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Physics in Perspective

Book Reviews

Harry Nussbaumer and Lydia Bieri, *Discovering the Expanding Universe*. Cambridge: Cambridge University Press, 2009, 226 pages. $59.00 (cloth).

Harry Nussbaumer, Professor Emeritus at the Swiss Federal Institute of Technology, and Lydia Bieri, an assistant professor in the mathematics department at Harvard, have written an engaging history of our unfolding understanding of one of the most important discoveries of 20th-century astronomy—the expanding universe. Beginning with an overview of some seeds of modern cosmology planted in late medieval texts, they use primary documents to focus on the work of major contributors to the idea.

Their main goal is to correct a major popular misconception, which dates the beginning of the discovery of the expanding universe to Edwin Hubble's landmark 1929 paper. As is well known to many workers in the field, Hubble's six-page publication showed not only that distant galaxies are receding from us but also that their speed of recession increases with distance in what Fritz Zwicky soon called a "roughly linear" fashion. Nussbaumer and Bieri point out that despite the fact that Hubble's 1929 "results were regarded as highly significant," his article "was not seen as a stunning revelation," (p. 118) since such a relationship had been expected, and Hubble was not the only observer on the lookout for it (a statement that probably deserved a footnoted reference but did not get one). What Hubble's observationally based calculations did do was to "set landmarks that could not be disregarded, and served as guidelines in subsequent theoretical cosmological discussions" (p. 119). Furthermore, Nussbaumer and Bieri report that "nowhere in those six pages does Hubble propose, nor even mention an expanding universe," and that in addition to never having "claimed to have discovered the expanding universe," Hubble himself "probably never believed in such a scenario." (p. 119) Fully two dozen years after the publication of the famous paper, and only a few months before his death, Hubble presented a lecture in which he stressed that an expanding universe was only one among several possible interpretations of the meaning of the linear velocity–distance relationship, "and the right one was not yet known." (p. 120) Nussbaumer and Bieri suggest that "public relations, through the media," later "elevated [Hubble] to the status of discoverer of the expanding universe," in doing so "rewriting history." (p. 120)

In his interesting Foreword, Allan Sandage, who worked as Hubble's assistant from 1949 until 1953 before going on to his own career as a distinguished observational cosmologist, confirms that "although the discovery of the expansion is often attributed to Hubble with his 1929 paper, he never believed in its reality" (p. xv) and "had often argued against" it. (p. xiv) Sandage also focuses the reader on "the central theme of this important book," namely, a "proper accounting of the complete history, and in particular of the crucial role of [Georges] Lemaître in setting out the basis of the theory as we now know it." (p. xii)

If a book like this can be said to have a hero, Lemaître is it. This Belgian mathematician and Catholic priest, having taken "a fresh look at [Albert] Einstein's fundamental equations and incorporating the observational progress accomplished in those years," dropped Einstein's assumption that the universe was static, enabling him to become the first, in June 1927, to unequivocally suggest an expanding universe. (p. 99) Nor was this Lemaître's only coup; there were two others. In 1931, Lemaître became the first to suggest what we now call the Big Bang, and to suggest that the concepts of space and time have no meaning before that initial explosive event.

In Lemaître's own words, "If the world has begun with a single quantum, the notion of space and time would altogether fail to have any meaning at the beginning; they would only begin to have a sensible meaning when the original quantum had been divided into a sufficient number of quanta. If this suggestion is correct, the beginning of the world happened a little before the beginning of space and time." (p. 166) According to Nussbaumer and Bieri, Lemaître's third discovery was of what today we call dark energy: "For Lemaître, the cosmic evolution after the decay of the primeval atom is due to an imbalance between two opposing cosmic forces: gravitation and dark energy. Dark energy is the modern term…Lemaître equated it with vacuum energy." (p. 168) Still, it was a total surprise when the universe, based on observations in the 1990s, turned out to be accelerating, no doubt because of such "dark energy."

In a charming imaginary scene in the final two pages, Nussbaumer and Bieri imagine a contemporary social gathering of some of the major figures whose contributions their book has summarized: "If we had Einstein, [Willem] de Sitter, [Arthur] Eddington, [Alexander] Friedmann, Lemaître and Hubble gathered around a table for a glass of port, how would they comment on present day cosmology?" (p. 186) Our authors make clear that Lemaître is the winner of the trifecta, "with his expanding universe of 1927, the Big Bang, and his early association of the cosmological constant with vacuum energy." (p. 187)

From my point of view as a biographer, what makes the historical survey in *Discovering the Expanding Universe* so much richer than, say, an equivalent overview in an encyclopedia is the authors' use of primary documents to provide insight into the personalities of the scientists associated with the various insights and stalemates. Allan Sandage sets the tone for this humanizing of science in some charming personal revelations in the Foreword. In one, Sandage tells us at his own expense that when he admitted to Lemaître at their second meeting that he could not himself "envisage curved space and the beauties of Riemannian geometry, so necessary for relativity," as Lemaître—32 years Sandage's senior—called them, the priest advised Sandage, "gently, like a father to a son," that "Perhaps it might be best for you to change fields." (p. xvii)

Perhaps Lemaître could have been kinder to Sandage in the encounter that, happily, did *not* convince the younger scientist to switch professions. But in shedding light on their hero's personality, Nussbaumer and Bieri tacitly draw a distinction between Lemaître's gentlemanly and generous insistence on sharing credit even where it was not necessarily due to Hubble's miserly resistance to doing so where it clearly was. While Lemaître "rediscovered the dynamical universe completely independently of Friedmann, he always acknowledged that Friedmann had been the first to find dynamical solutions." (p. 111) By contrast, Hubble, "Even in his influential *The Realm of the Nebulae*, published in 1936,…avoided any reference to Lemaître. Was he afraid that a gem might fall from his crown if people became aware of Lemaître's pioneering fusion of observation and theory 2 years before Hubble delivered the confirmation?" (p. 132) To their own credit, despite their obvious favoring of Lemaître over Hubble, Nussbaumer and Bieri are careful not to dismiss the importance of the latter: "Just as there is no justification to glorify Hubble's publication as the 'discovery of the expanding universe,' and not even as the original discovery of the linear velocity-distance relationship, it would be unfair and wrong to belittle its importance. The publication was a significant milestone for cosmology." (p. 133)

Nonetheless, I found it amusing to learn that while Einstein "certainly did not underestimate the importance of Hubble's observations," which "provided unequivocal evidence for a non-static universe," (p. 146) Nussbaumer and Bieri's analysis of Einstein's diary entries, letters, and even publications reveals that the great man persisted in mispelling Hubble's name, which he repeatedly rendered as "Hubbel."

