POWERLINE ELECTROMAGNETIC FIELDS AND HUMAN HEALTH

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1. INTRODUCTION

1.1. Origins

The question whether powerline electromagnetic fields (EMFs) affect human health originated in the 1960s in the United States [see 1.1. note 1], 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. Dr. Becker was my mentor when I was a graduate student (1963-68) and my boss for 12 years thereafter.

Between October, 1974 and February, 1978, Dr. Becker and I were deeply involved in a long legal dispute [see 1.1. note 2] in New York regarding whether powerline EMFs were a potential health hazard. In the subsequent quarter century, concern regarding health risks of powerline EMFs grew and expanded to other sources of electromagnetic fields in the environment including cellular telephones, microwave ovens, electric blankets, microwave towers, and television and radio antennas.

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 still younger lawyer, largely inexperienced in the intricacies of both professions. Dr. Becker had been involved in scientific arguments, [see 1.1. note 3] but this did not prepare him for the contentiousness that subsequently developed regarding powerline EMFs. The consequences of the stand that Dr. Becker took regarding health risks of powerlines were catastrophic for him. By 1980 he lost his NIH grants, his Veterans Administration grant, his laboratory, and he was forced to retire at the age of 56.

I too lost my NIH grant, and my Department of Energy contract. We were both attacked [See Boffey, P.M.: Project Seafarer: Critics attack National Academy's review group, Science 192:1213-1215, and Schiefelbein, S.: The invisible threat: The stifled story of electric rays, Saturday Review, No. 15, pp. 16-20, 1979. Also, 60 Minutes, CBS, February, 1977 (interview of Dr. Becker by Dan Rather). 60 Minutes, CBS, April, 1977 (Mike Wallace interview of me and Dr. Becker] by the Chairman of Biology at Harvard University and by the President of the National Academy of Sciences. Contracts were awarded to investigators for the specific purpose of performing research designed to contradict the results of our research.[see 1.1. note 4] In the short period between 1974-80 I came to be regarded as a serious enemy by an uncomfortably long list of scientists, corporations, agencies, and their lawyers.

1.2. Personal Crisis

We saw the end coming as we lost our grants, one by one, and the pressure against our laboratory mounted steadily. It became difficult to do research, and we began to focus on a book we agreed to write dealing with the biological significance of electromagnetic fields. We wrote the book {*Electromagnetism and life* available from *http://www.ortho.lsuhsc.edu/Faculty/Marino/EL/ELTOC.html*] during the last year that the laboratory existed.

Our interests had already begun to diverge, and the book contract created considerable tension in our relationship. Dr. Becker is the originator of the stressor theory [see 1.2. note 1] of EMF bioeffects. As best I can remember, he first told me about it in detail in 1974. That conversation affected me profoundly. It provided professional focus and direction. If the stressor theory were true, it could be important because it suggested a previously unrecognized role of the neuroendocrine system in human disease.

After I met Dr. Becker, 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. 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. 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 on top of it. Now, if disease was really mediated by an aberrant response in the neuroendocrine system, caused in part by apparently innocuous factors in the environment like powerline EMFs, *that* would be important.

My concept of our book was that it should be focused on Dr. Becker's exciting insight into the possibility that EMFs were stressors. I wanted to marshal all the available scientific evidence and document the affirmative case. But Dr. Becker saw things quite differently. Although he was proud of his discovery of the effects of environmental EMFs, he seemed to regard it as one of the lesser of his insights into biology. Early in his career, before I knew him, he conducted a stunningly successful series of studies [see 1.2. note 2] dealing with the biological effects of electrical energy, particularly effects involving bone. 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.

Finally, Dr. Becker believed that the focus of orthopaedics 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, all four of them, presented me with a dilemma. I personally believed that the weight of the evidence was against his theory about PN junctions, that his theory about nerves was wrong, and that his theories about regeneration and stressor effects of EMFs were problematical. Actually, I thought the stressor theory was problematical, and the regeneration theory was very problematical. Consequently, at least for the book, I thought we ought to concentrate on the stressor theory. But the prospect of appearing disloyal to Dr. Becker, to whom I owed so much, was particularly disturbing.

I approached Dr. Becker with a proposal that I thought reflected the wisdom of Solomon. All four theories would be treated in the book, and the presentation would be organized in a four-step 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.

I know how to find evidence, and I know how to analyze it. I was trained to do exactly that in physics and in law. I am good at it, and I was good at it in 1980. Dr. Becker knew that and frequently complimented me regarding this ability. My thinking was that if he agreed to my proposal and it turned out that the evidence weighted against one of his theories, he would then take that theory off the table, and its associated evidence would be de-emphasized in the book. I thought that we might discount two or even three of his hypotheses by following this procedure. I hoped that I would be wrong because nothing would have pleased me more than to write a definitive analysis that defended Dr. Becker's views. It would be a small payback for everything he had done for me. Dr. Becker knew that in analyzing the evidence I would give him, not his critics, the benefit of the doubt.

Dr. Becker rejected my proposal. It wasn't even close. What he wanted to do was simply describe his theories and the evidence that he produced to support them, as well as other evidence that fit with them. 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 do it. I just couldn't do it. Ultimately, after painful discussions, we agreed to write two books.

He would write about his three favorite theories, and I would write about the EMF hypothesis. That's what we did. His three theories are contained in the first four chapters of our book, [*Electromagnetism and life* available from *http://www.ortho.lsuhsc.edu/Faculty/Marino/EL/ELTOC.html*] each of which designates him as the sole author. My analysis of the EMF issue is contained in the subsequent seven chapters, each of which designates me as the sole author.

1.3. Sorting Things Out

When I wrote my chapters I saw that the scientific evidence showed that environmental electromagnetic fields were potential health risks. 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 EMF biology. I turned down the jobs [see 1.3. note 1] that were offered to me in New York, but I found a job in Louisiana, which is where my wife and I and our four kids moved in 1981.

The Chairman of the Department of Orthopaedic Surgery, LSU Medical School, Shreveport, Louisiana, hired me as an Assistant Professor. He is probably the greatest man I ever met. One of the things I learned from him was the importance of staying cool. Emotion is the enemy of rational thought, I came to see.

I had become angry over the question of health risks from powerline EMFs. I was angry because the power industry had hired scientists specifically to attack me. 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 contract, 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 environmental EMFs. 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 EMFs, because I know enough about that subject to prevent making the mistake of exposing myself or my family to powerline EMFs. But the situation regarding EMFs 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 EMFs. I know the EMF 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 EMFs that I perceived? The power company says that

the EMFs from the powerlines are safe. If they are right, the power companies do not have to spend money to include safety features that would protect against exposure to EMFs. 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 EMF.

Many factors have been implicated as causing cancer in people. But EMFs were 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 EMFs.

What is the just responsibility of the power industry and its trade associations, particularly the Electric Power Research Institute (EPRI)? I think it is to "lay bare the truth, without ambiguity or reservation". [see 1.3. note 2] What occurred, however, was the opposite - a consistent pattern of obfuscation, misrepresentation, mis-characterization, and hiding data by EPRI and the power companies, motivated, as best I can tell, by simple greed.

EPRI 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 far more dollars than called for in the EMF bioeffects research contracts. In other instances EPRI and the power industry simply created companies whose major asset was a contract for research or analysis regarding powerline EMFs. The results produced by these contracts and released to the public never concluded that they had found evidence suggesting that powerline EMFs might be a health hazard. Thus, the situation was that almost everyone who **didn't** work for the power industry and EPRI was almost always finding evidence that suggested that powerline EMFs were health risks, but essentially everyone who **did** work for the power industry or EPRI was failing to find such evidence.

The industry was always well represented in all legal proceedings involving powerline EMF health-risk issues. In the legal dispute [see 1.3. note 3] in New York, 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. The industry fared poorly in that dispute, but it learned from its mistakes and entirely shifted its strategy. An integrated strategy was formed that would permit the industry to protect its interests wherever they might be jeopardized, either in court or in the court of public opinion. The lynch-pin in this strategy was a lawyer, Tom Watson. [see 1.3. note 4] 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 EMFs, even when the residents have no conscious awareness of the presence of electromagnetic fields, and have never voluntarily consented to be exposed.

I thought the situation was unfair. I wouldn't want my family exposed to powerline electromagnetic fields based on the present evidence, Watson's family isn't exposed to electromagnetic fields 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 and me to do so.

1.4. 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 fields 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 EMF issue. The legal dispute [see 1.l. Note 2] 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 was applicable to assessing powerline health hazards?

I began a study of the cellular biology of how stimuli in the environment are detected [see 1.4. note 1] by the body. Both in my own research, and in the research of others, I planned to learn where and how the body transduced electromagnetic fields. Although this question was important, it was not the first question to be considered. The question **how** the body detected EMFs would not be ripe until the fact that the body **could** detect them was first proven. Schwan confounded the issues of detection and mechanism and argued that absence of knowledge regarding mechanism of detection of powerline EMFs was evidence that no such mechanism existed. To me that view was illogical, and the Siren song of mechanism was best avoided until the phenomenon of detection of powerline EMFs was established.

If Dr. Becker's theory that environmental EMFs were biological stressors was correct, it would necessarily be the case that the presence of an external electromagnetic field would cause changes in the brain's electrical activity. It would be impossible for the brain to recognize the presence of EMFs in the environment and orchestrate a response without, itself, changing its electrical activity. I resolved, therefore, that my first studies would be geared toward detecting the body's detection of powerline EMFs.

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 EMFs 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 EMFs could be linked to disease. Early in this quest I settled on the immune system as a likely target for EMFs 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 EMFs. If the efficiency of the immune system were reduced by EMFs, then it is easy to see that the probability of disease [see 1.4. note 2] 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 EMF health risks, and it would be necessary to be able to distinguish a good EMF epidemiological study from a bad one.

As I saw it, the question whether powerline EMFs were health hazards 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. If the question were, for example, whether under a particular set of conditions a particular EMF 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 EMF-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.

