a concept, like dark matter or the idea of superspace, he does it right. In two appendices, he makes clear how a Higgs field gets a vacuum expectation value—by artificially making its mass-squared negative—and how supersymmetry can make this *seem* less artificial—by making the mass of the top quark large. It is a pity, however, that Kane puts less effort into explaining particle spin, the *sine qua non* of supersymmetry. Also, Kane practically ignores the important "flavor" problem: the proliferation of identical quark and lepton species and their inexplicable pattern of masses and mixings.

Kane devotes chapter 3 to effective theories, the modern way to organize our descriptions of nature by the energy or distance scales to which the descriptions apply. This idea says that our models of nature can be expected to be valid only within a limited domain of energies, because our basic framework—relativistic quantum field theory—gives answers that are insensitive to physics at energies much higher than those we are studying. (The same idea works in condensed matter, as Michael Fisher, Leo Kadanoff, and Kenneth Wilson taught us.) Thus, biologists need know nothing about atomic nuclei, chemists and atomic physicists need know nothing about quarks, and so on. This notion is one of the greatest advances of 20th century physics. It puts us ahead of the 19th century giants who thought physics was complete. (It is surprising, therefore, that Kane seems to believe we can extrapolate our current knowledge 17 orders of magnitude to the Planck energy of 10^{19} GeV. On page 21: "Most particle theorists . . . think we have finally reached the end of the line." On page 45: "Most particle physicists currently expect that quarks, leptons, *W* and *Z*, and gluons will ultimately be understood as having string-like extension. . . .")

Kane's chapter 5 on experimental methods is also much needed in popular expositions. The question of how we get evidence of phenomena beyond the reach of our own senses "would be a good unifying theme for a history of scientific discovery," he says. Media presentations of science and the work of some scientist-writers often attribute advances to pure thought or to mystical manipulations by white-coated lab nerds. Kane makes clear that a well-defined process of observation has evolved, despite the limitations of our senses, to reach out to 10^{10} light years and in to 10^{-16} centimeters. He describes the mammoth particle detectors and what they "see"—photons, electrons, muons, hadrons, and missing energy—and how these elements are "seen." He stresses that all measurements have errors and that physics signals are beset by deceptive backgrounds no matter how precise the detector. Kane even tells about triggering and trigger budgets! These discussions of experimental methods and effective theories are wonderful. There ought to be a whole book on them at this level.

One thing I don't like about Kane's book is his argument, repeated so often that it seems elevated to a physical principle, that such-and-such is true because most particle physicists think or expect or believe it is true. I noted two examples above, but the book is peppered with them. No particle physicist I know denies the beauty and appeal of supersymmetry. The absence of unambiguous signals for physics beyond the standard model may be consistent with supersymmetry, but that is not evidence for it. What most theorists "currently" think is no more important than it was 100 years ago. What experiment reveals is what counts.

Finally, I cannot identify the audience for *Supersymmetry*. Kane doesn't say. It can't be beginning physics students, or even the interested public; Kane says too little about spin (and other basics) for them. Media science reporters and politicians would profit from this book, but it seems too long for their attention spans. About halfway through, I thought I had the answer: This book is meant for me! Alas, it is not funny enough. *Supersymmetry* hasn't a shred of humor or lightness, so that excludes us atheists.

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The Discovery of Anti-matter: The Autobiography of Carl David Anderson, the Youngest Man to Win the Nobel Prize

Edited by Richard J. Weiss
World Scientific, River Edge, N.J., 1999. 144 pp. $28.00 hc
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Carl David Anderson (1905–91) received the Nobel Prize in 1936 for the discovery of the positron. In 1936, with Seth Neddermeyer, Anderson also discovered the positive and negative "mesotron," now called the muon. Thus he added three new fundamental particles to physics and pointed the way to the existence of antimatter. At age 31, Anderson was then the youngest person to receive the Nobel Prize. (Tsung-Dao Lee got the 1957 prize when he was 30.) Anderson wrote this autobiography during five years, beginning in his late seventies, at the request of his son and daughter-in-law, David and Melanie Anderson, who did the preliminary editing after his death.

Anderson began his long career at Caltech as an undergraduate. Then came his PhD thesis on photoelectrons produced by x rays, under the nominal direction of Robert Millikan. ("For this I thanked him," Anderson wrote, "but not once during the three years of my graduate thesis work did he visit my laboratory or discuss the work with me.") Then came postdoctoral work, again, loosely supervised by Millikan, during which Anderson built and ran the Caltech Magnet Cloud Chamber.

