LETTERS

Needed: Gatekeepers of Science Who Are Receptive to New Ideas

In David Mermin's critique in "Reference Frame" (March, page 11, and April, page 11) of Harry Collins and Trevor Pinch's book *The Golem* is a refreshing defense of the modern science community. Taken together, the critique and the book are reminiscent of the story of a committee of blind people trying to describe an elephant, with each person's perception being shaped by how he or she made contact with the beast. I believe both Mermin and Collins/Pinch present reasonable depictions of science from different perspectives.

For two decades, my contact with frontier science has been primarily as a challenger of the standard paradigm.[1] I have found that many of the gatekeepers of science—the referees—do not always abide by the basic tenets of the scientific method. My experience shows that if you formulate hypotheses that are considered too radical by the gatekeepers, you very quickly discover why sociologists write books like *The Golem*.

As a scientist, I am not obligated to function as a disciple of the standard paradigm, just as society is not obligated to support my pursuits. As a challenger of the *status quo*, I must convince its defenders that the changes I propose have value.

On the other hand, the history of science contains many examples of leaders in various fields effectively blocking challenges to their worldview.[2] Also, intellectual revolutions tend to be unpopular because they demand a change in the ways most people think about the world. In fact, resistance to change has been so pervasive that Max Planck declared that "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."[3]

Nevertheless, I believe that when professionals accept the role of referee, they are also accepting the responsibility for not letting skeptical conservatism become censorship. They must remember that science is not a popularity contest, even though the popularity factor has helped shape scientific change, triggered scientific disputes and given sociology a role to play in developing an understanding of the history of science.

The physics community is presently reevaluating its role in society and the way it is training future physicists. It should call for all physicists to study the history and philosophy of science so they will be better prepared as seekers of truth in a society where many argue that "truth is relative" and "perception is reality." Perhaps then they will also acquire the humility that comes with understanding that no matter how much we think we know about the endlessly fascinating universe, we may be only one experiment away from having our entire worldview changed.

References

1. See, for example, J. R. Fanchi, *Parametrized Relativistic Quantum Theory*, Kluwer Academic, Norwell, Mass. (1993).
2. See, for example, T. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed., U. of Chicago P. (1970); R. S. Root-Bernstein, *Discovering*, Harvard U. P., Cambridge (1989).
3. Quoted in Kuhn, p. 151.

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Reviews of *Dark Sun* Trigger Exchanges

I normally do not respond to negative reviews of my books—the books themselves are sufficient answer—but for *ad hominem* attacks such as the two reviews of *Dark Sun: The Making of the Hydrogen Bomb* that PHYSICS TODAY ran side by side in January (page 61), I'll make an exception.

I was hardly a "little-known journalist and novelist [who had] turned to writing sustained history," as Barton J. Bernstein dismisses me, when I began writing *The Making of the*

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Atomic Bomb. By then I had published six books and more than fifty articles in national magazines and had received Guggenheim, Ford and other fellowships. In any case, my Yale BA *cum laude* in history surely qualified me to work in Bernstein's chosen field.

I wonder in turn what qualifies Bernstein—an assembler rather than a writer of books, with no book-length original work to his name—to judge the merits of *Dark Sun*'s "framework" and "organization." It's certainly true that the first third of a book (not "about half the volume," as Bernstein erroneously counts it) subtitled *The Making of the Hydrogen Bomb* deals with Soviet atomic espionage and the Soviet fission program that culminated in Joe 1 in 1949, but those were the crucial factors that drove the US decision to accelerate work on a hydrogen bomb.

Bernstein's review is long on assertions and short on evidence. He does not identify the "serious weaknesses" he perceives in *The Making of the Atomic Bomb*. If, as he alleges, my work in *Dark Sun* "occasionally reaches . . . beyond the evidence," relies "heavily on others' scholarship" and is guilty of "often unduly personalizing events" (whatever that may mean), Bernstein does not say where and when. Nor does he make clear that my reliance on others' scholarship—as well as my extensive work of interviewing and of examining original documents—is carefully referenced with 92 pages of endnotes and bibliography. I take specific exception to his claim that I found the story of the nine atomic bombs transferred to Guam for use in the Korean War in secondary sources. To the contrary, I exhumed the full details of the story for the first time from the several thousand otherwise tedious pages of SAC commander Curtis LeMay's daily diaries.

Bernstein is particularly misleading in his characterization of my treatment of J. Robert Oppenheimer's 1954 security hearing. Oddly, he sneers that I rely on "the established literature" without identifying himself as a major contributor to that literature. He claims I fail to appreciate or to emphasize the serious doubts about Oppenheimer's loyalty entertained by "prominent people." Then he lists several prominent people—Luis Alvarez, Kenneth Pitzer, Lewis Strauss, Ernest Lawrence—whom I follow and characterize at length. Whether my evidence that Oppenheimer lied at the hearing about wartime espionage contacts is "dubious" I leave to the reader to weigh against Bernstein's bare assertion; one of my sources was the official history of the US Atomic Energy Commission, which drew on classified sources.

