Swimming Against the Current

The Life and Work of Reba Goodman

A Tribute and Remembrance

August 5, 2025

This is a story about what it's like doing research on electromagnetic fields and public health. About what happens when you serve two masters: science and medicine, on one hand, and business and politics, on the other. It shows what has so often happened to those who follow the data. Those who link the words "EMFs" and "cancer" in the same sentence pay the price. Careers are cut short.

I've told many similar stories over the years, but this one is about an old friend who deserved better. It's a story about Reba Goodman.



Reba Goodman (1927-2024)

I remember the day I first spoke to Reba: It was July 12, 1983. I called to interview her about a paper she had just published in *Science* magazine which showed that specific types of EMFs could have different effects on how genes express themselves. It was a remarkable finding. Indeed, it was supposed to be impossible,

according to the ruling dogma of bioelectromagnetics. Fields could transmit energy into the cell but not do anything like send a message to a specific DNA sequence.

Just as remarkable was that her paper was in *Science*—arguably the country's leading journal. It is a coup for any scientist, but especially so for a member of this often-ignored backwater of scientific research.

Reba picked up the phone and we were soon chatting like old friends, about the latest research and who was doing what. She was open and welcoming, rare qualities in a field dominated by tight-lipped types who were reluctant to talk to outsiders, especially a reporter. Few wanted to risk saying something that was out of line. Reba was not like that. She seemed to have nothing to hide. There were no conditions. No, "Don't say I told you." Or "Don't quote me on that." None of the usual demands that I had come to expect covering bioelectromagnetics.

Reba was a welcome breath of fresh air. So began a 40-year friendship. It ended last June when Reba died at the age of 96.

No Effects, Please

Reba Mirsky was not planning to do science when she enrolled at Indiana University in the fall of 1945. She was going to study music, as her mother had at Radcliffe College. But somewhere along the way, she was seduced by science —a passion she inherited from her father. Science would become a driving force, her refuge, for the rest of her life.

In 1949, with a degree in zoology and botany, Reba moved back to New York City, where she had grown up, to do graduate work in developmental genetics at Columbia University. She married her college boyfriend, Bob Goodman, and they soon had four young children. "I was holding a baby at the ceremony to get my PhD," she recalled.

Reba remained at Columbia for close to 40 years, rising from research associate in the medical school's Department of Pathology to full professor. She published hundreds of scientific papers.

When Reba began doing experiments on EMFs and genetics in the early 1980s, there were two separate communities of scientists involved. One of these, working under the umbrella of the newly formed Bioelectromagnetics Society (BEMS), saw the fields as potentially harmful. The other searched for possible benefits, ways to use the fields to treat medical conditions. This latter group had its own clubhouse, the Biological Repair and Growth Society, with the unfortunate acronym BRAGS.

BEMS was the creation of the armed services, notably the U.S. Navy. The stated objective was to do biological research, but this was not the military's dominant interest. Whatever health effects might be identified, the true mission was to make sure that the use of radars and communication systems would not be restricted.

Members of BRAGS, on the other hand, assumed that EMFs can induce biological changes—their work depended on it— but in the end they too were reluctant to acknowledge that any unintended effects could be harmful. They were harnessing pulses of electromagnetic energy, call them “waveforms” or “signals,” to do good. Most would never acknowledge that there were possible side effects. The assumption was that there would be no thalidomides.

For all their differences, the BRAGS doctors and the Navy researchers had one thing in common: Neither wanted to uncover any suggestion that EMFs might cause a health problem, and certainly not cancer. It would be bad for business.

Sine Wave Surprise

Reba’s first foray in the world of electromagnetics was on the BRAGS side. She collaborated with Andrew Bassett, an orthopedic surgeon at Columbia, and Ann Henderson, who had spent a decade doing genetic research at Columbia before moving across town to Hunter College. Bassett was already a bit of a star. He had done important work on the electrical properties of bone with Robert Becker and had later helped launch Electrobiology Inc., a company known as EBI. It marketed a device that used EM signals to help mend bone fractures.