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 guys. What I was mostly interested in was encapsulating their activities for the purposes of posing the question *Is this what we want?* 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 EMF epiphany 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 EPRI was wrong. Rather, it was to find the truth [see 1.4. note 3] about the relation between environmental EMFs and human disease, regardless of who might be hurt or displeased.

The ultimate issue would be whether EMFs affect 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 EMFs, 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 EMFs, 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 EMFs would not be one of them. On the other hand, if powerline EMFs 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".

1.5. Congressional Interest

While I was attempting to understand the EMF 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 an electromagnetic field is. [see 1.5. note 1] 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 EMFs. 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 rather evenly. The proponents of the powerline-EMFs-are-safe view had all the money on their side. They completely controlled the targeted research and the public spin involving powerline EMFs. Research that had the potential to yield results that implied powerlines caused health risks was not funded, and opinions that powerline EMFs were health risks were infrequently voiced in high government or industry councils. What was funded was usually irrelevant. The industry viewpoint was over-represented on each blue-ribbon committee, with the unsurprising result that their conclusions were broadly reassuring to the public and supportive of the industry.

On the other hand, it was distressingly easy for a print or 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 EMF issue simmered in the '80s.

The law that mandated the federal EMF program was one of the provisions in the 1992 Energy Policy Act. The law called for research to determine whether powerline EMFs "affect human health," and it required that this issue be addressed directly in a report to Congress by the Director of the National Institute of Environmental Health Sciences (NIEHS) [is Dr. Kenneth Olden, who was formerly Director of Howard University Cancer Center and Chairman of the Oncology Department at Howard's Medical School.]

A prominent aspect of the Congressional interest in the powerline EMF issue was the distrust [see 1.5. note 2] that developed regarding whether the industry would honestly evaluate the health risks of powerlines. 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 [see http://www.law.cornell.edu/uscode/42/13478.html. The due date of the report was subsequently changed by Congress to November, 1998] that set up the federal program to evaluate the health implications of powerline EMFs. In those hearings,[see 1.5. Note 3] high-level officials from the power industry strongly urged Congress to enact legislation aimed at determining whether powerline EMFs affected human health. This was a major shift in strategy on the part of the power industry.

The Director's report to Congress is due in November, 1998. In response to the question *Do powerline EMFs affect human health?* I think the Director will effectively say "I can't tell for sure." The reasons why this will probably be the bottom line go deep into the nature of science, and into the relationship between science and the larger society [see 1.5. note 4] of which it is a part. Those reasons are the subject of this report.

1.6. Why Continue?

Public and Congressional interest in the powerline EMF 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 EMF 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 EMF 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 = powerline-EMFs.[see 1.6. note 1] 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 EMF 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.

1.7. Tom Watson and the Rules of the Contest

My view is powerline EMFs do 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 EMF 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 EMFs are safe. If the calculations are not persuasive he shows that there are no mechanisms of interaction between EMFs 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 EMFs.

He says that the only acceptable evidence that a human being got cancer from exposure to powerline EMFs 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 EMFs 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 EMFs. Since the 1970s, he has acquired an enormous data bank of scientific reports, testimonies, and other pertinent documents. Watson knows the EMF scientific jargon 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 EMF 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 EMFs affect human health or that powerline EMFs 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 EMFs affect human health. *What are the rules for answering the question?*

1.8. Ultimate Goals

The EMF dispute has been generally styled as one involving only scientific knowledge, 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 the Director of the NIEHS [Dr. Kenneth Olden] to assess whether powerline EMFs affect human health. The facts 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.

I want to show that the question whether powerline EMFs affect 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-EMF 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?

EPRI and the power industry claim that the values which necessarily enter into the resolution of whether powerline EMFs affect human health ought to be the values of scientists, particularly the scientists that are associated professionally with their industry. But I think this is wrong, and that the values incorporated into the decision ought to be those of society, not those of any particular group of scientists. The opinion of scientists, as distinct from their knowledge, is not important except in proportion to their numbers in society. It's a case of one man, one vote.

These issues may be difficult to appreciate because they require a new look at science and at the relationship between science and 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 *Do powerline EMFs affect human health?* I suspect powerline EMFs are not the only problem whose existence 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 EMF powerline dispute, and then progressing toward the more concrete issues that animate the controversy. I am aiming to be understood by both scientists and laymen, and this 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.

2. TWO SCIENCES

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

2.1. Introduction

Following the end of World War II, 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 [see 1.1. note 2] he applied them to powerlines and concluded that powerline EMFs would not affect human health.

Schwan was cross-examined for 2 days in April, 1976 regarding his opinion about powerlines, and he fared poorly. [see 2.1. note 1] 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 EMFs 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 EMFs 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 [see 2.1. note 2] offered opinions regarding whether powerline EMFs affect human health. But their arguments were no different from those of Schwan. It dawned on me that Schwan and those who think like him were not 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 EMFs affect 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 EMF 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 Do powerline EMFs 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 that physicist's opinions about whether powerline EMFs and 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 EMF 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 hazards question.

2.2. Scientific Methods

There have been many studies of the philosophy of science.[see 2.2. Note 1] Generally, the aim in these studies was to identify what the authors considered to be the basic features of scientific practice, and this 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 cases, and in the absence of idiosyncratic ideas regarding how it ought to be done. Consequently, I employed representative sampling [see 2.2. Note 2] to facilitate identification of the rules and procedures of scientific reasoning that are used to establish a putative fact as scientific knowledge.

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 [see 2.2. note 3] in Table 1. [see 2.2. note 4] Four additional reports were not considered because they involved measurement or other activities (invention and discovery) that did not utilize formal reasoning.

2.3. Scientific Reasoning

A common feature of the reports summarized in Table 1 [see 2.2. note 4] 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 absolutely 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 [see 2.2. note 4] 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 osteopontin 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, [see 2.3. Note 1] which is an inferential reasoning process distinct from induction and deduction. 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 [see 2.3. Note 2] of the studies. 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). [see 2.3. note 3] 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.

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 [see 2.2. note 4] 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. 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.

2.4. 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 *s*cientific 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 5249 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 [see 2.4. Note 1] are fundamentally different objects. 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 EMFs affect human health.

3. PHYSICS AND POWERLINE HEALTH HAZARDS

Physics does not predict or preclude that powerline EMFs affect human health.

3.1. Schwan and the Linear Model

Historically, Herman Schwan was the first physicist who sought to explain powerline EMF bioeffects on the basis of the laws of physics. His analysis led to the conclusion that powerline EMFs do not affect human health., and his work [see 3.1. note 1] still constitutes the most lucid explanation of the application of the physical thought-style to the issue of powerline-EMF health risks. It is the cornerstone and the substance of every subsequent opinion in which the physical thought-style was employed to rationalize the same conclusion. [see 3.1. note 2]

Schwan assumed a model for the interaction between EMFs 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 EMFs 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 EMFs produced by the powerline at the putative locus of interaction to the strength of the EMFs that were already present in the fluids or membranes. Schwan pegged this relationship at 1/100 to 1/10, and used it as a threshold for deciding whether or not the powerline EMF could cause a bioeffect. Below the threshold, the powerline EMF 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 EMFs 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 EMFs could occur only if the powerline EMF 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 EMFs 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 [3.1. note 3] that could be affected by powerline EMFs that penetrated the body. 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 EMFs induced by powerlines. The second possibility was that, in principle, the powerline EMF 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 EMF-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, Schwan calculated the magnitude of powerline EMFs that would be unsafe, and it turned out to be impossibly high. Any attempt to create an unsafe powerline EMF 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 practicing scientists 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, early in the 1950s, when Schwan first considered EMF health hazards, the data was consistent with the linear model. Microwaves, the form of EMFs initially studied by Schwan, were known as early as the end of World War II to be capable of cooking tissue and interfering with heart rhythms, and no other physiological effects were then identified.

Unfortunately, the success of the linear model in explaining these two effects encouraged 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 EMF 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. When new data appeared, Schwan ignored it or attacked it without mercy. [see 3.1. note 4]

Schwan's analysis of EMF health risks was reasonable in the 1950s, but demonstrably incomplete in the 1970s. In the 1990s, when used to conclude that powerline EMFs are safe, it is unreasonable because the number of studies whose results do not fit the linear model is vast, and their number is 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 fields and biological tissue, and such attempts are being made. [see 3.1. note 5] In the meantime, in order to resolve the question whether powerline EMFs affect human health, it will be necessary to evaluate the biological literature to assess what scientific and public-health conclusions follow from that literature.

3.2. Nonlinear Interaction Models

At the present stage of development of physical theory, the model that successfully (or best) explains EMF-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 EMF 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). [see 3.2. note1] 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 insure 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.

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 EMFs 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 different 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. [see 3.2. note 2]

The example of the lava lamp shows that, even though the linear interaction model does not explain EMF-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 general laws of physics.

Physicists have not determined what nonlinear model could be used to explain EMF-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 EMF-induced bioeffects.

The analysis presented here does not prove that EMF 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, with regard to these laws of physics, powerline EMFs could be a health risk. Physics simply can't say.

3.3. 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, it might be the case that they explain almost nothing about complex systems such as biological organisms. The inability to predict or preclude powerline EMF 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 usually confined to the context of closed linear systems. That is, systems that can be modeled linearly as if they do not exchange energy with their surroundings. In these instances, the laws of physics can explain and predict. The operation of automobiles, space ships, atomic bombs, and powerlines are all achievements of 20th century physics. But earthquakes, volcano eruptions, the weather, the activity in lava lamps, and the behavior of living things can not 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 [see 3.3. note 1] the laws of physics to them.

3.4. Theoretical Limit of the Physics Thought-Style

Some effort is presently being devoted to identifying the particular nonlinear model applicable to powerline EMFs, and the day may come when it is possible to satisfactorily explain or even predict some EMF-induced bioeffects. Even if **that** occurs, however, it will **still** be impossible to resolve certain kinds of crucially important questions concerning the health hazards of powerline EMFs 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 (that is, the initial conditions and the boundary conditions).