More deeply offensive is Bernstein's dismissal of the testimony of I. I. Rabi in 1953 and "retired Los Alamos theorist" J. Carson Mark "in a retrospective 1994 interview" (as Bernstein notes nastily), that Edward Teller's obsession with megaton yields delayed the development of a US hydrogen bomb.

Readers in the physics community will know how to weigh these differences of opinion. They should also know, in assessing Bernstein's credibility, that I criticize him in *Dark Sun* (in an extended endnote on page 644 that he cannot have missed) for misreading a crucial document concerning the 1949 H-bomb debate. Nowhere in his review does Bernstein acknowledge this evident cause for bias.

As for Gennady Gorelik's incoherent review, his consistent misreading of my text makes it clear that English is not his first language. I did not fail, as he alleges, to explain Andrei Sakharov's "layer cake" idea; Gorelik can find it described on page 334, with further elaboration on pages 418, 516 and 524–25, as referenced in my index. Gorelik charges that *Dark Sun* lacks "scientific and social context." He must be confusing my deliberately limited narrative with the more comprehensive work he is himself preparing. My reconstruction of Soviet nuclear history benefited from rich Russian documentary resources that I arranged to have translated, as well as from extensive communications with Russian principals including Yuli Khariton, Yuri Smirnov, Victor Adamsky and Elena Bonner.

Gorelik compares *Dark Sun* unfavorably to David Holloway's *Stalin and the Bomb*. The comparison is specious—an apple and an orange. As I acknowledge, Holloway graciously arranged for me to read his book in galleys after I had drafted my own narrative. I found that reading to be a useful check; we had arrived independently at many of the same conclusions, using many of the same sources. As for "the proper use of patronymics," my book required a glossary to identify its 300-some characters; I saw no reason to confuse the reader further with the complexities of Russian names.

In a review in *Science* (vol. 269, p. 1455), Hans Bethe concluded of *Dark Sun*, "The book is full of suspense. Its only fault is that it kept me from doing other work." At the risk of keeping readers of PHYSICS TODAY from doing other work, I hope they will turn to *Dark Sun* itself to assess its worth.

RICHARD RHODES
Madison, Connecticut

I was astonished by the "Books" section of the January 1996 issue of PHYSICS TODAY. I cannot recall having seen in print anywhere, and surely not in a reputable physics publication, anything comparable: two lengthy lead reviews in parallel columns, both by reviewers of similar backgrounds, both highly negative about the same book—in this instance, *Dark Sun* by Richard Rhodes.

While both reviewers make clear their dislike of *Dark Sun*, their reviews are not especially informative. When not making unscholarly remarks ("often bloated and desultory"), they devote a good deal of energy to damning with extremely faint praise: "Rhodes copes pretty well . . ." and "As a popularizer, Rhodes is generally skillful and arresting. He is shrewd in picking good quotes. . . ."

Both reviewers include gratuitous remarks about Rhodes's previous book, *The Making of the Atomic Bomb*, a book that was not the one under review. Gennady Gorelik dismisses it ("he dealt adequately with the atomic phase of world history"), but Bernstein goes further: "It won accolades and prizes. It was a good 'read'—vivid writing about a great story. Its serious weaknesses were seldom noted." That is surely hitting below the belt. As the former book review editor and current editor of the *American Journal of Physics*, I believe that reviewers should restrict their attention to the book under review, or at least should not attack previous books by the same author. In this case, if Bernstein felt compelled to criticize Rhodes's earlier book, he had a responsibility to tell us something about its "serious weaknesses."

I am surprised that two historians, rather than a historian and a nonhistorian (I would have tried hard to get a physicist and a historian of science), were commissioned to write about *Dark Sun*. In so doing, in getting back highly critical reviews and in then deciding to publish them as lead reviews, PHYSICS TODAY certainly gives the impression that it was out to do a hatchet job on Richard Rhodes. I expect higher standards of fairness and of book reviewing from PHYSICS TODAY.

ROBERT H. ROMER
Amherst College
Amherst, Massachusetts

BERNSTEIN REPLIES: I find that Rhodes's response confirms my initial assessment of *Dark Sun*—and

also prompts me to add the following points to my criticisms of the book.

On the issue of his originality, Rhodes has chosen to make his "discovery" of the A-bombs on Guam his test case. Too bad for him. It was Roger Dingman who first published the story (based partly on Curtis LeMay's diary), in the winter 1988/89 issue of *International Security*. Similarly, Rhodes failed to get there first on such other matters as America's 1946–47 A-bomb situation (see David Rosenberg's essays) and the Soviet A-bomb's imitation of the US weapon (see David Holloway's *Stalin and the Bomb*). Putting it bluntly, Rhodes's claims of newness are often overblown.