Among the few things I do not admire about *Discovering the Expanding Universe* one is the rather insalubrious analogy Nussbaumer and Bieri use to describe the expansion itself. The authors tell us that Lemaître, in explaining his expanding universe model, "employed the same pictures we use today: the nebulae remain in the same configuration in space, but space itself increases its size in the course of time. Thus, the distance between the two nebulae covers the same fraction of space, but grows together with space. Therefore, any two nebulae draw away from each other." (p. 110) Instead of comparing the receding nebulae to raisins in a cake rising in the oven, the appetizing analogy with which I am most familiar, the authors compare them to "microbes on a bubble. When the bubble increases, each microbe realises that the others withdraw, and it has the impression—but only the impression—of being at the centre." (p. 110)

Another quibble I have with the authors is that they obviously have a sense of humor and are able to write more than workmanlike prose but are very chary in demonstrating those capabilities. They withhold their sense of humor until some fifty pages before the book ends, when they express their relief that Hubble "did not challenge de Sitter to a duel with rapier, pistol or even heavier weaponry" (p. 131) for failing to pay specific enough homage in a 1930 publication to the contributions of Hubble and Mount Wilson to the velocity–distance relation. Similarly, a rare instance of interesting writing comes only a few chapters from the end, when Nussbaumer and Bieri open a chapter called "Are the Sun and Earth older than the Universe," with a metaphor particularly apt for our current Great Recession: "At an early stage it was realised that the model of an expanding universe was laden with a heavy mortgage." (p. 153)

More important, however, than these slight reservations is that the authors do not make sufficiently clear that even a correct theoretical prediction is very different from an observational result. Nonetheless, I have no wish to challenge Allan Sandage's highly favorable endorsement of *Discovering the Expanding Universe* as "meticulously researched," (p. xi) "remarkable," (p. xvii) and "authoritative and definitive" (p. xvii)—nor, of course, am I in any position to do so.

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Patricia Fara, *Science*: *A Four Thousand Year History.* Oxford: Oxford University Press, 2009, xv + 408 pages. $ 34.95 (cloth).

If you are seeking an account of the history of science that is incomplete, biased, often misleading, occasionally incorrect, and both arrogant and snide, then this is the book for you. If, however, you are looking for an accurate, thoughtful, and sensible history of science, then you must seek elsewhere.

I realize that in attempting to discuss 4,000 years of science in 400 pages an author must make difficult decisions concerning what to include and what to omit, but some of the choices Fara makes seem to me bizarre. There is, for example, more space devoted to Franz Mesmer's theory of animal magnetism than to the work of Max Planck or Gregor Mendel. Surely quantum mechanics and genetics are more important in the history of science than animal magnetism. I also recognize that in writing such a book one must omit many of the actual details of science, but the lack of detail is often appalling. There is not a single equation in the entire book. After reading this book the reader will not know anything about Newton's laws of motion or his law of universal gravitation. Nor will the reader realize that there was a major change in the discussion of motion from the Aristotelian view that a force is required for motion to occur at all to Newton's law of inertia that tells us that an object moves with constant velocity or remains at rest unless acted on by a net external force. (In fact, the reader will not know what Aristotle's view of motion was; that detail is omitted.) We also never learn anything about Mendel's laws of inheritance, those of segregation and independent assortment, which are the foundation of modern genetics. The book also attempts to, and sometimes, although not often, succeeds in discussing the impact of science on society, and *vice versa*. Yet Faraday's discovery of electromagnetic induction, surely a part of scientific knowledge that has had an enormous impact on society, is nowhere mentioned. After all, it is the basis of almost all of our electrical energy production. Fara does not seem to regard this as important.

Despite the book's self-congratulatory attitude on including the contribution of other cultures to science, the author's accounts don't amount to very much. There is, in fact, no mention at all of anything in the New World. There is no discussion of the elaborate and complex work on the calendar by the Maya (which has even penetrated popular culture with the movie "2012" about the predicted end of the world), or of the architectural achievements of the Incas and the Aztecs. Fara includes brief discussions of Babylonian and Chinese astronomy, but the detail is lacking. She tells us that the Babylonians used astronomy in determining omens, but we never learn that they constructed a complex and beautiful theory of planetary motions by counting the number of days between heliacal risings and settings of the planets.* In the discussion of Chinese astronomy we learn of Shen Gua, who, in the eleventh century, initiated massive data collection on planetary positions, and used it not for scientific purposes but more as "an administrative astrologer who reformed the calendar in order to make better predictions about imperial rituals." (p. 52) Once again, no details are given. We have no idea how the measurements were made. The treatment of Islamic science in the Middle Ages is somewhat better and the author notes the debt of European science to those Islamic scholars who preserved and extended Greek science. Aside from these discussions there is little mention of science outside of Europe and the United States.

There are also some inexplicable errors for someone who has a degree in physics from Oxford University. In her discussion of Carl Anderson's famous photograph that demonstrated the existence of the positron, Fara concludes that "two particles have been created simultaneously, a negative electron curved downwards by a magnetic field, and its mirror image, a minute positive particle steered in the opposite direction." (p. 281) That was definitely not Anderson's conclusion, which was that a single positively-charged particle travelled upwards and passed through a lead plate. This was supported because the radius of curvature was smaller above the plate indicating that the particle travelled upward and lost energy passing through the plate. Fara also attributes the origin of special relativity to the need for precision in clock synchronization for measuring distances. She remarks of the newly invented devices for doing so that "many of their designs landed on the desk of a philosophical physicist who was originally more interested in thermodynamics than time—Patent Officer Albert Einstein." (p. 248) She does not seem to recognize the distinction between clock synchronization, which involves stationary clocks, and simultaneity, which involves moving clocks. The latter was considerably more important in the development of special relativity.

