Richard Doll and Alice Stewart

reputation and the shaping of scientific "truth"

Gayle Greene

ABSTRACT As the world watched the Fukushima reactors release radionuclides into the ocean and atmosphere, the warnings of Dr. Alice Stewart about radiation risk and the reassurances of Sir Richard Doll assumed renewed relevance. Doll and Stewart, pioneer cancer epidemiologists who made major contributions in the 1950s—he by demonstrating the link between lung cancer and smoking, she by discovering that fetal X-rays double the chance of a childhood cancer—were locked into opposition about low-dose radiation risk. When she went public with the discovery that radiation at a fraction of the dose "known" to be dangerous could kill a child, her reputation plummeted, whereas Doll, foremost among her detractors, was knighted and lauded as "the world's most distinguished medical epidemiologist" for his work. Their lives and careers, so closely intertwined, took contrary courses, he becoming "more of the establishment" (as he said), while she became more oppositional. When it was discovered, after his death, that he'd been taking large sums of money from industries whose chemicals he was clearing of cancer risk, his reputation remained unscathed; it is now enshrined in the "Authorized Biography" (2009) commissioned by the Wellcome Institute, along with Doll's denigration of Stewart as an "embittered" woman and biased scientist. Stewart lived long enough to see radiation science move her way, to see international committees affirm, in the 1990s, that there is no threshold beneath which radiation ceases to be dangerous; recent evidence from Chernobyl is bearing out her warnings. But a look at the making and breaking of these reputations reveals the power of status, position, and image to shape scientific "knowledge" and social policy.

Scripps College, 1030 Columbia Avenue, Claremont, CA 91711.
E-mail: ggreene@scrippscollege.edu.

Perspectives in Biology and Medicine, volume 54, number 4 (autumn 2011):504–31

As the world watched the Fukushima reactors spew incalculable quantities of radionuclides into the sea and air and wondered what effect this would have on our health and that of generations to come, the warnings of Dr. Alice Stewart about low-dose radiation risk assumed a terrible timeliness. As industry, governments, and the media attempted to quiet the alarms, assuring us that radioactive releases will dilute and disperse and become too miniscule to matter, the reassurances of Sir Richard Doll, foremost among Stewart's detractors, also became relevant. It is clear, as proponents and opponents of nuclear energy thrash it out, that there is not much more scientific consensus about the hazards of low-dose radiation exposure today than there was half a century ago, when these pioneer radiation epidemiologists locked into opposition. Their arguments are reiterated as mainstream radiation scientists invoke the Hiroshima studies to assuage fears about Fukushima, while critics cite Chernobyl as a warning.

Stewart's career trajectory reads like a cautionary tale to anyone considering challenging received opinion in such a high-stakes, highly politicized area as radiation science. Though she began with honors that came to few women of her time, the first woman under 40 and the ninth ever to be elected to the Royal College of Physicians (in 1946), once she discovered that fetal X-rays double the risk of a childhood cancer, she never again received major funding in the United Kingdom. Her findings—published in the *Lancet* (1956) and expanded in the *British Medical Journal* (1958)—were not welcomed: the arms race was ratcheting up, the governments of the United States and United Kingdom were promoting "the friendly atom," and nobody wanted to hear that "a tiny fraction" of a radiation dose "known" to be safe could kill a child. Studies of the Hiroshima and Nagasaki survivors, the basis of radiation safety standards throughout the world, assured that risk diminished as dose decreased until it disappeared altogether. Stewart was suggesting there was no threshold beneath which radiation ceased to be dangerous.

First to launch a study to discredit her was Dr. Richard Doll, later, Sir Richard. They moved in the same circles, sat on the same committees and editorial boards; their lives were so intertwined that Stewart taught medicine to the woman Doll later married. Stewart was born in 1906, received her medical degree from Cambridge in 1936, and worked more than 20 years at Oxford; Doll, born in 1912, received his degree from St. Thomas Hospital Medical School in 1937 and was appointed Regius Professor at Oxford in 1969. They both started out with left-wing ideals and an interest in the environmental causes and prevention of disease, both taking part in the Socialist Medical Association that campaigned for the National Health Service after the war, he moving further left than she, joining the Communist Party. But for his demonstration of the link between lung cancer and smoking in the1950s, he was made Regius Professor, knighted, and had a building named after him that became home to Oxford's Cancer Research U.K. Epidemiology Unit and the Clinical Trial Service Unit, both of which he helped found. He spent the latter part of his

career at Oxford's prestigious Imperial Cancer Research Center, becoming, in his own words, ever "more of the establishment"—"about as establishment as the Bank of England," a colleague quipped (Keating 2009b, p. 211)—while Stewart became more oppositional. Drawn into international controversy by her 1970s studies of the Hanford nuclear workers—which found a greater risk to low-dose radiation than was being claimed—she testified on behalf of workers, veterans, and downwinders, while Doll testified against them.

Stewart died in relative obscurity, in 2002, with only a handful of radiation scientists appreciating the importance of her contributions, while Doll went out, three years later, on a cloud of hyperbole, hailed as "the world's most distinguished medical epidemiologist," as "the greatest epidemiologist of our time" (Espinosa-Brito 2005; Tucker 2005). But a year after his death, a front-page *Guardian* headline rocked the world of radiation science: "World-famous British scientist failed to disclose that he held a paid consultancy with a chemical company for more than twenty years while investigating cancer risks in the industry" (Boseley 2006). A letter from a Monsanto epidemiologist was found among Doll's papers at the Wellcome Institute, renewing his contract as consultant, at the rate of £1,000 a day (in 1986, when this letter was dated, that would have earned him in a week what a worker made in a year); only the year before, he had taken part in a review of Monsanto's Agent Orange that cleared dioxin as "only weakly and inconsistently carcinogenic" (quoted in Talbot 2007). Evidence was also found that he received £15,000 from the Chemical Manufacturers Association and Imperial Chemical Industries, the largest producer of vinyl chloride, even as he was exonerating vinyl chloride.

These disclosures inspired a flood of detractions and defenses. But Doll's reputation has survived unscathed, to be enshrined in Conrad Keating's *Smoking Kills: The Revolutionary Life of Richard Doll* (2009). The "Authorized Biography," commissioned by the prestigious Wellcome Institute, depicts Doll as "the quintessence of the scientific ideal," "the ultimate in dedication, perseverance, and integrity" (Keating 2009b, pp. 205, 454), and portrays Stewart as a confused, "embittered" woman whose scientific work was tainted by political sympathies. This is the reputation she had in mainstream radiation circles, the reputation Doll did his best to perpetuate when he wrote her entry in the *Oxford Dictionary of National Biography*, after she died, describing her as having damaged her "reputation as a serious scientist."

Stewart would not have been surprised by his opinion—he made no secret of it—though it did mystify her: "Why we couldn't have been working together all those years, I don't know, since we shared a common goal, to understand the causes of cancer," she said to me. I didn't know either, and the long interview I had with Doll at Oxford in 1998 gave me no deeper insight into their vexed relationship. I could only conclude this part of my biography, *The Woman Who Knew Too Much: Alice Stewart and the Secrets of Radiation* (1999), with a series of

questions: was it a difference of opinion about radiation risk that motivated his antipathy, or personal dislike, or sexism, or rivalry?

But information has come to light since their deaths that makes me see their relationship in a new light. More than a conflict between two striking figures who took contrary courses in their lives and work, which is how I read it in 1999, I now see a story about the making and breaking of reputations and the power of reputation to shape scientific "knowledge." Doll was a man with "the instincts of a politician," as his biographer writes. With his backward sweep of white hair, his high brow, patrician profile, and "notoriously elegant dress" (Keating 2009b, p. 439), he could have been sent by Central Casting to play the role of Distinguished Scientist, and he played the part brilliantly, using his status, position, and the podiums afforded him to project an image that had considerable influence on scientific opinion and public policy. From early in his career, he had a knack for producing radiation risk estimates pleasing to the powerful. When Prime Minister Anthony Eden sought a go-ahead for nuclear testing, Doll produced a report that concluded, "The present and foreseeable hazards from external radiation due to fall-out from the test explosions of nuclear weapons . . . are negligible"—thereby giving the okay to the H-bomb trials that took place in the Pacific in 1957 and 1958 (Keating 2009b, p. 218). His complacency was not shared by the more than 9,000 scientists who signed Linus Pauling's petition warning that weapons testing would produce millions of cancers and birth defects, a petition that helped bring about the 1963 moratorium on above-ground testing. But Doll's reassurances about low-dose radiation have helped create the climate of complacency about radiation risk that has enabled the nuclear industry to win public endorsement and move forward, resurrecting itself from its ruins at the end of the last century, when it crumbled under its costs, inefficiencies, and catastrophes.

