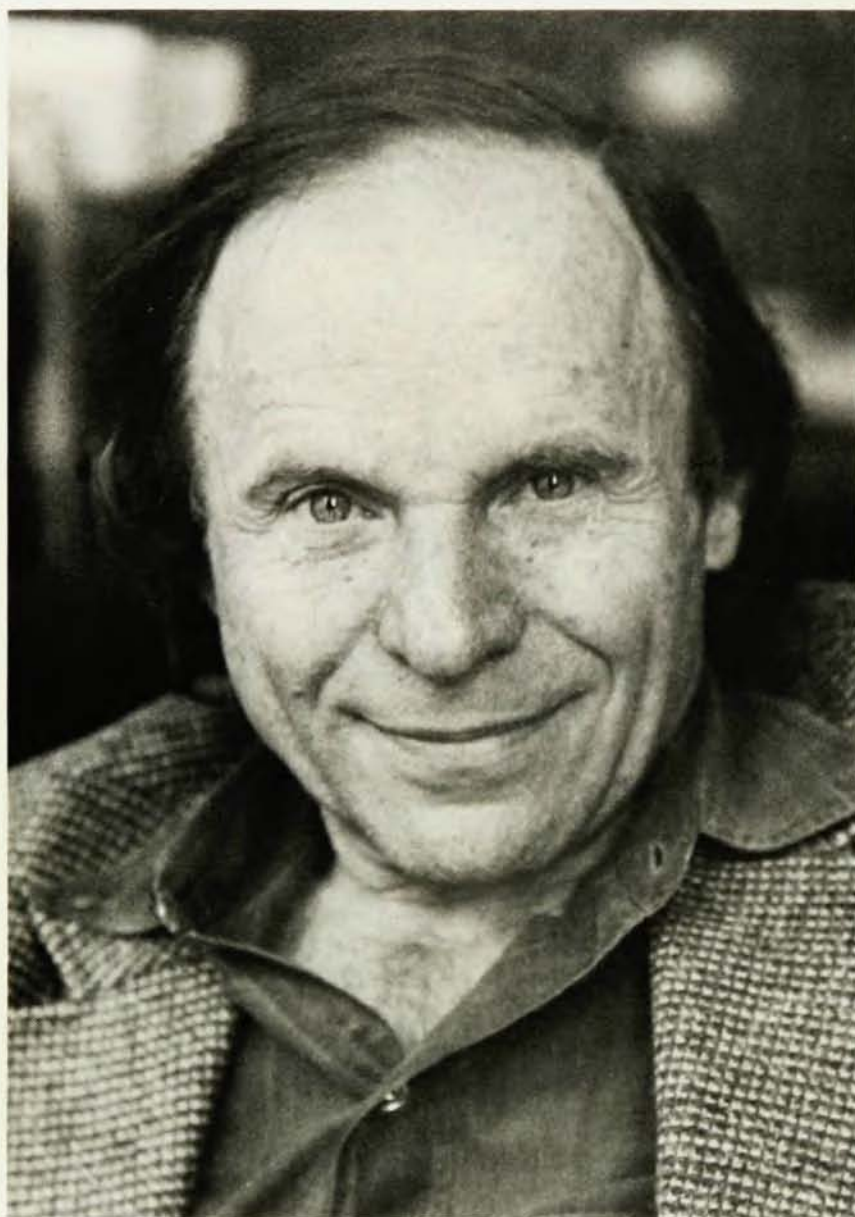




Philip Morrison— a profile

Valued for his scientific contributions to the Manhattan Project, to theoretical physics and to astrophysics, he has also contributed to the public understanding of science and has been one of the most thoughtful advocates of arms control.

Anne Eisenberg

When Philip Morrison, Institute Professor at MIT, came to the Polytechnic Institute of New York recently to give the Sigma Xi lecture, a diverse group attended. The group included physicists, chemists, engineers; people who admired Morrison for his sustained fight against red-baiting in the 1950s (in 1953 a national newsletter called him "the man with one of the most incriminating pro-Communist records in the entire academic world"); and people in the humanities who had enjoyed his book reviews, films, articles and textbooks. The diversity of the audience reflected the diversity of Morrison's career.

