis of Hypothesis No. 3, there was an effect under Hypothesis 6.

The state of nature would be that the positive study was positive because W was altered by the electromagnetic energy, and the study that was judged negative because W was not altered was actually positive because V was altered (54).

Thus, the studies would be consistent because both would imply that the electromagnetic energy was detected by the body.

I now want to show that in the reported electromagnetic-energy animal-growth-rate studies, the apparent inconsistencies disappear when the hypothesis, purpose, and plan of the investigators are considered. For the present, I do not consider the problem of rigged experimental designs or fabrication or falsification of data; rather the focus is on statistical considerations, which are the rational basis of a decision concerning the occurrence on an effect.

Powerline Electromagnetic Energy and Growth Rate

In the 1970s, Richard Phillips and his colleagues at Battelle performed two apparently identical but independent experiments dealing with the effects of powerline electromagnetic energy on the growth rate of mice. In each experiment, three generations of mice were exposed essentially continuously to electromagnetic energy under conditions designed to avoid artifacts that they perceived to be responsible for earlier positive results in experiments performed by my colleagues and me (55, 56).

The results of their first experiment showed that the average weights of both the male and the female mice were less than their corresponding controls (Table 3). In the second experiment the average weight of the male and female exposed mice were significantly greater than the corresponding controls (Table 4). The investigators averaged the results of the first experiment with those of the second experiment and concluded that the data provided no evidence that powerline electromagnetic energy can affect growth.

Table 3. Influence of 60-Hz vertical electric field, 100 kV/m, on development and variance in development in mice. The average value ± SD (in grams) are listed at the indicated number of days after birth. Values of mean weight and standard deviation that differed significantly from the corresponding controls are indicated by an asterisk. N is given in parentheses. See R.D. Phillips, L.B. Anderson and W.T. Kaune: *Biological Effects of High-Strength Electric Fields on Small Laboratory Animals*, Richland, WA, Pacific Northwest Laboratories. DOE/TIC-10084, Contract E4-76-C-06-1830, 1979. The statistical tests for the mean and variance (here and in Table 4) were the t test and the F test, respectively. These tests were performed by me. EE, electromagnetic energy.

			Weight (grams)				
			Day 1	**Day 14**	**Day 28**	**Day 35**	**Day 70**
First Generation	**Male**	**EE**	*1.8 ± *0.2 (30)	*7.0 ± 0.8 (30)	*16.4 ± *2.4 (27)	23.8 ± 2.4 (27)	*34.6 ± 2.1 (27)
		Control	2.0 ± 0.2 (28)	7.5 ± 0.7 (28)	19.7 ± 1.9 (27)	24.8 ± 2.4 (27)	36.9 ± 2.1 (26)
	Female	**EE**	1.8 ± 0.2 (34)	7.1 ± 0.6 (34)	16.4 ± 1.9 (34)	*20.3 ± 1.6 (34)	*28.0 ± 1.4 (34)
		Control	1.9 ± 0.2 (28)	7.4 ± 0.6 (28)	17.1 ± 1.8 (27)	21.3 ± 1.5 (27)	29.9 ± 1.6 (26)

			Weight (grams)				
			Day 1	**Day 14**	**Day 28**	**Day 35**	**Day 70**
Second Generation	**Male**	**EE**	*1.8 ± *0.2 (23)	7.5 ± 0.6 (22)	*20.4 ± *2.2 (22)	26.2 ± *1.6 (11)	35.9 ± 1.6 (22)
		Control	2.0 ± 0.2 (28)	7.3 ± 0.8 (28)	18.6 ± 3.0 (28)	25.8 ± 2.4 (24)	36.0 ± 2.1 (28)
	Female	**EE**	*1.8 ± *0.2 (22)	7.6 ± *0.4 (22)	*19.0 ± *1.7 (22)	23.6 ± 1.6 (11)	*29.2 ± 1.8 (22)
		Control	1.9 ± 0.1 (28)	7.2 ± 0.9 (28)	17.2 ± 2.8 (27)	22.9 ± 2.0 (23)	30.7 ± 1.9 (27)

			Weight (grams)				
			Day 1	**Day 14**	**Day 28**	**Day 35**	**Day 70**
Third Generation	**Male**	**EE**	1.9 ± 0.2 (33)	7.2 ± 0.7 (33)	19.0 ± 2.2 (33)	*25.0 ± 2.1 (31)	*34.2 ± 1.8 (32)
		Control	1.9 ± 0.2 (34)	7.4 ± 0.8 (32)	18.4 ± 2.6 (34)	26.2 ± 2.5 (32)	36.9 ± 2.7 (32)
	Female	**EE**	1.8 ± 0.1 (24)	7.0 ± 0.8 (24)	16.7 ± 2.4 (24)	*22.1 ± 1.8 (24)	*28.6 ± *1.1 (23)
		Control	1.8 ± 0.1 (30)	7.2 ± 0.8 (30)	17.5 ± 1.9 (29)	23.0 ± 1.4 (29)	29.9 ± 1.9 (28)

*P < 0.05

Table 4. Influence of 60-Hz vertical electric field, 100 kV/m, on development and variance in development in mice. The average value ± SD (in grams) are listed at the indicated number of days after birth. Values of mean weight and standard deviation that differed significantly from the corresponding controls are indicated by an asterisk. N is given in parentheses. See R.D. Phillips, L.B. Anderson and W.T. Kaune: *Biological Effects of High-Strength Electric Fields on Small Laboratory Animals*, Richland, WA, Pacific Northwest Laboratories. DOE/TIC-10084, Contract E4-76-C-06-1830, 1979. EE, electromagnetic energy.

			Weight (grams)				
			Day 1	**Day 14**	**Day 28**	**Day 35**	**Day 70**
First Generation	**Male**	**EE**	1.9 ± 0.2 (17)	7.4 ± 0.9 (17)	20.3 ± 1.8 (17)	27.3 ± 1.4 (17)	37.0 ± 2.1 (17)
		Control	1.9 ± 0.2 (28)	7.6 ± 0.7 (28)	20.4 ± 2.4 (28)	27.5 ± 1.7 (28)	36.7 ± 2.1 (28)
	Female	**EE**	1.8 ± 0.2 (23)	7.3 ± *1.0 (23)	*16.5 ± 2.1 (23)	*22.8 ± *1.5 (23)	29.5 ± *2.5 (23)
		Control	1.9 ± 0.2 (27)	7.6 ± 0.6 (27)	18.8 ± 1.8 (25)	23.7 ± 1.0 (25)	29.0 ± 1.6 (24)

			Weight (grams)				
			Day 1	**Day 14**	**Day 28**	**Day 35**	**Day 70**
Second Generation	**Male**	**EE**	2.0 ± *0.2 (28)	7.2 ± 1.2 (28)	20.4 ± *3.7 (28)	26.2 ± 2.8 (28)	*37.0 ± 2.3 (28)
		Control	2.1 ± 0.2 (23)	7.1 ± 0.6 (18)	19.5 ± 1.9 (21)	26.4 ± 2.0 (21)	35.4 ± 2.4 (20)
	Female	**EE**	*1.9 ± *0.2 (36)	7.1 ± 1.1 (36)	18.1 ± 2.3 (36)	23.2 ± 1.9 (36)	29.4 ± *1.3 (36)
		Control	2.0 ± 0.1 (30)	7.5 ± 0.9 (19)	17.9 ± 1.7 (29)	23.2 ± 1.9 (29)	29.3 ± 1.9 (28)

			Weight (grams)				
			Day 1	**Day 14**	**Day 28**	**Day 35**	**Day 70**
Third Generation	**Male**	**EE**	2.0 ± *0.1 (35)	*8.0 ± 0.7 (35)	19.8 ± *3.2 (34)	26.8 ± *2.6 (34)	*38.9 ± 2.3 (33)
		Control	2.0 ± 0.2 (30)	7.5 ± 0.6 (30)	19.6 ± 1.9 (30)	26.5 ± 1.6 (30)	36.4 ± 2.3 (30)
	Female	**EE**	*2.0 ± 0.1 (29)	*7.8 ± 0.6 (29)	18.0 ± *2.8 (27)	*22.3 ± *1.9 (27)	*29.9 ± 1.8 (27)
		Control	1.8 ± 0.2 (34)	7.3 ± 0.5 (34)	18.0 ± 1.5 (34)	23.3 ± 1.3 (34)	28.5 ± 1.8 (34)

*$P < 0.05$

How were they able to justify averaging the results of two independent, statistically significant experiments to conclude that no effects were seen? It was done by assuming a linear model for the interaction between electromagnetic energy and tissue. The investigators assumed that differences observed in the weights of individual mice in the control group were due to random fluctuations, and that any effect due to electromagnetic energy would be linear. In this model, an effect due to the field must be consistent from animal to animal and from experiment to experiment, regardless of all factors or conditions other than those explicitly controlled. If, for example, the electromagnetic energy produced an increase in the weight in one animal and a decrease in a second animal, that result would violate either the assumption that uncontrolled factors were unimportant, or the assumption that the response was deterministic. For this reason, when Phillips found that the electromagnetic energy mice in the second experiment were not smaller than the controls, as was the case in their first experiment, he concluded that the absence of a consistent change in the average meant that there was no effect due to the electromagnetic energy.

The chain of reasoning in the Phillips study began with the assumption that a linear model governed any possible response of the mice to the electromagnetic energy, and went as follows: because no consistent effects on the average weight of the exposed mice were found, there was no linear response, and therefore no response at all; consequently, the experiments furnished no evidence suggesting that the electromagnetic energy was detected by the body; because there was no evidence of detection, the study provided no evidence of possible health risks. The important point regarding this reasoning is that its validity is entirely dependent on the validity of a linear model. In this model, consistency of change in the average value of the weight is an absolute requirement.

When Phillips visited my laboratory in September 1976, I objected to his plan to assume a linear interaction model. Although Phillips' experimental procedures were similar to experiments performed by my colleagues and me (55, 56), we did not assume a linear model in the evaluation of the data, and therefore did not require consistency in the average value of body weight as a pre-condition before concluding that the electromagnetic energy caused an effect. Instead, we evaluated the data as planned comparisons to assess whether there was or was not a difference between the exposed and control groups at the ordinary level of scientific certainty (5%). Because we did not assume that the effects of electromagnetic energy would necessarily be linear in nature, the character required to be manifested by the data was not consistency in change in the average value, but rather consistency in the finding of a difference between the exposed and control groups in particular experiments. Our rationale was that this kind of consistency would justify a conclusion that the electromagnetic energy had been detected by the animal. It is plainly true that consistency in the mean is sufficient but not necessary to support this conclusion.

I would interpret Phillips' studies (57) not the way he did, but rather the same way I interpreted my own studies. His data showed that powerline electromagnetic energy consistently affected the body weight of exposed animals, even though the effect could not be predicted in individual experiments.

Beyond Linear

The difference between Phillips and me regarding our interpretations of our powerline electromagnetic energy studies on body weight in animals was related to our attitude regarding the public-health implications of our work. Phillips sought the strongest possible evidence regarding the biological effects of powerline electromagnetic energy — a consistent effect on the average value — and planned to deny the existence of any kind of lesser evidence. Had he found the type of evidence he sought, powerline electromagnetic energy would have been conclusively established as a health risk and it would be unthinkable that the power industry would routinely carry out involuntary exposure to powerline electromagnetic energy. The position of EPRI and the power companies who sponsored Phillips' work was that until this kind of conclusive evidence had been obtained, the scientifically proper public-health strategy was to do nothing.

I never accepted the industry position, hence I thought Phillips' efforts were entirely misplaced. From my viewpoint, the conclusive evidence that Phillips sought might be impossible to obtain. There might be no such thing as a consistent effect on the mean of body weight or any other dependent variable in a powerline electromagnetic energy study. That state of the evidence would not prove that electromagnetic energy doesn't cause human diseases. It would prove only that a conclusive demonstration of powerline-electromagnetic energy health risks was not possible. Consequently, for public-health purposes, I thought the linear model was overkill. Consistency in the mean would have provided conclusive evidence; but consistency in change would be enough to warrant an inference of electromagnetic energy detection, and that alone might justify the implication of health risk.

Change, as reflected in experimental data, is typically measured by the variance. Consequently, I analyzed the published electromagnetic energy reports, other than the ones by Phillips or me, to assess whether they provided evidence that electromagnetic energy exposure consistently resulted in change. I searched the literature for all studies that might plausibly be viewed as similar to the studies we conducted. I looked for studies that involved exposure of animals under laboratory conditions to power-frequency electromagnetic energy for long periods of time for the purpose of assessing the effect on body weight. I included every such study I could find that had analyzable data.

Some of the studies reported an effect of electromagnetic energy exposure on the average weight, and some did not report such an effect. Juxtaposition of the latter reports with the positive reports was what gave credence to the idea that the electromagnetic energy growth-rate studies were inconsistent, and hence not a proper basis for setting public-health policy. But when I analyzed these studies, I found that they manifested a consistent effect on change in weight (Table 5). The studies involving effects of electromagnetic energy on body weight were therefore consistent if the effect searched for was change rather than increase or decrease. Only if the added condition that the change always occur in the sample mean were added, could it be said that the studies were inconsistent. I prospectively tested and verified the idea that powerline electromagnetic energy is detected by the body as manifested in a change in growth, even though the electromagnetic energy does not result in a consistent change in the average body weight (58).

Table 5. Electromagnetic energy effects on variance in body weight of mammals. The studies that used low-frequency fields and presented sufficient data to permit analysis are included. The means ± SD are listed; the number of animals is given in parentheses. M, male; F, female. In most of the studies, the average value of the body weight was chosen as the basis of comparison, but this need not have been the case because there is no logical or biological requirement that the average weight of the exposed animals should be altered as a consequence of powerline electromagnetic energy exposure as a condition for accepting the conclusion that powerline electromagnetic energy was detected by the animal. The variance is also an appropriate statistic for assessing whether powerline electromagnetic energy was detected by the body, and it is logically as probative of the occurrence of detection as is the average. The statistical tests for the means were t-tests, which were performed by the investigators. The tests for variance (F tests) were performed by me. The F value and the corresponding probability are listed in the last two columns. The rejection region for F is P < 0.025, which corresponds to a probability of type-1 error of P < 0.05. EE, electromagnetic energy.