For this project, Anderson built a large vertical cloud chamber and a heavy air-core magnet that produced a field of 25 kilogauss. When he first put the unwieldy apparatus, resembling an "obese pig," into operation, Anderson obtained "dramatic and completely unexpected" results: approximately equal numbers of positive and negative particles where only electrons were expected.

Anderson continued the measurements with his first graduate student, Seth Neddermeyer. They first interpreted the thin "wrong-curvature" tracks they observed as upward-moving electrons. However, with the insertion of a lead plate in the chamber, the change in curvature above and below the plate showed the particles' direction of motion. The first track thus analyzed turned out to be an upward-moving positive electron! This event, and subsequent data, led to Anderson's Nobel Prize.

To obtain more intense, higher-energy cosmic rays, the pair transported their magnet cloud-chamber to the summit of Pikes Peak, Colorado. Analyzing the cloud-chamber photos after a summer at the Peak, they found positive and negative tracks that were different from electrons and protons and appeared to have intermediate mass. While they were still pondering their high-altitude results, Millikan ordered the cloud chamber and its team to Coco Solo, in the Panama Canal Zone, to investigate the latitude dependence of sea-level cosmic rays. After their return, toward the end of 1936, Anderson and Neddermeyer proposed that the high-altitude tracks were new, unknown particles that (on account of their mass) they called "mesotrons."

Succeeding chapters of Anderson's autobiography deal with the award of the Nobel Prize, the development of rocket launchers at Caltech during World War II, and Anderson's postwar cosmic-ray research using a B-29 bomber. An interesting (and apparently little-known) wartime episode involved Anderson's being asked by Arthur H. Compton in May 1942, "to head a project to design and build an atomic bomb." Anderson turned it down "on purely economic grounds." Five months later, General Leslie R. Groves offered the job to J. Robert Oppenheimer, who accepted. Anderson observes: "I believe my greatest contribution to the World War II effort was my inability to take part in the development of the atomic bomb. Thinking so brings me peace of mind."

Anderson's autobiography gives valuable insights into the early days of cosmic-ray and elementary-particle research in America, and especially at Caltech. He describes his barely funded research and tells of the joys and challenges of "small science," remarking: "To find the positive electron and the two muons cost about $15,000."

This small book is well worth reading, but I must say (to put it gently) that it is seriously under-edited. Thus, Anderson describes the cloud chamber as counter-controlled, but fails to mention the role played by Patrick Blackett and Giuseppe Occhialini, at the University of Cambridge, who invented the coincidence counter-triggered cloud chamber in 1932 and who observed and identified electron–positron pair production. Nor does Anderson point out that Cecil Powell, Occhialini, and Cesare Lattes, at Bristol, discovered Hideki Yukawa's nuclear-force meson in 1947. In fact, the unwary reader could easily conclude from Anderson's account that the Anderson–Neddermeyer "mesotron" (now known to be the muon) was the particle predicted two years earlier by Yukawa and not a confusing look-alike. In his account of the Nobel Prize award, Anderson never mentions Viktor Hess, the discoverer of cosmic rays, with whom he shared the prize. A few editorial footnotes could have avoided these omissions and possible misconceptions.

Also, figure 4 is printed upside down, so it looks exactly like a downward-moving electron, and not an upward-moving positron as it should. The captions are exchanged on figures 25 and 26.

These criticisms aside, I am glad that the autobiography of this remarkable scientist has become generally available, and I enjoyed reading it.

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A Radar History of World War II: Technical and Military Imperatives

Louis Brown
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When World War II is called "the physicists' war," the image evoked is of the mushroom cloud over Hiroshima. But nuclear weapons merely administered a horrific *coup de grace* to an already-defeated enemy. Radar, on the other hand, played a pivotal role in key battles that turned the tide of war in favor of the Allies, and for that, too, physicists can claim a fair share of the credit. Louis Brown, a nuclear physicist at the Department of Terrestrial Magnetism of the Carnegie Institution in Washington, DC, offers in this book a compendious and scholarly history of the development of radar.

The idea of using radio waves for echolocation dates back to the early years of the twentieth century, but it was not until the mid-1930s that all the key elements were in place: transmitters and receivers in the meter-wavelength band, modulators capable of generating microsecond pulses, and high-speed cathode-ray tubes to display the results. Most of these developments were byproducts of civilian work on broadcast television. At that time, laboratories in the US, Britain, Germany, and the USSR had all begun radar experiments on a modest scale. Japan did not take notice until 1941, but then hurried to catch up. As a result, all of the principal belligerents in the war entered it with some capability in radar.

Only Britain, however, had thought through the tactical use of radar in a real battle situation. This was a product of the vision of Air Vice-Marshal Hugh Dowding, who conceived a system based on the "Chain Home" (CH) radar network, linked to filter centers that evaluated the pic-