The author also repeats several of the social-constructivist myths about the history of science as if they were absolute truths. "This assertion that scientists fiddle their readings may seem shocking, but there are some striking examples, including the British astronomer Arthur Eddington's claim to have confirmed Albert Einstein's general relativity theory, and the American physicist Robert Millikan's measurement of the charge of an electron." (p. 88) Fara is either unaware of recent scholarship on these subjects, or just doesn't recognize its relevance. As noted, Fara often accuses scientists of ignoring evidence, while not recognizing that she herself does the same thing. This is part, as discussed below, of her persistent efforts to denigrate scientists and their achievements. As Daniel Kennefick has shown,** the exclusion of data from the 1919 eclipse expedition was done not by Eddington, but by Dyson, and the exclusion was quite justified on experimental and methodological grounds. (To be fair, Fara bases her account on the work of Earman and Glymour who overlooked this point). In the case of Millikan she further notes that "Millikan discarded around two-thirds of his readings." (p. 273) This is correct, but she fails to note

* Noel Swerdlow, *The Babylonian Theory of the Planets* (Princeton: Princeton University Press, 1998).

** Daniel Kennefick, "Testing relativity from the 1919 eclipse—a question of bias," *Physics Today* **62** (March 2009), 37–42.

that much of Millikan's exclusion of data was quite justified and that the result of his "cosmetic surgery" (he also engaged in selective calculations), did not change his final value of *e* by more than a few parts in 5,000. Fara does, however, note that Millikan did obtain a value for *e* very close to the one accepted today, but she later goes on to compare Millikan to the discoverers of N-rays saying that they were no worse than "(Millikan, who sacrificed neutrality by jettisoning inconvenient readings)." (pp. 273–274) The reader who might be interested in further pursuing these claims will find no help in this book. As discussed below the references in this book are quite inadequate.

As is typically the case in a book intended for a general audience, the citations and references are rather sparse. Fara does give us sources for direct quotes, but, as noted above, for many of her controversial statements she provides no references. At the end of the book she does give us the sources she has used for various sections of her book, but these are rarely of any use in tracking down specific claims. In the case of Millikan, my own paper, "Millikan's Published and Unpublished Data on Oil Drops,"* a likely source, either directly or indirectly, is not mentioned nor is any other reference. Given Fara's faulty interpretation I am happy to be omitted.

In Fara's history, evidence and reasoned and critical discussion play little, if any, role in the production of scientific knowledge. Consider her discussion of N-rays, a presumed discovery by Blondlot and others in the early 20th century. "They were eventually proved wrong by a *devious* American visitor [Robert W. Wood] who *sneakily* removed the vital prism while his hosts proudly continued making measurements. Easy to laugh—but initially N-rays seemed no more implausible than radioactivity. Easy to criticize—but were the Nancy enthusiasts acting so much more unethically then Becquerel, who abandoned his schedule and developed some plates early." (p. 273, emphasis added) To anyone who knows anything of the history of these two episodes the comparison is ludicrous.

In Fara's history, scientists do not succeed because they have made important contributions to science but because they are adept at currying favor with the rich and powerful or very good at self-promotion. In attempting to oppose the "Great Men Pursuing Ultimate Truth" caricature of science, which she says others espouse, she constantly denigrates those who have made real contributions to science. Thus, we have Aristotle's "self-centered arrogance"; (p. 43) Galileo as a "far more effective publicist than Kepler" (p. 115) who used a "conjuror's panache" (p. 116) to persuade people of his views; and Copernicus was "an undistinguished church administrator." (p. 110) (Even if true, one may ask what relevance this has to an evaluation of Copernicus's contributions to astronomy.) "Lavoisier became an icon of revolutionary chemistry not because he was indubitably right, but because he persuaded influential people that he was"; (p. 172) Humboldt was "a skilled self-promoter"; (p. 208) Cuvier was "an expert string puller"; (p. 232) and "Like many scientific heroes, Einstein was an expert self publicist." (p. 249) The list goes on. Even those scientists who agree with her political views, which are quite apparent throughout the book, are not immune. Fara correctly points out, time after time, that women have been excluded from science. In discussing Sigmund Freud, however, she remarks that "this forceful authoritarian welcomed female disciples, perhaps anticipating (mistakenly) that they would be more subservient than his rebellious male colleagues." (p. 301) No evidence for such speculation is given. In Fara's world, not even a good deed remains unpunished.

Given the incompleteness of the book, the lack of detail, the misinterpretations and bias in presenting the science, the occasional errors, and its consistently snide and superior attitude towards science and scientists, I do not recommend this book. Under ordinary circumstances, after reading a few chapters I would have told the editors that I did not wish to review this book.

* Allan Franklin, "Millikan's Published and Unpublished Data on Oil Drops," *Historical Studies in the Physical Sciences* **11** (1981), 185–201.

I persevered because I wanted to save my colleagues from wasting their money on buying this book or wasting their time in reading it. Oxford University Press should be ashamed.

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Lawrence Badash, *A Nuclear Winter's Tale: Science and Politics in the 1980s*. Cambridge, Mass: The MIT Press, 2009, xiii + 403 pages. $40.00 (cloth).

On Halloween 1983, planetary physicist Carl Sagan presented to a press conference in Washington, D.C., the preliminary results of a computer-based study on the environmental effects of nuclear war. Sagan informed the 600 scientists and journalists in attendance that he and a team of researchers associated with NASA's Ames Research Center had discovered that a nuclear war could possibly trigger what team member Richard Turco called a "nuclear winter." Sagan and Turco, together with their collaborators—planetary scientists Thomas Ackerman, James Pollack, and Owen Brian Toon (often referred to together as TTAPS)—published a technical report on their findings in the December 23 issue of *Science.* Environmentalist Paul Ehrlich and colleagues followed the TTAPS paper in the same issue with a report on the long-term biological effects of nuclear war.