Ref.	**Species**	**EE**	**Exposure Duration**		**Sex**	**Body Weight**		**F**	**P**
				Exp. No.		**Control**	**EE**		
a	Pigs	30 kV/m 60 Hz	Conception–birth	1	M	536 ± 74.2 (28)	553 ± 157.5 (56)	**4.50	<0.001
					F	510 ± 91.7 (29)	518 ± 135.0 (56)	**2.16	0.015

				Exp. No.		Control	EE		
Ref.	**Species**	**EE**	**Exposure Duration**		**Sex**	**Body Weight**		**F**	**P**
				2	M	576 ± 129.2 (29)	532 ± 109.3 (71)	**1.40	0.130
					F	573 ± 123.8	*488 ± 118.0 (71)	**1.10	0.36
b	Monkeys	2 gauss 20 V/m 72–80 Hz	1 year		M	2290 ± 510 (14)	*3060 ± 470 (14)	1.18	0.39
					F	1290 ± 700 (16)	1260 ± 920 (16)	1.73	0.15
c	Rats	150 kV/m 60 Hz	Conception–21 days		M	47 ± 6.7 (56)	45 ± 13.7 (58)	**4.18	<0.001
					F	43 ± 8.2 (56)	44 ± 12.9 (58)	**2.47	<0.001
d	Rats	80 kV/m 60 Hz	Conception–weaning	1	M	66.5 ± 31.1 (123)	65.6 ± 35.4 (58)	1.29	0.070
					F	60.8 ± 29.4 (119)	59.4 ± 25.8 (126)	1.30	0.075
				2	M	45.1 ± 27.9 (268)	42.9 ± 40.0 (220)	2.06	<0.001
					F	42.7 ± 20.6 (295)	42.7 ± 31.2 (270)	2.29	<0.001
				3	M	41.7 ± 16.4 (188)	41.9 ± 29.6 (199)	3.25	<0.001
					F	38.9 ± 15.7 (204)	41.3 ± 28.8 (208)	3.36	<0.001
e	Rats	3 kV/m 4.45 Hz	36 days		M	414 ± 17 (47)	*362 ± 9 (47)	**4.18	<0.001
f,g	Rats	2 kV/m 3.45 Hz	28 days	1	M	398.5 ± 30.1 (16)	395.9 ± 40.6 (16)	1.82	0.13
				2	M	349.1 ± 29.3 (16)	358.1 ± 25.5	1.32	0.30
				3	M	398.6 ± 34.2 (16)	388.3 ± 21.3 (16)	2.58	0.038

*P < 0.05, compared with control mean
**Contains round-off error due to uncertainty in sample size

a. Sikov, M.R., Rommereim, D.M., Beamer, J.L., Buschbom, R.L., Kaune, W.T. and Phillips, R.D.: Developmental studies of Hanford miniature swine exposed to 60-Hz fields. *Bioelectromagnetics* 8:229–242, 1987.

b. Grissett, J.D., Cupper, J.L., Kessler, M.J., Brown, R.J., Prettyman, G.D.L., Cook, L.L. and Griner, T.A.: *Exposure of Primates for One Year to Electric and Magnetic Fields Associated with ELF Communications Systems.* Pensacola, FL: Naval Aerospace Medical Research Laboratory (NAMRL/1240), 1977.

c. Rommereim, D.N., Kaune, W.T., Anderson, L.E. and Sikov, M.R.: Rats reproduce and rear litters during chronic exposure to 150 kV/m, 60 Hz electric fields. *Bioelectromagnetics* 10:385–389, 1989.

d. Seto, U.J., Majeau-Chargois, D., Lymangrover, J.R., Dunlap, W.P., Walker, C.F. and Hsieh, S.T.: Invesetigation of fertility and in utero effects in rats chronically exposed to a high-intensity 60-Hz electric field. *IEEE Trans. Biomed. Eng.* 31:693–701, 1984.

e. Noval, J.J., Sohler, A., Reisberg, R.B., Coyne, H., Straub, H., Straub, K.D. and McKinney, H.: Extremely low frequency electric field induced changes in rate of growth and brain and liver enzyme of rats. In: *Compilation of Navy Sponsored ELF Biomedical and Ecological Research Reports, Vol. 3*. Bethesda, MD: Naval Medical Research Center, B11, 1974.

f. Mathewson, N.S., Oosta, G.M., Levin, S.G., Diamond, R.S. and Ekstrom, M.E.: *Extremely Low Frequency Vertical 45 Hz Electric Field Exposure of Rats: A Search for Growth, Food and Water Consumption, Blood Metabolite, Hematological, and Pathological Changes*. Bethesda, MD: Armed Forces Radiobiology Research Institute, SR77-2, 1977.

g. Mathewson, N.S., Oosta, G.M., Oliva, S.A., Levin, S.G. and Diamond, S.S.: Influence of 45-GHz vertical electric fields on growth, food and water consumption, and blood constituents of rats. *Radiat. Res*. 79:468–482, 1979.

With regard to the studies involving the effects of powerline electromagnetic energy on body weight, therefore, if the hypotheses, purposes, and plans of the investigators are taken into account in evaluating the data from a reasonably similar series of animal studies, the implications of the studies are generally consistent in the sense that they indicate the existence of a cause-effect relationship between powerline electromagnetic energy and changes in body weight (59).

The Nonlinear Model and Consistency of Electromagnetic Energy Bioeffects

If declining the assumption of a linear model generally leads to an explanation of intra-experimental-design inconsistency, then it ought to be possible to show that this is the case in other lines of research besides those involving body weight. The Henhouse studies are another group of similar experiments that can be evaluated for this purpose.

In 1982, Delgado and colleague reported that electromagnetic energy caused skeletal abnormalities in chicken embryos. The report led to follow-up studies, some of which confirmed the effect and some which did not. One proposed hypothesis to explain the apparent inconsistencies was that they were due to differences in the exposure systems used in the studies. If everyone used exactly the same apparatus and procedure, then consistent results might be obtained. The exposure systems were therefore rigorously standardized and similar experiments were carried out in three laboratories in the United States and three in Europe. The result was that significantly more defective embryos were found among the electromagnetic energy-exposed eggs, even though that result was not obtained in each laboratory (Table 6).

Table 6. Proportions of normal living embryos (means ± SE). Approximately 100 embryos in the MEF and in the control group were studied at each laboratory. On the basis of ANOVA, there was a significant difference between the electromagnetic energy and control groups, F(1,54)=12.09, P=0.001. See Berman, E., Chacon, L., House, D., Koch, B.A., Koch, W.E., Leal, J., Løvtrup, S., Mantiply, E., Martin, A.H., Martucci, G.I., Mild, K.H., Monahan, J.C., Sandström, M., Shamsaifar, K., Tell, R., Trillo, M.A., Ubeda, A. and Wagner, P.: Development of chicken embryos in a pulsed magnetic field, *Bioelectromagnetics* 11:169–187, 1990.

Principal Investigator	Location	Sham Exposed	Exposed
A.C. Martin	London, Ontario, Canada	0.936 ± 0.024	0.794 ± 0.024
K.H. Mild	Umeå, Sweden	0.916 ± 0.026	0.874 ± 0.026
J.C. Monahan	Rockville, MD, USA	0.903 ± 0.030	0.778 ± 0.030
J. Leal	Madrid, Spain	0.829 ± 0.041	0.796 ± 0.057
W.E. Koch	Chapel Hill, NC, USA	0.784 ± 0.027	0.785 ± 0.035
G.I. Martucci	Las Vegas, NV, USA	0.730 ± 0.050	0.699 ± 0.044

The sponsors of the international cooperative effort that led to the data in Table 6 went to extraordinary lengths to ensure that all of the participating investigators followed exactly the same experimental design and procedure. It is unlikely that this kind of inter-laboratory synchronization of experiments will be attempted again soon because of the high costs. Ironically, a line of argument subsequently developed holding that effects of electromagnetic energy on skeletal development in chicks is not important for the purposes of evaluating potential health hazards of electromagnetic energy, even though that was largely how the studies were initially justified (60). But even if this view were accepted, the Henhouse effort would still be important because, far better than could have been imagined, it revealed the role of normally uncontrolled variables in altering the manifestation of electromagnetic energy transduction. This was also the real message of Phillips' growth-rate studies. If neither the Battelle investigators nor the Henhouse investigators could eliminate the impact of these factors, despite great efforts and the expenditure of millions of dollars, it is safe to conclude that they cannot be eliminated. The most parsimonious explanation for both studies, therefore, is that the biological systems were highly sensitive to initial conditions that were not — and could not be — controlled despite all reasonable efforts to do so. As I showed in the previous section, this is a fundamental, defining property of nonlinear systems.

The apparent intra-experimental-design inconsistencies in the studies involving the effect of powerline electromagnetic energy on cellular transcription can also be resolved on the same basis that afforded resolution of the apparent inconsistencies of the body-weight studies and the Henhouse studies. The case of apparent inconsistency in transcription studies began when Goodman and her colleagues reported that powerline magnetic fields affected cellular transcription. They did many different experiments and the reported effect of the electromagnetic energy was different under different circumstances. Goodman's studies elicited much interest because they suggested a link between the powerline electromagnetic energy issue and orthodox molecular biology. However, Saffer and Thurston conducted similar studies and found results that they said refuted Goodman (61).

They focused on a particular set of conditions (57 mG, 20 minutes' exposure), and reasoned that either exactly the same data that Goodman observed under those conditions must be observed in their laboratory (irrespective of the myriad differences in other environmental factors between the two laboratories), or Goodman's inference that power-frequency magnetic fields can alter cellular transcription was wrong. When Saffer and Thurston measured the average amount of mRNA produced by cells, the results did not differ from the average of the controls. But the variance in their experimental data differed significantly from that of the controls, showing that the powerline electromagnetic energy was detected by the cells in their study, resulting in alterations in message for protein. This was exactly the conclusion reached by Goodman.

The apparent intra-experimental inconsistencies in calcium studies can also be resolved. In a series of studies Adey and colleagues, and others, reported that electromagnetic energy had a significant effect on Ca^{2+} in a system involving *in vitro* exposure of parts of animal brains to electromagnetic energy. These studies were the impetus for Albert and his colleagues who conducted a similar series of experiments. They compared the average value of Ca^{2+} in exposed and control dishes containing brain tissue, and found no consistent change in average value in a series of 7 experiments (Table 7). They interpreted this data to indicate that the electromagnetic energy exposure had no significant effect on Ca^{2+}, a conclusion that was apparently inconsistent with the findings of Adey and others. However, the data can be analyzed using the L test to assess whether electromagnetic energy exposure caused any change in Ca^{2+}. The results indicated that electromagnetic energy exposure produced a statistically significant effect. The study was therefore consistent with the results of Adey and others if the plan to interpret the results is modified to allow nonlinear effects to be recognized.

Table 7. Mean and standard deviation of percentage Ca^{2+} released from chick brain tissue slices. See: Albert, E.N., Slaby, F., Roche, J. and Loftus, J.: Effect of amplitude-modulated 147 MHz radiofrequency radiation on calcium ion efflux from avian brain tissue, *Radiat. Res.* 109:19–27, 1987. The authors concluded that the electromagnetic energy had no effect, but this was not true as assessed on the basis of the L test (L = 28.371, $P < 0.0005$). The overall effect was due almost equally to an effect of the electromagnetic energy on variance (L = 14.314) and the mean (L = 14.057).

Experiment Number	Percentage Ca^{2+} Released from Tissue Slices in Test Chamber	Percentage Ca^{2+} Released from Tissue Slices in Control Chamber
1	24.8 ± 3.1	23.0 ± 3.0
2	15.4 ± 2.5	17.4 ± 4.7
3	34.6 ± 2.1	32.2 ± 4.9
4	45.6 ± 3.8	40.1 ± 0.7
5	38.3 ± 5.2	40.7 ± 8.7
6	26.4 ± 3.3	28.3 ± 4.8
7	24.1 ± 2.9	27.5 ± 2.1

Studies that reported a positive effect of electromagnetic energy on Ca^{++} include:

- Bawin, S.M. and Adey, W.R.: Sensitivity of calcium binding in cerebral tissue to weak environmental electric fields oscillating at low frequency, *Proc. Natl. Acad. Sci. USA* 73:1999–2003, 1976;
- Bawin, S.M., Adey, W.R. and Sabbot, I.M.: Ionic factors in release of $45Ca^{++}$ from chick cerebral tissue by electromagnetic fields, *Proc. Natl. Acad. Sci. USA* 75:6314–6318, 1978;
- Blackman, D.F., Elder, J.A., Weil, C.M., Benane, S.G., Eichinger, D.C. and House, D.E.: Induction of calcium ion efflux from brain tissue by radio-frequency radiation: Effects of modulation frequency and field strength, *Radio Sci.* 14(6S):93–98, 1979;
- Adey, W.R.: Frequency and power windowing in tissue interactions with weak electromagnetic fields, *Proc. IEEE* 68:119–125, 1980;
- Blackman, C.F., Benane, S.H., Elder, J.A., House, D.E., Lampe, J.A. and Faulk, J.M.: Induction of calcium ion efflux from brain tissue by radiofrequency radiation: Effect of sample number and modulation frequency on the power-density window, *Bioelectromagnetics* 1:35–43, 1980;
- Blackman, C.F., Benane, S.G., Joines, W.T., Hollis, M.A. and House, D.E.: Calcium ion efflux from brain tissue: Power density versus internal field intensity dependencies at 50 MHz RF radiation, *Bioelectromagnetics* 1:277–283, 1980.

Apparent inconsistencies have also been manifested in human studies. In 1966, Howard Friedman and Dr. Becker studied the effect of electromagnetic energy on the reaction time of human subjects. The subjects were instructed to press a key as quickly as possible after the appearance of a red light, and the results indicated that the electromagnetic energy significantly affected reaction time performance. In 1995, Podd and colleagues repeated the experiment, and concluded that the electromagnetic energy had no effect on reaction time. But even though the two studies were similar regarding exposure conditions and laboratory data acquisition, they differed markedly regarding their hypotheses and associated statistical designs. Friedman and Becker evaluated their data using an F test, to evaluate the effect of electromagnetic energy on variance. In contrast, Podd and colleagues used an ANOVA which entails an assumption of linearity. A true comparison, therefore, would require the use of the F test to evaluate Podd's data. When I did this, the result was that the implications of Podd's data were consistent with those of Friedman and Becker's data and showed that electromagnetic energy affected human reaction time (Table 8).

Table 8. Effect of electromagnetic energy on human reaction time performance. See Podd, J.V., Whittington, C.J., Barnes, G.R.G., Page, W.H. and Rapley, B.I.: Do ELF magnetic fields affect human reaction time?, *Bioelectromagnetics* 16:317–323, 1995.

	All Blocks			**Block 1**		
	Mean ± SD	**F**	**P**	**Mean ± SD**	**F**	**P**
No field	220.7 ± 13.6	3.2453	0.0316	219.0 ± 13.4	3.9019	0.0164
0.1 Hz	224.3 ± 24.5			225.5 ± 26.5		
0.1 Hz	224.3 ± 24.5	2.0056	0.1319	225.5 ± 26.5	1.4642	0.2688
0.2 Hz	218.0 ± 17.3			219.9 ± 21.9		
No Field	220.7 ± 13.6	1.6181	0.2187	219.0 ± 13.4	2.6710	0.0590
0.2 Hz	218.0 ± 17.3			219.9 ± 21.9		

	Block 2			**Block 3**		
	Mean ± SD	**F**	**P**	**Mean ± SD**	**F**	**P**
No field	220.6 ± 13.9	2.5963	0.0660	225.3 ± 15.3	2.0116	0.1309
0.1 Hz	221.3 ± 22.3			223.2± 21.7		
0.1 Hz	221.3 ± 22.3	1.6238	0.2170	223.2± 21.7	2.8297	0.0493
0.2 Hz	217.9 ± 17.5			216.3± 12.9		
No Field	220.6 ± 13.9	1.5851	0.2286	225.3 ± 15.3	0.7109	0.7095
0.2 Hz	217.9 ± 17.5			216.3± 12.		

	Block 4			**Block 5**		
	Mean ± SD	**F**	**P**	**Mean ± SD**	**F**	**P**
No field	218.4 ± 12.9	5.2296	0.0054	220.4 ± 13.8	2.6113	0.0632
0.1 Hz	226.4 ± 29.5			225.2± 22.3		
0.1 Hz	226.4 ± 29.5	3.4423	0.0258	225.2± 22.3	1.5013	0.2557
0.2 Hz	214.7 ± 15.9			221.0± 18.2		
No Field	218.4 ± 12.9	1.51921	0.2497	220.4 ± 13.8	1.7393	0.1863
0.2 Hz	214.7 ± 15.9			221.0± 18.2		

	Sham-Exposure Comparisons		
	Mean ± SD	**F**	**P**
Block 1	219.0 ± 13.4	1.0760	0.4527
Block 2	220.6 ± 13.9		
Block 2	220.6 ± 13.9	1.2116	0.3779
Block 3	225.3 ± 15.3		
Block 3	225.3 ± 15.3	0.7109	0.7095
Block 4	218.4 ± 12.9		
Block 4	218.4 ± 12.9	1.1444	0.4135
Block 5	220.4 ± 13.8		

The data was collected in blocks of 30 trials each. When the data was combined, the result was that the 0.1 Hz condition differed from the control, a result that was generally consistent with the result found by Friedman and Becker (see Friedman, H., Becker, R.O. and Bachman, C.H.: Effect of magnetic fields on reaction time performance, *Nature* 213:949–956, 1967). When the data was analyzed block by block, the implication was the same; of 15 comparisons, 5 were significant at a 5% level, and 7 were significant at a 10% level.

