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Suppression, Bias, and Selection in Science: The Case of Cancer Research

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In recent years the long-rejected theory of the bacterial etiology of peptic ulcers has been resurrected and transformed into consensus knowledge. The history suggests that the stability of consensus knowledge on the noninfectious nature of chronic disease may be open to question. Cancer research has a similar history in which alternative bacterial programs were not only rejected and forgotten, but actively suppressed. Two types of accountability are analyzed. On the one hand, while nonmainstream researchers are rightly held accountable to the strictest standards of their field, the standards themselves should be evaluated because they are defined hierarchically in ways that create biases against the nonmainstream research programs. On the other hand, the general research field is accountable to the public, and it should evaluate alternative research programs according to fair scientific standards. The cancer research field presents a massive policy failure on both counts; new policies are needed to allow for the evaluation of potentially safe and efficacious nontoxic therapies that have been 'orphaned' because they are not patentable and are therefore unprofitable.

Keywords: Cancer; alternative medicine; sociology of science; science policy

During the late nineteenth century, many scientists hoped that the advances achieved in the microbiology of infectious diseases would provide a model for the problem of cancer, and they searched for a possible cancer microbe. However, by the beginning of the twentieth century, the infectious theory of cancer had been largely discarded in favor of noninfectious etiologies (theories of disease causation). The change could be justified based on the apparent instability and lack of uniformity of microbes that had been cultured from tumor samples. In other words, scientists could argue

plausibly that the microbes cultured from tumor samples were secondary infections or laboratory artifacts.

However—and of some interest for a general understanding of accountability or integrity in science—the apparently legitimate grounds for the rejection of the bacterial theory also coincided with an emerging view of cancer etiology and treatment that held strong ties to industrial interests. As Ralph Moss (1996) documented in his classic book *The Cancer Industry*, the emergence of radiation treatment during the first decades of the twentieth century and chemotherapy during the years following World War II were part of a new industrial–medical complex that acted ruthlessly to eliminate competition in the area of cancer treatment. Throughout the twentieth century a few researchers in Europe and North America supported the older infectious theory that one or more pleomorphic (form-changing) bacteria or fungi played a central role in cancer etiology for both humans and other animals. However, when researchers and clinicians presented their theory, data, and therapies on nonviral microbes, they were either ignored or suppressed.

The history of the rejection of the bacterial research tradition is of some interest today because microbial theories of chronic disease have begun to reemerge. During the 1980s the Australian researcher Barry Marshall overturned long-held beliefs about the noninfectious nature of peptic ulcers by demonstrating the role of the bacterium now known as *Helicobacter pylori* in peptic ulcer etiology, and by showing the promise of antibiotic treatment (Marshall *et al.*, 1988). Likewise, evidence appears to be accumulating in favor of the role of mycoplasma (a kind of bacteria that lacks cell walls) in arthritis and other chronic diseases, such as multiple sclerosis (Mattman, 1993) and Gulf War syndrome, a chronic disease associated with veterans of the Persian Gulf War (Nicholson and Nicholson, 1997). If the pattern of reassessment of the noninfectious nature of chronic disease continues, it is possible that the current view that bacteria associated with tumors are secondary infections of little or no etiological significance may need to be revisited. For example, a literature is emerging on the role of the peptic ulcer bacterium *Helicobacter pylori* in a variety of gastrointestinal cancers (Parsonnet, 1993; Parsonnet *et al.*, 1994), and research is accumulating on the carcinogenic potential of mycoplasma as well (Tsai *et al.*, 1995). Although the role of bacterial agents in cancer etiology may be due for some revision, the problem of revisiting the issue is clouded by a history of bias and suppression.

THE SUPPRESSION OF THE BACTERIAL RESEARCH TRADITION

In *Can Bacteria Cause Cancer?* (Hess, 1997) I reviewed some of the major twentieth-century cases of researchers who advocated the theory that bacteria play an under-recognized role in cancer etiology, and I demonstrated that the research tradition was not simply ignored but actively suppressed. To adopt the terms of Brian Martin (Martin *et al.*, 1986: 2–3), the case study material revealed a continuum between intellectual suppression and repression, that is, between marginalizing techniques and extralegal violence (see also Martin, 1997). Advocates of the bacterial tradition who did research and avoided clinical applications tended to experience milder forms of suppression, such as loss of jobs, grants, or publication venue. The pattern corresponds to my earlier research on suppression among North American parapsychologists, who recounted similar incidents of relatively mild suppression (Hess, 1992). In contrast, advocates who went on to produce and use clinical products experienced much more severe forms of suppression.