The horrific predictions were worthy of the Halloween date. It was already known that nuclear fireballs rising into the stratosphere from a nuclear war would contain high concentrations of nitrogen oxides capable of depleting most of the ozone layer. They would also carry millions of tons of soil dust into the air, darkening the skies. Added to these disasters, TTAPS now predicted that urban fires ignited by nuclear bombardment would send hundreds of millions of metric tons of sooty black smoke high into the atmosphere, at least to the top of the troposphere. Available data indicated that sooty smoke is even more absorbent of visible sunlight than dust. Together, the dust and smoke would shut down the greenhouse effect that warms earth. According to a computer model used by TTAPS, for a "baseline" nuclear war releasing 5,000 equivalent TNT megatons of energy the average ground-level sunlight would drop to just a few percent of normal, land temperatures would drop to −15 to −25°C within three weeks and remain there for several months. Even a war releasing as little as 100 megatons of energy in many small bursts would reduce land temperatures by as much as 5–10°C, comparable to the approximate 8°C decline during the last ice age. Ehrlich and co-authors (including Sagan) predicted that photosynthesis would cease during the cold, dark nuclear winter, causing a collapse of the world's food supply. Even after the atmosphere cleared, all life on the planet would be wracked by devastating ultraviolet radiation, high radioactivity, low oxygen levels, toxic chemicals, contaminated water, diseases, and much else. Many species would become extinct, including, quite possibly, *Homo sapiens*. While an estimated half a billion people would die in a nuclear war itself, billions more, including those in non-belligerent nations, would die in the aftermath. Nuclear war was very likely nuclear suicide.

The sudden discovery that a nuclear winter was possible, and with such devastating consequences, set off a firestorm of scientific controversy, public debate, political posturing, and intensive research that peaked during the next three years, then declined to near negligible interest by the end of the decade. Utilizing available contemporary documents and drawing upon extensive interviews and correspondence with the participants, historian of science Lawrence Badash provides the first full-scale history of the discovery of nuclear winter and of the scientific and political debates that ensued. It is a well-researched, evenly balanced, and fully detailed yet generally accessible account of a very significant episode in the history of contemporary science. What makes it so significant is the remarkable confluence of many diverse elements within and across science and politics in the emergence of the nuclear-winter phenomenon. Badash's tale of nuclear winter provides one of the best examples of the contemporary tendency toward multi-disciplinary and often multi-national research leading to the discovery of large-scale phenomena that are otherwise beyond the scope or perspective of any one discipline. Like the similar discovery of global warming and the human contribution to it, nuclear winter raised significant political, social, even economic implications for its time. In the United States it was a time marked by the Reagan administration's views that a nuclear war might be "winnable" or at least "survivable" and that the nation might even be able to defend itself against attack. Just six months earlier the administration had announced the Strategic Defense Initiative ("Star Wars"), which, along with the positioning of middle-range nuclear missiles in Europe, significantly increased Cold-War tensions. Paralleling these decisions were the rise of the environmental and nuclear-freeze movements, the civilian nuclear catastrophes at Three Mile Island (1979) and Chernobyl (1986), and increased public attention to popularized science and technology through NASA media events and televised science series offered by Sagan and others. Badash sees in the tale of nuclear winter "an exemplar of the twentieth-century interaction between science and society." (p. xi)

Among the many disciplines that converged in the discovery of nuclear winter (NW), and within the TTAPS group itself, were atmospheric studies of Martian dust storms undertaken by Sagan and colleagues; studies of the particulate microphysics of dust from volcanic eruptions, notably Mount St. Helens in 1980, and from the asteroid hypothesis proposed at that time for the extinction of the dinosaurs; preliminary work on the nature and effects of large-scale forest fires; a National Academy of Sciences study in 1975 on ozone depletion caused by a nuclear war; and the emergence of the sophisticated computer modeling of complex phenomena in concert with the development of bigger and faster supercomputers. By the early 1980s the TTAPS group was experienced in all of these areas, and, writes Badash, under Sagan's direction it was oriented toward the quantitative study of planet-scale atmospheric effects, now including those occurring on earth.

Badash also points out the remarkable circumstances that all of the strands woven into the tapestry of nuclear winter emerged at about the same time and entirely from federally funded national research laboratories rather than from university or industrial venues. Moreover, even though some of the work was done in nuclear weapons laboratories, no weapons scientists participated in the research leading directly to the concept of nuclear winter. Badash suggests that these help to account for why it took so long to recognize the possibility of NW and why it finally occurred in the early 1980s. Each of the constituent disciplines had concentrated on its own internal concerns and traditions, while weapons scientists focused on the immediate blast and fallout effects rather than on the longer term destructive consequences. Only national laboratories possessed the advanced supercomputers required for the necessary modeling. In addition, national laboratory researchers, such as the TTAPS group centered at Ames, had freer cross-disciplinary interactions, and once NW became a topic of interest they could shift funds more rapidly to it from other projects than could academic researchers. In fact, redirected funds within national laboratories were the main source of support for such research. Yet federal authorities proved the least receptive to the results emerging from their labs. The Reagan administration was so dismissive of the potential policy implications that a Defense Department grant of $25 million over three years (compared with $3.5 billion for SDI research) was the only direct funding for NW research.