As a positive control I compared the results between different blocks in the no-field condition. No differences would be expected, and none were found.

A final example of how the electromagnetic energy bioeffects studies are consistent when the assumption of a linear model is avoided is provided by the work of Stern and colleagues. In two experiments, they said they found no evidence that electromagnetic energy disrupted the operant behavior of rats. This conclusion was opposite to that of Thomas and colleagues, whose experimental procedures were duplicated by Stern et al. But their data actually supported the conclusion of the earlier study (Table 9).

Table 9. Effect of electromagnetic energy on operant behavior of rats. See Stern, S., Laties, V.G., Nguyen, Q.A. and Cox, C.: Exposure to combined static and 60-Hz magnetic fields: Failure to replicate a reported behavioral effect, *Bioelectromagnetics* 17:279–292, 1996. EE, electromagnetic energy.

	Experiment 1								
Behavioral Measure	**Condition 1** (0.26G DC; 0.5G, 60Hz)			**Condition 2** (0.27G DC; 0.5G, 60Hz)			**Condition 3** (0.27G DC; 0.7G, 60Hz)		
DRL (resp/s)	**Mean ± SD**	**F**	**P**	**Mean ± SD**	**F**	**P**	**Mean ± SD**	**F**	**P**
Control	0.066 ± 0.003	1.8595	0.2755	0.065 ± 0.0002	16.000	0.100	0.064 ± 0.003	20.2500	0.0064
EE	0.066 ± 0.002								
FR (resp/s)									
Control	1.420 ± 0.026	1.0937	0.4665	1.442 ± 0.089	2.2033	0.2315	1.224 ±0.065	0.8977	0.5404
EE	1.481 ± 0.027			1.310 ± 0.133			1.265 ± 0.061		
DR (pellet/min)									
Control	1.045 ± 0.114	110.1537	0.0002	1.102 ± 0.061	2.2950	0.2204	1.202 ± 1.202	1.0324	0.4880
EE	1.011 ± 0.011			1.107 ± 0.040			1.183 ± 1.183		

	Experiment 2					
Behavioral Measure	**Condition 1** (0.26G DC; 0.5G, 60Hz)			**Condition 2** (0.26G DC; 0.88G, 60Hz)		
DRL (resp/s)	**Mean ± SD**	**F**	**P**	**Mean ± SD**	**F**	**P**
Control	0.066 ± 0.013	1.1736	0.4191	0.061 ± 0.0044	1.7778	0.2509
EE	0.065 ± 0.0012			0.060 ± 0.0033		
FR (resp/s)						
Control	1.844 ± 0.0212	1.9975	0.1908	1.793 ± 0.0437	6.7662	0.0174
EE	1.874 ± 0.0150			1.823 ± 0.0168		
DR (pellet/min)						
Control	1.257 ± 0.0657	15.2937	0.0009	1.374 ± 0.0607	2.5484	0.1399
EE	1.224 ± 0.0168			1.308 ± 0.0969		

	Experiment 2		
Behavioral Measure	**Condition 2** (0.27G DC; 0.72G, 60Hz)		
DRL (resp/s)	**Mean ± SD**	**F**	**P**
Tuesday	0.071 ± 0.028	1.2258	0.4055
Friday	0.058 ± 0.0031		
Friday	0.058 ± 0.0031	7.6961	0.0127
Control	0.068 ± 0.0086		
Tuesday	0.071 ± 0.028	9.4337	0.0076
Control	0.068 ± 0.0086		

Table 9 (cont.)

	Experiment 2		
Behavioral Measure	**Condition 2** **(0.27G DC; 0.72G, 60Hz)**		
DRL (pellet/min)	**Mean ± SD**	**F**	**P**
Tuesday	1.223 ± 0.0336	11.9189	0.0041
Friday	1.374± 0.1160		
Friday	1.374± 0.1160	22.6015	0.0007
Control	1.311± 0.0244		
Tuesday	1.223 ± 0.0336	1.8963	0.2279
Control	1.311± 0.0244		

A reasonable interpretation of the large number of statistically significant comparisons is that they indicate transduction of the electromagnetic energy resulting in changes in operant behavior. The study was therefore consistent with the earlier study that it was intended to replicate, as determined on the basis of the F test. See Thomas, J.R., Schrot, J. and Liboff, A.R.: Low-intensity magnetic fields alter operant behavior in rats, *Bioelectromagnetics* 7:349–357, 1986.

It is unnecessary to labor further regarding the point that intra-experimental-design inconsistency in electromagnetic energy bioeffects studies is an artifact that results from differences between investigators regarding hypotheses, purposes, and plans to evaluate data. When apparently inconsistent studies were evaluated on a common basis, the inconsistencies disappeared. This was the result in each instance of apparent inconsistency that I analyzed. I expect that, ultimately, some exceptions will be identified, but it is difficult to imagine that they would amount to anything other than exceptions to the general rule. It can be concluded, therefore, that despite differences in models and statistical methods that were chosen and utilized by particular investigators in particular studies, the bottom line is that there is clear and convincing evidence that powerline electromagnetic energy was consistently detected by the various biological systems that were studied. It is simply not possible to gloss over the existence of this consistency.

Reproducibility of Nonlinear Phenomena

The conflict that Saffer and Thurston claimed was created by their results in relation to Goodman's results was apparent, not real, because it could be explained by taking into account the investigators' reasoning. The actual changes observed depended on the ionic composition of the solutions used, the temperature, the pH, the presence or absence of trace amounts of contaminants in the solution, the passage number of the cells, as well as many other factors, in addition to the field of 57 mG for 20 minutes. It is impossible to reproduce these conditions, and consequently it is impossible to reproduce specific changes in the average amount of expressed message. The same reasoning explains all the other cases of apparent inconsistency.

In general, the inability to precisely reproduce all conditions that can impact the biological system under study may or may not be a significant concern. If the phenomenon under study can be adequately explained on the basis of a linear model, then the consequences of the inability to precisely duplicate the laboratory conditions will be unimportant as long as the contribution to the variance in the dependent parameter due to the uncontrolled variables is less than the magnitude of the consistent effect caused by the independent variable. In this case, it is possible to replicate data between laboratories because the consequences of the differences between the laboratories are immaterial. But the situation is quite different if the linear model is not applicable, as in the case of powerline electromagnetic energy bioeffects. In this case, small differences between conditions in different laboratories can have disproportionately large consequences. Because it is impossible to reproduce these conditions, it is impossible to reproduce the data.

One can decide that a nonlinear model is needed whenever intra-experimental-design inconsistencies inferred on the basis of a linear model can be resolved by eliminating the assumption of linearity. The consistency that is required to rationalize a judgment that a phenomenon exists is consistency in observation of the phenomenon, not consistency in the measurement of data (which is impossible for nonlinear phenomena).

Allowing the possibility that powerline electromagnetic energy bioeffects can be nonlinear does not entail that no electromagnetic energy effects are linear. In other words, evidence of a nonlinear effect under one set of circumstances is not evidence against linearity under other circumstances. The best way to understand *nonlinear* is as the most inclusive term describing physical or biological systems. *Nonlinear* therefore includes *linear*, and *linear* is seen as a special case. For example, a pendulum is a nonlinear physical system that can be modeled as a linear system for situations involving small angular displacements.

As we have seen, the need for a nonlinear model can sometimes be manifested by employing statistical tests that involve comparisons of average values, but without the assumption of consistency in the average (which is equivalent to assuming a linear model). In other cases, applicability of the nonlinear model is manifested by employing statistical tests that involve comparisons of variance. In either case, if the underlying study hypothesis is accepted (null hypothesis rejected), then occurrence of detection of the electromagnetic energy can be inferred. Because either statistic can be used to rationalize detection, the most sensitive experimental hypothesis ought to include them both, with appropriate protection against family-wise statistical error.

Biological Generalizations Generally

The human-health implications of the fact that powerline electromagnetic energy can be detected by the body must be judged. That means all the evidence must be evaluated in some way according to some standard, because biological generalizations always require a framework of methods and standards. In this section I will show it is generally true that opinion, purpose, and values are important at this level of biological reasoning. In the next section, I will show that this is particularly true of the judgment regarding powerline electromagnetic energy health hazards.

Two hypothetical examples are sufficient to show the importance of subjective considerations in the formation of biological generalizations (62). First, consider the conclusion that decreased cyclin-E/CDK2 activity (Chapter 2, Table 1) causes loss of anchorage, which the authors suggested was generally true, based on their observations in KD cells. Assume that another group performed a similar study using XYZ cells, but did not find such a relationship. Is the abductive generalization suggested by the original authors now less reliable? If replicability were required, then the failure to confirm the initial results would cast doubt on their reliability. But failure to find something is not necessarily good evidence that the thing sought does not exist. Thus the hypothetical second report would not have proved that the phenomenon does not exist generally, just as it was the case that the first study did not prove that it does.

In practice, the attitude adopted toward such a mixed state of the evidence usually depends on the interests of the person or group deciding the significance of the mixed results. An author of a review article might hedge a decision (“the data is conflicting, and no firm conclusion is possible”). But there will be others who must take a position, perhaps because one conclusion or the other would materially influence the design of their experiments. Ordinarily, in resolving the question, many factors would be considered including known or suspected properties of the cells, degree of respect for the investigators, the reputation of their laboratories, whether the laboratories were in industry or academia, the track record of the investigators, insider information, style of presentation of the results, the relative prestige of the investigators’ institutions, and perhaps even the nationality of the investigators. The point is that, in the face of mixed results, which is commonly the case, the cognitive value of the scientific evidence in a particular area depends on who is evaluating it, why he is doing so, and how he does it. There is no necessarily right or wrong means of performing these analyses.

As another example of the role of judgment in forming biological generalizations, consider the conclusion that vigilance caused an increase in brain blood flow (Chapter 2, Table 1). Assume that exactly the same change in blood flow occurred when subjects were exposed to powerline electromagnetic energy. To avoid the difficulty of mixed results that was just discussed, assume further that the study was replicated many times, and always with the same result. Would such evidence indicate that powerline electromagnetic energy would affect human health? Because a change in blood flow accompanies every cognitive act and every sensation, it could be argued that changes in brain blood flow caused by electromagnetic energy were normal physiological responses, and thus not hazardous. On the other hand, a change in blood flow also accompanies every pathological change and perhaps the rule should be that it would be better to err on the side of caution and tentatively regard the exposure as a hazard, at least in the cases where the exposure is involuntary. Thus two opposite conclusions are possible on these facts and again, the validity of the scientific inference depends on the reasoning principle chosen.

It can be seen that formation of scientific generalizations in the biological thought-style generally involves non-empirical elements, including opinion, purpose, and values. These elements are outcome-dispositive principles, and they cannot be chosen scientifically. Individual scientists differ in education, perspective, attitude, approach, experience, integrity, and ethical orientation. Disagreements can therefore be expected regarding how the biological thought-style ought to be implemented in a given case, for example, that of assessing whether it is a scientific fact that powerline electromagnetic energy affects human health.

The Generalization About Whether Powerline Electromagnetic Energy Affects Human Health

Suppose that a group of scientists were identified who shared a common set of scientific reasoning principles that, for example, included how certain kinds of measurements and observations should be made, how the data should be analyzed, assumptions deemed to be reasonable, and general laws. The principles provide a group with a frame of reference for deciding what should be accepted as scientific fact. When a group of scientists commonly accept a particular set of principles, I shall refer to them as a *thought-group*. Members of different thought groups have different opinions regarding the relative truth status of different statements (Figure 3). Thought-groups may be large such as the groups consisting of radiation biologists, immunologists, microbiologists, or biochemists, or they may be small such as National Institutes of Health study sections or blue-ribbon committees charged to decide whether powerline electromagnetic energy affects human health.

The investigators who performed electromagnetic energy studies while employed at Battelle comprise a thought-group regarding electromagnetic energy biology, because perusal of their estimated 500 electromagnetic energy publications and presentations indicates that they have a shared set of non-empirical principles. For example, they think that animal studies can be used to discern the existence of health risks to human beings. They think that mathematical modeling of electromagnetic energy-animal interactions can help determine the extent to which electromagnetic energy may be a health risk. They think that whether or not electromagnetic energy is presently recognizable as a health risk cannot presently be adequately assessed, and that therefore more research is needed. They regard the occurrence of linear dose-response relationships as an important relationship in ascertaining whether electromagnetic energy effects in animal are real. These principles do not exhaust the shared reasoning principles among the Battelle investigators. They do indicate, however, that the Battelle investigators can be considered as a thought-group. No Battelle investigator has publicly opined that powerline electromagnetic energy affects human health. It is reasonable to infer that this result is a consequence of the particular principles that are shared by the group. Others who did not share these principles might not agree with the result. Any ad hoc committee that interacts for the purpose of forming collective opinions necessarily defines a thought-group. For example, the experts chosen by the NIEHS to write a draft report for the NIEHS Working Group constituted such a group (Table 10).

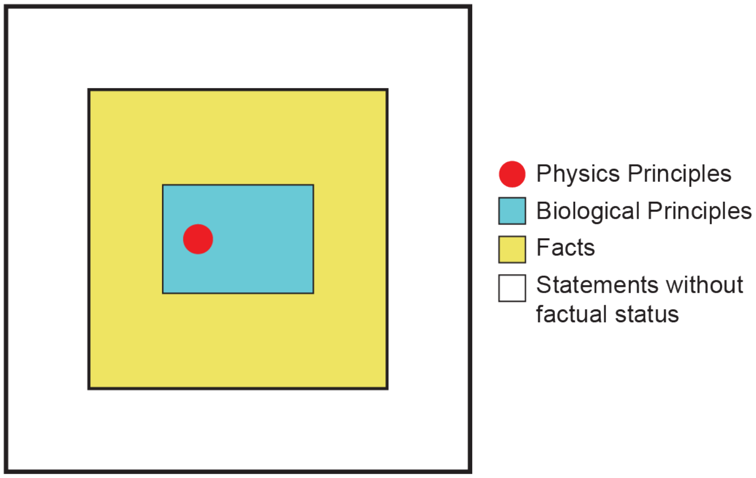

Figure 3.Relationships Among Scientific Principles and Facts

It would be improbable for the reasoning principles accepted by the NIEHS group to be identical to those of the Battelle investigators. Perhaps it is the case, for example, that the NIEHS group would require a different degree of certainty than would the Battelle investigators in assessing whether a given series of biological observations could properly be interpreted to indicate that powerline electromagnetic energy affects human health. Identifying differences in principles is possible, but that is not the point here. I want only to indicate that it is likely that some pertinent reasoning principles differ between the NIEHS and Battelle groups. If so, then the two groups will not agree on the factual status of some statements (see Figure 4). Whether or not powerline electromagnetic energy affects human health could be one such point of disagreement, depending on the consensus of principles adopted by the NIEHS committee. It is important to recognize that such a disagreement would not be based on data or measurements or observations, but rather on how the information was interpreted in the light of the axioms adopted.