In general, the social interests behind the suppression were the medical profession and allied industrial interests, particularly radiation therapy industry during the early twentieth century and the pharmaceutical industry after World War II (Moss, 1996). The definition of cancer as a noninfectious, progressive disease became closely linked to a cytotoxic (cell-killing) therapeutic strategy. In other words, if cancer is defined as the uncontrolled growth of human cells due to irreversible genetic damage, the best strategy for treatment is to destroy the tumor, either through surgery, radiation, chemotherapy, or, more recently, targeted specific cytotoxic immunotherapies.

In contrast, the bacterial research tradition emphasized the importance of changing the biological terrain of cancer by altering nutritional patterns and awakening the host's immune response. Tumor immunology has only recently received recognition with the development of biological therapies for cancer. Interestingly, the new respect for tumor immunology has been accompanied by very selective historical reconstructions. For example, during the first decades of the twentieth century William Coley, M.D., pioneered a bacterial vaccine therapy for cancer at New York's Memorial Hospital that has become marginalized and nearly forgotten, but he has retrospectively been recast from a marginal advocate of a failed cancer therapy to the founding father of North American tumor immunology. Although Coley has a new place of respect in the

history of cancer research, his bacterial vaccine has not earned a corresponding place. Rather than reexamine his bacterial vaccine, tumor immunologists focus attention on patentable, cytotoxic, specific immunotherapies such as the interleukins. Thus, Coley is constructed today as the precursor of modern specific immunotherapy rather than the founder of a nontoxic bacterial vaccine treatment for cancer, and his role as a researcher who was interested in bacterial agents as causes of cancer continues to be ignored.

Bacterial vaccines such as Coley's toxins failed for a complex set of reasons that included their competition with conventional therapies but were not limited to that factor. Bacterial vaccines and sera were less easy to patent than radiation technology or chemotherapy products, and they were more difficult to standardize and administer, thus making industrial production and standardized therapeutic application more difficult. In some cases, such as the work of the American physician Virginia Livingston, the bacterial therapies were part of a broader dietary and nutritional approach to cancer treatment. Dietary and nutritional therapies were also very difficult to patent and produce industrially, and consequently they suffered similar marginalization.

The more extreme forms of suppression of the bacterial research tradition tended to involve the use of state power. One example involves the case of Royal Raymond Rife, an inventor who developed an electronic frequency machine that he claimed could destroy cancer-causing microbes. To block his growing network of clinicians and researchers, the medical profession persuaded a disgruntled partner of Rife to sue the company, and it threatened loss of license to doctors who used the therapy (Lynes, 1987: 89–99). As a result of the trial, Rife ended up an alcoholic and nearly bankrupt, and subsequently the clinics were all closed (*ibid.*). Today the machines are one of a number of alternative cancer therapies that are banned by the U.S. Food and Drug Administration.

In the case of Gaston Naessens, a biologist who developed a serum and later a camphor-based drug called 714-X, suppression took the form of charges of illegal practice of medicine in France (Bird, 1990). Years later, after he moved to Quebec, the medical profession prodded the provincial government into charging him with murder because his treatment had allegedly led to the death of a terminally ill patient. When the case went to trial, the jury sided with Naessens, who argued that the patient had been too close to death for his therapy to take effect. In another case, the German physician and researcher Kurt Issels, whose nontoxic treatment for cancer included

vaccines, fought a long legal battle against charges of manslaughter and fraud. Again, he won the battle, but at great personal expense (Thomas, 1975). A final example in this category involves the American doctor Virginia Livingston, who faced expensive civil litigation in efforts to obtain Medicare insurance (Livingston, 1989). She also faced a cease-and-desist order for her treatment that came from the state health department (American Cancer Society, 1990: 107).

The many examples of suppression of bacteria-and-cancer therapies and researchers are common to the alternative and complementary medicine field, and they have been documented in other books for many other types of alternative medicine (e.g., Carter, 1993; Lisa, 1994). Such patterns of suppression have continued into the 1990s. Furthermore, journalists and writers who discuss the cases have been subject to lawsuits and other forms of suppression. Although the researchers, clinicians, and other advocates often win the cases, the legal prosecution usually drains them financially and emotionally. Rife, for example, became an alcoholic as a result of the trials mentioned above (Lynes, 1987), and Livingston (1989) reported that her legal battles had drained her financially.

