technologies for both military and commercial communication purposes.

At a transistor symposium in Murray Hill, New Jersey, in 1952, Bell Labs disclosed key materials breakthroughs that had been made in the fabrication of junction transistors. At about the same time, the US Air Force drafted Bell Labs' expertise to develop a network of early-warning radar stations. The cold-war arms race with the Soviet Union had begun, and the fledgling semiconductor industry was destined to be backed by the US government at a pace that was further accelerated by the launch of Sputnik and the subsequent space race with the Soviet Union.

Riordan and Hoddeson offer much insight into the personal workings of great scientists and inventors. Even as major breakthroughs were occurring in the 1950s, they recount, Bardeen, excluded from subsequent work by the increasingly touchy and difficult Shockley, had begun work on superconductivity, ultimately leaving to join Frederick Seitz at the University of Illinois in the summer of 1951. Further, Shockley himself became increasingly disenchanted with Bell Labs when he was passed over and Jim Fisk appointed as director of research. Shockley teamed up with fellow Caltech graduate Arnold Beckman to form Shockley Semiconductor Laboratory, in Palo Alto, California, in 1956. Shockley Semiconductor soon recruited such outstanding scientists as Gordon Moore and Robert Noyce. But even though Shockley thus proved himself again to be a prodigious recruiter of talent, he was unable to manage the creative talent he had brought together with Beckman's backing. A group of eight, led by Moore and Noyce, resigned in September 1957 to form their own company, backed by Fairchild Camera and Instruments.

Silicon Valley owes a significant portion of its genesis to Shockley Semiconductor, and Shockley has been referred to as the "Moses of Silicon Valley" by his longtime friend Seitz. But Shockley himself profited little from his efforts.

Crystal Fire provides a remarkable look into these highlights—and much more—of the story not only of one of the greatest inventions of the 20th century but of the birth of the information age. It is a must-read for every solid-state physicist, device engineer and materials scientist, as well as for those interested in the intimate coupling of fundamental science with application.

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The Truth of Science: Physical Theories and Reality

Roger G. Newton
Harvard U. P., Cambridge, Mass., 1997. 260 pp. $27.00 hc ISBN 0-674-91092-3

Roger G. Newton is a theoretical physicist already well known for his highly technical, foundational work in quantum mechanical scattering theory. In *The Truth of Science*, he gives us an exemplary nontechnical but thoughtful and clear description of science and its relation to truth. It is intended for an educated general reader and requires no familiarity with the mathematical aspects of physics. This book should prove to be of interest to a wide audience, since the question of the truth and objectivity of science has recently been brought to the fore, even among scientists, by the so-called Science Wars.

This latter expression refers to an extensive and ongoing exchange of volleys between the "hard-science" and the "sociology-of-science" camps. These represent opposite ends of a spectrum: Members of the first group take the laws and theories of science to represent an objective and accurate picture of the world, while extremists in the second see the very form and content of science as a purely social construct. Recently, a minor skirmish even took place in the pages of PHYSICS TODAY (July 1996, page 11 and January 1997, page 11). Among the combatants in the larger campaign have been distinguished scientists (some Nobel laureates), historians and philosophers of science, and social constructivists. Unfortunately, the discussants have too often talked past each other, without taking proper cognizance of arguably valid points made by the other side. While neither extreme is wholly defensible, the real problem, it seems to me, is just where one should come down between these extremes. There certainly is an important issue here and the stakes are high: whether (1) science gives us reliable knowledge about the way the world actually is, or (2) simply offers us a plausible story about the way the world might be.