Table 10. The investigators and their designated area of expertise (designated by NIEHS) are:

Larry Anderson, Ph.D., Staff Scientist—Group Manager, Battelle Pacific Northwest National Laboratories (*in vivo* cancer studies)
Gregory Blumenthal, Ph.D., Research Fellow, NIEHS Laboratory of Computational Biology and Risk Analysis (*in vivo* noncancer studies: neuroendocrine)
Joseph Bowman, Ph.D., National Institute of Occupational Safety and Health (epidemiologic studies on occupational exposure)
Elisabeth Cardis, Ph.D., International Agency for Research on Cancer (epidemiologic residential adult studies)
Charles Graham, Ph.D., Senior Advisor for Life Sciences, Midwest Research Institute (clinical human laboratory studies)
Richard Luben, Ph.D., University of California at Riverside (*in vitro* studies, excluding differentiation)
Kenneth McLeod, Ph.D., Associate Professor, SUNY at Stony Brook, Musculo-Skeletal Research Lab (*in vitro* studies: cell differentiation)
Mat-Olof Mattsson, Ph.D., Associate Professor, Dept. of Cellular and Developmental Biology, Umea University, Sweden (molecular biology studies)
James Morris, Ph.D., Staff Scientist, Battelle Pacific Northwest National Laboratories (*in vivo* noncancer studies: immunotoxicity, hematology, reproduction and development)
Charles Polk, Ph.D., Professor Emeritus, Dept. of Electrical and Computer Engineering, University of Rhode Island (theoretical mechanistic studies)
Walter Rogers, Ph.D., Associate Professor of Environmental Science, University of Texas School of Public Health (*in vivo* noncancer studies: neurobiology and neurobehavior)
Claire Sherman, Ph.D., Radiation Effects Research Foundation (epidemiologic residential childhood studies)
Michael Yost, Ph.D., University of Washington (exposure characterization studies)

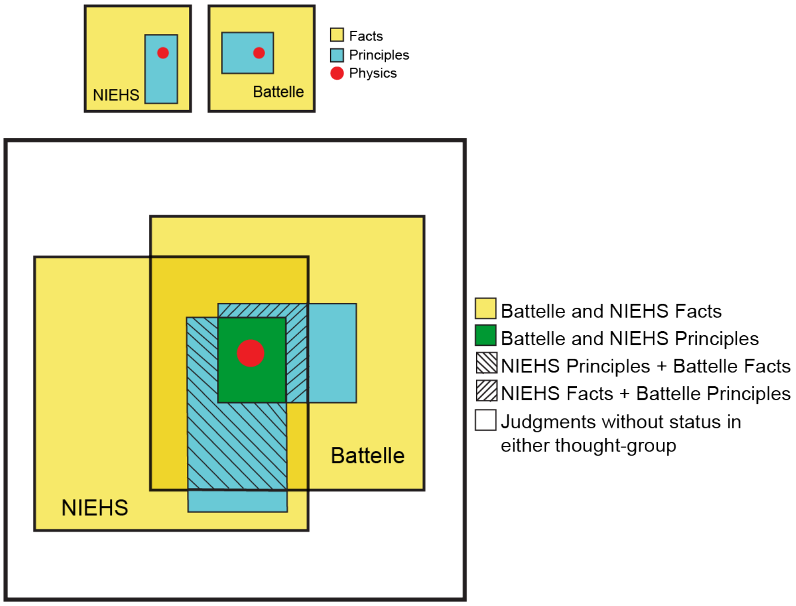

Figure 4 Disagreement between thought groups regarding the truth status of particular statements.

In some instances, thought-groups are sharply defined because they were explicitly assembled on the basis of homogeneity of thought regarding a particular conclusion. Such was the case, for example, with the two groups of scientists who testified in a court proceedings in New York regarding whether powerline electromagnetic energy affects human health (Table 11). There was essentially no intra-group disagreement regarding the ultimate issue, but complete inter-group disagreement regarding it. The reason for the disagreement was the adoption by the two groups of materially different reasoning principles in evaluating the scientific data. Watson's group, for example, emphasized the absence of conclusive evidence, and the absence of known mechanisms, and the inability of Battelle investigators to replicate some biological effects reported by others. The landowners' witnesses, on the other hand, did not require that the evidence be conclusive, and largely rejected as irrelevant many of the concerns of Watson's experts. As a consequence of their choices, the two groups flatly disagreed regarding whether powerline electromagnetic energy affects human health.

Table 11. Scientists who testified under oath regarding their opinion whether powerline EMFs affect human health. The group that represented the Landowners answered the question affirmatively. The group sponsored by Watson on behalf of the power company answered in the negative.

Landowner Group	**Watson Group**
Dr. Harris Busch, Baylor College of Medicine, Houston, Texas	Dr. Stuart Aaronson, National Cancer Institute, Bethesda, Maryland
Dr. Andrew Marino, LSU Medical Center, Shreveport, Louisiana	Dr. Richard Bockman, Memorial Sloan-Kettering Cancer Center, New York, New York
Dr. Jerry Phillips, Cancer Therapy and Research Center, San Antonio, Texas	Dr. Roswell Boutwell, University of Wisconsin, Madison, Wisconsin
Dr. Lennart Tomenius, Stockholm, Sweden	Dr. Edmund Egan II, University of Buffalo, Buffalo, New York
	Dr. Lucius Sinks, National Cancer Institute, Bethesda, Maryland
	Dr. Herbert Terrace, Columbia University, New York, New York
	Dr. Margaret Tucker, National Cancer Institute, Bethesda, Maryland
	Dr. Ken Zaner, Harvard Medical School, Cambridge, Massachusetts

A fourth example of a biological thought-group is provided by the Radiation Study Section of the National Institutes of Health. The hostility of this panel (and its predecessor) towards research proposals involving the study of nonionizing radiation is legendary in the electromagnetic energy community. The attitude of the Radiation Study Section, however, is entirely consistent with the principles of radiological science espoused by the type of expert normally appointed to the panel.

The reasoning principles of radiation-panel experts can be inferred by considering the critiques they provided me regarding powerline electromagnetic energy proposals that I submitted (63). Perusal of the critiques makes it clear, I think, that the radiation-panel experts have empirical reasoning principles that result in highly skeptical opinions regarding the existence and importance of electromagnetic energy-induced bioeffects. It is unthinkable that the Radiation Study Section would accept the statement *powerline electromagnetic energy affects human* *health* as a scientific fact. The important point is that this result is a consequence of the opinions and values of the members of the Radiation Study Section thought-group, and does not follow in any scientific fashion from the biological evidence. The validity of their decisions is based on legal principles (they were duly appointed by somebody at the National Institutes of Health), not on scientific principles (there is no reason to believe that their opinions are objectively correct, or broadly acceptable to non-radiological scientists).

This analysis shows that a group judgment regarding whether powerline electromagnetic energy affects human health depends strictly on the opinions, purposes, and values that are commonly held by its members. Different groups hold different principles and, consequently, can be expected to make different judgments.

Rendering Unto Caesar

As best I can tell, there is no serious dispute (or no serious basis for a dispute) regarding the single most important scientific fact pertinent to deciding whether or not powerline electromagnetic energy affects human health. The important, resolved issue is that biological effects caused by electromagnetic fields of the type produced by powerlines actually exist. These effects are real. In the 1970s, this view was accepted by only a handful of scientists when the evidence for it was first marshaled by me. Today, however, it is the overwhelmingly dominant view among knowledgeable experts, and it is not possible to find a modern rational analysis that leads to a contrary conclusion.

The conclusion that biological effects due to electromagnetic fields actually exist is pivotal in the analysis of potential health hazards, and I hope the reader appreciates its significance. Were it the case that electromagnetic energy bioeffects did not exist, then all of electromagnetic energy biology would be a chimera having no meaning or significance within the framework of science. An assertion that powerline electromagnetic energy affects human health would, in that case, be entirely vacuous. On the other hand, because the available evidence clearly shows that electromagnetic energy bioeffects do exist, it is as certain as anything in science can be that there exists a mechanism within the body that is capable of detecting and transducing electromagnetic fields into the language of biology — electrical changes in the nervous system, enzymatic activity, and protein expression.

The existence of electromagnetic energy bioeffects and their necessary implication regarding mechanisms give rise to different kinds of issues. There exist scientific issues, which are in the domain of scientists. There also exist non-scientific issues which are properly in the domain of the layman (which, whether for reasons of arrogance or ignorance, have frequently been addressed by scientists).

It is a scientific issue whether electromagnetic energy-induced changes actually occurred in cells or animals in particular studies. The further question regarding the choice of the model that best fits the data is also a question properly addressed by scientists. Elucidation of the biophysical principles that explain how the body detects powerline electromagnetic energy probably constitutes the most fundamental and difficult challenge to scientists. The rewards to humanity if we choose to fund an effort to meet this challenge are potentially great because we would gain information about ourselves, about how we work, as opposed to information about the nature of the planet or the structure of subatomic particles, as was obtained in other massive government science programs.

But the immediate question is not whether we have the political will to expend the money necessary to understand the electrical structure of our bodies. The question is what implications can properly be drawn from the presently available data regarding whether powerline electromagnetic energy affects human health. This question is not a scientific question because it cannot possibly be answered on the basis of laboratory data alone. It can be answered only on the basis of laboratory data and a set of rules that instruct the decision-maker regarding how and under what conditions the answer ought to be obtained. These rules are an indispensable aspect of generalizing from the biological data to make decisions about electromagnetic energy and public health. We need consider the situation involving only one rule, to understand the necessity of rules.

At least five qualitatively different standards for evaluating the evidence can be delineated. One possibility is that the evidence must be conclusive before the existence of a public-health risk is accepted. *Conclusive* would correspond to a standard such as *beyond a reasonable doubt*, or more than 99% certain. The typical scientific standard of 95% is another possibility. Perhaps the standard should be *clear and convincing* (75%) or a *preponderance* of the evidence (51%). Finally, it could be argued that a decision regarding whether powerline electromagnetic energy will affect human health should be made on the basis of an evaluation of the evidence in which the question is answered affirmatively if the evidence shows that such an effect is reasonably possible, say 25%.

My personal view is that 51% is certainly enough, and 25% may be enough. Others, I know, disagree profoundly with this opinion. Proponents of >99%, 95% and 75% can probably be identified. But whether you agree or disagree with my opinion that the standard should be at most 51%, it should be recognized that the choice is a sociological question not a scientific question. It is not the laws of science that dictate that the degree of certainty should be this percentage or that percentage, but rather it is the opinion of the larger society that properly sets the applicable norm (64).

I think it is clear that before deciding the substantive issue regarding whether powerline electromagnetic energy affects human health it is first necessary to decide what the rules of decision-making shall be. It is similarly clear that the choice of the applicable rules rests not with the narrow constituency of scientists, but with the larger society.

The Proper Choice

The fact that the biological evidence consistently shows that powerline electromagnetic energy are detected (65) by the body raises the possibility that powerline electromagnetic energy affects human health. Whether this inference is acceptable is a sociological question not a scientific question because it can be resolved only by incorporating societal values, not by performing scientific studies. The essential societal value I would incorporate is the prohibition against involuntary human experimentation (66). The consequences of an erroneous decision are truly significant for the people who are involuntarily exposed to powerline electromagnetic energy, but relatively insignificant for the power companies. My personal sympathies lie with the involuntarily exposed resident along the powerline right-of-way, rather than with the power companies and their shareholders who would ultimately be required to pay the higher costs needed to design and build safer powerlines. I would therefore opt to protect the individual, rather than the power company or the aggregate of society. On this basis I would accept no higher than 51% certainty as sufficient. I think the scientific evidence meets this standard.

Summary

The biological studies consistently show that powerline electromagnetic energy can be detected by exposed subjects. For this reason alone, powerline electromagnetic energy should be presumed to affect human health

5. EPIDEMIOLOGY AND POWERLINE ELECTROMAGNETIC ENERY HEALTH RISKS

Introduction

Historically, the methods and procedures of epidemiology have worked well in identifying and characterizing health risks due to infectious agents. Epidemiology has also successfully identified risks due to some non-infectious agents, including the links between smoking and lung cancer and between thalidomide and birth defects.

The first epidemiological study that considered the possible health menace of powerline electromagnetic energy was performed by Dr. Becker in the early 1970s (67). He found an association between environmental electromagnetic energy and cancer, and interpreted it to generally support the stressor hypothesis regarding the mechanism of action of electromagnetic energy. Subsequently, many hundreds of studies were performed and interpreted to support many different opinions concerning the health menace of powerline electromagnetic energy. In this Section, I will describe how the electromagnetic energy epidemiological studies were performed and evaluated. I will show that the scientific meaning and public-health significance of the electromagnetic energy epidemiological studies depends entirely on the evaluative criteria utilized to individually and globally assess the studies.

Clinical Study Standards: Randomization

I served on the Institutional Review Board (IRB) of the LSU Medical School for many years, including 5 years as Chairman. During that time, I read many applications for IRB for permission to conduct human experimentation. Although the purposes of the studies varied, most were clinical studies aimed at determining whether a particular drug or device was effective in treating a particular disease. Typically, the plan of study was approved by the Food and Drug Administration which stipulated that if the study was performed as proposed, and if the data obtained was as expected, then the existence of a cause-effect relationship between the study drug or device and an improvement in the disease could validly be inferred.

A fundamental aspect of the studies approved by the IRB was the use of randomization of study subjects to treatment or control groups. Statistical methods used to evaluate the data were based on the assumption of randomization, and the conclusion of a cause-effect relationship was based on the statistical evaluation. In contrast to clinical studies, electromagnetic energy epidemiological studies never used randomization of subjects because a randomized trial to assess whether electromagnetic energy affects human health is ethically impermissible.

The lack of randomization in electromagnetic energy epidemiological studies had serious consequences with regard to what could validly be inferred. For example, suppose that the risk for cancer in an electromagnetic energy-exposed group was found to be greater than the risk in the control group. The salient question would then be whether the association of increased risk with electromagnetic energy exposure was a cause-effect relationship, or was a mere association such as that between stock-market prices and hemlines. In the absence of randomization, it is impossible to have reasonable assurance that no factor was associated with both electromagnetic energy exposure and cancer, and that this factor, not the electromagnetic energy, was the true cause of the disease. If that were the case, then the correlation between electromagnetic energy and the disease could not validly be interpreted to indicate a cause-effect relationship. Because there are always many such potential causes, an observed increased risk in an epidemiological study could equally be explained as the result of an uncontrolled factor (68). Similarly, it is always possible that a finding of no increased risk could be equally explained by a failure to control for a pertinent risk factor. It follows that every electromagnetic energy epidemiological study is intrinsically inconclusive to some degree. It is a matter of human judgment whether the degree of uncertainty in particular studies or groups of studies is sufficient to warrant a particular conclusion. Reasonable people may differ regarding this judgment (69).

Other Clinical Study Standards

Aspects of clinical study designs other than randomization also contribute to their reliability. Many of these design features are possible in epidemiological studies, but they have rarely been incorporated into the design of electromagnetic energy epidemiological studies.

Two of the missing features are particularly important. First, every approved clinical study has an experimental hypothesis. Usually, the hypothesis is that a particular drug, device, or surgical procedure will be more efficacious than a suitable control, and the purpose of the study is to evaluate the hypothesis. The hypothesis is stated before the data is analyzed and is usually based on laboratory results that provide some basis for concluding that the study has merit and is worth the risk of exposing human beings to novel situations. A statistical test closely associated with the experimental hypothesis is used to objectively assess whether or not the experimental hypothesis was supported. In the absence of a hypothesis of some kind, one could have no confidence that statistical associations found in the data after it was collected were causal. They could be, but there is simply no basis for deciding.

Second, in a clinical study the drug is administered only to the patients in one of the two study groups. The second group, the controls, receive the same degree of attention, but they do not receive the study drug, and consequently can serve as the basis for evaluating its effect. Further, the dose of the drug is recorded so that it is possible to identify which patients received the drug, and how much they received. If the investigator could not determine who did and did not receive treatment, how much treatment was received, and whether treatments other than the study treatment were administered, then assessment of the effect of the drug would be impossible.

As discussed below, these routine and fundamental features of clinical studies are absent in electromagnetic energy epidemiology studies.