At that time most of the world's nuclear weapons were aimed at urban and industrial centers or at military targets close to such centers. The TTAPS report was the first to identify sooty smoke from urban fires as the trigger for a possible nuclear winter. Although atmospheric chemists Paul Crutzen and John Birks had estimated the climate effects of smoke from nuclear-war forest fires in a widely read paper published a year earlier, the TTAPS group was the first to make quantitative estimates of the size and duration of the urban fires produced in ten different nuclear-war scenarios, the amount of smoke produced, the total mass of smoke and dust particles carried into the atmosphere, and the density of the clouds and their height and lifetimes in the atmosphere. Owing to their previous Martian, asteroid, and volcano work, the TTAPS group was also unique in that it possessed the only computer model of the perturbing effects of aerosol clouds injected into a planet's atmosphere. Although it was only a one-dimensional model, for the vertical axis, it was capable of determining the effects on light and temperature at Earth's surface. After making many assumptions and roughly estimating values of the many physical parameters, the TTAPS team fed their model into a Cray supercomputer at the Ames Research Center, and out came the prospect of nuclear winter.

Thanks to Sagan's media skills and his vigorous promotion of nuclear winter as a direct challenge to the nation's nuclear policies, the public, politicians, and other scientists immediately took notice. This naturally provoked a variety of responses. Yet, Badash argues, the possibility of nuclear winter did not seem to change any attitudes or commitments among leading figures regarding current nuclear policies (unfortunately, no data were available on the opinions of the general public). The Reagan Administration argued that the possibility of NW only reinforced its primary policy of deterrence, making the horrors of nuclear war even worse. And it argued that a defensive system such as SDI was now even more necessary in order to reduce the possibility of NW by destroying the incoming warheads on the U.S. side. But, according to TTAPS calculations, even the one percent of warheads predicted to escape the SDI net would be enough to set off a nuclear winter. Badash is sympathetic to the administration's position. The administration's "successful action in constraining NW research and sidelining discussion of its political consequences seems to have been motivated by honest doubts about the phenomenon and preference for its own policy choices." (p. 309) Most administrations do initially defend their policy choices against scientific objections by emphasizing doubts about the offending science. But was a point ever reached where the potential consequences outweighed the diminishing doubts, thereby requiring reconsideration of policy preferences?

Sagan and the anti-nuclear movement took the opposite lesson from the prospect of nuclear winter: the world's nuclear arsenal must be vastly reduced as soon as possible—according to Sagan by at least 90% or below the 100 megaton threshold for NW. He argued that no leader of a nation under nuclear attack could be expected to act "rationally" in avoidance of NW. In addition, the policy of targeting cities had to be revised, and the proliferation of nuclear weapons must be halted. Even a limited regional war could bring down nuclear winter across the globe. In support of mutual disarmament Sagan became "a one-man whirlwind of nuclear winter promotion," (p. 254) appearing at press conferences, engaging in public appearances with Edward Teller, the father of SDI, frequently testifying before Congress, welcoming Soviet nuclear-winter researchers to the United States, and returning the visits on several occasions. More than any other person, Sagan shaped the nuclear-winter debate.

However, to critics and skeptics Sagan's publicity activities were evidence that, for him, science had taken a back seat to politics and self-promotion. Most scientists, including the other members of the TTAPS team, avoided discussing the political implications of their work. Even some liberal supporters argued that the TTAPS study incorporated so many uncertainties that Sagan and colleagues should have waited until they had more precise data and more definitive conclusions about the severity of the effect. Badash also sympathizes with Sagan's position. In fact, he argues, Sagan *et al.* had a moral obligation to bring their shocking preliminary results immediately to the world's attention. They did not hide the preliminary nature of their work. Because the effect has such a low threshold, they argued, even the large uncertainties in the many variables of their model still resulted in the possibility of nuclear winter. Badash also points to the long tradition of civilian scientists offering the government unsolicited policy advice. Even individual scientists sometimes promoted counter evidence and arguments to controversial policy choices. In 1986, American Physical Society President Robert R. Wilson denounced the SDI effort as the product of bad policy advice. But here, too, one might inquire about limits.

Three years after the Halloween announcement, a flurry of research and a spate of official investigations finally came to fruition. Among the latter were reports published by the Department of Defense, the National Academy of Sciences at the request of the Defense Nuclear Agency, and the United Nation's Scientific Committee on Problems of the Environment (SCOPE). All, including the DOD report, concluded the *possibility* of nuclear winter, but all were careful to note that uncertainties precluded precise predictions. Now seizing upon the prospect of nuclear winter, the administration viewed the DOD report, which deliberately neglected the biological effects of NW, as an invitation to increase funding for SDI. After numerous hearings, Congress, too, came to the conclusion that NW might possibly occur, but it too ignored the implications, even in preparing the civilian population for the potential catastrophe. Only the SCOPE report included the devastating biological effects of nuclear winter and the implications for the conduct of nuclear war on all sides.

The Chernobyl disaster of 1986 suddenly made nuclear winter "yesterday's story." As research continued on large-scale urban fires and now three-dimensional computer simulations of large-scale atmospheric perturbations (including a Russian study), by the late 1980s the possibility of NW was widely accepted, largely on the basis of the SCOPE and Academy reports, but neither report was regarded as authoritative on the precise consequences.

Politically sidelined and financially starved by the Reagan Administration, concern for the prospect of nuclear winter gradually faded by the end of the decade. After an initial flurry of interest, nuclear winter had long since disappeared from radio and television. Only print-media science reporters continued occasional follow-up reports. After the end of the Cold War and the dissolution of the Soviet Union, nuclear winter essentially disappeared into history, an artifact of a bygone era. Even as more recent studies suggest that a regional nuclear war may have less dire consequences for the climate than initially predicted, the lessons to be learned from *A Nuclear Winter's Tale* regarding interdisciplinary science yielding potentially catastrophic global results with profound political, social, and economic implications are as valuable today in the context of other similar events as they were on that frightful Halloween of 1983.

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Laura Dassow Walls, *The Passage to Cosmos: Alexander von Humboldt and the Shaping of America*. Chicago: The University of Chicago Press, 2009, xv + 404 pages. $35.00 (cloth).