In 1943, Stewart was invited by Oxford Regius Professor John Ryle to help build a department of Social Medicine, a new area that focused on the social and environmental causes and prevention of disease. When Ryle died in 1950, Social Medicine was demoted from a department to a "unit," leaving Stewart a lowly reader with tenure but no staff or funding. Having won a grant of £1,000, she set out to investigate rising rates of childhood leukemia. She devised a questionnaire for mothers who had lost children to cancer and mothers who had not (which gave her a control group), a questionnaire that cast a wide net, asking about exposure to automobiles, aerosols, hens, rabbits, dogs, colored sweets, fish and chips—and, "had you had an obstetric X-ray?" It was a revolutionary idea, "asking the mums," but Stewart, a mother herself, thought they might remember something the doctors did not. She included the question about X-rays because it was common practice in the 1940s and 1950s for doctors to x-ray pregnant women in the third trimester, to ascertain the position of the fetus. Within

the first 35 questionnaires, the answer leapt out: the children whose mothers had been x-rayed were running three to one with cancer—not just leukemia, but all kinds of cancers.

Doll countered with a study, coauthored by William Court-Brown, that found no association of fetal X-rays with leukemia (Court-Brown and Doll 1957). His findings were consistent with the Hiroshima studies, which found no excess of cancer in children exposed in utero to the blasts. Doll later admitted that his study was "not very good" (interview, Oxford, Oct. 9, 1998; Doll and Wakeford 1997): it lacked a control group, looking only at children who'd been x-rayed; it looked only at leukemia rather than all childhood cancers; and it did not follow subjects long enough for the effects of the radiation to become apparent. "But it didn't matter, the damage had been done," as Stewart said; "After the Court-Brown Doll study, we never got support from Britain again. If funding hadn't come through from America, we'd have been finished."

The Court-Brown-Doll study enabled doctors to go on x-raying pregnant women for the next two decades. Stewart spent those decades testing and retesting her hypothesis and expanding her database, until it included information about prenatal exposures to infections and inoculations, parental age, occupation, social class, smoking, and, later, ultrasound. With the instinct for inclusiveness that was a signature of her science, she developed a monumental set of data, the Oxford Survey of Childhood Cancer, the world's largest and longest-running study of childhood cancer, producing evidence so compelling that, in the 1970s, official bodies recommended against fetal X-rays.

She next found herself on a collision course with Doll when he came to Oxford as Regius Professor in 1969. He announced, in an interview with Georgina Ferry, that "there was little [at Oxford] in the way of epidemiology research," which enabled him "to bring several people with [him]," including his protégé and subsequent collaborator, Richard Peto (Ferry 1993, p. 11). He set about building his department, conferring research budgets, distributing chairs, "scouring the academic field all around the world" for talent (Keating 2009b, p. 273). Julian Peto, Richard Peto's brother and, like him, a statistician who worked with Doll, commented:

> It was odd that [Stewart] wasn't given a chair really because she was quite eminent and was a real founding father of the science. But . . . Richard created a new chair—social and preventive medicine and gave it to Martin Vessey—and that was a bit peculiar . . . it just seemed so natural that she should have it. . . . Martin . . . didn't have an outstanding reputation, and for him to be given that chair at 37 years old, over her, was the most extraordinary affront, and it really was a bit of power politics. (Keating 2009b, p. 304)

Stewart, in her sixties, was easily brushed aside. She had been immersed in her research, scrambling around for funding, and she had raised two children (on her own); after her son's death, she had helped care for his children—which left her

little time to do the kind of networking that creates allies. Made unwelcome at Oxford, she accepted a position at Birmingham. But then came the question, what to do with the Oxford Survey, which by this time consisted of 23,000 manila envelopes. "Since my office in Birmingham was a trailer, a sort of hut, and the records were prodigious, it became a real problem." She offered to leave the files at Oxford, feeling that the survey ought to continue: "We were building a database that would have allowed us to test several hypotheses about cancer. It ought to have been put on an ongoing basis. That's what you have to do if you're going to find the cause for cancer." But Doll had no use for the files—even though he would, several years later, launch his own study of childhood cancer, with great fanfare and £6 million funding (some from the nuclear industry), announced in 1992 by the U.K. Co-ordinating Committee on Cancer Research, as a "new" and "unique nationwide investigation into the causes of cancer in children," "the largest and most wide-ranging study" of its kind "to be carried out anywhere in the world."

"It's as though we'd never existed," Stewart said, "though it was hard to see the difference between his study and ours. . . . It's hard to describe, like a current I was swimming against. When the Medical Research Council put together a committee on epidemiology, Doll was made chairman, which gave him enormous influence. [He directed the MRC's Statistical Research Unit from 1961 to 1969.] After that, every department in the country was called in to consult—except us. We never got invited to official meetings, never got asked to give our point of view." She was thus excluded from the processes, decisions, reviews, commentaries that shaped medical research in the United Kingdom, all the while she was developing an international reputation as an authority on radiation risk and receiving invitations to speak and consult from researchers throughout the world.

Omissions are difficult to document, but here's one that leaps out. Doll, reminiscing (in "conversation" with Sarah Darby in 2003) about the early days of epidemiology, describes how he and several young men (all men) gathered around "those few senior people such as Professor Ryle at Oxford who were interested in developing the subject." He makes no mention of Stewart, though she was one of those young scientists: in fact, she is the one Ryle chose, when he was made Regius Professor at Oxford in 1945, to help launch his program in Social Medicine—which set her on the path to epidemiology. Doll simply writes her out of the story.

In 1974, Stewart, having found a trailer in Birmingham where she could store the Oxford Survey, was in the process of packing up to leave Oxford, when she got a phone call from Thomas Mancuso, a professor at the School of Public Health at the University of Pittsburgh. Mancuso, an epidemiologist expert in industrial medicine, had been commissioned by the Atomic Energy Commission (AEC) to do a study of the health of nuclear workers at Hanford, the vast

weapons complex in Eastern Washington that had produced plutonium for the Trinity and Nagasaki bombs. Since workers' exposure to radiation was well within the limit "known" to be safe, as determined by the Hiroshima studies, it was assumed that such an investigation would turn up nothing incriminating.

Mancuso had been looking at workers' health records for several years and had found no evidence of increased cancer. The AEC was urging him to publish, but he wanted another opinion, so he called in Stewart; she and her statistician George Kneale took the long trek to the United States. After several days studying the data, she declared, "I believe this industry is a good deal more dangerous than you are being told." The government terminated the study, going so far as to break in to Mancuso's office, attempting to seize his work. Stewart absconded with what data she had, taking them back to England, and continued to study them, with Kneale and Mancuso, for the rest of her life—consistently finding a 10 to 20 times higher cancer risk than was being claimed (Greene 1999). These findings were dismissed, as the fetal X-ray findings had been, on the grounds that "exposures were too low" to produce a cancer effect, "too low" as determined by the Hiroshima findings (Wing, Richardson, and Stewart 1999).

The Mancuso scandal inspired the efforts of antinuclear activists to pry radiation data loose from government control and open it to the scrutiny of independent scientists. In the last years of the 1980s, amidst breaking scandals throughout the nuclear weapons complex and the growing furor of citizens' groups, Stewart was much in demand as a speaker and expert witness. Though in her eighties, she was often the only respected scientist to respond to activists' calls. She made dozens of appearances: on a given trip to the States she would lecture at Yale, the University of Chicago, and in Fork River, Idaho, to a group of activists. "Audiences loved her," recalls Diane Quigley. "Her humor . . . her folksy expressions and tales, her obvious compassion, were irresistible. . . . She was equally at ease with all types of people, never talked down to them, her presentations were complex and ambitious, assuming her listeners' intelligence" (Greene 1999, pp.183–84).

Finally, after two congressional hearings and several Freedom of Information Act requests, activists succeeded in getting the Department of Energy to open the nuclear worker health records to independent scrutiny. In 1992, Stewart and Kneale took possession of approximately one-third of the health records of workers employed by the U.S. nuclear industry since it began in 1942, records not only from Hanford but from Los Alamos, Oak Ridge, Rocky Flats, Savannah River, and other nuclear facilities. It was a landmark victory, hailed by Keith Schneider on the front page of the *New York Times* as a blow for scientific freedom (Schneider 1990a, 1990b).

"It helps, in this area, to be long-lived," as she commented.

She lived to see another victory announced in a front-page *New York Times* headline, nearly a decade later: "U.S. Acknowledges Radiation Killed Weapons Workers, Ends Decades of Denials: Compensation Is Possible for Survivors of

Cancer Victims Who Worked on Bombs" (Wald 2000). This happened a few weeks after my biography went to press, too late to include it. It was clear that her story was not over.

The Oxford Survey brought Stewart into conflict with the medical profession, but the Hanford studies brought her up against more formidable authorities: the nuclear industry, the governments supporting the industry, the international regulatory commissions that set standards for radiation risk—and, ultimately, the Hiroshima studies on which these standards are based. Turning her attention to these studies, she was amazed at what she found.

The Atomic Bomb Casualty Commission, as it was originally called, began its studies of the Hiroshima and Nagasaki survivors in 1950, five years after the blasts. (The name was changed to "Radiation Effects Research Foundation" in 1975, to get the "atomic bomb" out, around the same time the "Atomic Energy Commission" was changed to the "Department of Energy.") According to its calculations, the death rate from all causes except cancer had returned to "normal" by 1950, and the cancer deaths were too few to cause alarm: all those expected to die from radiation effects had already died, and no further radiation-related effects were expected.