Epidemiological Studies

Epidemiological studies are traditionally divided into three general groups based on the timing of the identification of the case and control subjects. If the cases (subjects with the disease of interest) are identified prior to the control subjects, then the statistical comparison will involve a determination of whether the cases have a greater risk of exposure, and the approach is a *case-control* study. If subjects having or not having the exposure are identified first and then followed to determine the incidence of disease, the procedure is a *cohort* study (a metaphorical reference to a Roman military cohort which always moved forward, never backward). If the cases, controls, and exposures are identified at the same time (such as in the analysis of a list of persons who died from various causes subdivided into occupations), the procedure is a *cross-sectional* study. In this report, the focus is on epidemiological methodology itself, rather than on the less important issue of the implications of differences in epidemiological designs. Consequently, the studies are discussed without regard to the particular epidemiological design employed.

Absence of Hypotheses in Electromagnetic Energy Epidemiological Studies

In a study by Wertheimer and Leeper (WL), the cases were children who died with cancer, and the controls were normal children identified from birth certificates. The relationship between various predetermined classes of powerlines and the birth and death residences of the two groups was determined, and more than the expected number of cancer cases occurred among the subjects who lived near the powerlines.

For reasons never made clear, WL decided that the aspect of powerlines that might be linked with human disease was the magnetic field. Nothing prior to their study reasonably suggested that the magnetic field might be an etiologic agent, and in fact most animal studies had been performed using electric fields. Nevertheless, they chose magnetic fields for study, and constructed a coding system for identifying whether particular powerlines did or did not give rise to magnetic fields at the residences of the study subjects (WL wire codes). Subsequently, evidence of the validity of the WL codes as a surrogate for electromagnetic energy exposure was provided by measurements showing a relation between field strength and the coding system (70).

WL never explained why they chose to study cancer in relationship to magnetic fields rather than say diabetes or arthritis or mental retardation. Because there was no study hypothesis, no basis for studying magnetic fields, and no reason to choose cancer as an endpoint, it seems fair to characterize the WL study as the investigation of a subject (potential association of magnetic field exposure coded by a particular visual identification system, with the occurrence of childhood cancer), rather than a scientific study to test a specific hypothesis. They had an obvious interest in and aptitude for their subject, and because they paid for the study themselves, they were not required to justify its design or rationale to anyone.

The cause-effect relationship suggested by the association found by WL has great public-health significance because, despite an unprecedented degree of attention by the power companies who commissioned many similar studies, the apparent correlation discovered by WL has continued to stand up. But the absence of a hypothesis — whether or not justified under the circumstances that prevailed in 1979 — led to numerous subsequent electromagnetic energy epidemiological studies that also had no hypothesis. The resulting confusion significantly obscured the landmark status of the WL coding system and the public-health implications of their findings.

For example, in their next study they chose a control group that contained dead subjects (71). Again, they stated no explicit hypothesis but the hypothesis actually tested by the statistics they employed was whether electromagnetic energy exposure was more likely among people who died from cancer compared with a mixed group of controls, some of whom died from diseases other than cancer. The assumption cannot be made that the controls provided an unbiased estimate of the prevalence of electromagnetic energy exposure among the general population (which might be a reasonable assumption for a normal control group, as they used in their first study). Thus, the implicit hypothesis in the two WL studies are different, and possibly inconsistent.

In a subsequent study based in Seattle, no significant relation between acute non-lymphocytic leukemia and electromagnetic energy was observed (71). Although the authors used the WL wire codes for identifying electromagnetic energy exposure, the choice of non-lymphocytic leukemia as an endpoint was arbitrary and unjustified by any prior work. The authors seemed to suggest that there was some relationship between their study and those of WL in the sense that a statistical association in the Seattle study would have strengthened acceptance of a causal association in the WL study. It is difficult to understand why they thought that should be the case. Although WL never recognized it, their choices of all cancer as the endpoint and a normal control group (in their first study) was the ideal design to test the stressor theory of electromagnetic energy-induced disease. On the other hand, there was no rationale whatever for the investigators in the Seattle study to limit the study to a particular histological sub-type of cancer.

In a study based in Rhode Island, a unique coding system for identifying the presence of magnetic fields was used, and no link with childhood leukemia was found (71). The authors seemed to say that their study was pertinent to the WL study, though the chosen endpoint was childhood leukemia, not childhood cancer as in the first WL study. The authors of the Rhode Island study were clearly impressed that WL found a statistical association between childhood leukemia and wire codes when they searched through their data. But this association was not a planned comparison by WL, and therefore could not be used to conclude that magnetic fields and childhood leukemia were associated. It is always possible to rummage through data already collected to find unplanned statistical associations. The implicit hypothesis of the Rhode Island study seems, therefore, to have been related to an impermissible inference from the original WL study. It is difficult to be certain, however, because the authors of the Rhode Island study stated no hypothesis.

In a Los Angeles study, childhood leukemia was considered in relation to magnetic fields as indexed by the WL codes, 24-hour measurements, and spot measurements (71). An association with magnetic fields as indexed by the codes was observed, but not as indexed by the other surrogates. Because there was no hypothesis, the study seems best characterized as a historical narrative in which the author described a series of actions that led to various kinds of data, followed by an unplanned pattern of statistical analysis of the data followed by the expression of opinions regarding the meaning of the data.

In a study involving children who lived in Stockholm, Sweden, the cases were subjects who had either benign or malignant tumors, and controls were chosen from birth records (71). The magnetic field at each residence was measured and a unique system for coding for the presence of electromagnetic energy from powerlines and other sources was used to examine for possible statistical associations. As might be expected, some associations were positive and others were not. Thus it is possible to argue inconsistently regarding the implications of this study, based on which statistical associations are given credence. Since none of them were specifically planned, within the context of the study, there is no clearly correct choice.

In another series of studies, dead or diseased subjects were used as controls (71). Consequently, it is even more difficult to identify a plausible study hypothesis. The results were as follows.

- Subjects with lymphomas or leukemias matched with patients recently discharged from hospitals showed no association with electromagnetic energy exposure as indexed by residing within 50 m of a powerline.

- Leukemia cases were not affected by electromagnetic energy (defined as residence near transformers) compared with patients having other forms of cancer.
- Patients with leukemia were about 4 times more likely to have occupational exposure to electromagnetic energy, compared with subjects that had diseases other than leukemia.
- Occupationally exposed men were more likely to have leukemia than other forms of cancer.
- The risk among electrical workers of dying in England and Wales with acute myelogenous leukemia was elevated, compared with the risk of dying from other causes.
- The risk of dying from brain cancer among workers in 15 electrical occupations was greater than dying from other causes.
- The risk of dying with brain cancer was greater among white males with occupational exposure to electromagnetic energy, compared with the risk of dying without cancer.
- White men who were ever exposed to electromagnetic energy had a higher risk of brain cancer, compared with men who died from other causes.

What would be the possible inferences from these studies, even assuming that hypotheses had been stated? If the electromagnetic energy subjects had a particular type of cancer, say leukemia, and the control subjects had non-leukemia cancer, then the idea actually tested in a statistical analysis would be whether electromagnetic energy exposure was more likely among leukemia subjects, compared with subjects who died with another form of cancer. But it is hard to make sense of this comparison because the assumption cannot be made that the subjects who developed non-leukemic cancer provided an unbiased estimate of the prevalence of electromagnetic energy exposure among the non-diseased population. This is particularly true because the only plausible biological hypothesis yet proposed to explain the link between powerlines and human disease, namely the stressor hypothesis, suggests that any diseased control group will contain a higher proportion of electromagnetic energy-exposed subjects, compared with healthy subjects. Because the estimate of risk in an epidemiological study involves comparisons of risks between the cases and controls, the use of a disease control group can (and probably does) lead to an underestimation of the risk of electromagnetic energy exposure in the healthy population.

Epidemiological studies that employed a cross-sectional design constitute another group of non-hypothesis electromagnetic energy epidemiological studies whose theoretically possible hypotheses seem irrelevant if the goal is to reasonably estimate human health risks due to electromagnetic energy (72).

Misclassification

In any plan to assess a hypothetical cause-effect relationship it is necessary to distinguish between those who did or did not receive the electromagnetic energy exposure, to determine how much electromagnetic energy exposure was received, and to determine who received other potentially important exposures. None of these goals were achieved in any electromagnetic energy epidemiological studies. The question whether there were one or more studies where these goals were achieved sufficiently to warrant use of the studies in public-health planning is unresolved because there is nothing even resembling agreement regarding how close is close enough.

In a study in Stockholm, Sweden, for example, the investigators considered distances as great as 150 m to be within the zone of influence of powerline equipment. Not surprisingly, the mean field strength at the residences labeled as exposed was the same as that at the control residences. In an English study, persons who lived within 15 m of a transformer (M.E. McDowall: *Br. J. Cancer* 53:371:1986) were classified as exposed even though transformer fields do not extend that far. The control subjects in the study were also misclassified because not living within 15 m of a transformer in England is not a good surrogate for non-exposure because most English powerlines are underground. In a Rhode Island study, occurrence of electromagnetic energy exposure was predicated on the basis of mathematical calculations that seem hopelessly uncertain (71). In another English study, the surrogate for electromagnetic energy exposure was so bizarre that less than 1% of the study subjects were exposed (71).

Regrettably, the later epidemiological studies have essentially the same shortcomings in design as the epidemiological studies done 10–20 years ago; consequently the later studies are no more probative. For example, Linet and her colleagues examined the relationship between powerline electromagnetic energy and acute lymphocytic leukemia in children, and concluded that the study results “provide little evidence” of a link (73). But the authors gave no hint of what they meant by “little” or whether the evidence, even though it was “little,” was enough to, for example, warrant mandatory rules or governmental warnings about whether families with small children should live beside powerlines. Further, the Linet study had no hypothesis, and consequently the data analysis was arbitrary. The authors chose 2 mG as the dividing line between exposed and non-exposed subjects, and this made the results of the study negative. If 3 mG were chosen, however, the results would be positive.

Asymmetry in the degree of effort in classifying cases and controls also continues to occur. For example, an association between powerline electromagnetic energy and brain tumors in electric utility workers (P. Guenel, J. Nicolau, E. Inbernon, A. Chevalier and M. Goldberg: Exposure to 50-Hz electric field and incidence of leukemia, brain tumors, and other cancers among French utility workers, *Am. J. Epidemiol.* 144:1107-1121, 1996) was reported. The cases were identified on the basis of cancer diagnoses reported to the health insurance system, but the controls were matched simply on the basis of year of birth. Thus, the presumption was made that unless a subject was seen by a physician, diagnosed as having cancer, and reported to the health insurance system, then the subject did not have cancer for the purpose of this study. Consequently, every case is certain but every control is problematical.

Some problems regarding inferential limitation of electromagnetic energy epidemiological studies have actually worsened, occasioned by the development of computers and commercially available statistics software packages. In a study from Greece, (E. Petridou, D. Trichopoulos, A. Kravaritis, A. Pourtsidis, N. Dessypris, Y. Skalkidis, M. Kogevinas, M. Kalmanti, D. Koliouskas, H. Kosmidis, J.P. Panagiotou, F. Piperopoulou, F. Tzortzatou and V. Kalapothaki: Electrical power lines and childhood leukemia: a study from Greece, *Int. J. Cancer* 73:345-348, 1997) for example, 4 unvalidated surrogates for electromagnetic energy exposure were chosen and arbitrarily divided into 5 levels. The data was adjusted for 18 apparently irrelevant factors using the logistic equation, without explanation. The results of this complex design protocol are uninterpretable with reference to any identifiable standards of judgment.

Epidemiological Criteria for Causal Association

Because the electromagnetic energy epidemiological studies yielded statistical associations whose implications were problematical and significantly dependent on human judgment, criteria appropriate for use in evaluating the literature to reach an overall judgment must be delineated. These criteria ought to facilitate good or valid or generally acceptable opinions regarding the implications of the electromagnetic energy epidemiological literature. Unfortunately, the criteria often applied to evaluate the studies do not fulfill the obvious need for objectivity.

Koch and Hill

The difficulty in assessing the causative role of environmental factors in human disease is an old problem. More than a century ago Robert Koch, a German physician and microbiologist, recognized that a mere statistical association between two factors was insufficient to warrant a conclusion that the factors were causally associated, and he formulated several principles for use in assessing the veracity of apparent relationships in particular cases. His principles were formulated to facilitate evaluation of the role of microbes in diseases, because the environmental factors that were of interest to him were infectious agents.

Koch's general notion was that any claim that a particular microbial agent was responsible for a particular disease required that four criteria be satisfied. First, that the microbe occurs in every case of the disease. Second, that the microbe doesn't occur in other diseases. Third, that the microbe doesn't occur where there is no disease. Fourth, that the microbe can be isolated from a diseased subject, grown in culture, and used to induce the disease in a non-diseased subject.

Koch's criteria have proved durable and useful, but they are applicable only to infectious agents and they are insensitive. If the criteria are satisfied it can confidently be concluded that the microbial agent caused the disease, but the cause of the disease is left unresolved if the criteria are not satisfied.

In 1965, Austin Bradford Hill (1897–1991), an English medical statistician, published a set of criteria (Hill's criteria) that he suggested could serve to help evaluate the causal role of any environmental factor (A.B. Hill: The environment and disease: Association or causation? *Proc. R. Soc. Med.* 56:295–300, 1965). The criteria first appeared 11 years earlier in a little-known paper whose author listed them in an attempt to explain why he concluded that smoking and cancer were causally related (E. Wynder: Tobacco as a cause of lung cancer: With special reference to the infrequency of lung cancer among nonsmokers, *Penn. Med. J.* 57:1073–1083, 1954). Essentially the same criteria appeared again in 1964, and for the same reason, in the famous Surgeon General's report linking smoking and cancer (U.S. Public Health Service: Smoking and Health: Report of the Advisory Committee of the Surgeon General of the Public Health Service. Washington, DC: United States Department of Health, Education, and Welfare, PHS Publication No. 1103, 1964). Hill paraphrased those criteria in what the famous epidemiologist Abraham Lilienfeld considered to be more elegant language (A.M. Lilienfeld: The Surgeon General's "Epidemiologic criteria for causality": A criticism of Burch's critique, *J. Chronic Dis.* 36:837–845, 1983), and the criteria subsequently became best known as *Hill's criteria*.

Hill's first criterion involved the *magnitude of the statistical association* between an environmental factor and a disease, which is typically measured in epidemiological studies by the relative risk or odds ratio. Hill assumed, without any explicit justification, that a higher relative risk would imply more confidence in the causal role of the factor. It is difficult to see why this should be the case because the existence of a cause-effect relationship and the magnitude of the effect are independent concepts. Furthermore, observed statistical associations are affected by both the causal relationship and the presence of non-causal factors that introduce variance into a study. A low relative risk would be consistent with a high relative risk in the context of variance-inducing conditions, and with a true low relative risk in the case in which the variance was low.

Hill was obviously impressed by the high risks found in classic epidemiological cases including a risk of 200 for scrotal cancer in chimney sweeps, 30 for lung cancer in smokers, and 14 for death in the cholera epidemic of 1854 among customers supplied by the Southwark and Vauxhall Water Companies. Hill confused the concept of public-health significance, which is related to the magnitude of the effect, with the idea of causality which is not. It is not logical to regard the magnitude of the relative risk in an epidemiological study as probative of the existence of a cause-effect relationship.

Hill's second factor was *consistency of association*. The idea was that if the same or similar observations were made in studies by different investigators in different places at different times under different circumstances, the inference that the factor and the disease were causally related would be proportionately strengthened. No one can seriously quarrel with this idea in the case where consistency is observed. The real question, however, is what interpretation should be given to apparently inconsistent studies such as the electromagnetic energy epidemiological studies? The criterion of consistency of association cannot logically be accepted as necessary because it is entirely possible that a sought-after statistical association performed by different persons in different places and times under different circumstances should yield inconsistent results because there could be true causal associations in some of the studies but not in others. The criterion is therefore no help at all in evaluating the electromagnetic energy epidemiological literature.