A hundred years ago I would not have had to explain the title of this book; until the beginning of the twentieth century Baron Alexander von Humboldt (1769–1859) was an international celebrity. His role in shaping the intellectual landscape of the world, and especially that of America—including our science and our literature—was universally acknowledged. A native Berliner (like myself), and a world traveler, he had a special affection for the fledgling United States to the point of calling himself "half an American." His crowning work *Kosmos*, grandly subtitled in its third English edition *A Sketch of the Physical Description of the Universe*, had been translated into a dozen languages and had made him, according to one modern source, the most successful author of his generation. A hint of Humboldt's popularity in the United States, which at times approached "cult status," (p. 215) is the observation that the website http:\\www.placenames.com currently lists 205 entries for Humboldt, including in Nevada alone a river, a lake, a mountain range, and a county.

Today, Humboldt the man is largely forgotten in this country, for reasons that include the vagaries of scientific fashion as well as the violent anti-German sentiments stirred up by two World Wars. Some academics know of the German *Humboldt Foundation* with its scholarly grants, and a few readers remember the hilarious satirical portraits of Humboldt and the mathematician Carl Friedrich Gauss in Daniel Kehlmann's novel *Measuring the World*, which *Time* magazine included among the ten best books of 2006, but the general public is unaware of Humboldt's achievements. In her preface Walls announces her goal to recall him out of oblivion, and in the epilogue pleads again: "It is our obligation to let him come back…." Her book makes an eloquent, richly documented, and thoroughly persuasive case for that project.

Humboldt was trained as a geologist and quickly developed into a polymath (as well as one of the most loquacious scientists of all time). He had a lot to say to physicists. His view of nature was firmly grounded in physics and astronomy. In his travels, which he never undertook without hauling along an astonishing battery of astronomical and physical instruments, he compulsively gathered the reams of data that he would later augment with torrents of measurements supplied by a global network of friends and colleagues. Along with equally copious collections of biological and geological specimens, as well as cultural artifacts, he used this treasure trove of information as the raw material for his descriptions of the world. In *Kosmos*, he devoted fully three quarters of the first volume to a high-level popularization of astronomy, geology, meteorology, and physical geography. More important than his own modest contributions to physics was his reputation as the premier ambassador of science to the prominent people of his time. Walls demonstrates in detail how Humboldt and his books influenced, among countless others throughout the world, American opinion makers such as Jefferson, Emerson, Thoreau, Whitman, Poe, Muir, Morse, the anthropologist Franz Boas and the painter Frederic Church. In the history of scientific popularization Humboldt is a giant.

In the broader context of the history of science itself, Humboldt represents the transition from the Enlightenment to the Modern period, as implied by a remark attributed to the 19th-century physiologist Emil Du Bois-Reymond: "Every scientist is a descendent of Humboldt. We are all his family." (p. ix) So powerful was his global influence that historians of science use the phrase "Humboldtian Science" for the style of research he championed. From the Newtonian age he inherited a passion for measurement and a respect for mathematical analysis. To these he added three ingredients that uniquely characterize his approach: a commitment to large-scale international cooperation, a relentless effort to construct a seamless worldview from all the separate branches of science and the humanities, and an insistence that human beings must always remain at the center of the scientific enterprise, as its creators, its users, and its objects of study.

In order to animate her effort to revive Humboldt's legacy, Walls makes effective use of metaphors. Rivers, for example, which always fascinated Humboldt, symbolize the *confluence* of disparate strands of knowledge into a unified whole, while trees represent the opposite process of *ramification*—synthesis and analysis, respectively. Walls's most potent metaphor extends from the dust jacket to the penultimate paragraph: the image of the bridge. Humboldt was indeed a master builder of bridges on a great variety of levels—between the Old World and the New, between North and South America, between colonizers and natives, "science and sentiment" or objective and subjective knowledge, between physics and poetry, the head and the heart, holism and reductionism, between macrocosm and microcosm, to pick out just a few. Everything, according to Humboldt, is connected, and he aims to show us how. Accordingly, Walls's prologue is entitled *Humboldt's Bridge* and comments in detail on the frontispiece, a drawing based on a Humboldt sketch of two adjacent natural bridges spanning a crevasse in the Andes. With a keen eye and an artful pen Walls uses the image to announce the theme of her book. In the epilogue, a black and white illustration depicts another kind of bridge in an even more spectacular context. Fortunately it is also reproduced in full color on the dust jacket. It is an 1866 painting by Frederic Church of an immense double rainbow across an imaginary valley in the Andes, and can be seen, according to art historian Isabel Breskin, as "Church's paean to Humboldt's ideas." (p. 370) After you have read the book you cannot but agree with this interpretation. (I wonder whether Church noticed the visual pun concealed in his own canvas: the strange dark ribbon of sky between the two rainbows is known as *Alexander's Band*, though not after Humboldt.)

While rivers, trees, and bridges aptly symbolize aspects of Humboldt's work, his name is most famously associated with a phenomenon that combines, improbable as it sounds, the properties of rivers and bridges. This is the fabled *Casiquiare Canal* in Venezuela (a.k.a. *Casiquiare crossing, exchange, river,* or *passage*, as in the book's title). Many scholars vehemently denied its existence before Humboldt personally explored it and vouched for its reality. Indeed, it defies common sense. As a boy in Germany, where Humboldt's name was never eclipsed as thoroughly as it was in this country, I marveled at descriptions of this freak of nature. Decades later I was dismayed to find that a good friend, a prominent American geologist, refused to believe my account of it. The Casiquiare Canal is a river that achieves the impossible: it connects two catchment basins, that of the Orinoco, which flows North into the Caribbean, and that of the Amazon, which flows East into the Atlantic. It is as though there was a river in the Rockies that somehow manages to cross the Continental Divide. This review is not the place to explain how this counterintuitive trick works, but the Casiquiare does exist, and real people populate it. Better than any other symbol it captures the fascinating infectiously quixotic spirit of Humboldt.

