ing. They range over topics from the nature of the chemical bond and quantum chemistry to solid-state physics, nuclear physics, sickle-cell anemia, morality, politics and many others.

Why, then, does Rigden give the weirdly inaccurate figure of 202 references? Clearly, he merely counted up the footnotes listed at the end of the book, for these *do* total exactly 202. But the other 300-plus references are given in the text of the book itself. This was the publisher's policy, and while it may be unusual, a careful reading of the text could not possibly result in anyone's overlooking 300 references.

Did Rigden read it carefully? Consider also his claim that "Pauling's rejection of the molecular orbital approach needs an explanation that is not found in this book." This question is answered on page 124, by a quote from one of my in-person interviews.

The research for the book was extensive, including trips to California, Oregon, Seattle, Washington, Cambridge, Ithaca, New York, New Haven and other areas of the world for personal interviews with Pauling's contemporaries. I also conducted hundreds of hours of phone interviews with key persons (including phone interviews with an admittedly reluctant Pauling himself), assembled boxloads of Pauling correspondence and spent thousands of hours in libraries across the nation and over five years of my life on the project. Further, my book has been endorsed without qualification by many distinguished scientists and science writers, including Martin Gardner, Isaac Asimov, Robert Olby and many others.

Some of Rigden's other comments are also odd. Consider his remarks regarding John Slater's attack on Pauling's methods. He says, "The primary literature might well have shed some light on serious charges such as these." Note that on page 160 of my book, I quote the text of a personal letter from Slater to Pauling. Slater's letter contains a direct condemnation of Pauling's theory of ferromagnetism and a condemnation of Pauling's entire approach to science. A reviewer should be aware that in a biography a personal letter is universally regarded as a "primary source."

Perhaps there is a deeper problem: Scientists tend to prefer that biographies of scientists cover only the scientist's ideas and actual research. This is understandable, since proceeding in this way tends to hide the human side of science—the jealousies, competitiveness and occasional rancor that are part and parcel of any activity in which live human beings participate. But science *is* conducted by human beings. Nathan Reingold, in his book *The Sciences in the American Context* (Smithsonian Institution Press, Washington, DC, 1979), sums up one of the most widely held views on how to approach the history of science: "Unlike the older history of science, concepts and data will serve as important elements of human environments, *not as the principal objects of study justifying the endeavor*" (emphasis mine).

ANTHONY SERAFINI
12/90 *Hackettstown, New Jersey*

Was Sakharov's 'Dark Side' Deleted?

Something essential is missing from the picture of Andrei Sakharov painted in the August 1990 issue of PHYSICS TODAY, as if his life was rewritten (in Soviet style). It is one thing to forgive someone's early mistakes because of his good deeds later, and it is something else to miss a significant dark part of his life. Life accounts are supposed to be complete!

Reading about the many Sakharovs, I cannot help but ask myself, Where is the Sakharov who rose to prominence like a meteor *in the Stalin regime*? Everyone who knows the Communist or Nazi systems knows that no one got ahead who did not go along. Indeed, those who did not go along had to consider themselves lucky if they were not smashed—regardless of their talents. So where is the dark side of Sakharov, who was enlisted in classified work on atomic weapons, and who became a member of the Soviet Academy of Sciences at the age of 32? Where are the behind-the-scenes "achievements" that bought him the trust of Stalin and Lavrenti Beria?

LANCELOT I. KETHLEY
8/90 *Portland, Oregon*

Ernst Ruska's Wartime Generosity

I was quite surprised to read in Ernst Ruska's obituary (July 1990, page 84), written by Peter W. Hawkes, that Ruska "offered shelter in his apartment during the wartime bombing of Berlin to Jews, who were excluded from the public shelters." Were there really any identifiable Jews in wartime Berlin once the serious bombing had started?

J. FUTTERMAN
8/90 *Los Angeles, California*

HAWKES REPLIES: My information comes from the widow of Ernst Ruska, via Tom Mulvey, who has made many contributions to the history of electron microscopy and translated Ruska's historical volume *The Early Development of Electron Lenses and Electron Microscopy* (Hirzel, Stuttgart, 1980) into English. Ruska was profoundly shocked by the treatment of the Jews and declared, after one anti-Semitic massacre, that this would bring punishment on the German people. No doubt the Jews surviving in wartime Berlin were indeed not readily identifiable except to their friends.

PETER W. HAWKES
Centre National de la Recherche Scientifique
12/90 *Toulouse, France*

A Particle for All Reasons

I wish to propose the existence of yet another subatomic particle, the "so-on" [pronounced "sew on," though not to be confused with a jacket patch that proclaims one's unending loyalty to some musical group like Guns n' Roses (*sic*, in more senses of the word than one)].

This is the particle that explains events that cannot be explained otherwise, as in the sentence "This is obviously due to electric fluidity, quantum fluxes and so-on." The lifetime of a so-on will obviously be short—about as short as the conscience span of the speaker who invokes it, or roughly 10^{-50} seconds—and the particle itself will thus be undetectable, though a few attempts will be necessary to prove that its existence can't be proven. All of this at a suitable dollar amount, of course, assessed at more or less the reciprocal of its lifetime.

R. M. KREN
Department of Metachemistry
11/90 *University of Michigan, Flint*

Corrections

January, page 95—The estimate by Adrian Blaauw mentioned in Wolfgang Kundt's letter implies that all stars more massive than 4, not 14, solar masses would be required as progenitor stars of neutron stars.

December, page 28—The conjecture about the pair distribution function in superfluid helium-4 that was attributed to Roger Penrose and Lars Onsager should have been attributed to Oliver Penrose and Lars Onsager. ■