our profession," Feynman commented that while he was "excited by all the congratulations telegrams," Laurie's stood out and "meant an especially great deal" to him. "It was like another little prize" (page 158).

To Julius Ashkin, whose friendship with Feynman dated back to Los Alamos, and who had very carefully gone over and corrected Feynman's 1949 quantum electrodynamics papers and thus, according to Feynman, had made them "worthy" of consideration by the Nobel Prize committee, he wrote, "I owe to you not only the great pleasure of having had your personal acquaintance but also ultimately, no doubt, the fact that I received the Nobel prize" (page 160).

Of all the letters presented in the book, one group stands out by virtue of the care and consideration devoted to each of them. Those are the letters Feynman wrote to young students who asked him for advice on career decisions and prospects and those written to the youngsters' parents. The answer was always a variation on "Work hard to find something that fascinates you. . . . When you find it you will know your lifework" (page 229).

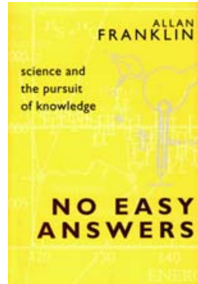
Among the most moving of these letters is the one Feynman wrote to Koichi Mano, a former student, who was despondent because he was tackling "humble" problems. Feynman issued a slight reproof: "The worthwhile problems are the ones you can really solve or help solve, the ones you can really contribute something to." And he listed some of the many problems, which Mano would call humble, that he had worked on, sometimes successfully, sometimes unsuccessfully: the coefficient of friction for highly polished surfaces, how to make electroplated metal stick to plastic objects, the design of a neutron counter, the theory of turbulence, and so forth (page 199).

It is difficult to convey the exhilaration and the increased admiration engendered by reading the letters of this remarkable human being. I urge that as wide an audience as possible read them for an uplifting experience.

No Easy Answers: Science and the Pursuit of Knowledge

Allan Franklin
U. of Pittsburgh Press, Pittsburgh, PA, 2005. $29.95 (258 pp.). ISBN 0-8229-4250-X

In the past 30 years the so-called constructivist sociology of science has produced a large number of studies on consensus building in science. Many of the analyses look at physics as a test case to show that controversies are more frequent than what philosophers and scientists often suggest. Building consensus in science involves negotiating what constitutes a fact in a given scientific community. Constructivist and relativist sociologists insist that the contingent aspects of the negotiations are important. By contrast, rationalist philosophers focus on the role of empirical tests and logical coherence as objective grounds for believing in the physical reality of phenomena and the explanatory value of theories.



The relativist conception of science, which minimizes the role of reason in science, has been criticized by many historians and philosophers of science. Allan Franklin is certainly among the most active critics of relativism. As a professor of physics at the University of Colorado in Boulder, he has published many important books on the role experiments and instrumentation have played in science. Titles such as *The Neglect of Experiment* (Cambridge U. Press, 1986), *Experiment, Right or Wrong* (Cambridge U. Press, 1990), and *Are There Really Neutrinos? An Evidential History* (Perseus Books, 2000) are technical contributions to the epistemology and history of modern physics and are written for professional scientists and historians and philosophers of science.

In *No Easy Answers: Science and the Pursuit of Knowledge*, Franklin wishes to present "an accurate picture of science," as he states in the preface, to both general readers and their colleagues who are in the humanities and social sciences and have no physics background. For their benefit, Franklin revisits, in a less technical manner, many of the case studies analyzed in his previous books. The picture he proposes is presented in contrast to the relativist's view largely diffused in such books as Harry Collins's *Changing Order: Replication and Induction in Scientific Practice* (Sage, 1985) and Andrew Pickering's *Constructing Quarks: A Sociological History of Particle Physics* (U. of Chicago Press, 1984). Franklin discusses Collins's book in chapter 13, which deals with the early search for gravitational waves; he takes up Pickering's book in chapter 14, which focuses on the history of the experiments on atomic parity violation.

Personally, I prefer those two chapters over the others because Franklin explicitly confronts previous interpretations of the events with his own. Four other chapters are devoted to the history of the neutrino; but curiously, the author does not contrast his narrative with the standard constructivist neutrino history provided by Trevor Pinch in *Confronting Nature: The Sociology of Solar-Neutrino Detection* (D. Reidel, 1986). Franklin's chosen cases are descriptive, with few extended analytical or philosophical discussions. Readers not familiar with the literature will learn through Franklin's book a great deal about the history of the electron, the neutrino, the magnetic monopole, and much else. Franklin also uses the well-known case of Robert Millikan's biased selection of data in his calculation of the electron's charge to raise the ethical question of the selection of data points in the analysis of experiments. He rightly distinguishes between "wrong" and "bad" physics: The former is part of normal science whereas the latter goes against the implicit, moral norms of the scientific community.