What can we, as modern physicists, learn from that Romantic hero Alexander von Humboldt? For one thing, he was among the pioneers of large-scale international scientific institutions and research programs. After reorganizing the German Association of Natural Scientists and Physicians in 1828, which served as a model for the British Association for the Advancement of Science and later the American Association for the Advancement of Science, Humboldt "launched the world's first international scientific collaboration" among Britain, America, and Russia to observe global geophysics and climate. (p. 110) Thus, he paved the way for the modern study of climate change. Closer to our narrow self-interest as physicists, if we had been more mindful of Humboldt's transnationalism in 1993, the Superconducting Supercollider in Texas might have escaped its inglorious demise.

With respect to Humboldt's second great achievement, the establishment of comprehensive, integrative branches of science, he has been credited with being the godfather of the science of *ecology*, which received its name just seven years after his death, at the height of the Age of Humboldt. (p. 327) Similarly, the "founder of American *anthropology*" Franz Boas was an avowed Humboldtian. *Evolution* benefited from Humboldt's influence on Charles Darwin, who wrote in 1832: "I formerly admired Humboldt, I now almost adore him." The field of *physical geography* was invented by Humboldt, which explains the remark that "geographers are the specialists most likely to know Humboldt's name and place in intellectual history." (p. 37) (His most famous contribution was his effective use of isolines, such as the contour lines found on hiking maps and the isotherms we see on weather maps. Thus, he was a pioneer of the science of Graphic Information Systems.) To a lesser extent archeology, astronomy, ethnography, vulcanology, oceanography and even linguistics benefited from Humboldt's researches. In every case, he supplied the breadth of vision of the true generalist that is required for shaping a congeries of facts and theories into a unified branch of knowledge. In physics, although we are amply supplied with narrow specialists, we would do well to seek out and value the rare voices of generalists who have the experience and wisdom to survey our field in its daunting diversity.

Thirdly, in addition to broadening our horizons beyond national boundaries and the confines of academic disciplines, Humboldt reminds us to use the power of our science wisely. He was, first and last, a profoundly ethical humanist. The issue that most clearly demonstrates this side of his thinking is the relation between the scientific study of "race" and the human tragedy of slavery. Walls writes: "Humboldt was the only major scientist during the nineteenth century to argue consistently, for six decades, that 'race' was not a biological category and that…'all are alike designed for freedom'." She quotes Stephen Jay Gould who urged that Humboldt should be recognized as "the hero of modern racial egalitarians." (p. 174) While scholars were busy arguing about the minutiae of racial distinctions, Humboldt celebrated the personal qualities, the achievements, and the histories of blacks, North and South American Indians, and other native peoples throughout the world. Science, he thought, must never be practiced for its own sake, but must always respect the worth and dignity of every human being.

Humboldt's rejection of the concept of race gave theoretical backing to his relentless opposition to slavery. For all his love of America, he considered slavery to be a horrid stain on its map. "Slavery is no doubt the greatest of all the evils that afflict humanity," he wrote. (p. 204) Walls concludes: "To Humboldt, none of his science counted so much as his moral argument against slavery." (p. 205)

Albert Einstein, whose style was in many ways the antithesis of Humboldt's, nevertheless would have agreed with his humanistic morality. In February of 1931, he addressed an audience of students at Caltech on the subject of the purpose of science: "If you want your life's work to be useful to mankind, it is not enough that you understand applied science as such. Concern for man himself must always constitute the chief objective of all technological effort…in such a manner as to assure that the results of our scientific thinking may be a blessing to mankind, and not a curse." Today atomic weapons and atomic power have been joined by global telecommunications, biomedical devices, supercomputers, and satellite technology, for example, as the fruits of pure physics. They can all bring blessings as well as curses. An infusion of the Humboldtian spirit, which aims to blend science, ethics, and aesthetics into a unified, humane worldview, might serve the physics community well in this age of specialization and belligerence.

Perhaps I am expecting too much. But at least I can recommend that every physicist whose concerns extend beyond the latest technical breakthrough read Laura Dassow Walls's fine and important new book. A century and a half after his death the garrulous old Baron still has a thing or two to say to us, and Walls is his worthy apostle.

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Sean Johnson, *History of Science*: *A Beginner's Guide.* Oxford: Oxford University Press, 2009, viii + 218 pages. $14.95 (paper).

In this intelligent and well-written volume, Johnson gives a five-chapter summary of the history of scientific developments from ancient times through the 20th century, then explores in two concluding chapters the "scholarly themes that have shaped the history of science." (p. 12)

An underlying purpose of the volume is to argue that history of science can be used to bridge the perspectives of the humanities and the sciences. Johnson himself is a perfect messenger for this cause; his first career was in physics and system engineering and he now teaches history of science and technology at the University of Glasgow. Because he truly understands both history and physics, both historians and physicists (and people with ties to both fields) will be pleased with his apt summaries. It was a joy to see someone get the science right; he does a good job, for example, in explaining the development of relativity accurately. At the same time, he handles historical context well; relativity is placed alongside other revolutionary advances of the early 20th century, including evolutionary biology and the rise of new electrical industries such as telephone, telegraphy, radio, and lighting.

My favorite part of the book is the last chapter, which manages, with admirable economy of words, to helpfully elucidate various recent scholarly trends. Particularly interesting is the description of the "shift in the history of science towards studies of language and discourse,"

(p. 187) which explains structuralism and post-structuralism. I also enjoyed the section on "the social turn," which includes a concise yet savvy explanation of social constructionism.

Care has been taken to make the book accessible. It has short chapters with headings to guide the reader to topics of interest. In addition, it features bordered boxes set into the text that provide the definitions of key terms (for example, instrumentalism and reductionism). This makes it easier for the nonspecialist to follow explanations and to look up terms later. The book also includes a good selection of books for further reading and a helpful index. Because of these features the book can be used as a reference text, for browsing, or read cover to cover.

As with any summary, one can quibble about what is left out. I would have preferred to read more, for example, about the development of quantum mechanics and the development of large-scale science with its expensive tools and related political issues.

Nonetheless, I would recommend this book for anyone who wants a thought-provoking, easy-to-read, yet in-depth introduction to the history of science and the scholarship that surrounds it. The book would also be useful for an introductory course in the history of science, either for undergraduates or for graduate students in related fields.