This normal process of physics is geared toward prediction because the ability to predict is what gives evidence of the ability to explain. But the method of physics is often useless with regard to attempts to explain what has already occurred. For example, it cannot be used to explain a specific observation recorded from a particular individual. In other words, 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 the conditions that **existed in the past are not known**. [see 3.4. Note 1]

3.5. Conclusion

This analysis showed that whether or not powerline EMFs affect human health cannot be ascertained within the physics thought-style. This fact does not imply that powerline EMFs are not a health hazard. Rather, it indicates only that the question cannot be answered if one chooses to think solely as a physicist thinks.

Although the hazards 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.

4. BIOLOGY AND POWERLINE EMF HEALTH HAZARDS

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

4.1. Introduction

There are two scientific methods for establishing scientific facts (Section 1). In principle, therefore, there are two ways in which scientific facts could be established that bear on the question whether powerline EMFs affect human health. The method of physics does not result in facts that materially support either side of the issue (Section 2). Here, I again consider the question of powerline EMF hazards, but in the context of the more general thought-style of biology.

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

Why do divergent opinions abound regarding the public-health significance of the EMF 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 EMFs affect 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.

4.2. The Biological Evidence

Biological evidence about the effects of powerline EMFs can come only from studies in which animals [see 4.2. note 1] or human subjects were exposed to electromagnetic fields and then observed to determine the consequences of the exposure. We expect that if it is true that powerline EMFs 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 EMFs can affect human health. Otherwise, we must conclude that no known evidence exists to support that inference.

The reported EMF 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 EMF exposure. For example, a group of investigators tested the hypothesis that exposure of lambs to powerline EMFs would alter melatonin patterns and thereby cause a delay in the onset of puberty. But lambs who lived under a 500kV powerline for 10 months did not exhibit detectable changes in serum melatonin patterns or onset of puberty. The investigators repeated the experiment, again with negative results, and argued that the studies were evidence against the theory that EMFs affect melatonin, which was a conclusion reached by other investigators who used **different** experimental designs. Whether or not it is justifiable, it is a fact that all EMF studies are viewed by some as dubious largely because of comparisons between negative and positive [see 4.2. note 2] studies in which a particular parameter was measured using different experimental designs.

A second reason for uncertainty regarding the implications of the EMF bioeffects studies is that there appear to be inconsistencies involving **similar** experimental designs within virtually every line of EMF biological research. A pattern has emerged during the last 25 years in which a report of an EMF 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. This 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 EMFs (1), [see 4.2.note 3] but others were unable to reproduce this effect (2). EMFs affected skeletal growth in chicks (3), but the same model system did not yield positive effects in the hands of other investigators (4). Sometimes EMFs affected growth rate of animals (5), but not in other cases (6). EMFs altered transcription (7) or not (8) in seemingly identical experiments performed by different investigators. EMFs were or were not associated with cancer (9,10), affected or did not affect melatonin levels in the blood (11,12), and did or did not induce a stress reaction (13,14), modify behavior (15,16) or affect cell growth in vitro (17,18), again depending on who conducted and evaluated the experiment.

The inter-experimental-design species of inconsistency (the species of inconsistency resulting from different designs) is not important for the simple reason that it takes no skill whatever to design and perform a study that finds nothing. I will not deal with this issue here, but will treat it in a later section dealing with trade-association science.

The issue I want to address here involves 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 is that the exposed subjects did not detect [see 4.2. note 4] the presence of the EMF, then the reports that failed to find a biological effect due to EMF exposure would reflect the objective state of nature. 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.

4.3. Possible Bases of Apparent Inconsistency

Early in the evolution of the dispute regarding whether powerline EMFs affect 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, and then spread to American and European investigators who reported EMF effects. Ultimately, however, as the EMF 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 EMF 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 EMF 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 EMFs, 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 EMF 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 EMFs 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: WE is greater than WC

Hypothesis No. 2: WE is less than WC

Hypothesis No. 3: WE is not equal to WC

Hypothesis No. 4: VE is greater than VC

Hypothesis No. 5: VE is less than VC

Hypothesis No. 6: VE is not equal to VC

Hypothesis No. 7: WE and VE are not equal to WC and VC

Suppose results supporting Hypothesis No. 1 and Hypothesis No. 2 were observed in two different studies. It could be argued (and has been argued) that the studies were inconsistent. 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 EMFs affected body weight - they differed only with regard to the direction of the change that was observed. Perhaps the thing that needs to be explained is why the two effects occurred, not why they occurred in opposite directions. Thus, the results are consistent or inconsistent depending upon one's attitude regarding the meaning of *consistent*.

Now consider an inconsistency between positive and negative reports, which is the classic case. This occurs when a study that tested Hypothesis No. 3 found results that supported it (that is, found that the average weight in the exposed animals was either greater or less than the corresponding weight in the control animals), but another similar experiment with the same hypothesis did not (that is, failed to reject the null hypothesis). In this case, the reports are inconsistent about their implications regarding the effect of EMF exposure on the average weight of the animals. The implication of the positive report would be that the EMF was somehow detected by the bodies of the exposed EMF animals, resulting in a change in the average body weight. The implication of the negative report would be that detection of EMFs did not occur because, if it had occurred, the results would not have been negative.

There is a possible state of nature regarding this case in which the implications of the positive and negative studies would actually be consistent with one another. Suppose in the study that was apparently negative on the basis of Hypothesis No. 3, the variance was viewed as the test statistic (Hypothesis No. 6) with the result that the study was positive (that is, the null hypothesis was rejected). The state of nature would be that the positive study was positive because W was altered by the EMF, and the study that was judged negative because W was not altered [see 4.3. note 1] would actually be positive because V was altered. Thus, the studies would be consistent because both would imply that the EMF was detected by the body.

I now want to show that in the actual EMF growth-rate studies, the apparent inconsistencies disappear when the hypothesis, purpose, and plan of the investigators are considered.

Further, the inference that the study was positive could be rationalized using an appropriate statistical test in conjunction with Hypothesis No. 7 even when **neither** the average alone nor the variance alone were individually sufficient for that purpose. For a discussion of an appropriate statistical test and its rejection regions, see the L test. [see 4.3. note 1]

4.4. Powerline EMFs 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 EMFs on the growth rate of mice. In each experiment, three generations of mice were exposed essentially continuously to EMFs under conditions designed to avoid artifacts that they perceived to be responsible for earlier positive results in experiments performed by my colleagues and me. [see 4.4. note 1]

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 EMFs 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 an EMF 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 EMF 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 EMF 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 EMFs.

The results of their first experiment showed that the average weight of both the male and the female mice were less than their corresponding controls (Table 1). [see 4.4. note 2] In the second experiment the average weight of the male and female exposed mice were significantly **greater** than the corresponding controls (Table 2). [seee 4.4. note 3] 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 EMFs can affect growth.

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 EMF, 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 EMFs were 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**.

I would interpret Phillips' studies [see 4.4. note 4] not the way he did, but rather the same way I interpreted my own studies. His data showed that powerline EMFs consistently affected the body weight of exposed animals, even though the effect could not be predicted in individual experiments.

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, [see 4.4. note 1] 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 EMF 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 EMFs 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 EMFs had been detected by the animal. It is plainly true that consistency in the mean is sufficient but not necessary to support this conclusion.

4.5. Beyond Linear

The difference between Phillips and me regarding our interpretations of our powerline EMF 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 EMFs - 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 EMFs would have been conclusively established as health risks and it would be unthinkable that the power industry would routinely carry out involuntary exposure to powerline EMFs. 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 EMF study. That state of the evidence would not prove that EMFs don't cause human diseases. It would prove only that a conclusive demonstration of powerline-EMF 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 EMF 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 EMF reports, other than the ones by Phillips or me, to assess whether they provided evidence that EMF 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 EMFs 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 EMF 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 EMF 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 3). [see 4.5. note 1]The studies involving effects of EMFs 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 [see 4.5. note 2] the idea that powerline

EMFs are detected by the body as manifested in a change in growth, even though the EMFs do not result in a consistent change in the average body weight.

With regard to the studies involving the effects of powerline EMFs on body weight, therefore, if the hypotheses, purpose, and plan of the investigators is 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 EMFs and changes in body weight. [see 4.5. note 3]

4.6. The Nonlinear Model and Consistency of EMF 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 EMFs 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 EMF-exposed eggs, **even though** that result was not obtained in each laboratory (Table 4). [4.6. note 1]

The sponsors of the international cooperative effort that led to the data in Table 4 [see 4.6. note 1] went to extraordinary lengths to insure 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 [Clinical and *In Vivo* Laboratory Findings, *NIEHS* April 6-9, 1998, pp. 104-105] subsequently developed holding that effects of EMFs on skeletal development in chicks is not important for the purposes of evaluating potential health hazards of EMFs, even though that was largely how the studies were initially justified. 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 EMF 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 EMFs 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 EMFs was different under different circumstances. Goodman's studies elicited much interest because they suggested a link between the powerline EMF issue and orthodox molecular

biology. However, Saffer and Thurston [see 4.5. note 2] conducted similar studies and found results that they said refuted Goodman.

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 EMFs were 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 EMFs had a significant effect on Ca2+ in a system involving *in vitro* exposure of parts of animal brains to EMFs. These studies were the impetus for Albert and his colleagues who conducted a similar series of experiments. They compared the average value of Ca2+ in exposed and control dishes containing brain tissue, and found no consistent change in average value in a series of 7 experiments (Table 5). [see 4.6. note 3] They interpreted this data to indicate that the EMF exposure had no significant effect on Ca2+, a conclusion that was apparently inconsistent with the findings of Adey and others. However, the data can be analyzed using the L test [see [4.3. note 1] to assess whether EMF exposure caused any change in Ca2+. The results indicated that EMF 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.

Apparent inconsistencies have also been manifested in human studies. In 1966, Howard Friedman and Dr. Becker studied the effect of EMFs 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 EMF significantly affected reaction time performance. In 1995, Podd and colleagues repeated the experiment, and concluded that the EMF 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 EMFs 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 EMFs affected human reaction time (Table 6). [see 4.6. note 4]

A final example of how the EMF 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 EMFs 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 7). [see 4.6. note 5]

It is unnecessary to labor further regarding the point that intra-experimental-design inconsistency in EMF 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 EMFs were consistently detected by the various biological systems that were studied. It is simply not possible to gloss over the existence of this consistency.