The Truth of Science opens with a preface and an introduction that address this question and, not unexpectedly, come down largely on side (1). Much of the rest of the book is presented as further brief for the scientists. It is a comprehensible, certainly technically correct and generally even-handed account of science—one almost universally subscribed to by the scientific community. Science comes across as an objective enterprise that discovers reliable laws and theories about nature, these converging toward truth. Are matters really so straightforward, though? While there are aspects of scientific practice (for example, the ever-increasing scope, accuracy and predictive power of our successful scientific theories) that support position (1) above, there are also other (external) factors (for example, the influence that social and even psychological elements have had on the structure of scientific theories) that lend credence to (2). This difference in outlook is what is at the heart of the Science Wars. While it is quite reasonable to position oneself between the extremes, there seems to be no objective set of criteria that will both command essentially universal assent and determine uniquely the proper location on the spectrum.

Roger Newton constructs an appealing case for a very positive and optimistic view of science on the basis of an often literate and nuanced examination of the history and content of scientific theories and of associated philosophical questions. His book is a useful addition to the general literature on the nature and goals of the scientific enterprise. He does make some conciliatory gestures toward the influence of external factors on science, but certainly not enough to bring into his fold those partial to a less sanguine view of science. Although I am inclined to give somewhat more weight to external factors than does Newton, I must say that he has made a good case for his view of science—perhaps about as good as one is going to find. One can only hope that continued, civil dialogue of the kind in this book will contribute to an accurate, widely accepted representation of science and of the type of knowledge it gives us, and, in the process, to an accommodation between polarizing views.

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Flash of the Cathode Rays: A History of J. J. Thomson's Electron

Per F. Dahl
IOP, Philadelphia, 1997. 526 pp. $49.50 hc ISBN 0-7503-0453-7

Per F. Dahl's *Flash of the Cathode Rays: A History of J. J. Thomson's Electron* is perhaps the first book-length monograph on the history of the electron to appear since David Anderson's *Discovery of the Electron* in 1964 (Princeton U. P., Van Nostrand). Dahl's book is timely, since 1997 was the centennial of Thomson's discovery of the electron. And although Dahl's account revolves around Thomson, the third Cavendish Professor of Experimental Physics at the University of Cambridge, the book also examines extensively Thomson's predecessors and contemporaries. And it extends its analysis to his followers, including Ernest Rutherford and Robert A. Millikan (it was Millikan who first measured the discrete charge of the electron).

On 30 April 1897, in his now famous Friday evening discourse at the Royal Institution, Thomson presented the result of his measurements of the "mass-to-charge" ratio ($m/e = 1.6 \times 10^{-7}$ g emu^{-1}) of cathode rays, and remarked: "This is very small compared with the value of 10^{-4} for the ratio of the mass of an atom of hydrogen to the charge carried by it. . . . These numbers seem to favor the hypothesis that the carriers of the charges are smaller than the atoms of hydrogen." Since then, this event has been regarded as the "discovery" of the electron. Yet, before Thomson, the same ratio of the cathode ray had been measured by other physicists, such as Emil Wiechert and Walter Kaufmann. Pieter Zeeman, with the help of Hendrik Antoon Lorentz, also calculated the same value for the rotating "ion" of an atom and obtained, before Thomson, almost the same small value. Further, Thomson was very reluctant to use the term "electron," which had been coined by the Irish physicist George Johnstone Stoney in 1891; instead, he stuck to the term "corpuscle," which he himself had chosen to designate the subatomic particles he discovered.

Therefore, the first question that Dahl raises—and answers—is, What constituted the "discovery" of the electron? And in what senses can Thomson be said to be *the* discoverer of the electron? For this, Dahl traces in some detail Thomson's research in 1896 and 1897 and compares it with that of Wiechert, Kaufmann and Zeeman. Unlike Wiechert and Kaufmann, who discarded the possibility of the corpuscularity of cathode rays because of the smallness of the value of m/e, and unlike Zeeman, who paid little attention to the ratio itself, Thomson was the first scientist truly to capture the radical meaning of the small value of m/e of the corpuscle: that the corpuscles are much smaller than hydrogen atoms and that ordinary atoms are built up from corpuscles.