Morrison is valued in the scientific community for his gift of language, for his wide-ranging intellect, and for his ability to pull together insights from different fields to shed light on a subject. Because he has spent considerable time writing about science—explaining and interpreting it for the public—he exists also in the imaginations of people outside science. He possesses what historian Alice Kimball Smith has called¹ a "rare sensitivity of spirit."

His career has included Los Alamos and Hiroshima in the 1940s, McCarthyism in the 1950s, the Peace Movement in the 1960s, and arms control from 1945 to the present. It began in Pittsburgh where he was reared and attended Carnegie Tech. After an initial interest in radio engineering, he majored in physics, and went on to do his doctoral work in theoretical nuclear physics with Oppenheimer at the University of California at Berkeley. They got along well; Morrison admired Oppenheimer and reminisces today about him: "There was only one difficulty most of us had with Robert. You had to be very careful with him, you couldn't give him too much of your problem, or he would solve it before you."

The Manhattan Project

Morrison had just gone to the University of Illinois at Urbana when the war broke out. Hired by the Manhattan Project, he went to Chicago to work with Fermi, and stayed there until 1944. Morrison became leader of the group that tested neutron multiplication in successive design studies for the Hanford reactors.

Then, in 1944, he was recruited for the Los Alamos effort by Robert Bacher. Morrison worked at Los Alamos in the group headed by Robert Frisch, who, with his aunt Lise Meitner, had discovered fission a few

years earlier. His job at Los Alamos was to extend work done at Chicago at which he was expert. "We made small critical assemblies to test the neutron behavior of the new plutonium and uranium fission materials being produced at the main plants and shipped to Los Alamos, in preparation for use in the two bombs. Our job was to study chain reactions in that stuff."

It was here that Morrison and his group did the famous experiments later characterized by Feynman as "tickling the dragon's tail." "No one had ever made a chain reaction that had so many prompt neutrons in it," Morrison comments. "All the chain reactions of reactors are mediated in part by delayed neutrons; otherwise they aren't controllable at all. The bomb, on the other hand, is made by fast, prompt neutrons, which of course are uncontrollable."

Morrison was concerned with building up experience on the passage from the controlled state to the uncontrolled state. This meant keeping the reaction in a partially contained state under active control, instead of relying on the inherent stability of the system. "We moved the system so carefully, but so rapidly, that it had no chance to build up on us—we hoped. We came very close to making explosions, stopping just in time. Feynman said this was like tickling the tail of a dragon, and so it was."

In *Disturbing the Universe*, Freeman Dyson characterizes² the spirit of Los Alamos as the "shared ambition to do great things in science without any personal feeling of jealousy." Morrison says that for himself the motivation was not science, but victory over the Germans.

In my group, two people died. We had the feeling of front-line soldiers with an important campaign at hand.

To begin with, we felt we were well behind the Germans. Rightly or wrongly, we were seized by the notion of this terrible weapon in the hands of the Germans, whose scientists we respected, admired, and feared greatly because they had been the teachers of our teachers and colleagues.

We felt ourselves a little like the English in 1940—a small band standing in the way. Could we possibly beat them? At first there was this terrible responsibility, and then in the end we became

more and more flushed with the fact that we had overcome them. But it wasn't a question of science. It was one of victory. I remember very well.

Morrison conveyed this atmosphere to us with a story of John Wheeler in Chicago: "When noontime came and the 12:00 o'clock bell rang, most of us would go to lunch at the nearby cafeteria. We'd learned, though, not to bother Wheeler. He brought his lunch and when the bell rang he took it and his Princeton notebook out. Then he went ahead to do what he regarded as his 'real work'. He was so conscientious he would never do this during work hours, only during lunch. And that was the attitude at Los Alamos as well."