Stewart's experience as a physician and researcher told her it would not be possible for a population to return to "normal" a mere five years after so devastating a holocaust. This was not a normal or representative population that could yield reliable information about the health effects of radiation exposure: it was a survivor population made up of the heartiest. The Oxford Survey had found that children incubating cancer became 300 times more infection sensitive than normal children (Stewart 1997). Children so immune-compromised would not have survived the harsh winters that followed the bombings, when food and water were contaminated, medical services at a halt, antibiotics scarce; but these deaths would not have been recorded as cancer deaths. The survivors' studies were also problematic on account of the guesswork that went into estimating radiation exposure; the radiation the bombs gave off was calculated according to tests done in the Nevada desert and was recalculated several times in subsequent decades. Most importantly, the extreme, high-dose radiation this population was exposed to could tell you nothing about the effects of chronic, low-dose radiation over time, the kind of exposure received by nuclear workers and people living in the vicinity of reactors or accidents.

"Radiation risk estimates based on A-bomb survivors would be substantially underestimating the cancer risk from protracted low-level exposure to radiation," Stewart maintained, and criticized the "outdated emphasis on evidence about radiation health effects based on studies of A-bomb survivors" which have been "used as a lens through which studies of radiation-exposed . . . populations are viewed" (Wing, Richardson, and Stewart 1999, p. 147). The data were "skewed," and calculations based on them were no better than "Bible arithmetic."

But there were powerful incentives to downplay radiation risk. The first

Western scientists and doctors allowed in to the devastated cities, in late 1945, were with the U.S. Armed Forces, under military escort. The Japanese scientists and physicians who had been on the scene told horrific stories of people who had seemed unharmed but then began bleeding from ears, nose, and throat, hair falling out by the handful, bluish spots appearing on the skin, muscles contracting, leaving limbs and hands deformed, many dying from some unidentified "atomic plague"—but their accounts were ignored or suppressed. When Tokyo Radio announced that people who entered the cities after the bombings were also dying of mysterious causes, American officials dismissed the allegations as propaganda intended to imply that the United States had used an "inhumane" weapon. As State Department attorney William H. Taft asserted, the "mistaken impression" that low-level radiation is hazardous has the "potential to be seriously damaging to every aspect of the Department of Defense's nuclear weapons and nuclear propulsion programs. . . . it could impact the civilian nuclear industry . . . and it could raise questions regarding the use of radioactive substances in medical diagnosis and treatment" (Jacobs 1986). A pamphlet issued by the Atomic Energy Commission in 1953 "insisted that low-level exposure to radiation 'can be continued indefinitely without any detectable bodily change'" (Caulfield 1989, pp. 99, 123). The Atomic Energy Commission was paying the salaries of the Atomic Bomb Casualty Commission (ABCC) scientists and "monitoring" them "closely—some felt too closely" (Lindee 1994, p. 107).

Doll was among the first "non-military foreign scientists" invited to Hiroshima, in 1957, to evaluate the Atomic Bomb Commission's work (Keating 2009b, p. 221). He told me he had been "satisfied with the Commission's procedures and conclusions"—which I had no reason (then) to doubt.

When Doll was knighted in 1971, two years after he came to Oxford, some of his left-wing friends found it an oddly incongruous honor for "Red Richard" (Keating 2009b, p. 275). "To suddenly become Sir Richard Doll, the most powerful person in medical academia, was an extraordinary change," comments Julian Peto: "He had real power and he used it, and Alice Stewart was an early victim" (Keating 2009b, p. 304). He had welcomed the Regius Professorship, since he "questioned whether he would ever conduct original research again," and became absorbed in university administration. He cut a fine figure, sporting around Oxford in an open MGB Roadster, wearing elegant suits, the details of which were overseen by his wife, who played the Oxford grand dame in their splendid Victorian house. "We have a communist who lives in great luxury . . . who's got original paintings of the sort that most communists would feel were the things that only the rich should have," was a view expressed by critics (Keating 2009b, pp. 270–76). Whatever the processes that transformed "Red Richard" to Sir Richard, he emerged in 1981, with the highly influential monograph *The Causes of Cancer*, as a powerful spokesman for industry.

In 1978, Joseph Califano, Secretary of Health, Education, and Welfare under

President Jimmy Carter, published an "Estimate Report" that gave a figure for workplace cancer of around 20%, unsettlingly high. Richard Peto was invited by the U.S. Office of Technology Assessment to provide a more "judicious" assessment. The report he and Doll produced, *The Causes of Cancer* (1981), attributed the majority of cancers to individual lifestyle choices, claiming that roughly 65% of U.S. cancer deaths were caused by diet and smoking, with only 4% of cancers due to occupational influences and a mere 2% to industrial pollution.

There were reputable scientists—Richard Clapp, Irving Selikoff (1992), Devra Lee Davis (1986), Samuel Epstein (1998) —who took issue with these figures, pointing out that Doll and Peto had excluded African Americans and people over 65. (African Americans suffer high rates of cancer and are the most likely to be exposed to toxins, since they have the highest-risk jobs and live in the poorest neighborhoods.) But the findings were welcomed by those who maintained that "'cleaning up the environment' is not going to make much difference in cancer rates" (Clapp et al. 2005, pp. 4–5). The report was embraced by the Office of Technology Assessment and the National Cancer Institute, and praised by the *New York Times* for bringing about "a less alarming view of the danger from carcinogenic pollutants" (Boffey 1982). It enabled the Reagan Administration, come to power in 1981, to weaken the Environmental Protection Agency, dismantle environmental, health, safety, and occupational legislation, and reduce by about 70% the number of cases against polluters referred to the Justice Department (Proctor 1995). It allowed Margaret Thatcher to move forward with an aggressive expansion of the nuclear industry. Doll was called as expert witness in dozens of court cases and official inquiries on U.K. facilities; he testified against plaintiffs suing British Nuclear Fuels Limited (in the Sellafield cancer case, as the media called it), against workers and veterans. He saved governments and industry untold millions in compensation and regulatory efforts.

"One of the biggest myths in recent years is that there is a cancer epidemic caused by exposure to radiation, pollution, pesticides and food additives. These factors have very little to do with the majority of cancers," Doll announced (Cookson 1994). In a puzzling reversal, the researcher who had declared in 1967 that "an 'immense' number of substances were known to cause cancer . . . besides cigarette smoking, exposure to nickel, asbestos, tarry products in gas production, and radioactivity, were major causes of cancer," now virtually dismissed environmental pollution as a cause of cancer and lashed out at those who saw such a link as "irrational" and unscientific (*Times*, June 8, 1967). He warned, in the Harveian Oration, that "environmentalists" might "whip up irrational prejudice, unfound in science" (*Daily Telegraph*, Feb. 7, 1983). He cautioned, in a letter to the *Daily Mail*, that environmentalists represented "an irrational ideology opposed to science, to industry and to progress" (June 3, 1992). He urged the public "to trust industry and industrialists, science and scientists"—these are "the people with the key to the future"—and ignore warnings by the powerful "anti-science Mafia" (Walker 1998).

Doll began to display what an observer described as a "penchant for splenetic and patronizing attacks on those who published findings running counter to his assertions" (Rory O'Neill, editor of *Injury Watch*, personal communication, Aug. 20, 2008). When asked why he had not considered an assessment of vinyl chloride by the International Agency for Research on Cancer in Lyon, which had found a link between vinyl chloride and brain cancer—as well as lung cancer, leukemia, and lymphoma—he called the investigators "incompetent" (Walker 2011a). Of the Swedish cancer researcher Lennert Hardell (2006), who had testified against dioxin in the 1985 court case, Doll wrote an unsolicited letter to the judge, claiming that "many of his published statements were exaggerated or not supportable . . . [and] should no longer be cited as scientific evidence" (Walker 2011b). Hardell termed Doll's science *epidemonology* (Walker 2011a). Of the work of Devra Lee Davis, a leading analyst of environmental toxins and cancer, he used the terms "uninteresting," "uninformative," "boring," "old junk" (Marshall 1990). It is difficult to square this with the claim made by his biographer in a 2009 podcast, that he was "always supporting of other scientists."

Most of Doll's criticisms of Alice Stewart were behind the scenes or after her death, but in 1996, he lit out at her in a Channel 4 British television documentary, "Sex and the Scientist," dismissing her methods as "a bit slapdash": "She was very enthusiastic, she got a great deal of cooperation throughout the country but she tended to accept results at their face value without detailed checking to test their accuracy." *Slapdash* is an odd term for a researcher who took 20 years corroborating the fetal X-ray findings and the rest of her life studying the Hanford data, and who published more than 400 studies in refereed journals—no mean feat, given the unpopularity of her findings. But Doll, to a nationwide television audience, described the Hanford work as "barmy . . . yes, it just wasn't a scientific analysis" (though it was a crucial part of the case that persuaded the U.S. government to grant nuclear workers compensation for cancer). When asked, in this documentary, how he perceived Stewart, Doll said, "Well, I don't really perceive her frankly. I mean I think that's . . . except when it's . . . except when I have to." And yet he did "perceive her" long enough to write that damning epitaph in the *Oxford Dictionary of National Biography*, to fix her reputation for all time.