In the course of my research, I also encountered claims of what Martin and colleagues would call repression, rather than suppression *per se*. For example, according to Rife's biographer, Rife's medical partner died under mysterious circumstances that federal investigators later ruled to be death by poisoning (Lynes, 1987: 97). A researcher who was attempting to write about the Rife microscope during the 1940s is reported to have been shot at while driving his car, and immediately prior to the trial mentioned above the 'only other quality "electronic medicine research lab" was destroyed by fire' (Lynes, 1987: 98–99). Livingston suggested that a tax audit of her husband may have been politically motivated and initiated by personal connections through opponents in the cancer establishment (Livingston, 1972: 79). Although the allegations are not well documented, they fit a pattern of extralegal threats and repression that has occurred among advocates of alternative medicine in the United States and other countries (Carter, 1993; Lisa, 1994).

SUPPRESSION AND THE PUBLIC

Rather than catalog the various cases in greater detail, I will focus here in somewhat more detail on one series of suppression incidents in the U.S. during the decades following World War II. There

are two main reasons for selecting this series of incidents. First, the SLAPP suits (strategic lawsuits against public participation, Pring and Canan, 1996) that continue to occur today make discussion of contemporary cases problematic for social scientists and journalists who do not have sufficient legal resources and protections. Second, when one examines the history of the marginalization and suppression of the bacterial research tradition in the United States, the critical juncture is probably the series of interventions by Cornelius 'Dusty' Rhoads during the decade or two following World War II. Rhoads served as chief of research for chemical warfare for the U.S. government during the war, and he was involved in some questionable human subjects experiments. When he assumed the leadership of the top cancer research institute and hospital in the United States, which today is known as Memorial Sloan-Kettering Cancer Center, he contributed greatly to the emerging chemical war on cancer, that is, the emerging chemotherapy industry.

In 1950 Rhoads wrote the Parke, Davis and Company and told them to stop producing Coley's toxins because they were being made at the hospital. The event seems innocuous enough, but in 1955 Rhoads stopped the production of the toxins at the hospital and all use of the vaccine there (Hess, 1997: 14). Because Coley had been affiliated with the hospital, and because his son had continued to use his father's vaccine in the treatment of cancer, Memorial Hospital was probably the key site if the vaccine were to survive in the United States. In 1963 the fate of the vaccine was sealed after the post-Thalidomide efficacy requirements had gone into effect for the drug approval process in the United States. The Food and Drug Administration ruled that Coley's toxins would not receive the grandparent status that had been accorded to other drugs; rather, it had to pass through the extremely rigorous and expensive new drug procedure. The vaccine has yet to be made generally available in the United States.

A second series of suppression incidents involves the network associated with Virginia Livingston (also known as Livingston-Wheeler), mentioned above for her work on a cancer treatment program that combined dietary therapy and bacteria vaccines. Livingston believed that she had isolated a pleomorphic (form-changing) bacterium that caused cancer in a way that was analogous to, for example, the bacteria that caused leprosy and tuberculosis. She believed that human cancers were better controlled by altering the biological terrain of the patient through a

healthy diet, particularly one high in raw fruits and vegetables. She also developed a heat-killed bacterial vaccine that was autogenous; in other words, it was cultured for each patient from the bacteria taken from their tumors. She claimed an 82% success rate in human cancers based on a random-chart review of 100 cases (Livingston-Wheeler and Addeo, 1984). Although her claim that the review was random is questionable (Hess, 1999: 220, based on comments from the journalist Robert Houston), the therapy did seem to produce some dramatic cases of long-term cancer control without the toxicities associated with conventional therapies. On the surface, then, one might think that the cancer research community would want to investigate it in more detail.

Livingston's credibility was enhanced by the network of credentialed North American scientists with whom she worked. Livingston for a while had an affiliation with Rutgers University; Eleanor Alexander-Jackson worked at Cornell and later Columbia; and Irene Diller worked at the Institute for Cancer Research in Philadelphia and edited the journal *Growth*. However, none held a university-based tenure-line position, probably due more to the gender biases of the time than to their credentials and research abilities. Another colleague, Florence Seibert, was a senior microbiologist and biochemist who was best known for having developed the PPD (purified protein derivative) skin test for tuberculosis, but when she became actively involved in bacteria-and-cancer research, she was already retired. Consequently, although Livingston and her colleagues had solid credentials, they lacked a strong institutional base from which they could wage a campaign for scientific change. Likewise, although they published in peer-reviewed journals, the journals tended to be second-tier, such as *Growth, Journal of the American Medical Women's Association,* and *Journal of the Medical Society of New Jersey*. It seems unlikely that the top medical journals would have accepted articles on bacterial agents in cancer, because the research program was considered a failure that had been rejected earlier in the century.

