cases from the past but on 20 years of developing an "intuitive 'feel'" for "what constitutes good science" (p. 3). When scholarly studies of the affair do appear, they will not consider Close's book a distanced and neutral reconstruction of already settled events but an insider's account that played a part in the closure of the events. Close has goals beyond scholarship: cold fusion is dissected in order to put good science in its best possible light, to prevent the public from forming a perception that "cold fusion" was something scientific. "If these events become regarded as a norm for science then public confidence would be threatened. It is important that the public see that the test-tube fusion story is *not* typical of normal science" (p. 2).

With that goal in mind, Close begins his interpretation in a curious way: "The idea that established science was somehow attempting to censor cold fusion research is utterly out of line with what science and scientists are all about" (p. 3). But in that case, why bring up this "conspiracy" idea not just once but several times? Close reports later, for instance, that Peter Bond, chairman of the physics department at Brookhaven National Laboratory, was asked by a reporter to comment on a claim that "the big labs *are* seeing fusion but are keeping it secret because the big oil companies have bought them off!" (p. 145).

Close never intends conspiracy as a plausible interpretation. Instead, it serves the rhetorical function of straw man: it exaggerates the social, political, economic, and psychological sides of science into an absurdity easily knocked over, so that the "correct" view of good science is alone left standing. Close constructs a strategic demarcation in which good science is put on one side of the border (those who refute cold fusion go here) and everyone and everything that kept cold fusion alive go on the other. The implication: if good science had been allowed to run its normal course, cold fusion would have been settled within days rather than months, no brouhaha at all, and the United States would not have spent $30 million chasing a chimera.

Candidates for a scapegoat for the fiasco abound: chronically misinformed journalists who produce "factoids" (p. 17) while they hype false hopes and pressure scientists to hold weekly press conferences in lieu of peer review; greedy university administrators who stoop so low for fame and gain that they make a $500,000 "anonymous" donation to the Cold Fusion Institute out of their university's own coffers as a way of reassuring the state of Utah that its $4.5 million was indeed attracting outside commercial investments; gullible politicians who fail to recognize that the good scientist/expert knows best (p. 35) and who challenge the authority of *Nature* with the arrogant response, "We are not going to allow some English magazine to decide how state money is handled" (p. 12); patent attorneys who make it impossible for scientists to share details of their research in a timely and cooperative manner.

As victims of villains from outside good science, Pons and Fleischmann themselves end up beyond the pale. Media hoopla catches them off guard, while patent lawyers and university administrators force them to put priority and secrecy above reliability and validity. In this pressure-cooker, Close suggests, Pons and Fleischmann's dealings with Steven Jones (the physicist from Brigham Young University doing research on muon-catalyzed fusion) could not remain a friendly rivalry but digressed into an obsessive, pathological drive to be first. Jones's report of a few observed neutrons coming from a similar kind of cell instantly convinced Pons and Fleischmann that their own heat measurements really represented fusion and that Jones was ready and willing to scoop them. Being "under intense pressure month after month," the two chemists "reacted irrationally while in the glare of media attention" (p. 327) by: forging ahead without consulting the long scientific literature on unsuccessful fusion attempts; releasing results prematurely; failing to double-check their findings; refusing to consult their knowledgeable peers in physics; choosing not to do the controlled experiments required to distinguish artifacts from facts; and, in the end, ignoring anything inconsistent with their claim to fame. Understandable psychology perhaps, but, for Close, not science.

This reading of the affair puts blame on external forces; real science rides in only to slay falsehoods. Such an image of scientists is certainly salutary for the profession at a time when allegations of laboratory fraud and mismanagement of research funds make the newspapers almost as frequently as cold fusion once did. Interestingly, however, the interpretation undermines Close's thesis that cold fusion is atypical; his reading is all too familiar for those acquainted with the rhetoric of earlier generations of "statesmen" of science. It is a play on the time-honored trope "If it works, praise science; if it fails, blame everything else."

Science could not get on without patent attorneys to protect the commercialization of new facts, without university administrators able to wangle funds from legislatures, without competition among specialists for priority and among universities for grant money, without the media to hype breakthroughs—real or promised. Close says as much: "Paradoxically, the very fact that test-tube *fusion* is news has grabbed public attention and ironically could be the headline that . . . attracts money from Congress—for *hot* fusion." (p. 48). If a viable fusion energy source does—many years and millions of dollars from now—emerge from (say) the Princeton tokamak reactor, no one will blame the media or university administrators or gullible politicians for anything, but neither will they get much praise. That will be reserved for good science. But when things go wrong, as they did for fusion of the cold kind, that necessary infrastructure—with all its interests, politics, pressures, passions, and pathologies—is cleaved off and blamed for not allowing good science to take its natural course. *Too Hot To Handle* makes it plain why there is nothing real about cold fusion (for now), but public understanding of science is not enhanced by its idealization.

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Epochs in Physics

The Joy of Insight. Passions of a Physicist. VICTOR WEISSKOPF. Basic Books, New York, 1991. xiv, 336 pp. + plates. $24.95. Alfred P. Sloan Foundation Series.

"Given a choice, I would have wanted to live as a scientist in the nineteenth century," remarks Victor Weisskopf in this autobiography. Born in 1908 and still active in science and public affairs in 1991, Weisskopf has instead played a primary role in the transformations that marked the 20th century. He was a participant in the development of quantum mechanics and nuclear physics in the '30s and a leader of the Manhattan Project during World War II. After the war he became an international statesman of science, furthering the goals of humanism and worldwide cooperation.