A few years earlier, Bassett had been prominently featured in a two-part investigation, *“The Electrical Connection,”* in *New York* magazine. The hook for the story was that bone healing was just the beginning. Bassett and his Columbia colleague, Arthur Pilla, would soon be using EMFs to treat cancer. The articles drew outsized attention, in part because they were written by Lally Weymouth, the daughter of Katherine Graham, the influential publisher of the *Washington Post.*

Together, Goodman, Bassett and Henderson showed that two different waveforms, both used by EBI, one to mend broken bones and the other to treat osteoporosis, had *different* effects on a how a cell’s genes may be expressed. “This study supports the hypothesis that [pulsed EMFs] induce specific modifications in normal cell function,” they wrote in the June 17, 1983, issue of *Science*. This was the paper that prompted my phone call.



Reba in her lab at Columbia

The Goodman-Henderson collaboration (without Bassett) would continue for another 20 years. Their work was supported by blue-ribbon agencies —the National Science Foundation, National Institutes of Health, Office of Naval Research and the Department of Energy, among others. They published numerous papers in peer-reviewed journals.

After their paper in *Science,* they turned their attention to other types of EMFs. In contrast to the complex modulations of the EBI waveforms, they experimented with the most basic of all signals, sine waves at frequencies like those used for transmitting and distributing electricity over power lines. Their segue from BRAGS to BEMS opened new avenues of research support. They soon had grants from both the electric utility industry and the U.S. Department of Energy (DOE).

These new sources of money were no different from the Navy. They, too, had an aversion to novel EMF effects. The Electric Power Research Institute (EPRI), an industry consortium, had a long track record of discrediting those whose work suggested potential ills. The best career move for EPRI contractors was to show that you couldn't find effects. In the upside-down world of EMFs, this was the surest path to continued funding.

Ann S. Henderson

Goodman and Henderson found effects. Within a couple of years, they had shown that those simple sine waves were just as likely to affect gene expression as the complex waveforms patented by EBI. Such changes in gene expression are a "common response" to all sorts of electromagnetic signals, they concluded in 1986. EMFs at specific frequencies could change gene expression "irrespective of waveshape."

A summary of their work on gene expression with both the EBI signals and sine waves was another coup. It was accepted by the *Proceedings of the National Academy of Sciences,* another rare achievement within the BEMS community. The fields, they wrote, were activating "a limited number of specific genes that were either previously silent or not detectable." More generally, the EMFs were causing a stress response. At the time Robert Becker, Bassett's former collaborator, was promoting similar ideas in his well-received book, *The Body Electric.*

Support for the Epidemiologic Findings

Public fears over power lines grew during the 1980s as epidemiologists documented links between EMFs and childhood leukemia and various types of occupational cancers. Concerns intensified in the summer of 1990 when the U.S. EPA classified power line EMFs as "probable human carcinogens," based largely on those human studies. When the White House tried to suppress the draft report, it added more fuel to the fire. (Self-serving disclosure: *Microwave News* broke the story.)

Many observers were unconvinced that the association with cancer was real without supporting data from lab studies. Goodman and Henderson stepped in to fill the gap. If, as they were reporting, EMFs could stimulate certain genes, might one or more of them trigger cancer? They tested their hypothesis with a genetic sequence known as a proto-oncogene in a leukemia cell line called HL60. Discovered in the late 1970s, proto-oncogenes are largely benign unless activated. Then they turn into oncogenes and can promote cancer.

About the time that the EPA EMF–cancer report hit the news, Reba spoke at a scientific conference in Washington and revealed that power-frequency fields could turn on proto-oncogenes. A reporter from *Science News* was at the meeting and filed a story. It began:

> Epidemiologic studies have suggested a link between extremely low frequency (ELF) electromagnetic fields and an increased risk of cancer, particularly brain cancer and leukemia. But those studies do not establish a cause-and-effect relationship, and scientists have very few clues to how ELF fields might influence cancer growth if a causal relationship exists. Now, two researchers in New York City offer one such clue, suggesting that genes normally expressed in cells dramatically accelerate their DNA transcription when exposed to ELF fields [in the lab]."