4.7. 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 EMF 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-EMF bioeffects can be nonlinear does not entail that no EMF 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 EMF 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. One way this can be accomplished is by use of the L test. [see 4.3. note 1]

4.8. Biological Generalizations Generally

The human-health implications of the fact that powerline EMFs 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-EMF health hazards.

Two hypothetical examples are sufficient to show the importance of subjective considerations [see 4.8. note 1] in the formation of biological generalizations. First, consider the conclusion that decreased cyclin-E/CDK2 activity (Section 1, 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 (Section 1, Table 1). Assume that exactly the same change in blood flow occurred when subjects were exposed to powerline EMFs. 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 EMFs 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 EMFs 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 EMFs affect human health.

4.9. The Generalization About Whether Powerline EMFs Affect 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*. Thought-groups may be large such as the groups consisting of radiation biologists, immunologists, microbiologists, or biochemists, or they may be small such as NIH study sections or blue-ribbon committees charged to decide whether powerline EMFs affect human health.

The investigators who performed EMF studies while employed at Battelle comprise a reasonably well-defined thought-group regarding EMF biology, because perusal of their estimated 500+ EMF 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 EMF animal interactions can help determine the extent to which EMFs may be a health risk. They think that whether or not EMFs are 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 EMF 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 (Figure 1). [see 4.9. note 1] No Battelle investigator has publicly opined that powerline EMFs affect 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.

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 EMFs affect human health (Table 9). [see 4.9. note 4] 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 EMFs affect human health.

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 EMF 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 [see 4.9. note 5] they provided me regarding powerline EMF proposals that I submitted. Perusal of the critiques [see 4.9. note 5]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 EMF-induced bioeffects. It is unthinkable that the Radiation Study Section would accept the statement *powerline EMFs affect 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 NIH), not on scientific principles (there is no reason to believe that their opinions are objectively correct, or broadly acceptable to non-radiological scientists).

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 8). [see 4.9. note 2] 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 EMFs affect 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 2).[see 4.9. note 3] Whether or not powerline EMFs affect 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.

This analysis shows that a group judgment regarding whether powerline EMFs affect 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.

4.10 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 EMFs affect 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 EMF bioeffects did not exist, then all of EMF biology would be a chimera having no meaning or significance within the framework of science. An assertion that powerline EMFs affect human health would, in that case, be entirely vacuous. On the other hand, because the available evidence clearly shows that EMF 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 EMF 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 EMF-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 EMFs 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 EMFs affect 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 EMFs 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 EMFs 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. [4.10. note 1]

I think it is clear that before deciding the substantive issue regarding whether powerline EMFs affect 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.

Each of us in our daily lives makes myriad decisions on the basis of incomplete and less-than-conclusive evidence. The legislators, executives, and judges whose decisions shape our society do the same. It would be amazing, I think, if most people expected that the evidence regarding powerline EMFs and human health should be conclusive or near conclusive, while accepting evidence that is far less than conclusive in decision-making generally, as well as in decision-making that specifically utilizes scientific data.

As examples, evaluation of the efficacy of drugs and medical devices, the safety of drinking water, the utility of mammographic screening, the risk from pesticides, the side-effects of drugs, the link between cigarettes and cancer, and the role of cholesterol in heart disease are typically based on 95% studies and an evaluation of the significance of the studies according to a standard that is far less than conclusive. There is no rational reason to treat a putative link between EMFs and health effects differently from the other cases where decisions are made in the public interest using scientific data. Whatever the rules are for using scientific data to make judgments that affect society generally, I think it should be the case that there is only one set of rules, and not different rules when different issues arise or where different parties are interested in the outcome.

4.11. The Proper Choice

The fact that the biological evidence consistently shows that powerline EMFs are detected [see 4.11. note 1] by the body raises the possibility that powerline EMFs affect 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. [see 4.11. note 2] The consequences of an erroneous decision are truly significant for the people who are involuntarily exposed to powerline EMFs, 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.

5. EPIDEMIOLOGY AND POWERLINE EMF HEALTH HAZARDS

A fair and honest doubt exists about the safety of powerlines growing out of the EMF epidemiological studies.

5.1. 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 EMFs [see 5.1. note 1]* was performed by Dr. Becker in the early 1970s. He found an association between environmental EMFs and cancer, and interpreted it to generally support the stressor hypothesis regarding the mechanism of action of EMFs. Subsequently, many hundreds of studies were performed and interpreted to support many different opinions concerning the health menace of powerline EMFs. In this Section, I will describe how the EMF epidemiological studies were performed and evaluated. I will show that the scientific meaning and public-health significance of the EMF epidemiological studies depends entirely on the evaluative criteria utilized to individually and globally assess the studies.

5.2. 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 several thousand applications that were made to the 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, EMF epidemiological studies never used randomization of subjects because a randomized trial to assess whether EMFs affect human health is ethically impermissible.

The lack of randomization in EMF epidemiological studies had serious consequences with regard to what could validly be inferred. For example, suppose that the risk for cancer in an EMF-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 EMF exposure was a cause-effect relationship, or as 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** EMF exposure and cancer, and that this factor, not the EMF, was the true cause of the disease. If that **were** the case, then the correlation between EMFs 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. [see 5.2. note 1] 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 EMF 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 [see 5.2. note 2] regarding this judgment.

5.3. 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 EMF 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 EMF epidemiology studies.

5.4. EMF 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.

5.5. Absence of Hypotheses in EMF 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 [see 5.5. note 1] as a surrogate for EMF exposure was provided by measurements showing a relation between field strength and the coding system.

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 EMF 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 [see 5.5. note 2] they chose a control group that contained dead subjects. Again, they stated no explicit hypothesis but the hypothesis actually tested by the statistics they employed was whether EMF 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 EMF 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 [see 5.5. note 2] based in Seattle, no significant relation between acute non-lymphocytic leukemia and EMFs was observed. Although the authors used the WL wire codes for identifying EMF 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 EMF-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, [see 5.5. note 2] a unique coding system for identifying the presence of magnetic fields was used, and no link with childhood leukemia was found. 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, [see 5.5. note 2] childhood leukemia was considered in relation to magnetic fields as indexed by the WL codes, 24-hour measurements, **and** spot measurements. 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, [see 5.5. note 2] the cases were subjects who had either benign or malignant tumors, and controls were chosen from birth records. The magnetic field at each residence was measured and a unique system for coding for the presence of EMFs 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, [see 5.5. note 2] dead or diseased subjects were used as controls. 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 EMF exposure as indexed by residing within 50 m of a powerline.
- Leukemia cases were not affected by EMFs (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 EMFs, 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 EMFs, compared with the risk of dying without cancer.

- White men who were ever exposed to EMFs 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 EMF 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 EMF

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 EMF 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 EMF-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 EMF exposure in the healthy population.

Epidemiological studies that employed a cross-sectional design [see 5.5. note 2] constitute another group of non-hypothesis EMF epidemiological studies whose theoretically possible hypotheses seem irrelevant if the goal is to reasonably estimate human health risks due to EMFs.

5.6. 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 EMF exposure, to determine how much EMF exposure was received, and to determine who received other potentially important exposures. None of these goals were achieved in any EMF 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, [J.T. Fulton, S. Cobb, L. Prevle, L. Leone and E. Forman: *Am. J. Epidemiol.* 111:292, 1980]occurrence of EMF exposure was predicated on the basis of mathematical calculations that seem hopelessly uncertain. In another English study, [M.P. Coleman, C.M. Bell, H.L. Taylor, M. Primic-Zakelj: *Br. J. Cancer* 60:793, 1989]the surrogate for EMF exposure was so bizarre that less than 1% of the study subjects were exposed.

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 [M. Linet, E. Hotch, R.A. Kleinerman, L.L. Robison, W.T. Kaune, D.R. Friedman, R.K. Severson, C.M. Haines, C.T. Hartsock, S. Niwa, S. Wacholder and R.E. Tarone: Residential exposure to magnetic fields and acute lymphoblastic leukemia in children, *N. Eng. J. Med.* 337:1-7, 1997]examined the relationship between powerline EMFs and acute lymphocytic leukemia in children, and concluded that the study results "provide little evidence" of a link. 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 EMFs 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 EMF 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 EMF 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.

5.7. Epidemiological Criteria for Causal Association

Because the EMF 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 EMF epidemiological literature. Unfortunately, the criteria often applied to evaluate the studies do not fulfill the obvious need for objectivity.

5.8. 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.B. Hill: The environment and disease: Association or causation? *Proc. R. Soc. Med*. 56:295-300, 1965]a set of criteria (Hill's criteria) that he suggested could serve to help evaluate the causal role of **any** environmental factor. The criteria first appeared 11 years earlier [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] in a little-known paper whose author listed them in an attempt to explain why he concluded that smoking and cancer were causally related. Essentially the same criteria appeared again in 1964, and for the same reason, in the famous Surgeon General's report [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] linking smoking and cancer. 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, 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 EMF 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 EMF 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 can not 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 EMFs 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 EMFs and cancer probably meets this definition of plausible because there exists evidence showing that EMFs 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 EMFs and cancer cannot meet this definition of *plausible*.

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 EMF epidemiological studies seems dubious.

Thus *plausible* can become (and has become in the case of EMF 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 [see 5.8. note 1] satisfied [Nothing remotely resembling a coherent set of rules: has emerged for judging what is or is not plausible. See "Biologic plausibility in causal inference: Current method and practice," *Am. J. Epidemiol*. 147:415-425, 1998].

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 EMF epidemiological literature. They have been employed to describe opinions about the public-health significance of EMF epidemiological studies, but there is no case where Hill's criteria were used to justify or explain an opinion regarding the significance of the EMF epidemiological studies.

5.9. Conclusion

The EMF 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 EMFs 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 EMF epidemiological data has no meaning.