Doll's version of Alice Stewart is the version his biographer gives us. Keating (2009b) portrays Stewart as embittered by her failure to achieve status and recognition, "antagonistic," "adversarial," "abrasive," "rude," "caustic," "paranoid," and without humor. This is the reputation barrister Stephen Sedley was familiar with when he decided not to call her to testify in a case against British Nuclear Fuels, owner of the Sellafield nuclear facility, "because of the way in which the establishment had branded her an eccentric . . . slightly nutty, loose cannon—because she wouldn't accept the conventional wisdom, *and now I gather she has turned out to be more right than wrong*" (Keating 2009b, p. 311, emphasis added). This is the reputation Klarissa Nienhuys, a scientist from the Netherlands, encountered at a

seminar at Groningen University, when a speaker implied "that this scientist had more or less flipped out and now these two crackpots, Stewart and Mancuso, had found each other"; when Nienhuys met Stewart, she found her "friendly, warm and human," "stimulating and nourishing me as an intellectual as hardly anyone ever had before" (Greene 1999). Steve Wing, a young epidemiologist who dreaded meeting Stewart on account of the things he had heard about her, was "totally taken" by the woman he met: "she is so wonderfully unassuming and down to earth and goes to such lengths to make you comfortable, to behave in a way that denies the inequalities of society" (Greene 1999).

How well did Keating know her? He does not say, though he slips easily onto a first-name basis, referring to her as "Alice" (Doll is always "Doll"). In the five years I spent writing my book, I got to know her well, and the person I knew bears no resemblance to the character Keating describes. Over the years, I spent several weeks with her, tagging along with her to her work in Birmingham, traveling to Wales and Bristol to meet her family; I heard her lecture at a conference in New York and watched her consult on a legal case at Rocky Flats and talked with her late into the night, many nights, at her cottage near Oxford—and I can tell you, she was excellent company, had a splendid sense of humor and an extended network of friends, colleagues, and fellow scientists who were there for her in her old age. "Belligerence," says Keating, "was, as often as not, her first emotional response" when she met with resistance (p. 310). But as the third of eight children, five of whom were male, tact was more likely to be her first response. Far from being "allergic to decision-making process," as Keating describes her (p. 309), she was legendary for the way she inspired assistants to do their best for her, mobilizing student volunteers and health officers to give their all—it was how she kept her projects going on such slender means (she often paid staff from her own salary). "The way she could motivate so many people to help her carry out her work . . . was extraordinary," says a Birmingham colleague Tom Sorahan. The way she inspired me to take on her biography, when I was writing another book and teaching full-time in California, was extraordinary, too.

Keating hints darkly and repeatedly at her quest for "status," but she was not ambitious for honors. She was pleased to receive the Right Livelihood Prize in 1986, the Alternative Nobel, as it's called, conferred in the Swedish Parliament the day before the Nobel to honor those who have made contributions to the betterment of society. She was happy to be awarded the Ramazzini Prize in 1991, the leading prize in Italy for epidemiology. But she had none of the status anxiety one associates with Oxbridge, and so little drive for self-promotion that more than one friend commented on it. "She was not prepared to go to the right places and be nice to the right people," observed Molly Newhouse, a lifelong friend and occupational health expert herself. "She could always think of something more interesting to do than to go to a vice-chancellor's garden party where you had to dress up and be nice to people" (Greene 1999, p. 256).

Doll, on the other hand, was, in his biographer's words, "a high-status person"

accustomed to being "treated in a deferential manner," "a natural patrician" (Keating 2009b, pp.143, 447). David Weatherall describes him as "a superior kind of person and used to getting his own way" (p. 284). "Few people, even his closest colleagues, had a personal relationship with him," observes Julian Peto (p. 284). "He emanated intimidation," says Marie Kidd, who worked with him on a lung cancer study (p. 143). The words *detached* and *detachment* recur in his biography; I find no mention of a sibling. "When asked if he was a 'family man,' Joan [his wife] replied, 'No. Richard likes his family, but he's very busy with his work." His daughter Cathy "did not think that he was a real dad to her" (pp. 256–57). Stewart, meanwhile, was changing diapers and incurring disapproval when she took time away from family to do her research. (She loved children, and was good with them.)

Michael Dunnill, who had helped bring Doll to Oxford in 1969, was one of many people watching that 1996 Channel 4 documentary who was shocked by Doll's insults: "he absolutely destroyed Alice Stewart on television. He said that her work was 'slapdash.' Doll can be ruthless. He destroyed her really" (Keating 2009b, p.312) But he did not destroy her. I was with her, watching that documentary, and I heard what she said: "I really—I'm really quite shocked! I knew he didn't approve of me, but that he'd say this! It explains a lot—the cold shoulders, the lack of offers or invitations. I see that I haven't made it up." Far from being devastated by Doll's remarks, she was relieved that the undercurrent of disapproval and dislike was out in the open for the world to see.

What she envied Doll for was not status or recognition—it was the support he had, the teams of researchers, "the best in Britain," as she said, statisticians like the Peto brothers, whom she admired. She has been much criticized for choosing George Kneale as her statistician, a man of enormous intelligence but painfully awkward personality, whose communication skills have never been up to clarifying or defending the unconventional methods he uses. But she never had Doll's resources to "scour . . . the world" for talent. I wonder what she might have accomplished if she had. She died with many theories untested.

"It is very difficult for the Alice Stewarts of the world to survive," said Sheldon Samuels, presenting her with the Ramazzini Prize. It's "a struggle not only to be independent but to find the support—social, intellectual, and material—that a scientist needs." "The wounds and scars received by advocates of unpopular opinion, and by persons publishing reports of adverse effects that are 'inconvenient' . . . are real but rarely presented in scientific journals," writes Morris Greenberg (1991, p. 717). "It hasn't been easy for Alice. . . . Yet she carried on, making her case in a sensible, unhysterical manner. She is a polite, gentle soul who doesn't express herself aggressively, just firmly. I respect also that she doesn't complain" (Greene 1999, p. 230).

She put up with slights, snubs, rebuffs, and rudeness that takes the breath away, eyes rolled, eyebrows raised, glances exchanged. She was called "senile, "barmy," "gone round the bend" as far back as the 1950s. Keating (2009b) professes "com-

passion" for the confused, benighted Alice, while characterizing her as "angry," humorless, "belligerent," "campaigning" (he calls her biography "tendentiously titled"; p. 292). These are familiar stereotypes, stock terms used for outsiders who are opposing insiders' positions, who are likely to look contentious and tendentious, since they are challenging accepted opinion.

But "embittered," Stewart was not. I came across "embittered" scientists while writing my book, independents more badly scarred than she. Thomas Mancuso, whose career and reputation were destroyed by the U.S. government, felt so hard done-by that he refused to speak to me, and then, when my book came out, complained to Bob Tredici that I hadn't interviewed him. But Stewart always said she was "extraordinarily lucky"—lucky to have a steady income, lucky to have bought a cottage in the Cotswolds when cottages could be afforded, blessed with a physical and psychological robustness that enabled her to carry on. She had a marvelous capacity to "make the best of it." She felt that being a woman had worked to her advantage: "If I'd been a man, I'd never have stood it—the pay was too low, the prospects too bad. As a woman . . . I was left to go my own ways." She even felt that being marginalized "was just right for me personally," and that her work was better for the opposition: "Since no one accepted our position, we had to dig in and prove that it was so." And since she had no full-time staff or department, she could take stands: "I speak out because there are not a lot of people who can. I have nothing to lose. A lot of people do."

None of the above tells us about the relative merits of Doll's or Stewart's science, but it does point to the distortions in the Doll-Keating version of her personality. Doll found it "unacceptable," says Keating, that Stewart "was making a political campaign out of the work she was doing": he saw this as "incompatible" with being "a serious scientist" (p. 303), implying that her objectivity had been compromised by her sympathies with the activists. Doll was himself, according to his biographer, a "neutral, dispassionate scientist" who kept aloof from "campaigning" (pp. 108, 230)—as though writing letters to the national press and denouncing opponents in public forums were not a form of "campaigning"; as though accepting large sums of money from industries while assessing the cancer risk of chemicals produced by those industries incurred no taint.

Yet it could be argued that Stewart was the one innocent in the whole agenda-driven fray: she blundered into it, as surprised as everyone else when her investigations turned up so lethal an effect from X-rays. She was never the banner-waving "campaigner" Doll and Keating make her out to be, but a scrupulous scientist who made every effort to keep her work untainted by sympathies. "I'm very sympathetic with the activist point of view," she said, "but I'm not an activist, and I'm determined that this sympathy not influence my interpretation of the data." "You must hear what the data are telling you, see what the numbers say": this was the first principle of her work.

It could be argued it was Doll's work that was slanted—toward the interests of "the ruling class," says Robert Park (2011) of the National Institute of

Occupational Safety and Health. "Doll's dealings with occupational issues were almost always with individuals from the management side, the data he analyzed was usually collected, described and documented by the management class. His deliberations in interpreting findings were within that same social context." Relying on company data provided him by Turner and Newall, Ltd., makers of asbestos products, for example, Doll found much lower rates of asbestos-related disease in the asbestos insulation industry than did Irving Selikoff, who looked at medical records provided by trade unions, records of workers who had daily contact with asbestos (Tweedale 2007b, p. 70). "Learning about the nature of industrial process exposures by listening to the architects and managers of the system rather than those who must function, succeed and survive within the lower reaches of it, cannot help but color one's fundamental perspective," Park points out. "The closest he came to most workers . . . was from a podium."