As for the Oppenheimer issues, it is evident from a careful reading of Rhodes's endnotes that he did not examine some of the crucial "perjury" materials, which were declassified by about 1991. Nor did he use the archives of Fermi, Urey, Feynman, DuBridge and Rabi, among others who supported Oppenheimer. If Rhodes had done more archival work (which is essential for any serious, well-researched history), he may well come to understand, appreciate and question why about a dozen prominent people (more than the handful he noted) had long suspected Oppenheimer, even though their suspicions were unreasonable.

My emphasis on the date of J. Carson Mark's anti-Teller remarks was not intended to denigrate Mark, whom I admire, but to stress the dangers of uncritically using retrospective eyewitness accounts of events that happened decades earlier. *Dark Sun* makes my case by drawing repeatedly on LeMay's blustery 1984 claims about events that happened 25–35 years earlier, by failing to mention that Cyril Smith's 1967 recollection was uncertain on some 1949 H-bomb matters and by relying without caveat on Willard Libby's confused oral history.

I am astonished to think that Rhodes believes an undergraduate *cum laude* degree is a sufficient qualification for writing reliable history. Much more is required.

Also, I am amused to see that Rhodes thinks I am biased against him because of what he asserts is a criticism of me in the book—and what I construe as a peculiar quibble too trivial to inspire bias.

Finally, Rhodes's response includes a type of distortion that mars *Dark Sun* itself in places. He rebukes me for inaccurately stating that "about half" of the book is on the Soviet fission program and atomic espionage, but strangely he omits the rest of my sentence, which made it clear that "about half" also includes the US fission program and LeMay's A-bomb activities. In doing so, Rhodes distorts my basic point, which is that about half of *Dark Sun* deals with non–H-bomb matters.

Barton J. Bernstein
Stanford University
Stanford, California

GORELIK REPLIES: I confined the scope of my review to my professional field, the history of Russian physics. I tried to point out, in quite a concrete way, some deficiencies on the Russian side of Rhodes's book, notwithstanding his ability as an analyst and writer. However, my main point was that, when Rhodes was doing his research, the available Russian documentary resources on H-bomb history were too poor, and government secrecy and self-censorship were still too strong, for him to have been able to write a realistic and comprehensive account.

Rather than reargue any of the points I covered in my review or those being raised by Rhodes, I prefer to simply use this limited space to draw people's attention to the fact that the Russians are now starting to release historically valuable information on the development of the Soviet H-bomb. President Boris Yeltsin has decreed that documents from the long-secret Soviet atomic archives be published. Declassified information on Soviet H-bomb history is already being presented at scientific meetings. Veterans of the Soviet atomic project are beginning to offer informative firsthand recollections.

The combined outcome is likely to be a wealth of new material that eventually will shine a bright light on both Soviet/ Russian and Western thermonuclear history.

Gennady Gorelik
Boston University
Boston, Massachusetts

Raise the *PT* Ante

The publisher's reply to the letters commenting on the March issue's split cover (May, page 91) is well put, and I hope your readers caught the irony of his words.

As members of the various American Institute of Physics societies, PHYSICS TODAY readers get this fine periodical practically free of charge. Some of us certainly wouldn't mind torn or even missing copies once in a while (although I have never experienced such a problem).

Perhaps you should start charging a fair price for PHYSICS TODAY. That might make it respectable in the eyes of any ungrateful readers.

Herbert Kobayashi
Edgewater, Maryland

Attempts at Humor—Again

In her letter (June, page 83) commenting on my letter concerning capital punishment as a topic for attempts at humor, Julie S. Link attributes to me a view that was nowhere expressed in my letter. While I suppose I am "against cancer" (whatever that means), I am not "dead set" against capital punishment. It may be an appropriate, and even necessary, response to some crimes.

Reuben E. Alley
Annapolis, Maryland

Postscript on Biedenharn Obit

In our obituary of Lawrence C. Biedenharn Jr (June, page 74), we should have noted that, after his retirement from Duke University in 1993, Biedenharn became an adjunct professor of physics at the University of Texas at Austin and held that position until he died.

In addition, Biedenharn received his PhD at MIT under the direction of Victor Weisskopf—not, as we wrote, John Blatt.

Edward G. Bilpuch
Horst Meyer
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Motion Proposed: Beyond Boltzmann

The ubiquity of Lévy distributions in nature, amply related in Klafter, Shlesinger and Zumofen's beautiful article, "Beyond Brownian Motion" (February, page 33), can be understood in a compelling way by studying Constantino Tsallis's generalized statistical mechanics.[1]

Tsallis's work goes back to 1988,[2] and it could well be thought of as being "Beyond Boltzmann."

References

1. C. Tsallis, S. V. F. Levy, A. M. C. Souza, R. Maynard, Phys. Rev. Lett. **75**, 3589 (1995).
2. C. Tsallis, J. Stat. Phys. **52**, 479 (1988).

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