cious attacks not only on JRO, but also on David Bohm, Wendell Furry, Philip Morrison, and many others.

The implicit charge that I am one of those "oddly reluctant to admit what actually happened" strikes me as very odd. I could more plausibly be charged with being naive in assuming that physicists today are aware of the most basic facts about the McCarthy era.

KURT GOTTFRIED
(kg13@cornell.edu)
Cornell University
Ithaca, New York

LUSTIG REPLIES: The readers of PHYSICS TODAY owe Ben Oppenheimer a debt of gratitude for recalling the atmosphere of extreme, and often irrational, anticommunism and the baneful effects of McCarthyism that afflicted the country in the years surrounding the removal of J. Robert Oppenheimer's security clearance. (I could not take the space to deal with that subject in my very differently focused article.) There is little doubt that the exhumation in 1953 of Robert Oppenheimer's previous communist associations and evasive actions was licensed, if not inspired, by McCarthyism, or that they played a role in the proceedings against him.

In recommending against restoring Oppenheimer's clearance (in spite of finding him unquestionably loyal), the majority of the review board set up by the Atomic Energy Commission cited four considerations: (1) ". . . his continuing conduct and associations [which] have reflected a serious disregard for the requirements of the security system," (2) ". . . a susceptibility to influence which could have serious implications for . . . security," (3) ". . . his conduct in the hydrogen-bomb program," and (4) his lack of candor. In the board's report, the third reason got as much space as the three others combined. Contrary to Ben Oppenheimer's assertion, I never stated that "Oppenheimer's clear opposition to the H-bomb was the only or even principal reason for the AEC's actions," but the record clearly demonstrates that it was one of the reasons.

In speaking up for J. Robert Oppenheimer and in expressing confidence in his loyalty, the American Physical Society did not explicitly point to McCarthyism and the prevalent anticommunism as a cause of his problems. Rather, by giving prominence to the H-bomb accusation, it chose to focus on the evil effects of persecuting scientists and others for their unpopular opinions and advice. I believe that this was an important statement for APS to have made on behalf of its members, science, and the country, and for that reason I included it in my history of the society. APS did not at the time (or ever, as far as I know) issue a broad, general attack on McCarthyism or a defense of anti-anticommunism. (And after all, there were communist spies at Los Alamos.) It is not clear from Ben Oppenheimer's letter whether he finds that to be naive, astute (because it wouldn't have done any good), or cowardly.

However, he does APS an injustice in stating that the society was not willing to help "our best researchers" (or even its ordinary members) until after the 1950s. To cite one early case to the contrary, consider what happened in 1948, when Edward U. Condon—who was then the director of the National Bureau of Standards—was pronounced by the House Un-American Activities Committee to be "one of the weakest links in our atomic security." The APS council issued a strong statement in his defense. On 5 March, in prominently overing the APS action, the *New York Times* reported that APS, in a move "unprecedented for an organization devoted exclusively to the affairs of pure science, entered the field of politics yesterday with a letter vigorously assailing the actions of the House Un-American Activities Committee in reference to Dr. Edward U. Condon. . . . The distinction between this message and those from other organizations lies in the fact that the American Physical Society prides itself on its aloofness from all matters except the intricacies of pure physics." The last sentence was slightly hyperbolic, but the newspaper's realization that APS was not in the habit of issuing political broadsides undoubtedly helped in the multifaceted and successful efforts of gaining clearance for Condon.

HARRY LUSTIG
(lustig@earthlink.net)
Santa Fe, New Mexico

Radiation Discoverer Rutherford Was Alpha Male in Deed and Word

Harry Lustig's article "APS and the Wider World" in your March issue (page 27) is readable, enjoyable, and packed with useful information. It does, however, contain a minor but significant error. Lustig states (on page 30) that "In December 1901, Ernest Rutherford, then at McGill University, gave two papers on radioactiv-

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ity, and a year later, Rutherford reported the discovery of (what were only later called) alpha particles, under the title 'The Magnetic and Electric Deviation of the Easily Absorbed Rays from Radium.' "

The facts are somewhat different. In January 1899, Rutherford published a paper in the *Philosophical Magazine* under the title "Uranium Radiation and the Electrical Conduction Produced by It."[1] In that paper, he stated (on page 116): "These experiments [i.e. the absorption of the radiation emitted by a uranium source in aluminium foil of increasing thickness] show that the uranium radiation is complex, and that there are present at least two distinct types of radiation—one that is very readily absorbed, which will be termed for convenience the α radiation, and the other of a more penetrative character, which will be termed the β radiation." This work was carried out in 1898 at the Cavendish Laboratory in Cambridge, England, where Rutherford was an 1851 Exhibition scholar under J. J. Thomson.