Hill's third epidemiologic criterion for causal association was *specificity of association*, but even Hill recognized that this criterion was insignificant because there are essentially no instances of specific relationships between environmental factors and particular diseases since diseases may have more than one cause. Hill consequently conceded that specificity of association was only a sufficient not necessary factor in judging the existence of true cause-effect relationships.

Hill's fourth criterion was *temporality*, by which he meant that a factor cannot properly be regarded as a cause if it comes after the effect. The criterion, however, is trivial because it is part of the definition of *effect*.

Hill's fifth criterion was an assumption—the now familiar assumption of *linearity*. He argued that if more of a putative cause produced more of the effect, then one could have greater confidence in the reality of the cause-effect relationship. Again, as with the third criterion, we have a listing of a sufficient but not necessary factor.

Hill's sixth criterion was *plausibility*, but he never explained what he meant by that term. At least three possible meanings of plausible can be identified on the basis of the way the term is used generally. *Plausible* can mean that a mechanism can be suggested to account for a particular observation. For example, an observation that addition of a signaling agent to a group of cells causes the cells to make proteins can be viewed as plausible because a putative mechanism, namely interaction of the signaling agent with membrane-bound receptors leading to initiation of a second-messenger system, can be postulated. On the basis of this meaning of *plausible*, the link between powerline electromagnetic energy and cancer is plausible because the occurrence of a stressor reaction mediated by serum corticoids, leading to impaired immunosurveillance and increased risk of cancer, can be postulated.

Plausible can also mean that a mechanism can be suggested and evidence for the mechanism can be provided. This definition would be met if the membrane receptor in the example above was identified and shown to initiate a particular sequence of intercellular changes following interaction with its ligand. The link between powerline electromagnetic energy and cancer probably meets this definition of plausible because there exists evidence showing that electromagnetic energy can affect serum corticoids, immune parameters, and central nervous system activity.

Plausible can also mean that the mechanism of action linking the cause and effect must be supported by an extensive amount of evidence such that it can be concluded that the mechanism has been proved. Such would be the case in the example above, for example, if all the intermediary steps following the ligand-receptor interaction were specifically identified up to and including the mechanisms that resulted in secretion of the newly synthesized proteins. The link between electromagnetic energy and cancer cannot meet this definition of *plausible*.

Thus *plausible* can become (and has become in the case of electromagnetic energy studies) a code word indicating one's general attitude, rather than a concept that is useful in arriving at an attitude. In its general effect, the criterion creates a bias against novel ideas. For example, Semmelweiss' exhortation that Viennese medical students should wash their hands after dissecting cadavers prior to examining women on the maternity wards as a means of avoiding childbed fever was implausible, coming as it did prior to the work of Lister and Pasteur. Only after recognition of the germ theory and the development of antisepsis were any of the plausibility criteria satisfied (74).

Hill invoked a seventh criterion he called *coherence*, which was actually a degree of his plausibility criterion. Semmelweiss' theory, for example, was not plausible but it would have been extremely implausible if Semmelweiss' peers had already accepted the view that microbes did not cause disease. A cause-effect relationship is coherent, according to Hill, if it does not contradict established fact. Hill gave no examples of the operation of the coherence criterion, and its value as an independent consideration in evaluating electromagnetic energy epidemiological studies seems dubious.

Hill's eighth criterion involved *experimental manipulation*. If a statistical association between an environmental factor and a disease is observed, and, all other things being equal, one repeated the study but removed the environmental factor, would the occurrence of disease be altered? This, of course, is the classic definition of the method of experimental biology and it is the proper one to show the existence of a cause-effect relationship. But such a study is not what is ordinarily meant by an epidemiological study.

Hill's last criterion was *analogy*. Given that thalidomide causes birth defects, he said that we can accept less evidence that another drug could cause the same outcome. There seems to be no logical basis for this criterion and, insofar as I can tell, it has not been used by others to judge epidemiological data.

Thus, Hill's criteria are no help at all in evaluating the electromagnetic energy epidemiological literature. They have been employed to describe opinions about the public-health significance of electromagnetic energy epidemiological studies, but there is no case where Hill's criteria were used to justify or explain an opinion regarding the significance of the electromagnetic energy epidemiological studies.

Summary

The electromagnetic energy epidemiological studies have the surpassingly great benefit of providing information about the actual object of interest — human beings — rather than laboratory animals. However, epidemiological studies have significant inferential limitations that arise, ultimately, from the way the studies were performed. Epidemiologists can't do randomized, controlled studies to evaluate the impact of powerline electromagnetic energy on human health. This fundamental distinction from the way human clinical studies are done and from the way laboratory experiments are conducted, combined with cost factors and with the relaxed standards for experimental design that have been accepted by epidemiological journals, results in uncertainty that requires adoption of decisional rules capable of investing epidemiological data with meaning. Standing alone, the electromagnetic energy epidemiological data has no meaning.

What is needed is an evaluation of the methods and procedures of electromagnetic energy epidemiology, irrespective of the results in particular studies, and a determination whether the data from such studies will be deemed acceptable for forming judgments regarding whether powerline electromagnetic energy affects human health (75). Further, if the data is acceptable, the method whereby the inferences will be drawn must be specified. It is possible, for example, that a fair committee of electromagnetic energy experts might conclude (and justify) that no conceivable results of electromagnetic energy epidemiological studies are worth considering. Any such conclusion regarding the electromagnetic energy epidemiological studies would require examples of epidemiological studies that the committee would consider applicable to the problem of evaluating cause-effect relationships involving environmental factors (76). Then, future studies could be scrutinized to ascertain whether they contained the needed elements that were missing from the earlier studies. The scientific validity of the decision would be guaranteed because of the process by which the committee was chosen and by which it functioned.

As discussed in the previous Section, it seems quite reasonable to expect that scientists will decide scientific questions, and laymen will decide how scientific data is to be used in forming public policy. Conceptually at least, the two decisional levels are discrete. In contrast, with epidemiological studies, there is no such separation. The scientific and public-health considerations are inextricably commingled when epidemiological data is evaluated. For this reason, I think it would be inappropriate to attempt to evaluate the electromagnetic energy epidemiological data with regard to the issue whether powerline electromagnetic energy affects human health via a process that was restricted to scientists only.

Those charged with defining the requisite criteria should approach their task on a limited pragmatic basis, and not attempt to devise criteria for guiding all disputes and inquiries. Koch, for example, in formulating his criteria, dealt with a particular problem, namely infectious agents. Similarly, the experts responsible for the Surgeon General's report formulated criteria aimed at helping to resolve a particular problem, namely the link between smoking and lung cancer. In both instances, the authors explicitly recognized that the proposed solution related to a particular problem, and did not necessarily encapsulate a philosophical approach applicable to all problems in scientific reasoning (77). It is possible, of course, that reasoning principles elucidated as an explanation and justification for why and how the electromagnetic energy epidemiological literature should be viewed will be relevant to other potential epidemiological issues, but that possibility remains to be determined, case by case.

Only when decisional criteria are established will it be possible to cut the present Gordian knot of controversy regarding the epidemiological significance of powerline electromagnetic energy studies (78). Personally, for two reasons, I am persuaded that the electromagnetic energy epidemiological studies show that powerline electromagnetic energy can affect human health. First, and most importantly, almost every study conducted has yielded a relative risk greater than 1.0, and the existence of a true cause-effect relationship is the only rational explanation for this global pattern that I can see. Second, the result is plausible in both the first and second sense of that term, as defined above.

6. BLUE-RIBBON COMMITTEES AND POWERLINE ELECTROMAGNETIC ENERGY HEALTH RISKS

Electromagnetic Energy Blue-Ribbon Committees

We believe that disease is the result of the operation of a causal chain. If we could identify links in the chain, perhaps it would be possible to prevent the operation of one or more of the causes, with the result that the disease would not develop or would be less severe. Despite advances in the treatment of disease and increased knowledge of the genes and other mechanisms that mediate disease, we know little about the causes of most disease. Why did this person develop this disease with this degree of morbidity at this time?

We attribute some causes of diseases to God or fate — an atavistic gene or a capricious microbe. Some causes, however, may originate at least in part from where people live or work. The possibility that powerline electromagnetic energy could be this kind of a cause has been with us since at least the 1970s. In response, from time to time, many expert committees were formed by stakeholders to evaluate the evidence and offer an opinion to the public about the health risks of electromagnetic energy.

The formation and functioning of these blue-ribbon committees of experts were complex sociological phenomena, with important differences between individual committees. But the defining characteristic of the blue-ribbon-committee approach to the evaluation of electromagnetic energy health hazards was the goal of seeking a consensus among the committee members regarding the meaning of the scientific evidence.

The first electromagnetic-energy blue-ribbon committee was appointed by the United States Navy to evaluate potential health implications of cellular, animal, and human studies funded by the Navy to assess the probable impact of a large radiating antenna proposed for construction in Michigan (79). The antenna's electromagnetic energy was similar in some respects to those of powerlines, although far weaker. The committee met in Washington, DC on December 6 and 7, 1973, and then issued a report evaluating the data provided by the Navy. The general tone at the meeting was surprise at the many different kinds of biological changes apparently caused by the electromagnetic energy used in the studies, which simulated the energy that would be transmitted by the antenna. The committee reached no firm conclusions regarding the safety of the antenna but it was concerned about the health implications for the state's citizens of the electromagnetic energy exposure the antenna would produce, particularly with regard to the population already at risk because of exposure to powerline electromagnetic energy.

In 1976 a second committee was appointed under the auspices of the National Academy of Sciences (NAS) to evaluate the health implications of the same antenna (80). The NAS committee, whose most prominent member was Herman Schwan, concluded that the antenna's electromagnetic energy was completely safe for the citizens of the state. Unsurprisingly, the committee said it could not identify with certainty any specific biological effects that would definitely result from exposure to the antenna's electromagnetic energy.

In 1984, the American Institute of Biological Sciences (AIBS), conducted a third review of the potential health risks of the antenna's electromagnetic energy and concluded that electromagnetic energy, generally, can cause a variety of biological effects, but that it was unlikely that the antenna would be unsafe, and would not cause health effects the committee considered adverse (81). Also in 1984 a blue-ribbon committee connected with the World Health

Organization (WHO) issued a report dealing with health risks of powerline electromagnetic energy which concluded that it was not possible to make a definitive statement about health hazards of powerline electromagnetic energy (82).

In at least two instances, the health risks of powerline electromagnetic energy were evaluated by self-organizing committees. In 1995, the American Physical Society (APS) issued a press release that said there existed no consistent, significant, and causal relationship between exposure to powerline electromagnetic energy and cancer (83).

The second instance occurred during a lawsuit in California where the San Diego Gas & Electric Company was being sued by a plaintiff who alleged that his cancer was caused by electromagnetic energy produced by the company's powerlines. Fourteen physicists, including 6 Nobel Prize winners, intervened in the case and submitted a friend-of-the-court brief supporting the position of the power company (84). They concluded that the scientific evidence strongly indicates that it is not scientifically reasonable to believe that powerline electromagnetic energy increase the incidence of cancer.

In 1997, a 16-person committee sponsored by the National Academy of Sciences concluded that there was no conclusive and consistent evidence of health hazards from powerline electromagnetic energy (85).

The most ambitious attempt, by far, to extract consensus regarding the health risks of environmental electromagnetic energy was carried out by the NIEHS. The effort consisted of multiple tiers of blue-ribbon committees that evaluated specified areas of electromagnetic energy bioeffects studies, and a super committee, the Working Group, that provided an overall assessment of all possible health effects of powerline electromagnetic energy. Based largely on this report, the Director of the NIEHS shall inform Congress by November, 1998, whether powerline electromagnetic energy affects human health (86).

The activities of the electromagnetic energy blue-ribbon committees frequently generated interest and awareness among scientists and the general public regarding man-made electromagnetic fields in the environment, and their potential health consequences. The 1973 Navy committee report was publicly released on the floor of the United States Senate. The 1977 NAS committee was the subject of a report in *Science* and was featured on two episodes of CBS' *60 Minutes*. The press release of the APS was widely reported in the *New York Times* and other prominent newspapers. The 1997 NAS report was also widely covered in the media, and it seems certain that this will also be the case for the soon-to-be-released NIEHS report.

Partly as a result of the electromagnetic energy blue-ribbon committees, whether intended or not, the public profile regarding environmental electromagnetic energy continued to rise and led directly to the NIEHS RAPID program, which for the first time made funds available for research by independent investigators to evaluate potential electromagnetic energy health risks.

But, in several important ways, the blue-ribbon-committee approach to evaluating electromagnetic energy health risks failed. First, no electromagnetic energy blue-ribbon committee delineated the limitation of the physical thought-style as a method for evaluating evidence and reaching an overall decision. In most cases, the role of physical theory was over-emphasized and disproportionate to its probative value. Not even one blue-ribbon committee recognized that the basic problem was political because all critical decisions necessarily had to be made on the basis of values, not science. Throughout my entire career, I never saw even one bone fide dispute about the science of electromagnetic energy — every dispute was based on values.

Second, the committees failed to recognize the basic nature of the electromagnetic energy-induced bioeffects that are pertinent to the issue of health risks from environmental electromagnetic energy. By adopting a too-narrow view of what could occur, the committees simply looked past what was actually occurring in the reported studies and thus failed to see the pattern of consistency that is manifested in the pertinent literature.

Third, the committees failed to identify decisional standards and to define dispositive terms. It is simply not possible to ascertain the meaning of committee reports because of the idiosyncratic reasoning principles and standards that were applied by individual experts, and the vague language that was used to state their findings.

The reasons why the electromagnetic energy blue-ribbon-committee approach failed merit consideration so that a reliable mechanism for making good public-health decisions regarding environmental electromagnetic energy can be designed at some future time. My goal in this Section is to explain the failure of the electromagnetic energy blue-ribbon committees. This requires discussion of (1) the process of appointment of committee members, and (2) the methods and procedures used by the committees to reach decisions.

The Appointment Process

If all the experts qualified to answer the electromagnetic energy question were identified and polled, then the majority vote would be the consensus regarding the issue among those qualified to offer an opinion. Such an opinion would be the most reliable consensus obtainable. But most reasonable definitions of a qualified expert would result in too many individuals to appoint to one committee or assemble in one place at a specific time. Consequently, the only practical means of obtaining the opinion of all qualified experts is to estimate it, based on representative sampling of the population of qualified experts. If the individuals whose votes were to be counted were truly representative of the population, then it would be reasonable to impute the results of the poll of the limited group to that larger population, thereby resolving the technical problem of having too many experts to assemble at one time.

On the other hand, if the individuals polled were not representative of all qualified experts, then a generalization of the committee's vote would be invalid. It is easy to see why this is the case. If members of the Sierra Club concluded that cutting redwoods would adversely affect the environment, or members of the National Rifle Association concluded that banning guns would adversely affect personal freedom, these conclusions might not easily generalize to the general population. The opinion of non-representative committees simply represents the opinion of that group of experts.

Representative sampling can occur only if the qualifications of the experts were first identified. It would then be possible to randomly choose persons for appointment to the committee. Although the details of how the electromagnetic energy blue-ribbon committee members were appointed were not disclosed, it is certain that none were chosen on this basis.