What is needed is an evaluation of the methods and procedures of EMF epidemiology, irrespective of the results in particular studies, and a determination whether the data from such studies will be deemed acceptable for forming judgments [see 5.9. note 1] regarding whether powerline EMFs affect human health. 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 EMF experts might conclude (and justify) that no conceivable results of EMF epidemiological studies are worth considering. Any such conclusion regarding the EMF epidemiological studies would require examples of epidemiological studies that the committee **would** consider applicable [see 5.9. note 2] to the problem of evaluating cause-effect relationships involving environmental factors. 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 EMF epidemiological data with regard to the issue whether powerline EMFs affect 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. [see 5.9. note 3] It is possible, of course, that reasoning principles elucidated as an explanation and justification for why and how the EMF 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 [see 5.9. note 4] of controversy regarding the epidemiological significance of powerline EMF studies. Personally, for two reasons, I am persuaded that the EMF epidemiological studies show that powerline EMFs 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 EMF HEALTH HAZARDS

The possible public-health menace of powerline EMFs cannot be reliably evaluated by non-representative experts in a consensus-seeking process.

6.1. EMF 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. 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 disease. Why did **this** person develop **this** disease 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 from where people live or work. The possibility that powerline EMFs could be this kind of a cause has been with us since at least the 1970s. In response, from time to time, various kinds of experts formed committees to evaluate the evidence and offer an opinion to the public about the health hazards of EMFs.

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 EMF health hazards was the goal of seeking a **consensus** among the committee members regarding the meaning of the scientific evidence.

The first EMF blue-ribbon committee was appointed by the United States Navy [see 6.1. note 1] to evaluate potential health implications of animal studies conducted to assess the impact of a large antenna proposed for construction in Michigan. The antenna's EMFs were similar in some respects to

those of powerlines, although far weaker. The committee, which included Dr. Becker, met in Washington, DC on December 6 and 7, 1973, and then issued a report evaluating the data provided. The general tone at the meeting was surprise at the many different kinds of biological changes apparently caused by the EMFs used in the studies. The committee reached no conclusions regarding the safety of the antenna's EMFs, but it was concerned about the health implications of EMF exposure, particularly with regard for what it said was a large population at risk because of powerline EMFs.

In 1976 a second committee was appointed under the auspices of the National Academy of Sciences [see 6.1. note 2] (NAS) to evaluate the health implications of the same antenna. The NAS committee, whose most prominent member was Herman Schwan, concluded that the antenna's EMFs would not cause a significant and adverse biologic disturbance. The committee said it could not identify with certainty any specific biological effects that would definitely result from exposure to the antenna's EMFs.

In 1984, the American Institute of Biological Sciences (AIBS), [see 6.1. note 3] Arlington, Virginia, conducted a third review of the potential health risks of the antenna's EMFs. The AIBS committee unanimously agreed that EMFs can cause a variety of biological effects, but that it was unlikely that the antenna's EMFs would lead to adverse public-health effects. Also in 1984 a blue-ribbon committee connected with the World Health Organization (WHO) [see 6.1. note 4] issued a report dealing with health hazards of powerline EMFs which concluded that it was not possible to make a definitive statement about health hazards of powerline EMFs.

In at least two instances, the health risks of powerline EMFs were evaluated by self-organizing committees. In 1995, the American Physical Society (APS) [see 6.1. note 5] issued a press release that said there existed no consistent, significant, and causal relationship between exposure to powerline EMFs and cancer.

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 EMFs produced by the company's powerlines. Fourteen physicists, [see 6.1. note 6] including 6 Nobel Prize winners, intervened in the case and submitted a friend-of-the-court brief supporting the position of the power company. They concluded that the scientific evidence strongly indicates that it is not scientifically reasonable to believe that powerline EMFs increase the incidence of cancer.

In 1997, a 16-person committee [see 6.1. note 7] sponsored by the National Academy of Sciences concluded that there was no conclusive and consistent evidence of health hazards from powerline EMFs.

The most ambitious attempt, by far, to extract consensus regarding the health hazards of environmental EMFs was carried out by the NIEHS. The effort consisted of multiple tiers of blue-ribbon committees that evaluated specified areas of EMF bioeffects studies, and a super committee, the Working Group, [see 6.1. note 8] that provided an overall assessment of all possible health effects of powerline EMFs. Based largely on this report, the Director of the NIEHS shall inform Congress by November, 1998, whether powerline EMFs affect human health.

The activities of the EMF 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 EMF blue-ribbon committees, whether intended or not, the public profile regarding environmental EMFs 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 EMF health risks.

But, in several important ways, the blue-ribbon-committee approach to evaluating EMF health risks failed. First, no EMF 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.

Second, the committees failed to recognize the basic nature of the EMF-induced bioeffects that are pertinent to the issue of health hazards from environmental EMFs. 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 EMF blue-ribbon-committee approach failed merit consideration so that a reliable mechanism for making good public-health decisions regarding environmental EMFs can be designed at some future time. My goal in this Section is to explain the failure of the EMF 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.

6.2. The Appointment Process

If all the experts qualified to answer the EMF 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 not be valid. 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 EMF blue-ribbon committee members were appointed were not disclosed, it seems

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 EMFs. 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 the presence of three power-company experts made it clear that the selection process was not random. The members of the 1984 WHO committee were nominated or appointed by the power companies of the countries that had representatives on the committee. As best I can tell, the 1984 AIBS committee was appointed 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 EMFs 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 EMF 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 EMFs affect 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.

6.3. Qualifications

The officials who appointed the EMF 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 EMF expert was. Similarly, when Portier appointed the NIEHS Working Group, he must have had in mind what he thought an expert in EMFs was. But neither Handler nor Portier, nor any official who appointed an EMF 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 EMFs.

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 EMFs were "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 EMF 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.

6.4. The Politics of Appointment to EMF Blue-Ribbon Committees: A Case Study

In early 1976, after Herman Schwan had filed his testimony on behalf of the power companies in the legal dispute, [see 7.1. note 1] I learned that he had been appointed to the 1976 NAS EMF 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 [see 6.4. note 1] that EMFs up to 100,000 times stronger were safe. 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 EMF 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." I learned from Hastings 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 EMFs. 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, EMF 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 EMF therapies. The gist of the power companies' position was that EMFs 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 (actually, the same power company for which the expert was testifying in the legal dispute). [see 7.1. note 1]

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 legal dispute [see 7.1. note 1] which had begun to consume my professional career. As a final, ending statement, however, Dr. Becker and I sent a statement to the NAS committee [see 6.4. note 2] in April that formally stated our experiences and our opinions (because my contacts with Hastings had been off the record).

I did not realize that our statement to the NAS committee would be a public document. Even if I had, I would not have guessed that anyone would be interested in it. 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. The *Science* report appeared in June, 1976. Soon thereafter, 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 EMF 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* [see 6.4. note 3] 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 [see 6.4. note 4] that he demanded be published, in which he called me everything but decent. I thought that publishing the manuscript was a good idea, [see 6.4. note 5] because the manuscript supported my contention that the NAS committee was pre-programmed to reach the conclusion it ultimately reached. But, in the end, the editors decided not to do so.

What is the point? When Handler appointed the 1976 NAS EMF 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 EMF

committee, and the 1998 NIEHS EMF committee, and each of the other EMF blue-ribbon committees, with the exception of the first one.

What makes the 1976 NAS EMF 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 EMF committee, and I think that's what happened in the other cases.

6.5. Rules and Procedures of EMF Blue-Ribbon Committees: A Case Study

The final reports of the EMF committees contained scanty information concerning the evidence considered by the committee members, who found and presented the evidence, whether the committee members meet face-to-face, how often they met, and the method and procedures followed in reaching their conclusions. In the case of the self-appointed EMF blue-ribbon committees the absence of documentation and detail was almost absolute. Nevertheless, all the evidence available to me suggested that the process was not pretty - that if the public saw what went on, they would not be pleased with how policy decisions that affect public health were made. My first opportunity to directly observe the activities of an EMF blue-ribbon committee while it was deliberating occurred recently. It wasn't pretty.

In April, 1998, I served on three of the NIEHS blue-ribbon committees at a meeting held in Phoenix, AZ. The meeting lasted three days, and I was on a different committee each day. After each daily session, I dictated notes regarding the activities of the committees to describe what was going on and how decisions were being made. What follows are those notes, unvarnished and unchanged except for the removal of some irrelevancies. I made no effort to edit or rearrange them into a chronological sequence or to make them committee-specific because my purpose then (and now) is to convey a sense of **how** the committees functioned, not to chronicle their deliberations. The notes contain my opinions because it was impossible for me to describe what was going on without including them.

NIEHS Blue-Ribbon Committee Meeting

April 6-9, 1998

Hyatt Regency at Civic Plaza

Phoenix, AZ

The seating was a square arrangement of tables, with the committee members assigned to specific locations. The chairman sat on one side, flanked by two committee members who had been assigned to write a contemporaneous record of the committee's deliberations. In most meetings they would be called secretaries, but here they were called rapporteurs, and the word was always spoken in French. The chairman and the rapporteurs occupied one entire edge of the table arrangement, thereby creating a teacher vs. student spatial arrangement.

Each committee member's seat was marked with a large sign that gave his name and affiliation. Both pieces of information were written on each side of a placard, which was folded in the middle so that it set well on the table and could be read by all the other committee members, regardless of where they

sat. Each placard also carried the logo of the NIEHS. The placard did not indicate, however, anything about the background or professional status of the committee members, most of whom were strangers to me. They could have been Ph.D.'s, M.D.'s, D.V.M.'s, or in the case of the foreign members of the committee they could have had still other kinds of academic degrees. The placard also didn't specify what the committee member did at the place where he worked. It would have been nice to know where they stood at home.

The attention to detail at the meetings was impressive. Very little was left to chance. For example, to make sure that scientists assigned to particular committees went to the right room, little colored dots were placed on each participant's name badge and the corresponding colors were posted on the wall outside the rooms where the committees met. The meeting rooms all had names, of course, but just in case we couldn't read we could always follow the colors.

The NIEHS provided an unlimited supply of coffee, cokes, tea, and bottled water, and a limited supply of muffins and cookies, and a $39 a day food allowance. The chairman and the rapporteurs got an additional $150.

