the major research areas in radio astronomy, but astronomy is a fast-moving field, and much of this material is already dated.

There are some awfully good parts to Burke and Graham-Smith's text: aperture distributions and beam pattern of telescopes; theory of interferometry; continuum and maser emission from stars; radio galaxies, quasars and the accretion disk model; and gravitational lensing. By and large, these are areas in which the authors or their colleagues have been personally involved, and it shows.

However, other aspects of the book are disappointing. The coverage is very spotty, with some very important areas receiving little real attention and others receiving too much. Most surprising to me was the treatment of the Galactic center. This is largely the domain of radio astronomy and is the nearest example we have to the hugely more powerful nuclei that power the quasars and radio galaxies that the authors treat so well. But with only one page of text (and five figures!), it is sadly glossed over.

Many of the figures and results are poorly selected and/or out of date. Emphasizing the historical development at some level has great merit, but I think the authors go too far. For example, they include the original (~35-year-old) 21 cm line map of Galactic spiral arms and state that this representation has not been improved upon. However, it certainly has, and perhaps the current best model is developed in a paper on the Galactic electron distribution that they quote extensively elsewhere in the book. Many figures are taken directly from sources in the literature, but without enough description; either more text or a modified figure would have helped a lot. The book is full of undefined and poorly defined jargon. The last chapter presents the broadest of broad summaries and a speculative look ahead, but it seems as though its predictions were made years ago because some, such as adaptive optics, have already been fulfilled.

These days, one can hardly even think about radio astronomy without worrying about interference generated by civilization's transmitters. Rohlfs and Wilson ignore this all-important topic. Burke and Graham-Smith end their book with a significant section on interference and frequency protection and, amazingly to me, end on an optimistic note that emphasizes the protected frequency bands. Practicing radio astronomers know full well that a protected band is no guarantee!

The fact is that we need to develop methods to observe through the interference. The new Green Bank Telescope, with its unobstructed aperture, is one possible way to deal with this problem, and I was disappointed to find no mention of this important aspect in either book. Just now being considered are the development of electronics, techniques and software that make interference reduction or excision possible to at least some degree. In one highly ironic endgame, terrestrial interference may force radio astronomers—who have traditionally been more hands on than most other astronomers—to operate remotely, in the most hands-off fashion possible, from the back side of the Moon.

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## The Theory of Superconductivity in the High-$T_c$ Cuprates

Philip W. Anderson
*Princeton U. P., Princeton, N.J., 1997. 446 pp. $49.50 hc*
*ISBN 0-691-04365-5*

How do I begin to write *the* review of Philip W. Anderson's *The Theory of Superconductivity in the High-*$T_c$ *Cuprates*? This is the question I asked myself as the deadline for this review first fast approached and then receded even faster. I thought this must be writer's block, which I would eventually get over, but the truth is that this is an unusual book to review and I will never be able to write *the* review; instead I must settle for writing just *a* review of the book.

The extraordinary nature of Anderson's book and the issues raised in it and by it demand a different approach to reviewing. I will catalog my review along the lines of my favorite Clint Eastwood western, *The Good, The Bad, and the Ugly*.

The Good: This book has long been expected, and it is a significant contribution to our understanding of one of the great problems in condensed matter physics. What's more, it is written by one of the most important physicists of the second half of this century. Our understanding of the theory of metals, magnetism and superconductivity has been profoundly influenced by Anderson. With this as the backdrop, the book's rebellious tone, beseeching us to overturn the Fermi liquid theory of metals introduced by Lev Davidovich Landau and promulgated by the Russian heroes and a cast of thousands, is most striking and must be taken seriously.

I know that the tone and content of Anderson's book will annoy many of the researchers working on high-$T_c$ superconductivity. This, however, is what makes it stand out over and above much of the work written to date on this subject. The intense efforts by the condensed-matter community to understand the normal state, from both the theoretical and experimental sides, would not have been as intense if Anderson did not insist on overthrowing the Fermi liquid theory of metals. This is also the case with regard to the Hubbard model and its derivatives. The extensive study of this model was spurred on in large part by Anderson, and his thoughts on this topic, discussed in some detail in the book, are deep and insightful. The extensive discussions on diverse experimental topics, such as photoemission, resistivity and the Hall angle, bring out Anderson's unique ability to see through the morass of details and find the grain of truth that connects these experiments.

The Bad: The book could have used a more thorough editing. For example, like everyone else working in the field, I went straight to the author index to see if my name was there. It wasn't. I looked further to see who else was (or wasn't) there. I would like to know, who is L. M. Varma? Also where can I find E. Teller's contribution? It is certainly not on page 423, as the index states.

Looking next at the text, I began to wonder if "Landon's expression" for the penetration depth, given on page 125, was discovered before London discovered it, and if so, why haven't I heard of him? There are several such errors that should have been caught in the editing process.

I also found it a bit disturbing that, when I would get deeply into a particular argument Anderson was developing, an annoying little letter would show up in the margin. Invariably, this was a note to let the reader know that the author had rejected that idea. I think it is good to see how a great mind operates, but the text would have been easier to follow if the currently accepted thoughts were presented in the main body, with the historical evolution of the ideas given in the footnotes.

The Ugly: Anderson has very strong views about certain theoretical approaches to high-$T_c$ superconductivity, and some of his points are well taken. However, I think it is inappropriate to criticize a researcher without mentioning his or her name. Two of the people who have contributed significantly to the field and receive little or no mention are Douglas Scalapino and David Pines. Another ugly issue is the chapter on central dogmas. I hate to be dogmatic, but I don't think the field is sufficiently developed that we can support without question cen-

tral dogmas, even if there is some truth to them.

I highly recommend this book to all physicists and, in particular, graduate students. In my view, *the* book on the theory of high-$T_c$ superconductivity has not been written yet. Nevertheless, one rarely finds a book in physics that rouses one's passions about a field, even if one is mostly angered by it. Few scientists have the courage to expose their thoughts and feelings in print as candidly as Anderson does. For the many Anderson wanna-bes out there, I would not recommend emulating his style of writing. But for Anderson, we can make an exception. I am happy that I had the opportunity to review the book, and I only wish I could have made it into the author index.

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## Selected Works of Hans A. Bethe: With Commentary

Hans A. Bethe
*World Scientific, River Edge, N.J., 1997. 605 pp. $68.00 hc*
*ISBN 981-02-2876-7*

> "This book contains a selection of my publications of the 70 years during which I have been active. For each paper I have written a historical note. . . . I hope the reader will find some of the papers interesting, as I still do."

With the economy and candor that are his trademark, Hans Bethe has once again said it all on page one. The rest of us are reduced to awe, kibbitzing and pedantry.

No physicist of this or any other era could, with a straight face, write that opening sentence, if by "active" is meant the publication of original research on hot topics in leading refereed journals. Thus, the first paper in the volume, Bethe's fourth publication, is the 1929 *Annalen der Physik* classic in which the level splitting of an atom situated in crystals is analyzed in exhaustive detail by group theory. And the last selection, published in 1996 in the *Astrophysical Journal*, deals with his most recent love affair, "Breakout of the Supernova Shock."

This is a very slim volume in comparison with Bethe's prodigious output. It does not contain any of the famous reviews—on the one- and two-electron problem or on solid-state physics in the 1933 *Handbuch der Physik*, or the Bethe bible, the 1935–37 *Reviews of Modern Physics* treatise on nuclear physics.