Goodman's colleagues praised the breakthrough. It has "the potential to be revolutionary," one member of the BEMS community told the *Science News* reporter.

The presentation caught the attention of Paula Kilberstis, the editor of *Cancer Cells,* a review journal recently launched by the Cold Spring Harbor Laboratory, the renowned DNA research center on the north shore of New York's Long Island. In a February 1990 letter to Goodman, Kilberstis called the work "a crucial complement" to the epidemiological studies linking power-line EMFs to cancer. She asked whether Reba would contribute a short "hypothesis" essay on how these fields might exert their influence on the molecular level.

This must have been an exhilarating moment for Reba. But it would not end well.

Summer Camp with Barbara McClintock

Kilberstis's letter was a sip of Proustian tea, reminding Reba of the summers she spent at the Cold Spring Harbor Lab during her late

teens and early twenties. There, Reba moved among science royalty. Each summer, the Lab served as a retreat for a who's who of modern genetics.

Her father, Alfred Mirsky, was a professor of biochemistry at Rockefeller University and an elite member of the club. He had helped unravel the role played by DNA in heredity; he was elected to the National Academy of Sciences in 1954. When Reba was 9, he spent a year in Linus Pauling's lab at Caltech.

Current and future leaders of genetics research and the then-emerging field of molecular biology brought their families to the Lab each year for a kind of science summer camp. They played tennis and went to the beach, all the while talking shop. Twelve of them, *twelve!* would go on to win a Nobel prize. Among them was Barbara McClintock, a path-breaking, meticulous and iconoclastic geneticist. (She was the first woman ever to win an unshared Nobel prize in medicine.) McClintock, who spent some 50 years working at the Cold Spring Lab, showed that DNA sequences could move from one chromosome to another —so called "jumping genes."

One summer, McClintock took Reba under her wing. Reba loved every minute. "When you worked for Barbara McClintock, you were hers," she recalled. "We ate every meal together. When she wanted to go somewhere, I went with her." Reba was especially taken by McClintock's independence. "I thought the way she lived her life was so fabulous; she didn't care what other people thought of her."

The two photos, below, of Reba at Cold Spring Harbor were taken by Joshua Lederberg, another summer camp regular. He would win a Nobel for showing that bacteria can exchange genes. He was only 33 at the time, the second youngest ever to win for medicine. Twenty years later, in 1978, four years after Reba's father died, Lederberg was appointed president of Rockefeller University.

Reba with McClintock and Carl Lindegren at Cold Spring Harbor Lab. Lindegren, a geneticist,
was the head of the Biological Research Lab at Southern Illinois University.

Reba with Sophie Dobzansky, another McClintock summer intern. Sophie was the daughter of geneticist Theodosius Dobzansky, at the time with Columbia University and later Rockefeller University.

By the time Reba met McClintock, she was already a major star. She had been elected to the National Academy in 1944 when she was only 42. She described her landmark discovery of jumping genes at Cold Spring Harbor in 1951, not long after it had been published in the *Proceedings of the National Academy of Sciences*. When she finished her talk, the room fell silent. She later described the audience's reaction as "puzzlement, even hostility." Overall, she said, the scientific community "didn't understand it, didn't take it seriously."

McClintock carried on. "If you know you are on the right track, if you have this inner knowledge," she once explained, "then nobody can turn you off… no matter what they say."

Thirty-two years later, in 1983, the Nobel Assembly of the Karolinska institute credited McClintock for her "great ingenuity and intellectual stringency." Her studies were compared to those of Gregor Mendel, the father of genetics. Like Mendel, according to the announcement: McClintock "carried out this research alone and at a time when her contemporaries were not yet able to realize the generality and significance of her findings."

Reba's father didn't want her to become a scientist. When she did, he didn't want her to use the Mirsky name. He had hoped Reba's younger brother, Jonathan, would follow in his footsteps. Instead, Jonathan Mirsky became a historian and prominent journalist of China. He died in 2021 at the age of 88.