Paul Tyler, then a commander in the United States Navy, chose the 1973 Navy committee members on the basis of who he knew and who he thought knew a lot about the biological effects of electromagnetic energy. I was present when Tyler explained the committee to

Dr. Becker, and asked him to serve on it. The 1976 NAS committee was appointed by Phillip Handler, president of the National Academy of Sciences. He refused to tell me how he chose the committee members, but his appointment of three power-company expert witnesses made it clear that the selection process was highly biased. In a New York case involving the safety of 765,000-volt powerlines, for two weeks I assisted the attorney who represented the State in the cross-examination of the industry experts. They fared so poorly that their client lost the case. I sent copies of their testimony to Handler after I learned from the committee chairman that the experts had been appointed, but Handler did not unappoint them. The members of the 1984 WHO committee were nominated by the power companies of the countries that had representatives on the committee. As best I can tell, the 1984 AIBS committee was chosen by H.P. Graves, the committee chairman. At least he was the one who contacted me and asked me to write a paper for submission to the committee. The 1997 NAS committee was almost certainly not chosen randomly from a defined pool of experts because too many of the members of the committee were publicly associated with an ambivalent or negative attitude toward the possibility that powerline electromagnetic energy could affect human health. The plethora of NIEHS blue-ribbon committees were probably chosen by Christopher Portier on the basis of his perception of their special competence. I do not believe that he would even claim that they were chosen randomly or were representative of an identified class of experts.

In each case, therefore, the electromagnetic energy blue-ribbon committees consisted of people who were not representative of a defined group of experts whose collective opinion or consensus would be the proper one for resolving the question of whether powerline electromagnetic energy affects human health pursuant to a consensus process. In each case, therefore, the conclusion represented only the view of that ad hoc committee, and does not generalize in any reliable manner.

Qualifications

The officials who appointed the electromagnetic energy committees must have had reasons of some kind for appointing those whom they appointed. For example, Handler maintained that Schwan was chosen for the 1976 NAS committee not because of his views but because of his expertise, indicating that Handler had an idea of what a suitable electromagnetic energy expert was. Similarly, when Portier appointed the NIEHS Working Group, he must have had in mind what he thought an expert in electromagnetic energy was. But neither Handler nor Portier, nor any official who appointed an electromagnetic energy blue-ribbon committee, disclosed these qualifications. Consequently, it is impossible to independently assess whether the people chosen were qualified to opine to the American public regarding powerline electromagnetic energy.

The NIEHS Working Group report, for example, tells us that one person was Division Leader, Molecular and Structural Biology Division, University of California, and that another person was Professor, Northwestern University Medical School, Department of Molecular Pharmacology and Biological Chemistry. But academic rank and job titles do not entail expertise in the biological effects of electromagnetic fields.

Each of the members of the 1998 NIEHS Working Group was an expert in some area of science, as attested to by the listed academic achievements and job titles. But common sense tells us that if scientific facts are to be established by a committee vote, then each person with a vote ought to consider all the available evidence. However, this principle conflicts with NIEHS' apparent goal of creating a committee whose members each had expertise in a specific area arguably pertinent to the issue. Thus, the Working Group undoubtedly were experts, but their expertise probably did not extend to all of the evidence presented. What is a professor of molecular pharmacology supposed to know about cancer or suicide or electromagnetic fields? What is a division leader of structural biology supposed to know about the immune system?

Expertise is a special competence in a particular area. It allows the expert to more reliably resolve some issues than would otherwise be the case. But expertise does not elevate the reliability of an expert's opinion regarding all issues. Expertise does not create an aristocracy whose members simply think better than others. Consequently, when experts make decisions regarding questions outside their expertise, the basis for accepting their opinions as scientific facts is destroyed. For example, nineteen members of the NIEHS Working Group voted to say that powerline electromagnetic energy was "possibly carcinogenic" to human beings, and 17 members voted that the evidence was "inadequate that they cause suicide or depression," and that there was "no evidence in experimental animals for powerline electromagnetic energy effects on the immune system." It is difficult to see how, even in principle, the best decision or even a good decision can emerge from a process in which all committee members have limited expertise but are given equal voice in all component judgments related to the basic issue. Consequently, no reliable meaning can be attached to the committee voting.

The Politics of Appointment to Electromagnetic Energy Blue-Ribbon Committees: A Case Study

In early 1976, after Herman Schwan had filed his testimony on behalf of power companies in the New York legal dispute involving the safety of proposed high-voltage powerlines, I learned that he had been appointed to the 1976 NAS electromagnetic energy blue-ribbon committee, along with other powerline experts from the same dispute. It was difficult for me to understand how the power company experts could possibly have been appointed to the NAS committee, considering that they had already said that electromagnetic energy up to 100,000 times stronger was safe (87). What disturbed me was not that these men had pre-formed opinions, but rather that opposing opinions were not represented on the committee. The other members of the committee appeared to be distinguished scientists in their respective areas of expertise, but I could see no nexus between their expertise and the question of whether the antenna's fields would be health risks. Few of the members of the committee had any connection with electromagnetic energy biology studies, and those that did had opined publicly in general support of Schwan's approach to the issue.

In January, 1976 I called J. Woodland Hastings, Head of Biology at Harvard, the committee chairman, and complained to him about what I perceived to be the unfairness and lack of credibility of the committee. Hastings was surprised to learn of the appointment of the powerline experts. He told me that he just assumed that everybody on the committee was an unbiased expert because "that's the way the NAS works." Hastings told me that the committee members had been picked by Phillip Handler.

I thought that Handler had erred badly in appointing the powerline experts to the committee, and this suggested to me that his other appointees might also have problems — in particular, they might not be qualified to render public-health opinions about electromagnetic energy. Hastings did not see it that way. He assumed that the other committee members were qualified because they were appointed by Handler, and Hastings' focus was on the 3 powerline experts. He told me that he would seek either to have Dr. Becker and me appointed to the committee for the purpose of balance, or have the powerline experts removed from the committee.

As I saw it, electromagnetic energy biology itself hung in the balance. The use of electromagnetic fields to treat human diseases and to control human development and physiology was an area that was just developing in 1976. The first FDA approved application of these techniques was still almost 3 years away, but work toward that goal was well underway in several laboratories, including our own. What concerned me was not only that bad advice might be passed off to the American public as good science because it was channeled into the public domain by the NAS; I was also concerned about the implications for potential electromagnetic energy therapies. The gist of the power companies' position was that electromagnetic energy produced no effects. If they produced no effects, they couldn't produce good effects. End of story. End of a new area of biology.

Over the next 2 months, Hastings dealt with the National Research Council (NRC), and in particular with Samuel Abramson, the project officer who was managing the committee. Hastings' naÏveté about the NAS committee seemed real. He was surprised to learn from the NRC that one of the power company experts was a major stockholder ("more than $10,000") in power companies.

But by March, 1976, I think Hastings realized that he had hit a brick wall in his attempts to revamp the NAS committee, because he refused to take my telephone calls or respond to my letters. At that point I resigned myself to the inevitable and turned my attention back to the powerlines dispute in New York. As a final, ending statement, however, Dr. Becker and I sent a statement to the NAS committee in April that formally stated our experiences and our opinions (because my contacts with Hastings had been off the record) (88).

I did not realize that our statement to the NAS committee would immediately become a public document. However, a writer for *Science* obtained the statement and wrote a report (Boffey, P.M.: Project Seafarer: Critics Attack National Academy's Review Group, *Science* 192:1213–1215, 1976) that described our criticisms of the NAS committee. Soon after the report was published, we were contacted by CBS' *60 Minutes*, and Dan Rather came to our laboratory and interviewed Dr. Becker regarding his criticisms of the NAS committee.

In February, 1977 the CBS' *60 Minutes* interview with Dr. Becker aired. In a letter published in the Detroit Free Press, Handler said that our charge that the NAS committee was stacked was "laughable" and "intolerable." The letter suggested that the antenna was safe, even though the NAS committee, which was supposed to be evaluating the question, had not issued its report.

The first semester of my personal experience with the NAS electromagnetic energy blue-ribbon committees ended, or so I thought, with the *60 Minutes* piece. The depth of the antagonism that we had engendered merely because, from my point of view, we had told the truth and called a spade a spade did not become apparent to me until two years later. In September, 1979 the April, 1976 *Science* report was re-told in an article in the *Saturday Review* (Schiefelbein, S.: The Invisible Threat: The Stifled Story of Electric Waves, *The Saturday Review*, pp.16–20, September 15, 1979). Handler went ballistic. He wrote the *Saturday Review* that the article was "willful and venal" and "insulting to several distinguished scientists and to the National Academy of Sciences." The letter included a manuscript that he demanded be published, in which he called me everything but decent (89). I thought that publishing the manuscript was a good idea because it supported my contention that the NAS committee was pre-programmed to reach the conclusion it ultimately reached, and I wrote a detailed response (90). But, in the end, the editors decided not to do so.

What is the point? When Handler appointed the 1976 NAS electromagnetic energy blue-ribbon committee, he fully expected that the committee would ultimately reach the conclusion that they did reach. Not only was the conclusion foreordained, so was the evidence that would be considered, the evidence that would be ignored, and the reasoning that would be followed. The same was true of the 1997 NAS electromagnetic energy committee, and the 1998 NIEHS electromagnetic energy committee, and each of the other electromagnetic energy blue-ribbon committees, with the exception of the first one.

What makes the 1976 NAS electromagnetic energy committee unique is that I had a window into the appointment process, and thus saw first-hand its essential unfairness. Handler would have never reacted as he did if he was really right and Dr. Becker and I were wrong. The take-home message is that no one can be trusted to appoint the judges who will decide an important public-health issue such as the potential health hazards of powerlines in a secret process pursuant to undisclosed criteria, because even prominent men have biases and make mistakes. If secret appointments are made, that result is tantamount to allowing the appointer himself to decide the ultimate issue because the people appointed will opine in predictable ways. That's what happened in the case of the 1976 NAS electromagnetic energy committee, and I think that's what happened in the other cases.

Summary

The possible public-health menace of powerline electromagnetic energy cannot be reliably evaluated by industry-bonded or otherwise conflicted experts in a consensus-seeking process.

7. POWER-INDUSTRY SCIENCE AND POWERLINE ELECTROMAGNETIC ENERGY HEALTH RISKS

Introduction

To decide whether powerline electromagnetic energy affects human health, it is necessary to produce scientific data by means of appropriate experiments, and it is necessary to analyze data to infer its meaning and overall significance. Production and analysis of data are distinct activities, and both are expensive. Over-simplistic as it may sound, whoever pays for electromagnetic energy bioeffects research and analysis determines what data is produced and the way it is interpreted.

Soon after the possibility that powerline electromagnetic energy was a health risk was raised in a legal dispute involving the New York Public Service Commission, power companies and their trade associations, particularly the Electric Power Research Institute (EPRI), became massively involved in electromagnetic energy bioeffects research (91). Subsequently, the power industry dominated funding of the effects of powerline electromagnetic energy, both in terms of absolute dollars and compared with dollars from non-industry sources.

More than twenty-five years have elapsed since the power industry began its electromagnetic energy activities, and it is now possible to evaluate the industry's role. I will show here that the power companies and their trade associations were deeply deceitful regarding the information they provided to scientists and to the public regarding the potential health hazards of powerline electromagnetic energy.

Powerline Electromagnetic Energy Research at Battelle

Battelle Pacific Northwest Laboratories (Battelle) is a private company that performs contract research of many different types for many different organizations. Battelle began powerline electromagnetic energy activities on behalf of the power industry in March, 1976, and this relationship has continued to the present, without interruption. The dimension of Battelle's involvement with electromagnetic energy is hard to discern exactly, but it far exceeds in scope and impact that of any other group or organization that has performed electromagnetic energy research. Battelle has probably received more than $100,000,000 in funding for electromagnetic energy research, and its employees have made more than 1000 presentations and reports dealing with electromagnetic energy bioeffects issues.

Battelle's electromagnetic energy research mostly involved the effects of powerline electromagnetic energy on rats, mice, and pigs. The experiments consisted of exposure of the animals to electromagnetic energy, followed by many different kinds of physiological measurements. Various investigators at Battelle designed and conducted the experiments, disseminated the results, and defended them in scientific forums. Most of the Battelle experiments, presentations, and reports were negative, by which I mean that the studies, either on their face or as interpreted by the Battelle investigators, failed to suggest that powerline electromagnetic energy was a health risk.

The Battelle investigators urged that the negative studies were presumptive evidence of powerline safety, and disinterested scientists who reviewed Battelle's negative studies frequently agreed that the negative results suggested that powerlines were safe. But the Battelle investigators designed their studies and handled their data intentionally to produce negative results, and to produce the perception that the results were negative even when they were positive. Under these conditions, the negative studies did not justify an inference of powerline safety because the negativity was *made*, not *found*.

Negative Results by Design

Battelle investigators designed and performed many electromagnetic energy studies in which the measured parameter had no plausible sensitivity to electromagnetic energy. In these cases the results were foreseeably negative because one would not expect an effect due to the electromagnetic energy. For example, in a study of the effects of powerline electromagnetic energy exposure on heart rate in rats (D.I. Hilton and R.D. Phillips: Cardiovascular response of rats exposed to 60-Hz electric fields, *Bioelectromagnetics* 1:55–64, 1980), the heart rate of the animals was measured only after the animals were removed from the electromagnetic energy and then confined in narrow tubes so that they could not turn, rear, or make other normal movements. It would be expected that the stress of confinement in the tubes would alter heart rate, thereby obscuring any effect due to powerline electromagnetic energy; not surprisingly, the study was negative.

In another study, Battelle investigators measured the effect of powerline electromagnetic energy on visual evoked potentials in the brains of rats (R.A. Jaffe, C.A. Lopresti, D.B. Carr and R.D. Phillips: Perinatal exposure to 60-Hz electric fields: Effects on the development of the visual-evoked responses in rats, *Bioelectromagnetics* 4:327–339, 1983). Such potentials are sometimes used to diagnose pathological changes in the visual systems of patients, but there was no evidence whatever to suggest that evoked potentials would be a worthwhile parameter to measure in connection with electromagnetic energy exposure. This was particularly the case in view of the method used by the Battelle investigators to measure the potentials. Normally, electrodes are attached to the head of the subject using an electrically conducting adhesive. This method of attachment minimizes the stress caused by the measurement process itself, thereby protecting the integrity of the results. The Battelle investigators, in contrast, drilled holes through the skulls of the rats and placed the electrodes directly on the brain, thereby making the measurements hopelessly insensitive to the effects of electromagnetic energy. The results were negative, but not finding an electromagnetic energy-induced change that one had no reasonable expectation would occur was not evidence that powerline electromagnetic energy was safe. Nevertheless, that was Battelle's rationale for the study and the way the results were interpreted.

The question whether powerline electromagnetic energy is a stressor is important because stress can worsen the consequences of *any* human disease, and Battelle investigators tried to show that powerline electromagnetic energy was not a stressor. In these experiments, however, they built special cages that confined the test animals in abnormally small areas. For example, mice were confined to cages that were only 2 inches high, and rats in cages that were only 4 inches. Federal guidelines for caging mice and rats stipulated cages having minimum heights of 5 and 7 inches, respectively, precisely because that was the veterinary consensus regarding what was appropriate for stress-free housing conditions for each species. The published results of Battelle's studies using abnormally small cages indeed failed to find evidence that powerline electromagnetic energy was a stressor, but that conclusion was foreordained by the way the animals were housed. Both the electromagnetic energy-exposed and the control animals were *already* stressed as a result of their crowded living conditions.

The pervasive consequences of the crowding were shown by the Battelle results obtained between March, 1976 and November, 1977. During this period, Battelle investigators found only two positive effects that they considered to be potentially adverse, out of more than 380 parameters that they measured in their chronically crowded animals. These overwhelmingly negative results were reported in almost 50 contemporaneous presentations and papers (92).

Negative Results by Analysis

Some Battelle experiments yielded positive results. On their face, positive results would appear to a disinterested scientist to suggest that powerline electromagnetic energy was *not* safe, following the logic used with the negative studies that led to the opposite conclusion. But positive results were the opposite of what Battelle's clients wanted, and Battelle invoked various artifices to insure that positive results were not recognized as positive. One way this was accomplished involved the device of replication.