The big dog was Christopher Portier, who was appointed by the NIEHS director to run the overall evaluation for the RAPID program. Portier was, basically, a mathematician who specialized in analyzing data risks from various pollutants in the environment. Portier seemed well-suited for the job from a public-relations point-of-view. He projected an aura of keen interest in his task, someone who would listen attentively to the various points of view, but someone who would not act imprudently or allow himself to be seen as a champion of one or other viewpoint.

Portier is a small thin man, with a ring of thick brown curly hair that girds his head like a halo. He looks like a balding Leonardo de Caprio. The default position on his face is a mild smirk, mediated by a slight curve of his mouth on the left side. The expression is most pronounced when he is confronted with an issue that he had not previously considered. In such a situation, he says nothing, he only smirks.

Portier is a smart man in at least two ways. He seems to be able to read science articles in fields beyond anything he has previously experienced, and to appreciate the important points being raised in the report. Second, he is politically aware. As a member of the federal bureaucracy, he must cooperate with an alphabet soup of other federal agencies that have some connection with the EMF area. It's a matter of comity. Portier might think, for example, that the representatives from the Department of Energy are in bed with the power industry, but nevertheless, he must treat them like colleagues and equals. He does the job admirably, never betraying his true feelings about the scientific sophistication of the sister federal agencies.

My impression - and it is only an impression because Portier has a zipped lip on the topic - is that he has a deep and abiding contempt for the research program of the Electric Power Research Institute. This is a common theme throughout the community of EMF investigators. Even so, the 1992 Energy Policy Act requires that the NIEHS cooperate with the power industry in seeking the truth about health hazards of EMFs. Consequently, Portier's hands are tied. The power company position, and its particular spin on the scientific evidence, is inside the tent.

Portier is a committee guy. Sometimes he calls them committees, sometimes he gives them other names, but he operates by appointing advisory groups whose chief role is to inform him, and act as his sounding board. Within the NIEHS, Portier has what appears to be quite a small staff to orchestrate his inquiry. Several other persons from outside NIEHS but inside the federal government are usually listed as part of Portier's staff. But taken together, the group is largely for logistical and tactical purposes, and

has no responsibility to directly evaluate the question posed by Congress.

Portier created a process to review and evaluate the evidence about EMFs. He asked for people's opinions about how to do that. Portier is always asking for people's opinions. There's actually little evidence that he accepts advice, but he is constantly in an advice-requesting mode.

Portier divided the EMF studies into three main areas, each of which was the subject of a symposium where scientists came to reason together. Each of the main areas was subdivided into sub-areas, resulting in the formation of about 30 committees, one for each sub-area. Some sub-areas were broad (brain cancer), some seemed important but hard to define (immunotoxicity), and some were obscure, and possibly irrelevant (cell calcium).

Portier, insofar as I can tell, decided essentially by himself who would be invited to the three symposia and appointed to the committees. The symposia themselves were completely open - O.J. Simpson or Monica Lewinsky could have come had they chosen to do so - but only the people chosen by Portier were on the committees.

Why would anybody serve on a committee? Well, it was an all-expense-paid trip to a nice location, just to talk about science, and no work was required. Second, like me, many of the invitees were NIEHS grantees. Having received from several hundred thousand to more than a million dollars to perform research, it would seem downright ungrateful and I think just plain wrong to refuse to attend the meeting. Third, particularly for the foreign scientists, one can imagine how honored they must have felt when asked to come to the United States and participate in the symposium.

Portier not only invited foreign scientists, he placed them in control of important parts of the various symposia. It was not unusual, for example, for someone from Finland or Switzerland to be placed in charge of a session, or for someone from Quebec to be afforded a prominent role in shaping the discussion of a particular scientific point. Portier could be certain that the foreign scientists would not complicate his life by stirring up the pot of competing interests regarding EMFs that exist in the United States. In most other countries in the world, I think that the question posed by Congress must look funny. No country on earth is concerned about the health hazards of EMFs as much as is the United States. No other country has spent as much money, litigated and politicked as much, published as many scientific reports, or, generally speaking, gotten as exercised by what's going on. There seems to be something peculiarly American in the idea that it's wrong for companies to save money by involuntarily exposing people to electromagnetic fields on the basis of a claim that the practice is safe, in the opinion of the scientists who work for the industry. There is no significant opposition to this idea any place outside the United States, except perhaps Sweden which is the only country where the quality of the EMF research exceeds that of the United States.

The foreign scientists seemed me to view the EMF question more like an academic exercise, than as an exercise to ascertain what is or is not safe for the American public. The foreign scientists seemed to laugh a lot and take things in stride, as if there were nothing serious going on.

Prior to the meeting, each invited scientist was given a list of scientific articles that Portier considered suitable for inclusion in the judging process. No one actually said that articles other than those chosen by Portier **couldn't** be considered. It was simply that, from a practical viewpoint, it would have been mighty tough to do that. And pointless. About 10-20 articles were provided for each of my three committees.

Portier designed a clever organizational scheme to control the symposia and keep the scientists moving

in the direction he desired. Generally, trying to control scientists is like trying to herd cats, but Portier did a good job. At each of the symposia, there were multiple committees (he dubbed them BOGs) meeting simultaneously. Each committee provided a report that detailed the workings of that particular committee. Portier chose the committee chairmen and the rapporteurs.

What was the meeting like? We sat around the table like a bunch of children at a back yard barbecue for our grandfather, Portier. When he spoke, we all listened. When he wanted something written down, it was written. When he wanted to discuss something in detail, we talked about it endlessly. When he was finished, we were finished. Nothing about the discussion would knock your socks off from a scientific perspective. "Have you measured such-and-such?" one member would ask another member. "Yes, we measured it but there was no effect." "Okay, write down 'no effect'," Portier said, and we moved on.

The process had no discipline whatever. Most members who spoke, were obviously reading reports at the meeting for the first time. There was simply no proportion between the seriousness of our endeavor and the process by which it was carried out.

Portier himself didn't chair any of the BOGs. But he attended many of them, and when he was present, he dominated. It was like being in a room with the President or the Pope. The other scientists in the room would address questions to him about particular reports or about scientific procedure. If he wanted something noted, it appeared in the report. If he wanted something omitted, it was omitted.

It was easy to understand why a sense of obsequiousness pervaded the room when Portier was present. Who was there? People from his staff. People from his advisory committee. People from other federal agencies. Foreign scientists. Industry scientists, and a handful of other scientists who admittedly knew almost nothing about the subject area of the meeting. Portier was in a position to get exactly what he wanted from that meeting. If he had wanted us to take a stand against beer in cans, I think that would have been a consensus position.

Portier is full of contradictions and inconsistencies. He said he didn't want the BOGs to be consensus-seeking committees, but rather committees that would provide him with a full range of opinion on a particular point. Nevertheless, every signal he sent was to the effect that we were to create a consensus of the scientists present.

In response to a question, Portier said that most of the chemicals that are presently recognized by his own agency as being harmful to the immune system do not have a well described mechanism of action. Thus, something can be harmful without being understood mechanistically. Nevertheless, Portier organized the three symposia such that there was a tight link between the question of mechanisms of EMFs (of which there are none known) and the question of whether EMFs affect human health. The gist of his strategy was to suggest that knowledge of mechanisms was somehow important in judging health risks, and that a firm conclusion regarding the latter couldn't be made in the absence of knowledge of mechanisms. It's hard to escape this conclusion, because the term mechanism was the second most frequently used word at the symposium (after "inconsistent" as in "inconsistent results"). I was particularly offended by this aspect of Portier's plan because it seemed to me to be hypocritical. Before he was elevated to the guru of EMFs, Portier did statistical analysis of data mostly associated with immunotoxicology. In that field, there exists an operational methodology for determining whether something is harmful. Now, when we ask the same question about EMFs, Portier simply ratchets up the degree of empirical evidence that's needed to conclude that a factor is a risk (now, we need at least some knowledge of mechanisms, he seems to suggest).

Another gross inconsistency manifested by Portier was that between his objective veneer and subjective attitude about how to make scientific decisions. At every meeting he organized, Portier gave a long detailed talk about his views of risk assessment, always accompanied by a handout that contained hard copies of his slides. They depicted an orderly, objective, rigorous decisional process. But, as if to temper this hard edge, on one of the slides Portier wrote "Experiments don't speak for themselves, we have to interpret them." Great. How true. But how do we interpret them? At one point, during the discussion of a particular paper in which an investigator had reported a statistically significant biological effect due to EMFs, Portier said "It's not enough to find effects, the effects have to be something you believe."

Portier said that he "wants to capture diversity of opinion", but how do you do that when you choose who is attending the meeting, tell them what to consider, and arrange for the people who will write the history. What you get from that process, I think, is what you want.

At one meeting I asked Portier the following question. "Suppose we had two studies, one of which showed that the measured parameter was statistically significantly increased due to EMF exposure, and a second independent replicate that showed the opposite result (statistically significant decrease). Are those results inconsistent?" "Yes, to me those results are inconsistent," he said. "Well", I said, "suppose my hypothesis was that EMFs affected the parameter, and that I had no hypothesis whatsoever regarding the direction of the effect? That is, my idea is that EMFs will be transduced and that, because the system is nonlinear, the dependent variable may be increased or decreased (because of sensitivity to initial conditions commonly found in nonlinear systems). Wouldn't you agree that, with this model and this hypothesis, that if the postulated results were observed, then the results should properly be labeled 'consistent.'?" Portier thought for a few moments and then said "Let's put Andy's concern aside and go on." Portier said he was doing it because my question was "too theoretical." But it wasn't theoretical, it happens all the time.

Throughout the sessions, confusion was obvious regarding both scientific reasoning within the context of particular studies, and how the results of groups of studies were to be generalized for the purpose of drawing an overall conclusion. Confusion regarding the supposed importance of a dose-response relationship as a criteria of validity of studies was a major problem. The relationship between the establishment of a mechanism and the establishment of validity of empirical data was another major problem. Many of the committee members were sensitive thoughtful persons, although many I thought held views regarding scientific reasoning that could have been shot down flat in an open debate. Nevertheless, they held these views sincerely and with an open mind. Real dialogue, however, never took place because it would have been unseemly to disagree with one of your colleagues, and downright rude to tell him why you thought he was dead wrong.