The absorption in the immediate task was complete. Only as work on the bomb drew to a climax did Morrison consider how it would be used against the Japanese. "We knew there would have to be a trial, but we thought suitable conditions could be made. For instance, I thought, as did many other people, that there was going to be a warning." But no explicit warning was given. The bomb was tested at Alamogordo 16 July and used on Hiroshima 6 August. Morrison says, "The military authorities rejected any demonstration as impractical. They felt Japan would not be deterred by the sight of a patch of scorched earth in the desert. The military had made up its mind. It would have taken a very powerful political presence—one that wasn't available—to sway them. The United States therefore gave no explicit warning. I think this was a moral failure."

Was Morrison surprised the scientists at Los Alamos were not more concerned with the implications of the bomb they were building? "Not at all. There was much discussion about this in the labs, quieter, of course, than those at the Met Labs in Chicago. But we were seized with a terrible responsibility, and our leaders were trying to make sure our attention was not diverted."

After the Trinity test, Morrison, who had been responsible for the design and final deployment of the plutonium core, again prepared and packed the equipment, this time to go to the Mariana Islands. When the bombs were dropped on Japan he was on the island of Tinian, from which the planes for both atomic attacks set off.

He was among the first Americans to visit Hiroshima after the war. "I had earlier decided that the most useful thing one could do would be to try to go through the entire process as a historical witness." At the invitation of Gen-

eral Thomas Farrell, assistant to General Leslie Groves, Morrison joined the 12-man group that went to Hiroshima just 31 days after the explosion to determine the effects of the atomic bomb released by the *Enola Gay*. They arrived in Yokohama the day after MacArthur, and followed him to Tokyo. "For me," Morrison said in an interview³ with Daniel Lang, "The first and main impact of Hiroshima's destruction had come... when we were flying down there from Tokyo. First we flew over Nagoya, Osaka, and Kobe, which had been bombed in the conventional manner, and they looked checkered—patches of red rust where fire bombs had hit intermingled with the gray roofs and green vegetation of undamaged sections. Then we circled Hiroshima, and there was just one enormous, flat, rust-red scar, and no green or gray, because there were no roofs or vegetation left."

Morrison walked through the city with Geiger counters and Lauritzen electrosopes and aided by an interpreter, a guide, and a policeman. "It had burst precisely at the spot we wanted it to, high over Hiroshima. There had been a minimum of radioactivity."

Arms control

After the war, Morrison returned to the US to find himself at the heart of the movement for international arms control, whose advocates operated in diverse ways—in arenas ranging from guarded offices to hearing chambers and press conferences at the Senate Office Building; dispensing the message through coded teletypes and rushed press statements; disputing with colonels and reconnaissance experts; persuading congressmen and reporters. A large number of concerned scientists—many of them organized into groups such as the Manhattan Project Scientists, the Association of Los Alamos Scientists, the Association of Oak Ridge Scientists, Atomic Scientists of Chicago—met in Washington in the fall of 1945. Out of this meeting the Federation of American Scientists was eventually formed. The Federation began operating in January of 1946, with Morrison as a member of the administrative committee.

Morrison described their original goals to us:

We—the people the press soon characterized as atomic scientists—wanted to turn over technical details of bomb production to a world authority under adequate controls. We sought to prevent a

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nuclear arms race by establishing this worldwide authority. The Federation believed that a continuing monopoly of the atom bomb by the United States was impossible. Without staff or salary, Federation members worked in Washington preparing reports on how to establish a worldwide atomic authority.

It seems to me that one finds in the story two distinct ways of meeting the sense of responsibility—indeed, of grave duty—that the Manhattan-project scientists as a whole felt then and feel still.

One of these is the way of the "insider." Oppenheimer—lucid, persuasive, wonderfully analytical—worked in secret with generals and diplomats, trying in a thousand ways to demonstrate what the facts implied. Szilard lived by the phone, buttonholing lobbyists and becoming himself the lobbyist *par excellence*. Both men acted inside the government, personally bringing their schemes before the individuals who had power, who wrote and passed laws.