McClintock was Reba's guiding light. Her mother was her hero. "She has given me purpose, resilience and a sense of humor," she jotted in her diary. Still, she credited her father with the direction her research would follow throughout her career. "My father got me interested in how DNA turns on and off," she said in an interview after she had retired.

Explaining the Epidemiology

The Goodman-Henderson hypothesis paper appeared in *Cancer Cells* at the end of 1990. The take-home message was in the title.

Lab work may clarify the epidemiology —how power lines EMFs could lead to cancer.

They were careful not to overstate the case, but —no surprise— it did not sit well with the electric utility industry or the Department of Energy. A rebuttal was soon on the agenda.

Two labs, one on each side of the Atlantic, were recruited to investigate the oncogene experiment. In the U.S. the job fell to Jeffrey Saffer, a molecular biologist who had recently joined the Pacific Northwest National Laboratory in Richland, Washington state, part of the DOE network. In England, the National Grid, the country's electrical utility, commissioned Cambridge University's Robin Hesketh and his close collaborator, James Metcalfe. The lab work was carried out by Hesketh's doctoral student, Adam Lacy-Hulbert; it would become part of his PhD thesis.

No one in British academia was closer to the National Grid than Metcalfe. Over a 25-year association (1994-2019), as a trustee and scientific advisor of the Grid's EMF Biological Research Trust, Metcalfe had outsized influence on what was considered good EMF science and which scientists would be funded.

Both labs failed to repeat the experiment. Saffer went to New York for a couple of weeks in November 1993, to try again in Henderson's lab. "We did not find any evidence for a magnetic field effect even using their cells and their exposure system," Saffer later told *Science* magazine.

Henderson saw it differently. She made her case in a detailed letter to the DOE project managers not long after Saffer had gone back to Richland: "We are convinced that our previous experimental results were replicated during their time in the lab —but only if the conditions used were the ones we employ in our experiments."

Showdown in Copenhagen

The conflict went public six months later, in June 1994. It became personal: a referendum on Reba's competence, or, as Saffer and the Cambridge group wanted everyone to believe, her incompetence. She became a proxy for the cancer link. She was now the target.

The scene was the annual conference of the Bioelectromagnetics Society, held that summer in Copenhagen. "Rumors circulating everywhere that Reba's work is no good!" was one of the first entries in my conference notebook. I soon learned that Saffer had submitted a paper to *Science* and that the editors had rejected it, without a formal peer review. They advised him to go to a specialty

publication. Instead, I found out later, Saffer sent the manuscript to *Nature*, which like *Science,* is a top-tier journal. He wanted to make a splash.

All three groups were in Copenhagen to present their latest findings, together with a few others doing similar work, notably Jerry Phillips, at the time a member of Ross Adey's lab at the VA hospital in Loma Linda, California.

Lacy-Hulbert of the Cambridge group led off the morning session on gene expression, as Hesketh watched nearby. Lacy-Hulbert declared, as many had already heard by then, that he had been unable to get the same results. But what he said was overshadowed by the way he said it. He was nasty and condescending. It was painful to be in the room.

Lacy-Hulbert came across as a hit man for the electricity industry. He was still a student, and here he was attacking a professor from a major U.S. university at an international scientific conference. He must have had the blessing —and most likely the encouragement — of his Oxford professors and their sponsor, the National Grid.

Goodman, Phillips and Saffer followed him to the podium, but I found it hard to move on after watching Reba getting mugged.

I recently asked Phillips about what happened in Copenhagen, now more than 30 years ago. "I remember sitting in an outdoor café with Adam Lacy-Hulbert and his mentor Robin Hesketh," he told me. "They spoke about Reba's work with disdain —their animosity toward Reba was palpable."

Reba in front of the Tivoli Gardens, Copenhagen, June 1994

My wife, Lesli, and I had dinner with Reba that night. It was not a happy occasion. It remains one of Lesli's lasting memories of our trip to Copenhagen. "They ripped her to shreds and Reba was devastated," Lesli recalls.