Franklin wisely selects his case studies to illuminate the influence that experiments have had in science. In addition to their obvious use in testing theories, experiments suggest new theories by uncovering new phenomena or by providing evidence for the existence of new entities like the electron or the neutrino. Also, experiments can have a life of their own independent of theory and can be devised simply to measure some constants of nature.

Franklin concludes that scientists had good reasons, "based on valid empirical evidence and reasoned and critical discussion," to assess, accept, or reject results as they did (page 227). For him, the cognitive aspects of science dominate any contingent social, or even psychological, factors. An important point rarely stressed in relativists' analyses is the fact that "there is very little instant rationality in science" (page 229): It took about eight years for scientists to clarify the validity of Enrico Fermi's theory of beta decay, and 30 years elapsed between the first reports of the solar neutrino anomaly and the solution of the discrepancy between theory and observation by confirming the existence of neutrino oscillations.

No Easy Answers is probably too technical for the general reader. Yet physicists will find in it a useful epitome of Franklin's past contributions and will be reassured of the legitimacy of their rationalist conception of science.

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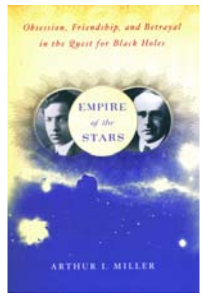
Empire of the Stars: Obsession, Friendship, and Betrayal in the Quest for Black Holes

Arthur I. Miller
Houghton Mifflin, New York, 2005. $26.00 (364 pp.). ISBN 0-618-34151-X

The quest to understand stellar evolution—how stars are born, live, and die—is a fascinating subject. Arthur I. Miller's *Empire of the Stars: Obsession, Friendship, and Betrayal in the Quest for Black Holes* traces the history of this intellectual adventure, intertwining it with the stories of the pioneering astrophysicists and theoretical physicists who have contributed to its success. Miller also examines the connections among the astrophysical processes, the making of the hydrogen bomb, and the role of general relativity in firmly establishing the existence of black holes in the astronomical universe.

The book, which is absorbing and scientifically well researched, is intended for general audiences but goes beyond the rational scrutiny of the history of science. It offers tidbits of personal lives of some key players and ventures into highly speculative sociocultural and psychological explorations that I find disturbing and unwarranted. For example, Miller writes that Arthur Stanley Eddington's only intimate, other than his sister, was Charles Trimble, a mathematics tutor. He speculates that the intimacy could have been more than just friendship: It could have been homosexual, and because homosexuality was illegal, it made Eddington "lead a life of concealment, a life on the edge with fragile psychological balance."

Aside from many such explorations, the real focus of Miller's book is Subrahmanyan Chandrasekhar (popularly known as Chandra), Eddington, and the controversy between the two concerning the theory of white dwarfs. On his long voyage from India to England in 1930, Chandra discovered, as a consequence of combining special relativity and quantum mechanics, the celebrated upper limit on the mass of a star that could become a white dwarf. For stars more massive than the Chandrasekhar limit of 1.44 solar masses, the electron degeneracy pressure is not adequate enough to withstand gravity. The star continues to collapse. During a meeting of the Royal Astronomical Society in January 1935, Chandra presented a rigorous proof of that startling result based on the exact relativistic equation of state for degenerate matter. In the dramatic sequel, Eddington, speaking after Chandra, ridiculed the whole idea of relativistic degeneracy. He claimed the underlying idea was wrong and had no validity.



Eddington's rebuttal came as a total surprise to Chandra. Eddington had known about Chandra's work and had followed it carefully in the months preceding the meeting but had given no warning that he disagreed. Being publicly refuted by Eddington, a towering figure with great international prestige, was traumatic to young Chandra.

I first learned about the eventful encounter in an article Chandra wrote in the *American Journal of Physics*, volume 37, page 577, 1969. The article led me to get to know Chandra well, and write *Chandra: A Biography of S. Chandrasekhar* (U. of Chicago Press, 1991), based on more than a decade of extensive conversations with him. Miller's account is totally different from Chandra's. For instance, the author gives an overblown, inaccurate description of Chandra's discovery. He describes it as "a great discovery concerning nothing less than the ultimate fate of humanity," and "like Einstein, [Chandra] had lifted a corner of a great veil, revealing a majestic yet terrifying picture of the fate of stars and of humanity." And further, according to Miller, Chandra's discovery had provided mathematical verification of black holes.

Chandra's theory of white dwarfs was certainly one of the most important discoveries of the 20th century, but it was not the theory of black holes. It did not incorporate general relativity—the basis of our current understanding of black holes—which was done by J. Robert Oppenheimer and Harland Snyder in the late 1930s.

Miller also writes that Chandra's story is "of one man's fight with the scientific establishment for recognition of