Weisskopf grew up in a completely assimilated wealthy Jewish Viennese family, for whom music and opera were serious matters. To attend an operetta, he notes, would have been "considered below our family's dignity." In intellectual, warm, and supportive surroundings Weisskopf grew up discovering socialism and Beethoven before girls. After two years at the University of Vienna, he arrived at Göttingen in 1928, just missing the birth of quantum mechanics, but as a graduate student he collaborated with Eugene Wigner on a celebrated paper on line shape. As do all accounts of these years,

The Weisskopf family at the beach in Belgium, 1912. Victor is at right. [From *The Joy of Insight*]

Weisskopf's reminds us that this was a time when there were giants in the Earth—Bohr, Pauli, Heisenberg, Dirac, Bloch, Oppenheimer, Wigner—but his vivid recollections bring these figures out of mythology and into a life filled with physics and with politics, music, theater, and the outdoors.

Theoretical physics has its frustrations and times of private doubt, as Weisskopf explains in his account of his famous error, discovered by Wendell Furry, in calculating the degree of divergence of the electron self-energy and the story of his calculation of the Lamb shift with Bruce French, which turned out to be correct despite the objections of Julian Schwinger and Richard Feynman.

"On the way to the Institute in Göttingen by bike." Left to right, Victor Weisskopf, Maria Goeppert, Max Born. [From *The Joy of Insight*]

Physicists will be fascinated by Weisskopf's tales of the beginnings of nuclear and particle physics and of the early years at CERN, but for most readers it will be the story of the development of the atomic bomb and efforts to control it that will be of greatest interest. This is an especially appropriate time to address the perennial questions: why did people work on the bomb? why was it actually dropped? should the development of thermonuclear weapons have been undertaken? With improved Soviet-American relations now offering a real possibility of ending a long chapter in this story, Weisskopf's observations are particularly interesting.

Weisskopf's leftist inclinations were tempered by first-hand experience. In 1936 he was offered a professorship in Kiev and was being considered for a job in Moscow as well. A visit there at that especially grim time revealed "an atmosphere of fear and terror." The reality of the Soviet regime was unmistakable when friends were arrested and sent to prison camps for their beliefs and associations. In 1938, Weisskopf, by then living in America, where he was a professor at the University of Rochester, visited Oppenheimer at his New Mexico ranch and found that "he still believed to a great extent in communism. We tried to convince him of the reality of Soviet life by describing the lack of freedom and the persecutions under Stalin that we had seen there." Soon the immediate concern was the Nazis, not the communists. The discovery of nuclear fission in 1938 and the invasion of Poland in 1939 caused fear that German physicists such as Heisenberg might create an atomic bomb. Weisskopf conveys the extent of the fear by revealing that "I wrote a letter to Oppenheimer suggesting that Heisenberg be kidnapped. . . . I even offered my services in carrying out this scheme." Needless to say, nothing came of what Weisskopf terms his "harebrained idea."

In 1943 Oppenheimer asked Weisskopf to come to Los Alamos. With candor he observes: "Today I am not quite sure whether my decision to participate in this awesome—and awful—enterprise was solely based on the fear of the Nazis beating us to it. It may have been more simply an urge to participate in the important work my friends and colleagues were doing. There was certainly a feeling of pride in being part of a unique and sensational enterprise. Also, this was a chance to show the world how powerful, important, and pragmatic the esoteric science of nuclear physics could be."

The defeat of Germany removed the primary motivation for work on the bomb, but work continued because, as Weisskopf says, "By then we were too involved in the work, too deeply interested in its progress, and too dedicated to overcoming its many difficulties. . . . I have often been disappointed that, at the time, the thought of quitting did not even cross my mind." From ten miles away, Weisskopf observed the first test at Trinity, and less than two days later he viewed the site of the explosion: "a flat area about 400 meters in diameter. . . . The tremendous heat . . . had transformed it into a gigantic mirror." When the war was over, the need to quit was obvious. Weisskopf simply states, "After Los Alamos, I refused to have anything to do with nuclear weapons development."

Weisskopf and the other physicists from the bomb project, who were viewed by the public as heroes for saving lives by shortening the war, struggled to control the atomic genie, which they knew could not be stuffed back into the bottle. In their naïveté they counted on the sort of altruism that was the general, if not universal, rule in the physics community. They failed to win international control of atomic energy, and the civilian control of atomic energy they won in the United States turned out to be largely illusory.

Weisskopf joined the faculty at MIT, where he continued his research and, with John Blatt, wrote his classic textbook on nuclear physics. His graduate students included Murray Gell-Mann, J. D. Jackson, and Kurt Gottfried. He returned to Europe to serve as director general of CERN from 1961 through 1965, after which he rejoined the physics department at MIT and served as department chairman. From 1976 to 1985 he was active as a member of the Pontifical Academy, working especially for control of nuclear arms.

Weisskopf's voice comes through clearly in the book, the voice more of a raconteur than a writer, happy to digress entertainingly on his favorite operas or poetry. It is a voice that has tried to infuse our century with the idealism and humanism that it so often has lacked, a voice informed with a full appreciation of the moral problems that he and his colleagues created by mastering the atomic nucleus. *The Joy of Insight* is much more than Weisskopf's autobiography: It is a first-hand account of the intellectual and political forces that shaped the 20th century.

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