Stewart did not, truth be told, think well of Doll's epidemiology. She faulted him for following trodden paths (such as the Hiroshima data). She would have been astonished to hear him described as *revolutionary*, though Keating uses that word in his subtitle. Even joining the Communist Party, though no doubt courageous and sincere, was something British intellectuals were doing in the 1930s and indicated, perhaps (as Stewart commented), that he was not adverse to "party lines."

She criticized Doll for not carrying his studies out for the length of time radiation studies require, for not casting a sufficiently wide net: "you have to include the noise rather than try to shut it out in the interest of time or tidiness." Doll, she said, was always in a hurry: "He has been heard to say that no survey that hasn't been completed within five years is worth its salt. This has had a very dampening influence on the whole field. You can't do it that way . . . you can't tidy it up like this. It needs a long time; it's got to have untidy edges." Doll admitted, of a study of ankolysing spondylitis he did with Court-Brown, that "we were rather keen to get it completed quickly as we knew the Americans were writing a report as well and we wanted to get ours out before theirs" (Cook 2004, p. 335). The exclusion of people over 65 and African Americans from the 1981 *Causes of Cancer* cut out a lot of "noise." Similarly, in the vinyl chloride study, "older, highly exposed workers were left out, as were entire plants" (Walker, "Sir Richard Doll").

In an interesting twist to their long, complex relationship, Doll has twice admitted publically that Stewart was right, and elsewhere stated positions that agreed with hers but that he did not publicize. First was his admission that she had been right about fetal X-rays, and that his own study was "not very good." He also told me, "it looks like Alice will be proved right on this —the A-Bomb studies are turning up effects other than cancer." (By 1998, when he said this, more radiation-related health effects had turned up in the survivors [*Hiroshima and Nagasaki* 1981; Malko 1998; Shimizu et al. 1992]; the longer these studies go on, the more effects they turn up, as Stewart predicted). Years later, I came across

a confidential memo written by Doll and Court-Brown to the Medical Research Council (MRC), after he visited Hiroshima: "It must be presumed that many of those killed in Hiroshima would have developed leukemia if they had survived so that the surviving population is a selected one . . . and this would have introduced a bias into subsequent incidence of various types of leukemia" (Alvarez 1987). That sentence could have been written by Stewart: Doll seems to have shared her reservation, but not wished to publicize it.

Then, too, there was a paper he wrote that was accepted by the *Journal of Radiological Protection* in 1955 but not published until 1996, which states, "there is *no threshold* dose below which no effect is produced" (my emphasis). Why had he not published this in 1955? It seems that Sir Harold Himsworth, the Secretary of the MRC, advised him not to: "Look, I think this is so speculative, I wouldn't publish this if I were you, it will only damage your reputation as a scientist." And where did Himsworth get the idea that the paper would damage his reputation? The Atomic Energy Commission (AEC) had "advised Himsworth that it really wasn't reliable and shouldn't be published" (Cook 2004, p. 335). It's easy to see why the AEC would not want a scientist suggesting there is no threshold beneath which radiation ceases to be dangerous. Nor would it be pleased to hear that "the time which has elapsed since the explosion of the bombs is short in comparison with the induction time usually observed in human cancer, and it is probable that the total number of cases attributable to the explosions will eventually be considerably increased"—as Doll states in his 1955/96 paper (p. 3).

So the AEC warned Himsworth who warned Doll against publication on the grounds that this paper might damage his reputation, and Doll withdrew the paper from publication. He only published it in 1996 because it was cited in a legal case. "So I said to our lawyers, 'Look, I've got to get hold of a copy of that paper.' And, of course, they were able to get hold of a copy because the other side had cited it. I got the paper, I read it, and I thought, 'This is bloody good.' The estimate really wasn't far from what we would make nowadays, and to cut a long story short I published it 40 years later," he told Cook (p. 335), seeming oblivious that there was anything odd or off-putting about his failure to publish it in the first place.

By 1996, official estimates of low-dose radiation risk had moved in Stewart's direction, with reports from the Biological Effects of Ionizing Radiation (BEIR V 1990) and the National Radiological Protection Board acknowledging that there is probably no threshold beneath which radiation ceases to have an effect (Strather et al. 1995). As low-dose radiation was acknowledged to be more dangerous than previously assumed, Doll's unpublished estimate came closer to "what we would make nowadays," and he took credit for knowing the risks all along—as it seems he did. Yet it was Stewart who went public with the warning—and paid the price—and Doll who was first to launch a study to discredit her and last on the scene to write a damning epitaph.

Yet he came to her funeral. At a country church not far from Oxford, a small

group of family and close friends had assembled, when in walked an aged man with a patrician air. A murmur ran through the gathering—Who's that? Can that be? He came late, left early, and, to my knowledge, spoke to no one.

"What had been that stubborn, vain old man's thoughts, as he mouthed the words of the hymns?" writes Margaret Drabble, who found the situation so intriguing that she put it in her novel, *The Sea Lady* (2007). "Had he repented of his attempts to block the woman's research, or had he attended the funeral in a spirit of triumph? In order, finally, to see her off, and to make sure that she had gone to earth for good. Rivalry endures until death, and after . . ." (p. 32).

Some said it was conscience, most saw it as a political show. But a show for what, for whom?

We all have a story or stories of our lives that make sense of who we are and how we came to be. Here is how Doll tells the story of the lung cancer discovery:

> after the war the mortality [from lung cancer] had gone on going up and . . . the MRC had a conference to discuss it. And the conference concluded—this is back in 1947 . . . that really we ought to try to find out to see if you could find any cause for [the increase]. . . . And, in fact, it was left to Bradford Hill and he asked me to help him. And we said, "Well, we'll interview patients attending hospitals for lung cancer." (Cook 2004, p. 33)

In fact, Doll was assigned to a committee that was commissioned to look specifically at links between lung cancer and smoking—as this summary of the MRC meeting indicates: "At this meeting it was agreed that Professor Kenneway, Dr. Percy Stocks, and Professor Bradford Hill should be asked to draw up a preliminary plan for large scale statistical *study of the past smoking habits* and other characteristics of persons with cancer of the lung" (emphasis added). "Doll was a young man put on a committee," says Stewart, who was present at the meeting: "Once you got in there, it was big as a house. You couldn't miss it." There were actually five papers on the dangers of smoking published the year Doll and Hill published theirs, 1950; cigarettes had been suggested as a cause of lung cancer as far back as the 1920s (Keating 2009b).

You can see, in Doll's account, an exaggeration of his role in the discovery. John Lilburne (2006) describes Doll as given to "telling Just-So" stories about how he came upon the smoking–lung cancer link, a tendency "to confabulate his own narratives, perhaps unknowingly":

> Almost all major scientists restructure their own narratives of discovery after they become famous, as one can see by examining the lab notes of Nobel prize-winners and comparing them with the subsequent autobiographical story. There is not necessarily anything very harmful about this very human trait, unless it obscures the contribution of others or misleads young scientists about how great work is done.

Lilburne notes that major recognition usually "comes relatively late in a scientist's career, and is therefore unlikely to have a major impact on his or her research. When it comes early, or the scientist remains active for decades, perhaps as the head of a research institute, such innocent confabulation can provide a sense of infallibility or invulnerability." Recognition did come early to Doll, and he remained active for decades.

Geoffrey Tweedale (2007a) comments: "He eventually constructed, perhaps unconsciously, his own narratives of his early tobacco work and the reaction of the tobacco companies. He dismissed pioneering work in other countries as unimportant and flawed, whilst mythologizing the roots of his own research, perhaps under the welter of adulation" (p. 234). And "a welter of adulation" there was. Take this obituary by David Simpson (2005):

> Days before he entered hospital, he attended the Green College summer dinner, a fine figure in a white tuxedo, charming friends young and old, and enthusing about the new building that bears his name. He drew envious glances from men, who told each other they would be happy to be half as active at 70, never mind 90, and admiring glances from women, some joking with each other that given the chance, they would leave home for him. Sir Richard Doll used to say that he wanted to die young as old as possible. That is exactly what he did; and thanks to his life's work, millions of others have the chance of doing the same. (p. 290)

A dazzling description, and a bit bedazzled, too.

Even the feisty, outspoken environmental scientist Devra Lee Davis (2007) comes under the spell:

> One evening, after a symposium in Lyon, France, I was thrilled to find myself having drinks with none other than Sir Richard Doll. His entry in *Who's Who* listed conversation as one of his hobbies, and sure enough, he was a captivating, engaging and scintillating man to talk with. Doll assured me that he was taking the time to speak with me because he wanted to help. I was honored by the attention. . . . I was flabbergasted and flattered. I had spent an evening with the great Sir Richard. (pp. 257, 262)

You can see how a person might get a bit puffed up by all this adoration. "On a personal level," writes Tweedale (2007a), "Doll was a charming and approachable man: but he also had a highly developed sense of his own importance that was later tinged with dogmatism and a belief in his own infallibility" (p. 234).