Dahl, however, pays less attention to the Maxwellian context of electromagnetic research during the last quarter of the 19th century—the context in which Thomson's training in electromagnetic theory took place. James Clerk Maxwell and Maxwellians like George FitzGerald and Oliver Lodge had developed a unique conception that viewed electric charge and current as epiphenomena of the electromagnetic field and the energy stored in it. When Thomson suggested the atomic, or materialistic, conception of electric charge in 1897, it immediately came into serious conflict with the Maxwellian conceptions.

Quoting Thomson's recollection, Dahl writes (on page 166) that FitzGerald thought that he, Thomson, "had made out a good case." But, in fact, it was FitzGerald who first published a critique of Thomson's corpuscle hypothesis. To save the Maxwellian dictum, FitzGerald identified Thomson's corpuscle with Joseph Larmor's free electron, which had been proposed as an end point of the ether strain. In FitzGerald's conception, the materiality of Thomson's corpuscle disappeared. This, in my view, is the true reason why Thomson was reluctant to adopt the term electron and its physical implications.

Dahl's book is certainly a welcome contribution to the historiography of the electron. It is, however, more synthetic than analytic. The full history of the electron has yet to be written.

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Planet Quest: The Epic Discovery of Alien Solar Systems

Ken Croswell
Free Press, New York, 1997.
324 pp. $25.00 hc
ISBN 0-684-83252-6

Worlds Unnumbered: The Search for Extrasolar Planets

Donald Goldsmith
University Science Books, Sausalito, Calif., 1997. 225 pp. $28.50 hc ISBN 0-935702-97-0

The Quest for Alien Planets: Exploring Worlds Outside the Solar System

Paul Halpern
Plenum, New York, 1997. 293 pp. $27.95 hc ISBN 0-306-45623-0

History will record the 1990s as the decade that saw the successful culmination of one of 20th-century astronomy's most vigorous quests: the search for extrasolar planets. As of this writing, some 10 giant planets have been discovered around nine Sun-like stars, and (perhaps most surprising) at least four terrestrial-sized planets have been observed around two pulsars, those extremely small, dense and distinctly non-Sun-like stars that mark one of the dead ends of stellar evolution.

Moreover, 1995 also saw the first confirmation of brown dwarfs: objects 13-to-80 times the mass of Jupiter that teeter on the brink of stardom but lack the mass to ignite normal fusion reactions. Brown dwarfs are very much a part of this story, because of the questions they raise about the nature of some of the new objects. There is no doubt of the brown-dwarf nature of Gliese 229 B, an object of 40-to-55 Jupiter masses, discovered by Shrinivas Kulkarni and his colleagues. And there is little doubt that the seven Jupiter-mass objects in nearly circular orbits around their respective suns, forever linked with the names Michel Mayor, Didier Queloz, Geoffrey Marcy and Paul Butler, are planets, even though some circle extremely close to their parent stars.

But it is just possible that the companion of HD 114762, discovered in 1988 by David Latham and his colleagues, with a minimum mass 12 times that of Jupiter, was the first extrasolar planet to be discovered. And it is possible that Marcy and Butler's "planet" 70 Virginis B is a brown dwarf rather than a planet. Clearly, much remains to be learned about the relationships of these objects, not to mention the mechanism of their formation. Such is to be expected at the beginning of a new field.

The discovery of these objects and the controversy over their nature make a dramatic story, and one well told in all three of these books: *Planet Quest* by Ken Croswell, *Worlds Unnumbered* by Donald Goldsmith and *The Quest for Alien Planets* by Paul Halpern. The Croswell and Goldsmith books, each written by a well-known Berkeley astronomer–author with a Harvard background, are intended for the intelligent and dedicated non-professional, while Halpern, an associate professor of physics at the Philadelphia College of Pharmacy and Science, has written one that is slightly less comprehensive; for example, it includes none of the tables summarizing the new planets and their properties that I found so useful in the Croswell and Goldsmith books, nor, in general, is it as detailed as the other two books.

All three cover much the same ground, but in quite different ways. Croswell's approach is chronological,