And then there were the rest of us: younger, less famous and less able. Ours was the way of the dissenter. In the way we acted there was a sense less of knowledge than of commitment. William Higginbotham, Joseph Rush, Louis Ridenour, John Simpson and scores of others in Washington spoke and wrote publicly for 3000 scientists back home at the project laboratories or crowding back into the universities, and also for the physicists and chemists who had not been in the project at all but felt about as we did. From shabby rented offices overcrowded and littered with mimeographed statements and pamphlets the 'atomic scientists' floated in the eddying stream of American public opinion.⁴

Morrison comments that "mutual deterrence was not the vision of 1946. The scientists sought true stability then, not metastability, not the top-heavy balancing rock on which we all breathlessly sit."

Morrison played many roles during the period, roles that called both upon his fertile mind and upon his considerable ability as a speaker. He worked for the Bulletin of the Atomic Scientists, composed FAS policy drafts, and appeared as a principal witness at hearings on atomic bomb policy. He worked on a report of ways to detect atomic bomb laboratories, testing sites and assembly plants. But no matter how carefully he and others stressed how an international authority could operate under adequate controls—indeed, no matter how many times they explained

what they meant by "under adequate controls"—they were accused of wanting to give away the bomb.

The arms race that Morrison had predicted grew as the scientists' movement for international controls waned after 1946. Morrison, who joined the faculty of Cornell University in 1946, remained in the fight for international arms control even as the public acclaim for scientists began to ebb.

McCarthyism

He was soon in need of defense himself. As an undergraduate at Carnegie Tech Morrison had joined the Communist Party, and he remained a member when he went to graduate school at the University of California at Berkeley, a school known at that time for its free-thinking, socialistic atmosphere. By 1941, Morrison was out of the party, but his political activities continued. At Cornell, he was deeply involved in the Peace Movement and in a variety of radical intellectual activities. It was not the involvement that was so noteworthy as much as the level of activity: a continuous string of speeches and appearances made Morrison one of the most politically active scientists throughout the fifties.

During this period there were many attempts to fire him. "What has Cornell University done about Morrison?" the right-wing newsletter *Counterattack* asked⁵ in March 1953, answering "Nothing!" In part the attempts were foiled by his situation, because, as a private school, Cornell was not quite as vulnerable to pressure as public schools. Nonetheless, considerable forces were exerted on Cornell, where his promotion from associate to full professor was held up for so long that the Physics Department began to talk of refusing to submit any further proposals for promotions until Morrison's was acted on.

His promotion finally became an issue before the Cornell Board of Trustees, who had him summoned. Even in those times, with Morrison the center of a series of attacks for such charges as "urging clemency for the Rosenbergs," the trustees were charmed by Morrison's intelligence and grace; they granted his promotion.

Morrison was also called before Senator William Jenner's Internal Security Subcommittee, where he talked frankly about himself and his early involvement with the Communist Party without naming other names; unsatisfied, the subcommittee continued to pry. For instance, they summoned another physicist for a special security clearance. This physicist was surprised but somewhat flattered to be called for special clearance. When he got there, he was taken aback to discover the committee had no interest in him; they

were only using the occasion as an opportunity to pump him about Morrison.

Morrison spent 19 years on the Cornell faculty before going to MIT. At Cornell, Morrison was famous not only for his social activism but also for his teaching. "Phil's a born teacher," Dyson, who was a colleague of Morrison's at Cornell, comments. "Whenever one didn't know what to do with a student, one sent the student along to Phil. He had an infinite supply of patience." Dyson says that it often seemed as if half the graduate students in the Physics Department were taken care of by Morrison, who spent hours talking to them, finding out which research ideas they could tackle.