Nevertheless, the women and some male colleagues developed good documentation of bacterial agents associated with both human and animal cancers, and they also demonstrated that bacterial vaccines made from the bacterial agents they cultured contributed to remission of tumors and host resistance in animal models. However, they lacked access to large clinical facilities that mainstream cancer researchers had. Without that access, it was

difficult to develop a credible body of research for the efficacy of vaccines in humans, and they were limited to case studies and case study reviews.

Indeed, mainstream cancer researchers were not only uninterested in the bacterial vaccines of Livingston and colleagues, but openly hostile toward them. For example, during the early 1950s Rhoads blocked Diller's plan to organize a symposium before the New York Academy of Sciences. He accused her of commercializing her work and therefore of not being qualified to sponsor a symposium. The assault on Diller's integrity was preposterous; it was based on her acceptance of several ultraviolet sterilizing lights, with no strings attached, from a private company (Livingston-Wheeler and Addeo, 1984: 73–74). The attack was also ironic given Rhoads' close relationship with the pharmaceutical and chemotherapy industry (Moss, 1996).

Another incident occurred in 1953, when Livingston and colleagues attempted to exhibit their work at the New York American Medical Association meeting. Because they had a television hookup that allowed visitors to see the purported cancer microbes, it created a sensation. Livingston described Rhoads's intervention, 'The publicity would have been great, but again the formidable Dr. Rhoads forbade the New York AMA publicity people to interview us. He also threatened to withhold further news releases from the press if they reported on our findings' (Livingston-Wheeler and Addeo, 1984: 79). Consequently, the press did not mention their research or the booth.

According to Livingston, Rhoads also intervened to alter a will that would have awarded the hospital where she worked $750,000 from the Black-Stevenson Cancer Foundation. She claims that when one of the directors of the grant lay dying from cancer in the Memorial Hospital, he was 'prevailed upon' to sign a codicil to the bequest that allowed Livingston's hospital to spend its money only with the permission of the Memorial Cancer Center. 'As it turned out,' she wrote, 'the only acquisitions that Dr. Rhoads would grant us were a new wing to be added to the hospital and the installation of a *high-voltage cobalt machine*' (1984: 88). Livingston then describes the impact of the loss:

> At the time of the announcement of the Black grant, we were elated. We could foresee establishing preventive clinics across the nation that would screen patients and immunize them when they were bacteriologically positive, clinics that would promote better life habits, better nutrition, safer and

cleaner surroundings, industrial and environmental control of carcinogens, earlier detection of precancerous lesions, and genetic counseling.

It was a great dream while it lasted. (Livingston-Wheeler and Addeo, 1984: 88).

The last comment from Livingston suggests that she had envisioned nothing less than a radically different understanding of cancer that included not only an infectious component but also a nutritional and environmental context. By understanding the nutritional and environmental factors that led to weakened immune systems that in turn created the conditions for infection, tumor genesis, and tumor promotion, she was decades ahead of her time. She was also defining the disease in a way that would lead to conflicts with the food industry and industrial polluters, not to mention the pharmaceutical industry. At the same time, her dream was bound to find a sympathetic, populist response from the general public.

Delinking cancer from nutritional and environmental factors, and cancer treatment from nontoxic and nonpatentable agents, continue to be crucial elements of the conventional understanding of cancer today. Conversely, the linkages forged among cancer etiology and hereditary factors, nonreversible genetic damage, and cytotoxic interventions continue to be central elements in conventional approaches to the disease. Livingston had hoped to forge a completely different set of etiological, political, and therapeutic linkages among nutrition, environmental toxin research, and nontoxic treatment systems.