Saffer and Lacy-Hulbert contended that Goodman and Henderson had run shoddy and incomplete experiments. They lacked

“essential controls.” said Saffer. The implication was that the changes that Goodman and Henderson had seen in the lab and had attributed to EMFs were most likely caused by something else.

One essential way to check the validity of an observation is to run the same experiment at the same time with a second set of cells that are not exposed to the agent under study —in this case, EMFs. These are called “sham controls.” If you see similar changes in the exposed cells and the shams, then the agent is unlikely to have been responsible. Using “positive controls” is a way to check that an effect is in fact possible. No point looking for change if it can’t happen. Other types of control abound. But the key requirement for replication is to repeat the experiment *exactly* the same way —the same cells exposed in the same way to the same type of EMFs. Everything must match.

What Saffer and Lacy-Hulbert both failed to mention in their talks is as striking as what they did say. Neither acknowledged that Phillips had already shown that EMFs could affect the expression of proto-oncogenes, including *myc* —the one that Goodman and Henderson had focused on. Phillips had published these results in peer-reviewed journals, up to two years before Copenhagen. He had run a different experiment, not a replication, yet if EMFs could affect proto-oncogenes, as Phillips saw, discrediting a single study would not in itself refute the cancer link.

Jerry Phillips at the University of Colorado, Colorado Springs, in 2006. He gave up lab research at the VA in 2000 and returned to science education at UCCS.

Saffer left out another important detail: At the time, he was working with a different cell line, known as JB6, which had been developed to screen for cancer promotion. Saffer’s preliminary results showed EMF-induced changes. Nor was he the first to see such effects. A group at the FDA in Washington had published, that very month, similar results with JB6. This too was no secret to many in the room: One member of the FDA team, Mays Swicord, was co-chairing the Copenhagen session on gene expression.

Saffer announced his JB6 findings a few months later at a DOE EMF conference in Albuquerque, New Mexico. I received an advance copy of the program a few weeks before the meeting and was stunned when I saw his abstract. I called Saffer in Richland

and asked why he hadn't dropped some hints in Copenhagen that he was seeing indications of cancer activity, albeit different ones. "In retrospect," he replied, "that may have been a wise thing to have done."

Saffer often tailored his message to the occasion. In that same conversation, he stressed that his failure to repeat the Goodman-Henderson HL60 experiment should only apply to those specific experimental conditions. If the message wasn't clear, he said, he would take responsibility. "I am aware of the problem that people are not listening to what I am saying or that I'm not saying it well," he told me, "I tried to be as specific as possible."

But, at other times, Saffer was quick to extrapolate his null HL60 findings to *all* EMF–cellular studies —and, further, to any possible link to cancer. For instance, in a conversation a few months later, he told me, "I do not believe that there has yet been a single demonstration of [a laboratory] effect that *proves* weak magnetic fields are capable of affecting cellular processes." That was his emphasis on the word "proves," and it's doing a lot of work in that sentence. There were in fact many published studies showing effects, not just Phillips's. Saffer was saying that all positive findings were from lousy experiments, just like those in New York.

At about the same time, he told the readers of *Nature* that, with respect to cancer, EMF effects on cells "may not exist."

In Albuquerque, Saffer put his JB6 findings on the record: "While magnetic fields do not alter gene expression in HL60 cells," he said, "they may serve as a co-[cancer]-promoter in JB6 cells."

Jeffrey Saffer (left) and Adam Lacy-Hulbert

Reba was there too and gave a talk. She began with a correction: on repeating the *myc* experiment, the observed increase in activity was smaller than first reported. It was now in the range of 30-50%, down from the original two-to-fourfold jump. Even so, she did not back off the central finding —EMFs could affect the expression of proto-oncogenes.

She then addressed Lacy-Hulbert directly: "The level of acrimony has gotten out of hand," she said, "It's intolerable." There had been no communication between their labs for two years, she said. "If we had worked together, all this could have been avoided."