Astrophysics

It was while Morrison was at Cornell that his interest turned from theoretical physics to astrophysics. "I was always rather interested in astrophysics," he recalls. "As a graduate student I published several small papers in nuclear astrophysical problems with Oppenheimer. At Cornell, though, I was actually trying to be a nuclear physicist until I took a sabbatical leave in 1952."

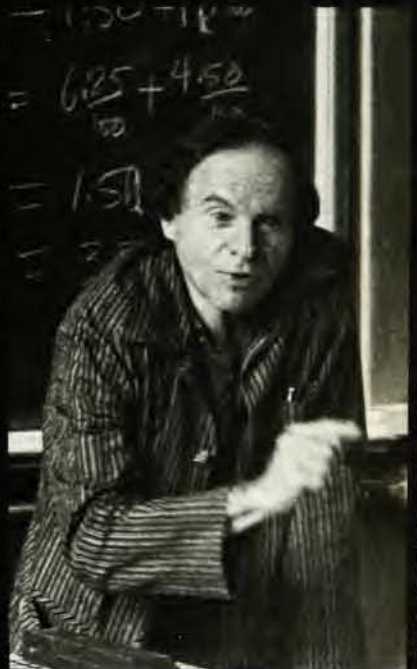
While on leave, Morrison determined to work on some of Bruno Rossi's problems; he knew Rossi's work from their days together at Los Alamos. "Along with many other scientists in the cosmic-ray domain, the early 1950s found me pushed into astronomy. The cosmic-ray people had always used this natural phenomenon as a source for high-energy particles—mesons were first discovered in cosmic rays—but in the early fifties machines became powerful enough to rival cosmic rays. Then, as machines improved, the cosmic rays were simply outcompeted. So cosmic rays were no longer of central interest from the point of view of their intrinsic physics; the interest was more in where they came from, first considering possible sources within the solar system, and then beyond. That interest gradually drew me and other scientists farther and farther into astronomy." He is pleased with the work he did on the origin of cosmic rays. "I do consider it as rather a high point. I regarded myself as a specialist in cosmic rays during the 1950s. At that time I proposed no single origin for them, but instead suggested they were highly hierarchical." Morrison argued that different places make different cosmic rays and that the highest energy concentrations might come from quasar-like objects such as the nearby radio galaxy M87.

At Cornell Morrison worked with Hans Bethe, a long-term friend and supporter. In 1956 they wrote a textbook together, *Elementary Nuclear*

Theory. "It was a useful and happy collaboration," Bethe says today. "He has ideas which are not obvious. His genius is to connect many different parts of physics." As an example, Bethe cites Morrison's discussion of the radiogenic origin of the helium isotopes in rocks. Morrison argued that the ratio of helium-3 to helium-4 is much greater in the atmosphere than it is in rocks, because it rocks helium-4 comes mainly from radioactivity, whereas in the atmosphere there is relatively more helium-3 produced by the cosmic ray-mediated disintegration of nitrogen. "It is a typical insight of Philip's to connect two opposite things—such as cosmic rays and terrestrial radioactivity—to determine the composition of samples taken from such places as hot springs."

Morrison is known not only for his ability to connect disparate elements, but for his willingness to challenge assumptions. His interpretation of M82, once touted as an example of an exploding galaxy, is one instance of this characteristic. Morrison suggested that what we were seeing is not an explosion, but rather an intergalactic dust cloud through which the galaxy is passing, the interaction giving rise to features that one might interpret as an explosion. "Although M82 looks superficially as though it were exploding in a mini-quasarlike way," Morrison comments, "in fact it seems pretty clear it isn't at all." Instead of there being one point-like center—a tiny engine that does everything for the device—the central object is the whole core of the galaxy, thousands of light years across, in which hundreds, even thousands or millions of new stars are suddenly formed. "The rapid bursts of star formations can create in some ways the same kind of activity as if there were a quasar-like object. In this case, however, the energy is primarily nuclear instead of primarily gravitational."