The meetings contained a cross-section of scientists, most of whom were narrowly educated in a particular specialty, and almost none of whom were in a position to see the big picture. Each of them was like someone in Plato's cave, chained in such a way that they could see only two-dimensional shadows, and not the three-dimensional reality that gave rise to the shadows.

When confronted with the problem of how to find scientific truth about public-health hazards of EMFs, Portier's first step should have been to analyze all previous attempts to accomplish this task, with an eye to discern why those attempts failed. One needs to talk to Portier for only 5 minutes to recognize that he has only the dimmest idea of the history of the problem he is attempting to solve. Being unaware of all of the previous mistakes, he was fated to repeat them.

One day, the Director called Portier and told him that he was to carry the water on the EMF project. It must have been an intoxicating day for Portier, but when he sobered up he must have realized that Congress is a political animal, that the laws passed are in response to political pressures, and therefore that there was a history regarding the issue with which he was charged. If he had paused and asked why, for the first time in the history of the Republic, has Congress taken such a step?, I think he might have evolved a decisional scheme that was more prudent and more reasonable. In particular, if he had looked at the reasons for the failures of the other blue-ribbon committees, he could have remedied them in the process that he was to design and implement.

What could have been done? First, there needs to be a recognition that there are going to be winners and losers at the end of the day when the question whether EMFs affect human health is answered affirmatively or negatively. If it's answered affirmatively, clearly that will cost the power companies money. Not only will they be required to widen some rights-of-way, they will probably be sued by people whom they have exposed to EMFs. On the other hand, if the question were answered negatively, then the people who live beside powerlines are simply going to have to live with that situation. Some will get sick because of it. They may not like it, but they will be stuck with it. Now, common sense tells you that in any situation where there are going to be winners and losers, there needs to be some procedure whereby people on each side of an issue can challenge the reasoning of the other guys.

Lawyers call the process cross-examination. It doesn't have to be that formal, but a winners-and-losers situation requires some recognition of the inherent adversarial nature of the situation, and an opportunity for one side to attack the spin on the evidence produced by the other side. The alternative to an adversarial process is a consensus process, and we already know that consensus processes don't work in the EMF area - that's why Congress wrote Section 2118 of the 1992 Energy Policy Act.

What did Portier do? He brought together a handful of scientists, many of whom were out of place in the BOGs to which they were assigned, and forced the process toward a consensus. As I sat there I thought why the hell should I talk? This guy doesn't want to hear what I've got to say. All of the papers that Portier assigned me to read had, in my view, severe inferential limitations. I kept thinking I wouldn't want some government committee making a decision on scientific evidence of the kind that might affect **my** family on the basis of a loosey-goosey generalized discussion by people who manifested various degrees of preparation, and who were never asked to explain why they had a particular opinion. In the committee meeting, it was impolite to ask someone to explain the basis for their opinion, and it rarely happened.

Portier's process was deja vu for a second reason. Past blue-ribbon committees usually summarized their work using terms that sounded definitive and clear to the layman, but which, on analysis, were quite the opposite. For example, there is no "convincing" evidence that EMFs are health risks, or EMF health risks have not been "proven", or EMF effects are not "robust" or "cause-and-effect relationships" have not been demonstrated. These simple-sounding terms are profoundly complex, and highly subjective. It's quite possible that I could be "convinced" that EMFs were health risks by certain items of evidence that are not sufficient to convince a power company scientist or stockholder. Portier repeated all of these mistakes in the documents and guidance that he provided to the symposium participants, and he even added some additional terms that had not appeared prominently in the EMF dispute. One example is "immunotoxicity." Is something immunotoxic if it causes **any** change in immune systems of animals? Only if it **reduces** the endpoint? Is it necessary to go further and show effects of EMFs on host-resistance endpoints? If the EMF caused a change in an immune endpoint, would that justify concluding that it "affects human health"? If not, what would be the state of the

immunotoxicology literature that **would** justify that conclusion?

Another word that was used a lot which seems at first glance to have a specific meaning but ultimately turns out to be subjective was "abnormal." No one could define the term in any meaningful or objective manner, and there was never any consensus regarding what it meant in the context of laboratory studies. Even the significance of the distinction normal-abnormal was obscure. Should investigators search for EMF bioeffects under the assumption that if such bioeffects occur and can be imputed to human beings, then it will be **assumed** that they are adverse for the subjects? Alternatively, as the power-company spokesmen argue, should the committee members be looking for biological effects in animals? Then, if they are found, determine which of those effects are abnormal? Then, determine which of those abnormal bioeffects can be imputed to exposed human subjects? No one seems to know.

There was much confusion regarding the argument that the observed effects were "small." The term had different connotations in different BOGs. Sometimes it referred to the difference between an exposed and a control group in relationship to the difference between control groups from different laboratories where the experiment was replicated. In other cases, it meant that the measured parameter was within the range of measurements that is ordinarily considered to be normal for the parameter in the species. In still other cases the term meant that the difference between the exposed and control group in the EMF experiment was small in comparison with the effect produced by the investigator's favorite chemical. If that chemical produced a difference in means of 1000%, and the EMF produced a difference of 50%, then the effect of EMFs was "small." The point is there needs to be some determination of what the reasoning rules will be, **prior** to evaluating the evidence. Are we to regard EMFs as affecting human health if they produce any change, or do we require that the change be bigger than X%, or outside the normal range, or greater than inter-laboratory variation in control groups?

At the meeting, like good little soldiers, we voted 100% that EMFs had not been "conclusively" shown to cause skeletal abnormalities in chick eggs. We then voted 100% that electromagnetic fields had not been "conclusively" proved to cause birth defects in animals. Most of the committee members voted to say that the results were "equivocal." But the words were never defined. Committee members always seemed to avoid defining terms that had decisional impact. Terms like "robust", or "equivocal", or "controversial", or "inconsistent", or "cause", were never defined, despite their enormous importance in conveying the committee's conclusions. What, for example, is the public to understand by the conclusion that the effects of EMFs on fetal development are "equivocal." Does that mean that it is okay to buy a house beside a powerline? Does it mean that there is no likelihood that a pregnant woman living beside a powerline will have a spontaneous abortion or give birth to a malformed child, at least partially as a result of the magnetic field from the nearby powerline? The BOG did not infuse the statement with any substantive meaning, and therefore it is unreasonable to expect that someone who reviews the BOG's work product, Portier, for example, could do so. It is even more unreasonable to expect that the NIEHS Director will do so.

During one meeting, a committee member tried to help the group decide whether EMFs affected reproduction in animals. He had a copy of the report of the 1997 NAS committee which had a Table listing positive and negative reports that showed that 30% of the reports were positive. On that basis, the committee voted to say that the results were "equivocal."

It's easy for a chairman of a committee to implement his own agenda or impose his own bias. In one committee, the chairman did not accept the existence of cause- effect relationships in biological systems unless they were 100% certain. He said so clearly. Effects were "definite" or there were no

effects. Nothing in between. At one point, during a discussion, he asked the committee to vote on whether or not the Henhouse studies showed that EMFs "could probably affect skeletal development in eggs". I asked him to clarify whether we were to vote on whether or not we thought the effects were "probably definite" or "definitely probable". Everybody laughed, but I wasn't trying to be funny.

That chairman is a good human being, the kind of guy you'd like to have for a neighbor. He would come over and help you move a refrigerator, he would attend your mother's funeral even though he never knew her, simply because you were his friend, and he would share with you the tomatoes in his garden. He doesn't kick his dog, his TV isn't too loud, and his kids don't have pierced tongues or pink hair. It's not that he isn't a nice guy. He simply isn't qualified.

Congress charged the Director to find out whether EMFs "affect human health." What exactly does that mean? What is the state of the evidence that would warrant an affirmative answer? In my mind, this issue must be resolved before the evidence is discussed. Apparently, as Portier looks at it, the issue doesn't have to be discussed at all.

There's something un-American about a process in which one man controls all important events, is not subject to any meaningful checks and balances, appoints himself as both investigator and judge, and renders a decision from which there is no appeal that has a pervasive effect on society, affecting the daily lives of hundreds of millions of people.

What do I expect? Two or more groups to present the evidence, and disinterested persons to judge the evidence and regulate the fairness of the process. Instead, we will be presented with Portier's view of the world, endorsed by the Director, and sent to Congress. It simply doesn't matter what is in Portier's report. He could exonerate EMFs, indict them, or take any other position. The point is that the process by which he has decided is fatally flawed. Decisions affecting the public interest ought not be made by one man, regardless how smart or honest he is. No one is that good.

In hindsight, it is clear that Congress' attempt to resolve the question of health hazards of powerlines by assigning the question to the NIEHS was doomed to fail. You simply can't throw $65 million at a problem and tell somebody "fix it." You must also specify what "fix" means. Otherwise, the money will be spent pursuing their notion of "fix," and then they will come back and ask for more money.

I think the Congress is unlikely to repeat this mistake again, and hence a search must commence for other mechanisms by which scientific data can be taken over into the public domain. The NIH deals in more or less certain science. It is ill-equipped to handle the inherently adversarial issue posed by Congress where things are not and cannot be. NIH has not mechanism, staff, nor tradition for resolving scientific disputes such as whether EMFs affect human health. I suspect that that infirmity would extend to any dispute where the public health is allegedly impaired by a pollutant under the control of an economic interest.

What will be the final result of the present process? A poorly documented, diffuse, vague, wishy-washy report in which terms are not defined, procedures are not specified, and the *ipse dixit* of scientists is presented as fact.

In 1996 I was hopeful that the NIEHS would design a credible program to evaluate the health risks of powerline EMFs. My enthusiasm dimmed significantly when I received a copy of NIEHS' proposed strategy for evaluating the health risks of powerline EMFs. Portier asked for comments on the proposed strategy, and in response I sent him a detailed comment. [6.5. note 1] need* It contained both an evaluation of the NIEHS proposal, and the outline of an alternative proposal.