That evening, Saffer called Reba and asked to meet. They had a drink at the bar, where, according to Reba, Saffer was very

apologetic. “It’s ruining my life,” he apparently told her, promising to make a public apology. It never happened.

The FDA group went on to reaffirm its JB6 findings in 1996 — with an additional worrisome observation: cancer promotion could be stimulated at *low* magnetic field intensities. They saw significant changes at levels approaching those seen in ambient environments.

The following year, Saffer published a final paper of his JB6 studies. Here, he wrote that he had *not* seen EMF effects in what he called “more rigorous” experiments. Why had the FDA seen them? Saffer contended, once again, that they had used inadequate controls. He also raised the issue of positive controls.

No Right To Reply

The full Saffer and Lacy-Hulbert papers would not be available to the public until 16 months after Copenhagen. In the end, they appeared in *Radiation Research,* a journal with a reputation for favoring EMF research that shows no effects. For years, this was facilitated by the editor in charge, John Moulder at the Medical College of Wisconsin. On the side, Moulder served as an expert witness for electric utilities in health litigation. It was a lucrative gig.

Both teams had first submitted their manuscripts to *Nature*, where they were peer-reviewed. Here again, the editors did not accept them —but they did offer to publish what was little more than an abstract of their failed replication The two letters would appear together as “Scientific Correspondence,” under the title “Cancer Risk and Electromagnetic Fields.” It would be a tease for what was to come.

On hearing about this, Reba immediately asked the editors at *Nature* to be allowed to reply in the same issue. “Considering the intensity of the controversy… it would seem appropriate that we be given the opportunity to submit a letter from our laboratory,” she wrote. Maxine Clarke, the magazine’s executive editor, replied that they would be “happy to consider a brief response.” But, she added, whatever was submitted would have to be peer-reviewed “to ensure that readers would find it persuasive.” Either way, she wrote, any comments would not appear in the same issue because the two letters had already been accepted for publication. They were in print less than a month later.

Reba sent Clarke a note few weeks later advising her that she had obtained a new, different batch of HL60 cells from the same source that had supplied both Saffer and Lacy-Hulbert. With those cells, she wrote, they did not see the same response as with their original HL60 cells.

The editors at *Nature* were unmoved. Reba got the news from the low end of the masthead. An assistant editor wrote that an independent referee, with access to all the manuscripts, felt that their “arguments do not substantially advance [the] discussion.” In any case, he added, the new data could not be included because they were “as yet unpublished in peer-reviewed journals.”

The two letters came out in the May 4, 1995, issue of *Nature.* At the time, neither the Saffer nor the Lacy-Hulbert papers had been published in a peer-reviewed journal. In fact, Lacy-Hulbert had not yet offered his manuscript to *Radiation Research.* Saffer had submitted his paper*,* but it had not cleared peer-review.

Reba didn't give up. In the end, a few months after the Saffer and Cambridge papers came out in *Radiation Research*, she was able to get part of her side of the story in print. Ironically, it was in one of *Nature*'s sister publications*, Nature Medicin*e. The article by the editor of the journal's News and Views section presented Reba's rebuttal, which the editors at *Nature* had declined to include:

> "Despite efforts to reproduce exactly the experimental conditions in Goodman's laboratory, differences in EMF exposure systems, the degree of shielding and the different sources of HL60 cells could well explain the dichotomy."

Not long afterwards, in January 1995, Phillips sent me a fax, spelling out his hopes for the future: It was time to move on, he wrote: "Many reports in the literature indicate that EMF-induced changes in gene expression must be occurring, and it is these changes that we must identify and study in detail."

The opposite happened. The spotlight kept shining on the failed replication. Throughout 1995, it was the media's go-to EMF news story —and became a key rationale for discounting the cancer risk. Here, for example, is the opening sentence of *Science* magazine's story:

> "Perhaps the most plausible piece of laboratory evidence linking electromagnetic field exposure to cancers has just become much less plausible."

If the oncogene experiment could not be trusted, the argument went, maybe neither should the epidemiology that had dominated the EMF conversation for the past 15 years.