There is evidence Rutherford was aware of the existence of a third type of radiation emitted by uranium, but the discovery of the γ radiation is usually attributed to Paul Villard in 1900.[2]

References

1. E. Rutherford, Philos. Mag. Ser. 5 **47**, 109 (1899).
2. P. Villard, C. R. Acad. Sci. **130**, 1010 (1900).

MONTAGUE COHEN
McGill University
Montreal, Quebec, Canada

LUSTIG REPLIES: I am obliged to Montague Cohen for pointing out that I mischaracterized the paper that Ernest Rutherford delivered at the December 1902 meeting of the American Physical Society. Its very title (which I cited in my article) should have stopped me from carelessly writing that it announced the discovery of the alpha particle; and as it was, the absence of the word "alpha" in the paper's abstract led me to mistakenly conclude that the term had been coined later. The significance of Rutherford's paper was that the direction of the deflection proved that the alpha particle was positively charged, and its magnitude led to the determination of the ratio of the particle's charge to its mass.

HARRY LUSTIG
(lustig@earthlink.net)
Santa Fe, New Mexico

More on the Sociology of Science—and a Note on Kant's Position

Mara Beller's article "The Sokal Hoax: At Whom Are We Laughing?" (PHYSICS TODAY, September 1998, page 29) only reinforces my conclusions arrived at during 40 years of experience since my wife, Malka Lipkin, earned a degree in sociology in 1959 at the University of Illinois and told me about the courses she took explaining how scientists work and the so-called scientific method. Nothing written by historians and philosophers of science about research in physics and how physicists work needs to be taken seriously. They haven't a clue.

Reality today means driving a car in which the driver is guided by an electronic navigation system controlled by signals received from satellites and interpreted by computers using Albert Einstein's general theory of relativity. Every aspect of modern life seems to feel the impact of devices using lasers, computers, magnetic resonance imaging, and solid-state electronics, which would not work without quantum mechanics. Are relativity and quantum mechanics reality or simply texts? Try and live without them in today's society.

Niels Bohr, Max Born, Werner Heisenberg, and Wolfgang Pauli were great physicists. But they never dreamed how their remarkable revolutionary discoveries would completely transform our everyday experiences a half century later and make them an inseparable part of the reality of the life of the common man. Their many papers about reality are completely out of date, and history has bypassed them. But historians have not. Somehow these outdated papers seem to be the only ones that historians and philosophers ever read. They behave like name droppers who are completely devoid of common sense. The great papers that led to the revolutionary discoveries get lost in the confusion.

A number of years ago, I was asked to give a talk about the impact of the discovery of the antiproton at a celebration of some anniversary of the discovery. I made the rounds of the postdocs to hear what they thought about the antiproton. Instead, I heard: "This is also the 100th anniversary of the birth of Niels Bohr. What did Bohr really do?" They knew about the Schrödinger equation, the Dirac equation, the Heisenberg uncertainty principle, the Pauli exclusion principle, the Born–Oppenheimer approximation, and all that. But where were the Bohr equation, the Bohr principle, or the Bohr approximation? Ah yes, there was the Bohr–Sommerfeld quantum theory. "But this is all wrong! Who needs it? What did Bohr do to deserve all this fame?"

I would like our current historians to explain for the next generation what people like Bohr did to make their names worth remembering, not to pontificate about their outdated philosophical utterances. Otherwise, the next generation will not even know who those people were, let alone what they might have said.

I recall a very profound remark made back in 1958 by another great physicist, Eugene Wigner. I had asked him about the collective model of the nucleus recently proposed by Aage Bohr and Ben Mottelson, for which they later were awarded the Nobel Prize. I had heard that Wigner did not like it. "Yes," he said, "I think that this model is wrong. But you know, the old quantum theory of Bohr and Sommerfeld was wrong, too. And it is very difficult to see how we could have ever found the right quantum theory without going through this stage."

I also recall a talk by Paul Dirac about his discovery of the now-famous Dirac equation. When he was asked whether he was bothered by the appearance of the unphysical negative energy states, his answer was, more or less: "No. I had successfully solved the difficulty of finding a description of the electron which was consistent with both relativity and