Paul Joss, a theoretical astrophysicist at MIT, comments on Morrison's work: "Both with M82 and with his supernova model, Morrison proposed testable models that gave us something to attack, challenging us and forcing us to rethink." Morrison's supernova model is an attempt to account for the visible light that comes from supernovae "without worrying too much about the causes of the explosion." The central idea of his theory is that the observed light from the supernova consists of two portions: those photons that reach the observer directly along a straight line and those that interact at least once, travelling along a dogleg path. Because the original outburst is so brief, even the small delays that arise from the somewhat greater length of the dogleg path are significant. Simple geometrical arguments





J. Robert Oppenheimer (left) and Major W. A. Stevens in May 1944, selecting a site for the atomic-bomb test. (Photo by Kenneth Bainbridge, courtesy AIP Niels Bohr Library.)

show that the locus of the secondary emission points (places where light from the supernova is absorbed and then reemitted as fluorescence) form a sequence of expanding ellipsoids whose focal points are the point of the supernova outburst and the position of the observer. Because fluorescence efficiencies are typically a percent or less, the total energy of the explosion is from 100 to 1000 times more than can be detected on earth in the visible region.

Joss says, "Phil's work on supernovae is a very good example of his impact on astrophysics. He has a way of looking at fundamental assumptions and asking, 'Why do we believe this?' In supernovae, for instance, there was a standard picture, one that was probably right in a primitive sense, that is, supernovae result from violent explosions in massive stars, causing in turn both a very large expulsion of matter into interstellar space and a very large amount of electromagnetic radiation. But Phil noted that if you take a star the size of the sun and blow it up, you are not going to get a tremendous amount of visible light. The energy that comes out is 10^{10} or 10^{11} times the luminosity of the sun, and if it radiates as a blackbody, then that energy is not going to come out as visible light, it is going to come out as x rays. The expansion of the exploding material, increasing the size of the radiating surface, won't help either, because by the time the material has expanded as much as it has to—through several orders of magnitude times its original size—it will have undergone such adiabatic cooling it will hardly radiate at all. So the reason that one can see this visible light has to be more complicat-

ed. What Phil did was come up with a very specific model. It's been controversial, but that's not the point. It was a testable model that made specific predictions and challenged astrophysicists to reconsider some of their basic assumptions about the supernova phenomenon."

Teaching

Morrison has been at MIT since 1964, first as Francis Friedman Visiting Professor, and then as a permanent faculty member since 1965. Morrison's interest in educational theory influenced his move to MIT. "Gerald Zacharias invited me to the school. He had an intense interest in science education, an interest he knew I shared." MIT was a center of educational innovation, and Morrison was associated with the Physical Science Study Committee at its inception and coauthor of its secondary-school text *Physics*. Morrison, together with Don Holcomb of Cornell, also wrote a physics text for college students, *My Father's Watch*. Although not widely used, it had a special appeal to teachers introducing adults to physics, perhaps because of Morri-

son's care to relate scientific arguments to history, art and philosophy.

Throughout Morrison's career he has interpreted science for the public in popular articles, in science films, and in monthly book reviews for *Scientific American*. These book reviews in all fields of science are particularly well-known. One hundred years ago, Charles Darwin wrote⁶ of the scientist Robert Brown: "He was rather given to sneering at anyone who wrote about what he did not fully understand. I remember praising Whewell's *History of the Inductive Sciences* to him, and he answered, 'Yes, I suppose that he has read the prefaces of very many books.'" Morrison is vulnerable to the same sneer, yet few would comment so of his incisive performances each month in *Scientific American*. Instead, one senses a polymath interested in every nook and cranny, as Morrison somehow makes his way through the 500 books he receives each month, choosing and then reviewing thoroughly the handful he selects as interesting and instructive.