The incidents also suggest that Livingston was attempting to circumvent suppression from the cancer establishment by making a direct appeal to the public. The appeal could have occurred through press coverage of her work at the AMA or other conferences or from outreach through the planned national network of clinics. In either case, once she had built up a broad public following, it would have been much more difficult to suppress her treatment program for cancer. In the North American alternative cancer therapy community, the strategy was only realized with some success in the 1980s and 1990s, when the number of American and Canadian cancer patients who frequent the alternative hospitals on the Mexican side of the U.S.–Mexico border has reached a figure into the tens of thousands. Likewise, the decentralization of the mass media that occurred during the 1980s and the 1990s—not only via email and the Internet but also with the growth of small presses, talk radio, cable television, health food store publications, and

direct mail solicitations—has made appeals to the public more difficult to suppress than in the 1950s. These cases would suggest that the public (or the various publics that can be mobilized) is the key to understanding the success or failure of suppression. Although victims of suppression may have recourse to the courts, they are usually fighting against deep pockets and such a high degree of infiltration of the legal system by industrial interests that recourse to the courts is less efficacious than the appeal to the public. Of course, when a case goes to a trial by jury, the public enters into the legal system, and at this point the two venues of appeal intersect.

SUPPRESSION, SELECTION, AND ACCOUNTABILITY

Because communities of scientists produce such a great quantity of research claims, and because those claims are often contradictory, a process of selection is necessary to sort through which claims are worthy of greater attention and further investigation. An ideal community of scientists would evaluate the competing claims according to universalistic, technical values such as the strength of evidence, the logic of arguments, and related concerns such as simplicity or parsimony. In this ideal community scientists would also possess interests, but those interests would be universalistic ones. In other words, scientists would make decisions to allocate their scarce resources of time and money to problems that were most likely to produce results that were highly valued by the general public.

Yet, decades of research in the history, sociology, and philosophy of science have shown that the selection of knowledge in real-world scientific communities often deviates substantially from a universalistic ideal. On the one hand, particularistic values drawn from the contexts of gender, time period, nationality, and so on bias the evaluation process (Hess, 1997: ch. 3). Likewise, professional, state, and industrial interests play a shaping role in both the allocation of resources and the evaluation of research claims. In short, particularistic values and interests play a nontrivial role in the selection of scientific knowledge.

Yet, within the category of particularistic selection processes there are differences between mere 'bias' and suppression. The well-known cases of bias in knowledge selection—such as sexist and racist psychologies of intelligence or the ways in which special

interests shape the mandates of state and private funding organizations—are examples of how particularistic values and interests enter into the selection process to bias it in certain ways. Yet, those processes do not necessarily involve suppression. Although biases inform suppression, the latter involves a different kind of process. For bias to become suppression, the original ideal of universalistic values—justification by evidence and logic—has to be subverted to the point that those values have become irrelevant. At that point the suppressed researcher has no option but to move outside the expert community and make a direct appeal to the public.

In the negotiation of bias and suppression, two forms of accountability are involved. On the one hand, nonmainstream researchers are rightly held accountable to the strictest standards of their field. They must not only reach but often exceed the methodological standards for acceptable knowledge production in their field. That is often a difficult goal to achieve because the economic conditions for the production of scientific knowledge have grown astronomically in the twentieth century. As I have discussed elsewhere, medical research is characterized by a 'ladder of evidence,' with the gold standard of the randomized, clinical trial at the top (Hess, 1999). Researchers who are marginalized because of their views will not have access to the gold-standard methods, and thus they will be driven down the ladder of evidence to epistemologically less secure, and politically more vulnerable, methods. Thus, the politics of suppression can continue to play itself out in an apparently 'accountable' or integrity-based discourse of methodological strengths and weaknesses, as long as the political and economic conditions of the hierarchical ordering of methods are obscured from view. One sees this to some extent in the Livingston case, where she lacked access to hospitals and the funding that would enable her to do clinical trials. She and her colleagues were driven down the methodological hierarchy to case study reviews and animal experimentation.

On the other hand, the community of peers in the field of research should be accountable to the general public, which grants scientific research tax exemptions and taxpayer support. In cases where the knowledge is highly applied, the problems are extremely complex, the research is only in the beginning stages, and the industrial or professional economic stakes are great, the pressures for failure of the second type of accountability are likely to be great. Such has occurred in the case of cancer research. Gradually, the public is

waking up the massive waste of taxpayer dollars on basic research and toxic cancer treatments that has led to few advances in survival rates (Moss, 1995). The massive policy failure to evaluate potentially safe and efficacious nontoxic, orphaned therapies—whether they are bacterial vaccines, nutritional supplements, dietary programs, or nontoxic pharmaceutical products—provides perhaps an ideal case for the study of the nonaccountability in research and the challenge for a substantial policy intervention, even if studies and changes will not help the millions of cancer patients who have died because profits have been put before people.

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