When the Battelle investigators found a positive effect, they routinely repeated the experiment. Superficially, this practice appeared to be an honest procedure, predicated on the possibility that the positive effect might have been a statistical fluke. Only the *positive* effects were usually replicated, however, even though the negative results might *also* have been statistical flukes. Thus, the routine procedure of replicating only positive effects created a pervasive bias in favor of the general conclusion that powerline electromagnetic energy studies were negative. Adding to this bias was the way the Battelle investigators interpreted the overall result when the replicate of a positive experiment was negative. In those cases, the Battelle investigators arbitrarily concluded that both experiments, taken together, were negative.

In some instances, both the first study and the second study of a particular type were positive; in that event the study was repeated a third time. If the results of the third study did not exactly match the results of the first study and the second study, then the set of three studies was considered to be a negative study. For example, they observed an inflammation of the prostate glands of rats that were exposed to electromagnetic energy for 30 days (93). The experiment was repeated, with the same result. The experiment was repeated for a third time, but the 67% increased rate of prostatitis in the electromagnetic energy-exposed rats was not statistically significant. The investigators concluded that, overall, electromagnetic energy had no effect.

Battelle's strategic use of replication forced the inherent uncertainty in biological studies to favor the point-of-view of Battelle's clients. In theory, the results of biological studies must be certainly yes, certainly no, or somewhere in the middle. The Battelle investigators arbitrarily interpreted the two most likely outcomes in favor of the power industry.

Negative Results by Omitting Positive Results

If an investigator performs an experiment and then withholds some of the data, without explanation, it's easy to see that a disinterested scientist who reviews the published data might be misled. Relevant data was routinely withheld by the Battelle investigators.

For example, in one of their endocrinology studies, the Battelle investigators exposed a group of male rats to powerline electromagnetic energy for 30 days to assess whether or not the electromagnetic energy was a stressor. The experiment consisted of recovering the blood of the exposed and control animals and analyzing for the presence of changes in corticosterone levels, which would indicate that the electromagnetic energy was a stressor. I had previously performed the same experiment several times, and reported that corticosterone levels were altered as a consequence of the electromagnetic energy exposure (94).

Using a fluorometric technique, the Battelle investigators found that corticosterone in the blood of the electromagnetic energy-exposed animals was 123±17(units of ng/ml), which was less than that in the control animals (175±50). Portions of the same samples were sent to the University of Rochester to be analyzed by competitive protein-binding radioassay, a different and perhaps more specific method of measurement. Using the radioassay method, the corticosterone levels in the exposed animals were found to be even more significantly different than the levels in the control animals (34.9±7.7 compared with 287.0±137.9).

The experiment was repeated using twice as many rats as previously. When the results were analyzed using the fluorometric method, the exposed animals were again lower than the controls (150±16, compared with 193±32). The radioassay measurements also showed that the levels in the electromagnetic energy-exposed rats were lower than in the controls (43.4±10.6, compared with 82.8±22.1).

The experiment was repeated a third time; in this case the blood samples were sent to the University of Kansas for analysis. Again, the levels were lower in the electromagnetic energy-exposed animals (51.5±9.9, compared with 90.8±15.8). In a fourth experiment, rats were exposed for 120 days (4 times longer than the exposure in the first three experiments). Again, the levels were lower in the electromagnetic energy-exposed rats compared with the control rats (52±10 and 91±16, respectively). Battelle wrote to the study sponsor: “The data appears to be consistent with similar findings reported by Marino.”

But then the 30-day experiment was repeated a fourth time, and there was no difference in the blood levels of corticosterone between the exposed and control rats (42.1±11.6 and 35.6±9.5, respectively). And the 120-day exposure experiment was repeated with the result that the corticosterone levels in the exposed animals was lower than in the controls, but not significantly so (64.4±6.2) compared with 76.5±8.0). When the Battelle investigators published their results, they included only the second of the four 30-day experiments, and the two 120-day experiments, and they concluded that electromagnetic energy exposure had no effect on corticosterone levels (See N.J. Free, W.T. Kaune, R.D. Phillips and H.-C. Cheng: Endocrinological effects of strong 60-Hz electric fields on rats, *Bioelectromagnetics* 2:105–122, 1981).

The easy ability to hide data or to disclose only that portion that comported with the position of the study sponsor is one of the fundamental weaknesses in the use of trade-industry research results for making public-health determinations about the safety of powerline electromagnetic energy. In the endocrinology experiments, for example, if the Battelle investigators had disclosed all the data, the results would likely have been interpreted by disinterested scientists to show that powerline electromagnetic energy was a stressor. But nothing is more clearly demonstrated by an analysis of the history of electromagnetic energy bioeffects research than the fact that investigators or organizations that find results suggesting that powerline electromagnetic energy is a health risk do not have their research contracts renewed. Thus, every instance of a positive effect found by the Battelle investigators created a conflict of interest for them, and in many cases this resulted in their failure to disclose pertinent data that should have been disclosed. In the endocrinology experiments, the Battelle investigators hid the data because it suggested *exactly* the inference that the power industry sought to avoid.

The Battelle studies involving rats and mice consisted of 12 different kinds of experiments, each of which was headed by a principal investigator who was answerable to the Task Leader, R.D. Phillips. Every instance in which it was possible for me to compare internal Battelle documents with the results of their published experiments I found serious instances of hiding of data, resulting in an altogether different public perception than if *all* the data were disclosed.

In the Battelle Cardiovascular Function studies, for example, male rats were exposed to powerline electromagnetic energy for 30 days and then removed from the field and placed in narrow tubes so that wires could be attached to facilitate measurement of heart rate. In the 1-hour period following removal of the rats from the field, the heart rate of the exposed animals did not differ from that of the controls. The investigators intended to repeat the experiment after 4 months' exposure, but found that the male rats grew too large to fit into the tubes. The experiment was therefore begun again with female rats, resulting in data for male and female rats after 1 month's electromagnetic energy exposure, and for female rats after 4 months' exposure.

When the Battelle investigators reported their results on heart rate (D.I. Hilton and R.D. Phillips: Cardiovascular response of rats exposed to 60-Hz electric fields, *Bioelectromagnetics* 1:55–64, 1980), they described only the results for male rats and for female rats exposed for 4 months, and concluded that there were no significant effects due to the electromagnetic energy. But their report was misleading for several reasons. First, the unpublished data from the female rats exposed for 30 days *was* statistically significant, and showed an effect due to electromagnetic energy (95). This was remarkable because it suggested that the effect of the electromagnetic energy could not be obscured even by the stress of confinement. Second, the reported data for female rats exposed was not the same as that in their monthly report, which seemed to show that the electromagnetic energy significantly affected the heart rate for about the first 20 minutes after the rats had been removed from the electromagnetic energy (96). Thus, the conclusion of their publication that there were no electromagnetic energy effects was not true if all the data was considered.

Battelle's Reproduction and Development study also resulted in data that was never publicly disclosed. The reproduction study began in January, 1978, and was intended to refute an earlier study published by me and my colleagues (97). The plan was to produce 3 successive generations of mice, and to code the data in such a way that some of the people who worked on the experiment could not determine what the results were during the experiment (98). In February, a second version of the same experiment began in a separate exposure facility 50 feet down the hall from the first exposure facility. Both experiments were scheduled for completion in December, 1978.

Some time prior to November 22, 1978, after only two generations had been born in each of the two experiments, the data codes were broken and the data was analyzed. The interim analysis showed that the electromagnetic energy affected the growth rate of the mice in both experiments, whereupon the experiment was changed to a 4-generation study. The fourth generation was born around March, 1979, but its existence was never disclosed.

The results from the first 3 generations showed that the electromagnetic energy consistently affected the growth rate of the mice. However, as described in Section 3, because the results were not exactly the same in the two experiments, the Battelle investigators concluded that there were no effects due to the electromagnetic energy. Because the data from the fourth generation of mice was never disclosed, we can only speculate about how it might have affected the overall interpretation of the study. Perhaps Battelle's procedure of averaging the results of two positive experiments would not have yielded a negative result if the data from the fourth generation was also included. In that case, even scientists who accepted the averaging procedure would be constrained to agree that the overall results of the study were positive.

Negative Results by Argument

Battelle investigators frequently characterized their data as negative even when it was probably positive. By undercutting the obvious implications of their work, the Battelle investigators denied its use to those who might disagree with the power industry position. An outstanding example was the Battelle study of the effects of powerline electromagnetic energy on reproduction and development of pigs, which lasted more than 5 years and cost more than $7 million. During the study it began to appear that powerline electromagnetic energy produced many different biological effects. When the Battelle investigators published the study they identified a broad range of problems and claimed that these problems, not the electromagnetic energy, were responsible for producing the biological effects in the pigs. Among the problems were infections, electrical fires, hysterical female pigs, and statistical fluctuations. In each instance where the data apparently disclosed a positive effect, the Battelle investigators chose a non-electromagnetic energy cause and explained away the positive result.

When this Keystone Kops of powerline electromagnetic energy studies was published by EPRI, the written record extended to 7 volumes. Even if all of the data was present, Battelle's written and oral reports were so thoroughly hedged, it looked like the study was negative. The Battelle investigators pooh-poohed the inference that data which looked positive was actually positive. Obviously, independent investigators would be reluctant to assert that data was positive when the Battelle investigators themselves would not make that claim. The overall result, therefore, was that the Battelle pig study was generally accepted as negative.

Negative Significance of Concededly Positive Results

Battelle developed a novel strategy for insuring that inferences based on their data could not undercut the position of the power industry, even in those cases where the Battelle investigators admitted that the data was positive. This was accomplished by intentionally compromising the significance of the data using a confounder. The strategy was based on mathematical modeling that, on the surface, seemed designed to resolved a bona fide problem—the important issues of electromagnetic energy dosimetry and scaling.

What electromagnetic energy strength should be used in animal studies that will ultimately serve as a basis for answering the question of human risk? Should the animals be exposed to the same strength of electromagnetic energy as the people who live hear the powerlines? More? Less? The Battelle investigators performed many mathematical studies that seemed designed to deal with the dosimetry issues. On the basis of these calculations, they claimed that animal studies should be done at about 5 times the strength of the powerline electromagnetic energy to which people were exposed.

But the Battelle investigators arrived at the factor of 5 by making a series of assumptions in their calculations. By changing the assumptions, one could produce an infinite number of factors, each of which was as valid as the factor of 5 suggested by the Battelle investigators. Nevertheless, on the basis of their calculations, the Battelle studies were done using electromagnetic energy many times stronger than powerline electromagnetic energy.

Early in the course of the work, Battelle investigators discovered that the strong electromagnetic energy caused the hair on their mice, rats, and pigs, to vibrate (99). Since these animals, but not people, are completely covered with hair, one consequence of using high electromagnetic energy was to destroy the potential scientific significance of any positive effects that might occur. Any such effects could equally be attributed to chronic irritation of the animals due to causing the hair on their body to oscillate continuously, as well as to electromagnetic energy interacting with body tissues. The overall result was that the Battelle investigators reported *some* biological effects due to electromagnetic energy, thereby avoiding the absurdity of *always* failing to find *anything*, but they did not jeopardize the position of the power industry in doing so because the implications of the positive effects could be explained away. For example, Battelle investigators found that powerline electromagnetic energy retarded fracture repair in rats. As a potential explanation, they suggested that the hair vibration caused by the electromagnetic energy may have increased muscular activity in their fractured legs, thereby inhibiting repair (E.J. McClanahan and R.D. Phillips: The influence of electric field exposure on bone growth and fracture repair in rats, *Bioelectromagnetics* 4:11–19, 1983).

The artifact of hair stimulation was used like an ace in the hole. During an electromagnetic energy blue-ribbon committee meeting, for example, a suggestion by a disinterested scientist that the positive results from a particular Battelle study suggested that powerline electromagnetic energy might be a health hazard typically resulted in a remark from the Battelle representative pointing out the potential role of the irrelevant mechanism of hair stimulation. Thus, Battelle's calculations rationalized the use of high electromagnetic energy which, in turn, virtually guaranteed that any positive data could not be used for evaluating human health risks.

Unreliability of Contract Research

There is a right way and a wrong way to do science. Scientific misconduct is the general name for the wrong way. I think that, in specific experiments, the powerline electromagnetic energy research at Battelle was scientific misconduct. But the problem posed by the type of research performed at Battelle and other similar companies is far more serious for science and society than isolated cases of scientific misconduct. The *process* that produced the scientific data published by Battelle differed too greatly from the process normally employed to produce scientific data. Battelle's data, therefore, simply cannot be treated like data that was produced in the normal way. It does not matter what the data says or doesn't say, the process followed tainted every result.

The conduct of powerline electromagnetic energy research at Battelle differs markedly from the manner in which honest and competent research ought to be done.. The ultimate goal of the Battelle electromagnetic energy research was the economic advantage of the power industry, not scientific truth. Specifically, they sought to produce scientific information that supported the positions of the directors of the power companies. The willingness of the power companies to pay the hefty price for Battelle's electromagnetic energy research reflected the power industry's judgment regarding priorities affecting its business, and had no necessary connection with scientific truth or public priorities. The industry's priorities translated into Battelle's goals which in turn determined Battelle's specific activities. If the industry-Battelle axis did result in scientific truth or if it fostered the welfare of the general public, those benefits would be accidents, not the result of design.

Battelle research was almost always reactionary. It is not possible to identify a single fruitful line of research that was initiated by Battelle investigators. On the other hand, it is almost always possible to identify a line of powerline electromagnetic energy research that each Battelle report was designed to rebut, replace, or otherwise undercut. The hypotheses for the bulk of their studies was that a previously reported electromagnetic energy-induced bioeffect was an artifact, and in most cases the Battelle investigators supported their hypothesis. It is simply impossible for honest electromagnetic energy investigators to establish scientific truth under these circumstances because anybody can perform a study and not find something that was found in a previous study by someone else. It takes no skill whatever to do this.

As a rule, the Battelle investigators had few publications prior to beginning powerline electromagnetic energy research. This suggests that the Battelle investigators did not have the training and expertise necessary to perform the studies that they were hired to perform. The trade associations were indeed free to hire anybody they wanted to perform their research because it was trade-association dollars that were spent. Legally, therefore, no one can insist that their dollars should be spent only for innovative research done by competent investigators. On the other hand, it would be foolish to treat the Battelle work product as if it were done by competent scientists pursuant to innovative experimental designs.

Nothing about the research at Battelle was released to the scientific community or the public except for material that was approved by the power industry. The experimental designs of the Battelle investigators and the data they obtained were not made available because the Battelle research was a private contractual affair between Battelle and a particular power company or trade association. I was able to obtain the detailed information concerning Battelle studies presented here only because of the intervention of Vice-President Walter Mondale, who directed the Department of Energy to release copies of letters and reports that had been copied to the Department by the Electric Power Research institute, which was a partner with the Department for the purposes of researching the health risks of high-voltage powerlines. The research program at Battelle sponsored by the Electric Power Research Institute in conjunction with the Department of Energy and masterminded by Richard Phillips was dishonest and rigged to support the industry's desired conclusion, and I told him so (100).

Summary

Neither scientists nor the public can rely on power-industry research or analysis to help decide whether powerline electromagnetic fields affect human health because power-industry research and analysis are radically misleading.

All claims, conclusions, reports, publications, and presentations by Battelle investigators should be doubted or disbelieved because they were primarily intended to serve the interests of Battelle's clients, not the public or scientific truth. In some cases, the Battelle version of the facts may be correct, but the point is that the likelihood of biased information is too great for scientists or the public to believe what Battelle says to the same extent and in the same way they might believe information provided by disinterested scientists whose only goal was truth. Information from Battelle regarding the health risks of powerline electromagnetic energy can be rehabilitated and perhaps used in public-health decision-making only in circumstances that provide a mechanism to challenge the responsible Battelle investigators regarding the details of their work.