I think he had a story he told himself, a story he told to the national press, to an admiring biographer, to the many audiences his position provided him, a story of a scientist dedicated to truth and the betterment of humanity, a story he believed—and that was in part true. Though the self-image had, perhaps, slipped a little out of alignment over the years, what with compromises made, positions taken, leaving him a little too convinced of his own rectitude, that only made him a more persuasive a teller of the tale. "Man ends by fully believing the story

he has told so many times and continues to tell, polishing and retouching here and there the details," writes Primo Levi (1988): the "transition to self-deception is useful" since the self-deceived are more convincing narrators, "more easily believed by the judge, the historian, the reader" (p. 27).

"I've done nothing but try to help her," Doll said to me, said it more than once, seeming keen to convince me he had behaved honorably toward her. The Doll I met was a man of formidable charm, witty, winning, every bit the gentleman—though I could tell, as we chatted, that I was being sized up by a shrewd observer, and I felt my questions bounce off a hard, polished surface. I came away thinking, this is a man well versed in the art of public relations (an impression I doubt anybody ever got of Alice Stewart), adept at the presentation of self to create effect, a master craftsman, and enormously persuasive: a Channel 4 "Power List" program on October 31, 1998, listed him as 122 among the most powerful (or influential) people in Britain.

And he left a dynasty. "A high proportion of the scientists who were to dominate the field passed through the doors of his department," boasts Keating (2009b, p. 276). Many who leapt to his defense after the 2006 revelations were people who had worked with him or under him. With such a flock of followers, his reputation is assured.

So secure is Doll's reputation that the 2006 revelations bounced off it like Teflon—as I discovered, in 2008, when I came across Richard Horton's review of Davis's *Secret History of the War on Cancer* in the *New York Review of Books*. Horton's review was very peculiar. It started out sounding sympathetic to Davis's arguments about the "misplaced emphasis on treatment over prevention" and about the strategy of "doubt promotion," whereby aspersion is cast on scientists who dare to suggest that environmental pollution has a role in cancer causation. But then Horton spun about-face, put off by the "vitriol and innuendo" in Davis's discussion of Doll, concluding that her book was a tissue of "vague exhortations," and that the real reason cancer was on the rise is not environmental but that people smoke and eat too much—and how dare she say such things about this revered scientist?

This hit a nerve. My biography of Stewart had been turned down by Verso Press on account of a reader who had this same kind of apoplectic response to Stewart's criticisms of Doll. But that was in the 1990s, when Doll's name was sterling—how could his reputation be so untarnished, two years after those conflicts of interest had been revealed?

I wrote to the *New York Review of Books*, saying that if Horton was shocked by what Davis said about Doll, he should hear what I learned about Doll while writing *The Woman Who Knew Too Much* (Greene 2008). To my amazement, the *Review* published my letter—usually Doll's detractors get listservs and e-mail and blogs to air their opinions, while his defenders get the *London Times*. I received

a flood of mail, all of it anti-Doll. Horton wrote no response to my letter, though it is the *Review*'s policy to give authors the opportunity to reply.

The Woman Who Knew Too Much was never published in England on account of what we said about Doll. Not only did Verso turn the book down, but later, the Women's Press, which accepted it for U.K. publication, withdrew their offer when their legal department warned of libel issues. I never took it to another British publisher. "I know of no one who has had critical things published about Doll whose work has not been thoroughly obstructed by lawyers," writes Martin Walker, who himself received a warning from Doll's solicitors after the critique he wrote in *The Ecologist* in 1998. In Britain, where many journalists are "frightened off by legal matters, there has been next to no serious debate inside or outside science about these matters."

That's what a "mafia" looks like, not the little band of outsiders nipping at Doll's heels.

Thus has the esteemed Sir Richard Doll constructed the story of his life and been in a position to make it stick. And so is the "Authorized Biography," which is receiving laudatory reviews in mainstream journals (see McMichael 2010; Samet and Pineles 2010; Shetti 2010), on its way to becoming the official story. It was inevitable that this biography be commissioned, given Doll's iconographic status—but that Stewart's story was written was pure chance. I just happened to interview her for a book I was writing on cancer and the environment, and I just happened to have the time her story required—though it was time carved out of teaching responsibilities half a world away and from leaves paid for by myself.

A year after Doll died, when the media were publicizing his industry ties, the MRC issued a press release in the *Sunday Times*, signed by the head of the MRC and other luminaries: "It is with dismay that we now hear allegations against him that he cannot rebut for himself" (MRC 2006). But Doll himself showed no reservations about speaking ill of the dead when he wrote that damning entry on Stewart in the *Oxford Dictionary of National Biography*, and nobody from the MRC or the Wellcome Institute came to her defense. Far from defending her, the Wellcome Institute (which houses her papers as well as his) commissioned a biography that gave Doll the last word.

I'm afraid it mattered that Doll looked like a grandee and Stewart looked like a granny. She dressed like a woman who had other things on her mind, never gave a thought to what she wore, had not a shred of image-consciousness. And she was the bearer of bad news—"In the old days they killed the messenger who brought bad news, these days they just cut off your funding," as she'd say—whereas Doll dispensed bromides. "We are, for the most part, winning the fight against cancer" was another of his reassurances (Doll 1990, p. 508).

Those who have power have access to venues that enable them to secure more power. Stewart existed so far outside institutional structures that she had no such venues, whereas Doll dominated British epidemiology for the last half of the

20th century, defining who was in, who was out, who was scientifically respectable, and who was fringe, and effectively silencing his critics. He shaped cancer research in the United States and the United Kingdom, assuring that occupational cancer remains a low priority and putting a virtual end to inquiry into the environmental causes of cancer. The Imperial Cancer Research Fund that he represented has never, as Martin Walker (1998) observes, carried out research on "the possible carcinogenic effects of exposure to . . . environmental factors" (p. 82). "His contribution on occupational and environmental cancer has been a disaster," says Rory O'Neil.

Keating (2009b) asserts that "millions of people are alive today who would otherwise be dead had Richard Doll not made his enduring contribution to medical science" (p. xi). Perhaps. But Chris Talbot (2007) points out that it may have been a draw between the lives he saved and the lives he cost: on account of positions Doll took on Agent Orange, vinyl chloride, and radiation, people were denied compensation, endured further exposure, suffered, and died. His defense of dioxin enabled the United Kingdom to refuse its ex-servicemen compensation, even as Australia, New Zealand, and the United States were granting compensation to theirs. Richard Stott, in a blistering article in the *Sunday Mirror* (2006), denounced Doll as among those responsible for Britain's heartless refusal to acknowledge the damage the bomb tests had done its servicemen: they "should hang their heads in shame for the misery, torment, crippling disease they have caused."

Stewart saved lives, too: putting an end to the x-raying of pregnant women, she spared the human race untold numbers of malignancies and mutations. But with her, there's no negative side to the ledger.

The confusion sown by Doll and those who have accepted his reassurances about low-dose radiation has enabled proponents of the nuclear industry to proceed as though the risks were understood and under control, to pass off as "clean" this energy source that has polluted half the globe. The Chernobyl catastrophe released hundreds of times the radioactivity released by the Hiroshima and Nagasaki bombs combined: 57% of it spread outside the former USSR, contaminating more than 40% of Europe and the entire Northern Hemisphere (Yablokov, Nesterenko, and Nesterenko 2009).

As technicians struggle to contain the Fukushima reactors, as experts disagree about radiation released and new cover-ups come daily to light, the Hiroshima studies are invoked by way of reassurance. "The risk of cancer is quite low, lower than what the public might expect," a researcher from the Radiation Effects Research Foundation informs the *New York Times*, explaining that the Hiroshima studies show that "at very low doses, the risk was also very low" (Grady2011b). The Department of Energy assures us that the "miniscule quantities" of radiation in the radioactive plume spreading across the United States pose "no health hazard" (Broad 2011). British journalist George Monbiot (2011) cites the Hiro-

shima studies as evidence that low-dose radiation produces low rates of cancer and no genetic effects, pointing to these as "scientific consensus." In a shrill, much publicized debate with Helen Caldicott, on television and in the *Guardian*, he chides, "you have to go with the scientific consensus, rather than with a few outlier papers," and calls Caldicott "unscientific" (Doll's term for opponents)—though she is a physician and he is a journalist.

As Fukushima is upgraded to a "level 7" disaster, on a par with Chernobyl, the public looks for information about Chernobyl, only to find, in mainstream media, human interest stories, denials, and evasions. "There is no evidence of a major public health impact attributable to radiation exposure two decades after the accident at Chernobyl," announced the *New York Times*, a few days after the Fukushima reactors began to destabilize (Grady 2011a). So says a World Health Organization study that found "minimal health effects" and estimated that only 4,000 deaths "will probably be attributable to the accident ultimately," a report the *New York Times* had publicized in 2005, quoting an expert who explained that the worst effect of the accident was a "paralyzing fatalism" that leads people to "drug and alcohol use, and unprotected sex and unemployment"—the "lifestyle" causes invoked by Doll and Peto (Rosenthal 2005).