"I judge my job to see what's inside, and then to unpack it. The nice part of the book-review column in *Scientific American*, and what makes it different from others, is that I don't need to review all the important books. I am not obliged to say, this is a lousy book but we have to review it because it is the work of an important author." Instead, Morrison tries to take a variety of books representing either a good popular approach or an approach at an introductory level. The reviews—serious, generous, often more entertaining than the original volumes—are a reflection of the intellectual energy that consumes Morrison; they are also the result of the peculiar ability he has to

Vatican Conference on nuclei of galaxies, 1970: Morrison, an unidentified priest, Donald Osterbrock, Martin Rees and Edwin Salpeter. (Courtesy of AIP Niels Bohr Library.)



read almost as rapidly as he can turn pages.

Throughout Morrison's book reviews, books and films, there is a stress on the evidence rather than on neatly packaged conclusions or indeed on the personality of the presenter. "The key thing in a science film is to show the evidence," Morrison says, "but the media believe more in testimony and atmosphere." Morrison tells an anecdote to illustrate this conflict. In his film "Whispers from Space," which Morrison considers his best, he spends half the program establishing and demonstrating experiments that are at least 100 years old. For instance, to illustrate one of the most important features of blackbody radiation, the viewers see a kiln loaded with dishes and piggybanks. These gradually heat up until all detail is lost: first the dishes disappear, then the piggybanks, until the viewer is left with a bland, smooth space. When the executive producer saw the clip, he exclaimed, "You're spending all this time and money on a thing you tell me was discovered 150 years ago. We can't do that old-hat stuff."

Morrison comments, "So long as science is seen largely as a personal view, so long as science films have a speaker who mainly ignores the evidence and presents the history of science as his own concoction of ideas and insights, it is possible to talk of Bermuda triangles and flying saucers. It's good enough if someone says it. If you invent myths and don't explain, people can't test the foundations of your beliefs, or be prepared to change when the foundation changes. Then another myth comes along and beats your myth. That's how the creationists can come along with their demands for equal time: as far as they are concerned, it is myth against myth."

Essentially Morrison was a radical as a youth and remains that way today. His deep involvement in arms control extends from 1945 to the present. Two years ago he, his wife, and four Boston-area colleagues published *The Price of Our Defense: A New Strategy for Military Spending*. The book aimed at limiting the upward-spiraling arms trade and thus lightening what Morrison calls "the thermonuclear sword hanging over all mankind, sharper and heavier each decade." The authors take a look at how much the US needs to spend to maintain its national security, and propose a program for decreasing land, sea and air forces to give a "prudent military structure prepared for eventualities short of all-out nuclear attack." Against an all-out nuclear attack, the authors argue, there can be no defense; one must rely on deterrence alone.

How well has the book done? "The Pentagon was interested," Morrison

comments. "It sold quite well in bookstores in Washington. It's also been popular with people in the peace movement. But we are in a period when the Russians are perceived as standing 10 feet tall. There are no signs that the government is considering the nuclear-arms cuts we proposed. In fact, it's quite the opposite." Morrison continues to act as a gadfly to the defense establishment with an energy characteristic of all his political struggles. One of his targets is the Air Force, which he says is on the edge of obsolescence. "Of course, it can't accept that, and so it tries harder. As the largest industrial organization in the world, it is up to all the sorts of things you would expect from a huge organization that cannot face its own obsolescence. The MX system is a perfect example; its chief value lies in its ability to keep the Air Force in the strategic-missile business."

Morrison continues to have a deep concern about nuclear weapons. "It is one of the great failings of the American political process," he says, "that there is a huge hue and cry against nuclear reactors, and nothing much about bombs. I think to some extent this had to do with displacement. People can't deal with bombs, and they displace their concern onto reactors, which turn out to be vulnerable objects. It's a most important phenomenon, the absence of attention to one, and the irrational attention to the other. But since the summer of 1981 I see a decisive change."

One of Morrison's most striking characteristics is the immense energy he has spent writing about science for the public. Why do this? "In part," he replies, "I think it is simply that I have a flair for it. But I imagine it's more than that. I feel very keenly an obligation to maintain the social nexus in which I've learned and become a scientist. The one obligation society makes on you is that you must explain your craft, because that is the cultural treasure you can pass on. People in the future will need the information."

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