This was the message that Saffer and the Cambridge team wanted everyone to believe. The failure to repeat Goodman-Henderson meant that EMFs could not affect gene expression *and* that in fact, there is *no* cancer link.

A Final Chapter at Columbia

Bob Goodman died in 1989, and Reba remarried three years later. Her collaboration with Henderson wound down, as she spent less time at Hunter and more in her lab at Columbia, not far from her home in Englewood, New Jersey.

For her part, Henderson took on more administrative work at City University of New York (Hunter College is a part of CUNY). In 2011, she became the associate provost and the dean for graduate sciences, a position she held until 2016. She remains active today. When I interviewed Henderson not long ago, she was busy writing grant proposals.

I asked her whether she ever had doubts about the EMF work. “Of course, I doubted the results,” Henderson replied. “In all cases, the experiments were coded so even I did not know experimental from control results. They were not beautifully consistent, nor would the values knock your socks off. But, they were positive.”

Beyond the science, Henderson had sharp words for the way she and Reba were treated. “The marauders,” she called them, “would not have dared do this if we were male. They were hardly a noncommitted and unbiased group.”

Reba continued to work at Columbia, now partnering with Martin Blank, a long-time member of the biophysics faculty. Together, they turned out a good number of papers, but it wasn’t the same. The utility campaign to discredit the science had succeeded. DOE closed its EMF research program. Other funding dried up too. Without new papers coming out, the issue went dark and lost its audience. No one cared much about power line EMFs anymore.

What happened to Reba was nothing new in bioelectromagnetics research. Many of the most productive and original scientists have been attacked when they identified effects that were unwelcome to their sponsors. Robert Becker is one of the best-known examples. His lab at the VA Hospital in Syracuse, New York, was closed after he advised caution in the siting of power lines. Among the many others are Robert Liburdy, Hugo Rüdiger and Henry Lai. When Jerry Phillips moved from studying power lines EMFs to cell phone radiation, Motorola, his sponsor, tried to stop him from publishing a paper showing effects on DNA.

Looking back on the Goodman-Henderson controversy today, it’s impossible to say whose experiments were right. Maybe they were all right. But it doesn’t matter. Science isn’t about replicating a single experiment. It’s about the body of work from all laboratories and looking for a coherent picture. Some 100 proto-oncogenes have been identified to date. How many might EMFs activate? We don’t know, because the experiments have not been done.

I asked Phillips if he thinks power-frequency EMFs can affect the expression of proto-oncogenes. “Absolutely,” he replied, “I have no doubt.”

You can see why Phillips is so sure when you look at the overall gene expression literature. Few know it better than Henry Lai, an emeritus professor at the University of Washington, Seattle, and one of the world’s best-known EMF researchers. In his retirement, Lai has been keeping track of what gets published. By his count, there are now more than 200 papers on static and power-frequency EMFs and gene expression —*over 90% show effects.* Among them is a 2020 study from Iran that saw EMF-induced changes in *myc* expression.

Quashing the cancer link turned into a mission to discredit Reba. It was successful. “I always believed that the National Grid was out to demonstrate that the Goodman and Henderson work lacked credibility and that EMF research was nothing but junk science,”

Phillips told me as I was finishing up this article. “Money has always been the motivation, period.”

Reba’s motivation was different. In 1999, she became a professor emeritus. Using her own money, she kept her lab open for few more years. Yet, in some ways she never left. As she later wrote in her diary: “In my mind, I am still in the lab… I miss it so much!!!”

When McClintock was 90, she described her life in research:

> “I just have been so interested in what I was doing, and it's been such a pleasure, such a deep pleasure, that I never thought of stopping … I've had a very, very satisfying and interesting life.”

Reba might have said the same thing.

Reba Goodman, Ann Henderson, Jerry Phillips, Jeffrey Saffer, Adam Lacy-Hulbert, gene expression, DNA, Andrew Bassett, Robert Becker, Henry Lai, BEMS, BRAGS,

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