The *Times* did not mention the two other studies that came out in 2006, "The Other Report on Chernobyl" (Fairlie and Sumner) and "The Chernobyl Catastrophe" by Greenpeace, both of which gave much higher casualty estimates than the 2005 report. Nor did it mention *Chernobyl: Consequences of the Catastrophe for People and the Environment*, translated into English and published by the New York Academy of Sciences in 2009—which estimates casualties at 985,000 (Yablokov, Nesterenko, and Nesterenko 2009), orders of magnitude more than the 2005 report. Drawing on "data generated by many thousands of scientists, doctors, and other experts who directly observed the suffering of millions affected by radioactive fallout in Belarus, Ukraine, and Russia," this report incorporates more than 5,000 studies, mostly in Slavic languages (as compared with the 350 mentioned in the 2005 report, most of which were in English). The authors are Dr. Alexey Yablokov, environmental advisor to Yeltsin and Gorbachev, and Dr. Vassily Nesterenko, former director of the Institute of Nuclear Energy in Belarus, who flew over the burning reactor, giving us the only measurement of the radionuclides released. Nesterenko, together with Andrei Sakharov, founded the independent Belarusian Institute of Radiation Safety (BELRAD), which treats and studies Chernobyl children; when he died, in 2008, as a result of radiation exposure, his son Dr. Alexey Nesterenko—third author on this study—took over as director and senior scientist at BELRAD. Dr. Janette Sherman, translator and contributing editor, is a physician and toxicologist.

Comparing contaminated areas of Belarus, Ukraine, and Russia with the so-called "clean areas," the studies find significant increases in morbidity and mortality in contaminated regions: not only more cancer, especially thyroid cancer, but a wide array of non-cancer effects (as Stewart predicted there would be)—

ulcers, chronic pulmonary diseases, diabetes mellitus, eye problems, mental retardation, and a higher incidence and greater severity of infectious and viral diseases. In fact, every system in the body is adversely affected: cardiovascular, reproductive, neurological, hormonal, respiratory, gastrointestinal, musculoskeletal, and immune systems. The children are particularly damaged: "Prior to 1985 more than 80% of children in the Chernobyl territories of Belarus, Ukraine, and European Russia were healthy; today fewer than 20% are well." (p. 2). In the animals, too, there are "significant increases in morbidity and mortality . . . increased occurrence of tumor and immunodeficiencies, decreased life expectancy, early aging, changes in blood and the circulatory system, malformations" (p. 255).

Parallels between Chernobyl and Hiroshima are striking: data collection was delayed, information withheld, reports of on-the-spot observers were discounted, independent scientists denied access. Yablokov and colleagues (2009) note that "The USSR authorities officially forbade doctors from connecting diseases with radiation and, like the Japanese experience, all data were classified," and "the official secrecy that the USSR imposed on Chernobyl's public health data the first days after the meltdown . . . continued for more than three years," during which time "secrecy was the norm not only in the USSR, but in other countries as well" (pp. 3, 33).

But the parallels are political more than biological, for the Hiroshima data have proven to be an "outdated" and inapplicable model, as Stewart said, for predicting health effects from low-dose, chronic radiation exposure over time. Thyroid cancer, for example, increased "exponentially" in the years after Chernobyl, appearing earlier and more virulently than in Hiroshima (a three-year as opposed to a 10-year latency). Scientists (Doll among them) denied that this could be a radiation effect since doses were too low—"too low" as determined by the Hiroshima data—yet epidemiological studies kept turning up a link with radiation. Finally, in 2005, a case-control study headed by Elisabeth Cardis confirmed "a very strong dose-response relationship" between thyroid cancer and radiation exposure, at doses "not thought to be sufficiently high" to produce this effect (p. 729).

The Hiroshima studies find little genetic damage in the survivors, yet "Wherever there was Chernobyl radioactive contamination, there was an increase in the number of children with hereditary anomalies and congenital malformations. These included previously rare multiple structural impairments of the limbs, head, and body" (Yablokov, Nesterenko, and Nesterenko 2009, pp. 130,125). Such anomalies and malformations are especially pronounced in the children of the "liquidators," the men and women called in to put out the fire, decommission the reactor, and clean up the site—15% of whom were dead by 2005 (these were young, healthy men and women). The correlation with radioactive exposure is so striking as to be "no longer an assumption, but . . . proven," write the authors (p.133), and the damage will go on for "at least seven generations" (p. 211). As in humans, so in other species, "gene pools of living creatures are actively transforming, with unpredictable consequences": "It appears that [Cher-

nobyl's irradiation] has awakened genes that have been silent over a long evolutionary time" (pp. 323, 237).

The inescapable conclusion is that "there is no threshold for ionizing radiation's impact on health. . . . Even the smallest excess of radiation over that of natural background will statistically . . . affect the health of exposed individuals or their descendants, sooner or later " (p. 42). Radioactive contamination does not "dilute and dissipate": as Stewart warned, it becomes part of what we breathe and eat, adding, over time, to cancers and genetic damage throughout the world. "Even more than the cancer threat is the genetic damage," she cautioned: "that's what you ought to be really afraid of, the possibility of sowing bad seeds into the human gene pool."

As low-dose radiation is demonstrated to have health effects beyond those predicted by current theoretical frameworks, the discrepancy between the models and the evidence becomes glaring. Rudi Nussbaum (2009), professor emeritus of nuclear physics, Portland State University, describes an increasing "dissonance between evidence and existing assumptions about . . . radiation risk," a gap between new information and the "widely adopted presuppositions about radiation health effects" (p. 321). The mechanisms by which low-dose radiation does its damage are not well understood. Stewart hypothesized that rather than killing the cell outright, as a high dose of radiation does, it allows it to attempt repair, and it's the attempt that produces the mutations that leads to cancer and genetic aberrations. There are "many different complex cellular interaction scenarios [that] can be hypothesized," write David Brenner and Rainer Sachs (2006, p. 254), that may lead to an understanding at the molecular level. A 2004 report of the Committee Examining Radiation Risks of Internal Emitters (CERRIE) suggests that internal emitters from radiation ingested or inhaled have been insufficiently taken into account or understood, and points to newly discovered biological effects of radiation, "genomic instability (ongoing, long-term increase in mutations within cells and their offspring), bystander effects (cells next to those that were irradiated can also be damaged) and minisatellite mutations (inherited germline DNA changes)"—which "need further research."

"The Chernobyl accident created an unique possibility for specialists in radiobiology and radiation protection for examination of their hypotheses and theories," writes Michael Malko (1998), a researcher at the Joint Institute of Power and Nuclear Research in Belarus. But rather than using this evidence to expand their knowledge of radiation effects, experts invoke the old studies to dismiss new evidence. Chernobyl is a better predictor of the consequences of Fukushima than Hiroshima is, but Yaboklov's book has met, as he said in a press conference in Washington, D.C., in 2011 "mostly with silence"—while the Hiroshima studies continue to be cited.

I keep hearing Doll's words, "it looks as though Alice will be proved right. . . ." But we see, from scientists' adherence to the old models, how powerful are the incentives not to let those models go.

REFERENCES

Alvarez, R. 1987. Statement before the NAS Advisory Committee on the Biological Effects of Ionizing Radiations, BEIR V, March 2, Environmental Policy Institute.

Biological Effects of Ionizing Radiation (BEIR V). 1990. *Biological effects of ionizing radiation*. Washington, DC: National Academies Press. http://www.nap.edu/catalog.php?record_id=1224#toc.

Boffey, P. 1982. Cancer experts lean toward vigilance, but less alarm, on environment. *NY Times*, March 2.

Boseley, S. 2006. World-famous British scientist failed to disclose that he held a paid consultancy with a chemical company for more than twenty years while investigating cancer risks in the industry. *Guardian*, Dec. 8.

Brenner, D., and R Sachs. 2006. Estimating radiation-induced cancer risk as at very low doses: Rationale for using a linear no-threshold approach. *Radiat Environ Biophys* 44: 253–56.

Broad, W. 2011. Radiation over U.S. is harmless, officials say. *NY Times*, March 22.

Cardis, E., et al. 2005. Risk of thyroid cancer after exposure to 131-I in childhood. *J Natl Cancer Inst* 97:724–34.

Caulfield, C. 1989. *Multiple exposures: Chronicles of the radiation age*. New York: Harper and Row.

Clapp, R., et al. 2005. *Environmental and occupational causes of cancer: Review of recent scientific literature*. Cancer Working Group of the Collaborative on Health and the Environment, Lowell Center for Sustainable Production. Lowell: Univ. of Massachusetts.

Committee Examining Radiation Risks of Internal Emitters (CERRIE). 2004. Report. Oct. 20. http://www.cerrie.org/index.php.

Cook, C. 2004. Oral history: Sir Richard Doll. *J Public Health* 26(4):327–36.

Cookson, C. 1994. The weapons of a killer: The search for what causes cancer has thrown up hundreds of suspects, from smoking to driving. *Financial Times*, Dec. 13.

Court-Brown, W. M., and R. Doll. 1957. Leukemia and aplastic anaemia in patients irradiated for ankolysing spondylitis. *Spec Rep Ser Med Res Counc (GB)* 295:1–135.

Darby, S. 2003. A conversation with Sir Richard Doll. *Epidemiology* (14)3:375–79.