When the plan for the first NIEHS symposium was posted on the NIEHS Web site, it became clear that Portier intended to implement his strategy, with no changes. I posted a note [6.5. note 2] need* aimed at explaining why the strategy was futile, hoping that perhaps this would induce him to consider the problems that he ignored. I also sought him out at a meeting and tried to use the few minutes that he had available for me to emphasize my basic point. I urged that he make every expert that is part of his process write a report that detailed and explained and justified his position. That is the only way we will be able to **identify** the expert's opinion. Second, allow those who disagree with the expert the opportunity to confront him with evidence that is inconsistent with his position. Only in that way will we know the **quality** of the expert's opinion.

When NIEHS published its plans for a second and third symposia, I realized I had wasted my time. I never again took seriously Portier's requests for information and advice, and I never again tried to offer it to him.

The mantra that was often repeated at the NIH symposium was that a decision regarding whether powerline EMFs affect human health must be made on the basis of the "best science available". Unfortunately, this will not be the case. There is actually a significant danger that the decision won't be made on the basis of any science at all, but rather that it will be determined essentially by the process.

7. POWER-INDUSTRY SCIENCE AND POWERLINE EMF HEALTH HAZARDS

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.

7.1. Introduction

To decide whether powerline EMFs affect 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 EMF bioeffects research and analysis determines what data is produced and the way it is interpreted.

Soon after the possibility that powerline EMFs were health risks was raised in a legal dispute [see 7.1. note 1] involving the New York Public Service Commission, power companies and their trade associations, particularly the Electric Power Research Institute (EPRI), became massively involved [see 7.1. note 2] in EMF bioeffects research. Subsequently, the power industry dominated funding of the effects of powerline EMFs, 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 EMF 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 EMFs.

7.2. Powerline EMF research at Battlelle

7.2.1 Introduction

Battelle is a private company that performs contract research of many different types for many different organizations. Battelle began powerline EMF 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 EMFs is hard to discern exactly, but it far exceeds in scope and impact that of any other group or organization that has performed EMF research. Battelle has probably received more than $100,000,000 in funding for EMF research, and its employees have made more than 1000 presentations and reports dealing with EMF bioeffects issues.

Battelle's EMF research mostly involved the effects of powerline EMFs on rats, mice, and pigs. The experiments consisted of exposure of the animals to EMFs, 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 EMFs were health risks.

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*.

7.2.2. Negative Results by Design

Battelle investigators designed and performed many EMF studies in which the measured parameter had no plausible sensitivity to EMFs. In these cases the results were foreseeably negative because one would not *expect* an effect due to the EMFs. For example, in a study of the effects of powerline EMF exposure on heart rate in rats [D.I. Hilton and R.D. Phillips: "Cardiovascular response of rats exposed to 60-Hz electric fields," *Bioelectromagnetics v.* 1:55-64, 1980] , the heart rate of the animals was measured only after the animals were removed from the EMF 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 EMFs; not surprisingly, the study was negative.

In another study, Battelle investigators measured the effect of powerline EMFs 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 EMF 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 EMFs. The results were negative, but not finding an EMF-induced change that one had no reasonable expectation would occur was not evidence that powerline EMFs were safe. Nevertheless, that was Battelle's rationale for the study and the way the results were interpreted.

The question whether powerline EMFs are stressors is important because stress can worsen the consequences of *any* human disease, and Battelle investigators tried to show that powerline EMFs were not stressors. 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 EMFs were stressors, but that conclusion was foreordained by the way the animals were housed. Both the EMF-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.[see 7.2.2 note 1]

7.2.3. 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 EMFs were *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-EMF 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 EMFs for 30 days.[see 7.2.3. Note 1] 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 EMF-exposed rats was not statistically significant. The investigators concluded that, overall, EMFs 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.

7.2.4. 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 EMFs for 30 days to assess whether or not the EMF 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 EMF was a stressor. I had previously performed the same experiment several times, and reported that corticosterone levels were altered [see 7.2.4. note 1] as a consequence of the EMF exposure.

Using a fluorimetric technique, the Battelle investigators found that corticosterone in the blood of the EMF-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 fluorimetric 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 EMF-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 EMF-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 again were lower in the EMF-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 EMF 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 EMFs. 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 EMFs were stressors. But nothing is more clearly demonstrated by an analysis of the history of EMF bioeffects research than the

fact that investigators or organizations that find results suggesting that powerline EMFs are health risks 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 EMFs 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 EMF 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 EMF. But their report was misleading for several reasons. First, the unpublished data [see 7.2.3. note 2] from the female rats exposed for 30 days *was* statistically significant, and showed an effect due to EMFs. This was remarkable because it suggested that the effect of the EMF 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, [see 7.2.3. note 3] which seemed to show that the EMFs significantly affected the heart rate for about the first 20 minutes after the rats had been removed from the EMF. Thus, the conclusion of their publication that there were no EMF 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 [see 7.2.3. note 4] began in January, 1978, and was intended to refute an earlier study published by me and my colleagues. The plan was to produce 3 successive generations of mice, and to code the data [see 7.2.3. note 5] in such a way that some of the people who worked on the experiment could not determine what the results were during the experiment. 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 EMFs 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 EMFs 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 EMF. 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.

7.2.6. 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 EMFs 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 EMFs 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 EMF, was 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-EMF cause and explained away the positive result.

When this Keystone Kops of powerline EMF 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.

7.2.7. 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 EMF dosimetry and scaling.

What EMF 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 EMFs 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 EMFs 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 EMFs many times stronger than powerline EMFs.

Early in the course of the work, Battelle investigators discovered that the strong EMFs caused the hair on their mice, rats, and pigs, to vibrate. [see 7.2.7 note 1] Since these animals, but not people, are completely covered with hair, one consequence of using high EMFs 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 EMFs interacting with body tissues. The overall result was that the Battelle investigators reported *some* biological effects due to EMFs, 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 EMFs retarded fracture repair in rats. As a potential explanation, they suggested that the hair vibration caused by the EMF 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 EMF blue-ribbon committee meeting, for example, a suggestion by a disinterested scientist that the positive results from a particular Battelle study suggested that powerline EMFs 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 EMFs which, in turn, virtually guaranteed that any positive data could not be used for evaluating human health risks.

7.2.8. 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 EMF 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.

To appreciate how radically research at Battelle differed from normal research, consider the following comparisons. The National Institutes of Health (NIH) and the National Science Foundation (NSF) are major sources of funding for scientific research in the United States. The primary goal of the research funded by both the NIH and the NSF is *scientific truth*, by which I mean scientific knowledge that is as reliable as can be obtained by human beings. The particular scientific knowledge sought is subject to priorities that are governed by the political process. The emphasis might be, for example, on breast cancer research, particle physics, developing the internet, HIV research, or space travel. As a result of competition among these and other priorities, the research favored by one group or another might not get done for lack of funds. But if it is done, the research is as reliable as human beings can make it.

Research funded by NIH and NSF is characteristically innovative. Both agencies explicitly seek new and novel solutions to particular problems, and the degree of innovation of a particular proposal is an important factor in the funding decision regarding it. Moreover, both agencies expect that proposed innovative research will be carried out by competent investigators having a demonstrated history of performing the types of studies proposed. It is difficult to imagine, for example, that the NIH would fund a grant application by a principal investigator who had never performed the type of studies

proposed in his application or, even worse, had attempted to perform the experiments but failed because he lacked the necessary technical capability.

NIH and NSF research is peer-reviewed, by which I mean an independent and disinterested group of scientists evaluates individual proposals, compares them with other similar proposals and ranks them accordingly. The actual mechanism of evaluation differs between the two organizations, but in both cases it is usually true that a specific proposal is actually reviewed by peers of the individual making the proposal.

Individual peer reviews are regarded as privileged communications and not disclosed to the public; the reasons for this have never been clear to me. However, essentially all other aspects of the funding of scientific research by NIH and NSF are open and public, and can be examined by anyone who chooses to do so. Thus, the actual funded research proposal submitted by each investigator is listed on the agency internet servers, and can be obtained from the agency. Each of the annual reports submitted by investigators of funded projects are also public documents. The rules of both NIH and NSF also require that *any data* obtained using federal funds be freely available to all other investigators who request it. Thus, the traditional openness and free availability of scientific information that has been characteristic of the development of science throughout its history is part of the tradition and the specific rules of both the NIH and the NSF.

The conduct of powerline EMF research at Battelle differs markedly from each of the above mentioned characteristics of scientific research funded by NIH and NSF, and the Battelle research suffers badly in comparison. The ultimate goal of the Battelle EMF 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 EMF 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.

Whereas NIH and NSF research is innovative and competent, 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 EMF 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 EMF-induced bioeffect was an artifact, and in most cases the Battelle investigators supported their hypothesis. NIH and NSF do not fund studies having such hypotheses. In contrast, the Battelle studies almost always had such a hypothesis. It is simply impossible for honest EMF 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 simply takes no skill whatever to do this.

As a rule, the Battelle investigators had few publications prior to beginning powerline EMF research. If their negative powerline EMF publications were erased, they would still generally have few publications in the peer-reviewed scientific literature. 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. In contrast to NIH and NSF research where documents describing the experimental design as well as the actual data are freely available to all interested persons, 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. No disinterested investigator was in privity with either party, and hence had no right to demand information regarding experimental design or the results of particular experiments. Thus, the legal nature of the relationship between Battelle and the power industry prevented full and open disclosure of pertinent scientific information. The essential nature of the industry-Battelle axis has been known since 1984.[see 7.2.8.note 1]

In summary, different masters are served by the open grant system and the secret contract system that governed Battelle's activities. Consequently, the results produced in each system regarding EMF research must be evaluated differently. All claims, conclusions, reports, publications, and presentations by Battelle investigators should initially be doubted or disbelieved because they were primarily intended to serve the interests of Battelle's clients, not 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 EMFs 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.

CONCLUSION (to be completed maybe)

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Items in BLUE are available on line. See http://www.ortho.lsuhsc.edu/Faculty/Marino/CV.html

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