Davis, D. L. 2007. *The secret history of the war on cancer*. New York: Basic Books.

Davis, D. L., et al. 1986. Increasing trends in some cancers in older Americans: Fact or artifact? *Toxicol Ind Health* 2(1):127–44.

Doll, R. 1990. Are we winning the fight against cancer? An epidemiological assessment. *Eur J Cancer* 26(4):500–508.

Doll, R. 1996. A tentative estimate of the leukaemogenic effects of test thermonuclear explosions. *J Radiol Prot* 16(1):3–5. Accepted for publication in 1955.

Doll, R., and B. Hill. 1950. Smoking and carcinoma of the lung: Preliminary report. *BMJ* 2(4682):739–48.

Doll, R., and R. Peto. 1981. The causes of cancer: Quantitative estimates of avoidable risks of cancer in the United States today. *J Natl Cancer Inst* 66(6):1195–1308.

Doll, R., and R. Wakeford. 1997. Risk of childhood cancer from fetal irradiation. *Br J Rad* 70:130–39.

Drabble, M. 2007. *The sea lady*. New York: Harcourt.

Epstein, S. 1998. *The politics of cancer revisited*. Fremont City, NY: East Ridge Press.

Espinosa-Brito, A. D. 2005. Sir Richard Doll: Obituary. *BMJ* 331:295. http://www.BritMedJ.com/cgi/eletters/331/7511/295#113076.

Fairlie, I., and D. Sumner. 2006. *The other report on Chernobyl*. Berlin: MEP Greens/EFA.

Ferry, G. 1993. No smoke without fire. *Oxford Today* 5(3):11–12.

Grady, D. 2011a. Precautions should limit health problems from nuclear plant's radiation. *NY Times*, March 15.

Grady, D. 2011b. Radiation is everywhere, but how to rate harm? *NY Times*, April 5.

Greenberg, M. 1991. The evolution of attitudes to the human hazards of ionizing radiation and its investigators. *Am J Ind Med* 20:717–21.

Greene, G. 1999. *The woman who knew too much: Alice Stewart and the secrets of radiation*. Ann Arbor: Univ. of Michigan Press.

Greene, G. 2008. Malignant maneuvers. *NY Rev Books*, June 26. http://www.nybooks.com/articles/21578.

Greenpeace. 2006. *The Chernobyl catastrophe: Consequences on human health*. Amsterdam: Greenpeace. http://www.greenpeace.org/raw/content/international/press/reports/chernobylhealthreport.pdf.

Hardell, L., et al. 2006. Secret ties to industry and conflicting interests in cancer research. *Am J Ind Med* 50:227–33.

Hiroshima and Nagasaki: The physical, medical and social effects of the atomic bombings. 1981. Committee for the Compilation of Materials on Damage Caused by the Atomic Bombs in Hiroshima and Nagasaki. New York: Basic Books.

Horton, R. 2008. Malignant maneuvers. Review of Devra Davis, *The secret history of the war on cancer*. *NY Rev Books*, March 6. http://www.nybooks.com/articles/archives/2008/mar/06/cancer-malignant-maneuvers/.

Jacobs, B. W. 1986. The politics of radiation: When public health and the nuclear industry collide. *Greenpeace* (July–Aug.):6–9.

Keating C. 2009a. Richard Doll: His revolutionary life. April 28. http://beta.podcasts.ox.ac.uk/richard-doll-his-revolutionary-life.

Keating, C. 2009b. *Smoking kills: The revolutionary life of Richard Doll*. Oxford: Signal Books.

Levi, P. 1988. The memory of the offense. In *The Drowned and the Saved*, 23–35. New York: Summit Books.

Lilburne, J. 2006. Scientists are only human. *Guardian*, Dec. 8. http://www.guardian.co.uk/commentisfree/2006/dec/08/scientistsareonlyhuman.

Lindee, M. S. 1994. *Suffering made real: American science and the survivors at Hiroshima*. Chicago: Univ. of Chicago Press.

Malko, M.V. 1998. Chernobyl accident: The crisis in the international radiation community. *Research activities about the radiobiological consequences of the Chernobyl NPT accident*. KURR-KR-2. http://www.rri.kyoto-u.ac.jp/NSRG/reports/1998/kr-21/Malko96-1.html.

Marshall, E. 1990. Experts clash over cancer data. *Science* 250:900–902.

McMichael, A. 2010. *Smoking kills*. Review. *Int J of Epidemiol* 39:1123–26.

Medical Research Council (MRC). 1947. Conference on Cancer of the Lung, Feb. 6, Report to MRC meeting March 21; A. Bradford Hill, Memorandum, "Proposed Statistical Investigation of Cancer of the Lung," MRC Nov. 10.

Medical Research Council (MRC). 2006. Press release. Science chiefs defend cancer pioneer. *Sunday Times*, Oct. 9. http://www.timesonline.co.uk/tol/news/uk/article665082.ece.

Monbiot, G. 2011. Prescription for survival: A debate on the future of nuclear energy between anti-coal advocate George Monbiot and anti-nuclear activist Dr. Helen Caldicott. Amy Goodman, March 30. http://www.democracynow.org/2011/3/30/prescription_for_survival_a_debate_on.

Nussbaum, R. 2009. Childhood malignancies near German nuclear reactors. *Int J Occup Environ Health* 15(3):318–23.

Park, R. 2011. Occ-Env-Med-L: A free forum for Occupational & Environmental Medicine. University of North Carolina. http://subscribe.occhealthnews.onet/.

Proctor, R. 1995. *Cancer wars: How politics shapes what we know and don't know about cancer.* New York: Basic Books.

Rosenthal, E. 2005. Experts find reduced effects of Chernobyl. *NY Times*, Sept. 6.

Samet, J., and B. Pineles. 2010. *Smoking kills*. Review. *Am J Epidemiol* 171(7):848–50.

Schneider, K. 1990a. Radiation records of 44,000 released. *NY Times*, July 18.

Schneider, K. 1990b. Scientist who managed to shock the world on atomic workers' health. *NY Times*, May 3.

Sellikoff, I. J. 1992. Influence of age at death on accuracy of death certificate disease diagnosis: Findings in 475 consecutive deaths of mesothelloma among asbestos insulation workers and asbestos factory workers. *Am J Ind Med* 22:505–10.

Sex and the Scientist: Our Brilliant Careers, 1996. Documentary, screened Aug. 19, Channel 4, UK.

Shetti, P. 2010. Richard Doll's smoking gun. Review. *New Sci*, Jan. 15.

Shimizu, Y, et al. 1992. Studies of the morality of A-bomb survivors. *Radiat Res* 130:249–66.

Simpson, D. 2005. Sir Richard Doll, 1912–2005. *Tob Control* 14:289–90.

Stewart, A. M. 1997. Letter. *Br J of Rad* 70:769–71.

Stewart, A. M., et al. 1956. Preliminary communication: Malignant disease in childhood and diagnostic irradiation in utero. *Lancet* 2:447.

Stewart, A. M., et al. 1958. A survey of childhood malignancies. *BMJ* 1:1495–1508.

Stott, R. 2006. Cloud over Sir Richard. *Sunday Mirror*, Dec. 10. http://findarticles.com/p/articles/mi_qn4161/is_20061210/ai_n16900920/.

Strather, J. W., et al. 1995. Radiation-induced cancer at low doses and low dose rates. *Radiol Prot Bull* 167:8–12.

Talbot, C. 2007. Medical research and big business: The case of Sir Richard Doll. World Socialist Web Site, Jan. 9. http://www.wsws.org/articles/2007/jan2007/doll-j09.shtml.

Tweedale, G. 2007a. Hero or villain? Sir Richard Doll and occupational cancer. *Int J Occup Environ Health* 13: 233–35.

Tweedale, G. 2007b. The Rochdale asbestos cancer studies and the politics of epidemiology. *Int J Occup Environ Health* 13:70–79.

Tucker, A. 2005. Obituary: Sir Richard Doll. *Guardian*, July 25. www.guardian.co.uk/news/2005/jul/25/guardianobituaries.obituaries/print.

U.K. Co-ordinating Committee. 1992. Unique national survey of children's cancer to begin in April. Press release, March 12.

Wald, M. 2000. U.S. acknowledges radiation killed weapons workers, ends decades of denials. *NY Times*, Jan. 29.

Walker, M. 1998. Sir Richard Doll: A questionable pillar of the cancer establishment. *Ecologist* 28(2):82–92.

Walker, M. 2011a. Scoop! http://www.whale.to/a/walker_a1.html.

Walker, M. 2011b. Sir Richard Doll: Death, dioxin, and PVC! http://www.whale.to/a/walker_a1.html.

Wing, S., D. Richardson, and A. M. Stewart. 1999. The relevance of occupational epidemiology to radiation protection standards. *New Solutions* 9(2):133–51.

Yablokov, A. 2011. Press conference. March 15. http://www.democraticunderground.com/discuss/duboard.php?az=view_all&address=115x283519.

Yablokov, A., V. Nesterenko, and A. Nesterenko. 2009. *Chernobyl: Consequences of the catastrophe for people and the environment*. New York: New York Academy of Sciences.