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Joao Magueijo, *A Brilliant Darkness: The Extraordinary Life and Mysterious Disappearance of Ettore Majorana, the Troubled Genius of the Nuclear Age.* New York: Basic Books, 2009, xxi + 280 pages. $27.50 (cloth).

Ettore Majorana was a brilliant Italian theoretical physicist with a remarkable set of accomplishments achieved in a very short time span. Unfortunately he was also a deeply troubled young man who in the summer of 1933, at age 27, withdrew from the world. He lived for the next four years as a virtual recluse in his Rome house, seeing almost no one, estranging himself from his family, and living on a very restricted diet.

Majorana resurfaced toward the end of 1937 and, to everyone's surprise, entered a competition for a chair of theoretical physics that had been launched by the Italian government. He was declared the winner and awarded a professorship in Naples. He moved to Naples in January 1938, but disappeared on March 26, 1938. On the previous day he wrote his senior colleague, Antonio Carrelli, what appears to be a suicide note and then boarded the overnight ferry from Naples to Palermo. However, the day afterward he wrote Carrelli a second note from Palermo:

> Dear Carrelli,
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> I hope that my telegram and the letter arrived simultaneously. The sea has rejected me and I shall return to the Hotel Bologna [in Naples] tomorrow, perhaps traveling together with these lines. However, I want to give up teaching. Do not think of me as a girl in an Ibsen play, because the case is different. I am at your disposal for further details.
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> Affectionately, E. Majorana

The presumption was that Majorana had boarded the ferry from Palermo back to Naples on the evening of the 26th and during the voyage jumped from its deck to his death. An alarmed Carrelli instituted a search for Majorana in Naples and Palermo. In Rome, Fermi attempted to mobilize government officials to organize a further investigation.

However, Majorana's body was never found and a considerable literature has been spawned around his disappearance, heightened because Majorana took his passport with him and withdrew a considerable amount of money from his savings before vanishing. Speculations range from Majorana entering a monastery to his having boarded a boat for Argentina instead of the Sicilybound ferry and subsequently having lived there for many years, perhaps working as an engineer. The most fantastic and at the same time the best known of such hypothesized endings is in a book by the well-known Italian novelist Leonardo Sciascia. He imagined Majorana as having foreseen the discovery of nuclear fission and the building of nuclear weapons and then withdrawing from the world rather than play a role in such developments.

Magueijo's book, the latest addition to this literature, though amusing in many points, is offensive in others and makes little attempt to present a balanced view of Majorana. This is unfortunate, for Majorana's unquestioned sufferings should be viewed with sympathy, rather than highlighting once again the notion of the misunderstood genius who is surrounded by a cadre of inferior beings.

But first to Majorana's real accomplishments. He clearly was a very gifted mathematician, recognized by his contemporaries as having both superior intuition and extraordinary abilities in calculation. There are a number of contemporary stories that display the facility Majorana possessed in such matters. He also seems to have had a remarkably good understanding of the meaning of experiments. The two best known of Majorana's small number of publications are his 1932 paper describing the spectrum of the infinite tower of particles belonging to a unitary representation of the Lorentz group and his 1937 paper in which he displayed a real representation of Dirac's gamma matrices and derived the equation for what we now call a Majorana neutrino, one whose mass term is not invariant under charge conjugation. The latter, in particular, is a very important part of contemporary physics.

The claim Magueijo makes that Majorana predicted the neutron is more questionable than his very real and documented accomplishments. It is indeed true that he realized almost at once that the results of Joliot and Curie's famous 1932 experiment should be interpreted as due to a neutron and not to an extremely energetic gamma ray, but Rutherford and Chadwick saw this as well. Furthermore, in a three-week period during which he never slept more than a few hours a day, Chadwick performed the experiment that proved the neutron's existence. He received the 1935 Nobel Prize for this and is justly recognized for the discovery of the neutron. It is possible that Majorana had been thinking about the neutron's existence earlier, but Rutherford had as well, at least since the time Majorana was in high school.

Credit should be given to Majorana for subsequently developing a theory of nuclear forces that involved the neutron. This work has been ascribed to Heisenberg and to Iwanenko who independently arrived at the same results at the same time as Majorana did. Fermi had heard from Majorana about his results and had wanted to report on them at a Paris Congress, naturally giving Majorana the credit. Majorana agreed that Fermi could do so, but only on the condition that it be attributed to an elderly professor of electronics who would also be present at the Congress. Fermi refused because, as Magueijo says, the elderly professor was *widely regarded as a moron* and Fermi *didn't have the guts for the prank.*

This short anecdote illustrates some of the problems I have with this book. Either because Magueijo actually believes what he is writing or wants to create a better story—the truth may well be a bit of both–he consistently builds up Majorana as a genius, on the level of Newton, Einstein, and Galileo. Conversely, Magueijo labels most of the other physicists Majorana was dealing with in Rome as jealous of his superiority, stupid, and puerile. Majorana's all too frequent bizarre behaviors are treated as the exploits of a superior, totally altruistic mind rather than accepting the much more obvious explanation of these being the workings of an enormously talented but disturbed young man who was having difficulties in his day-to-day dealings with others.

In the interests of full disclosure, I should admit that I am the nephew of Emilio Segrè, Majorana's university classmate and the man that probably originally introduced Majorana to Fermi. My uncle was admittedly often a difficult person to get along with, but his autobiography, while critical of many, has nothing but good things to say about Majorana. Having Emilio portrayed in Magueijo's book as winning a professorship in Palermo for *having tempered an adequate number of tuning forks* or describing his behavior after Los Alamos as *bragging moronically about* *his participation in the Manhattan Project* seems hardly accurate. Magueijo also suggests that the purpose of the anti-Semitic letter that Majorana wrote Segrè from Germany in mid 1933 was to break with the Fermi group. Surely there were easier ways to do this than by writing such a letter at a time when Einstein, Born, Franck, and so many others were being dismissed from their positions for being Jewish. But I can definitely reassure Magueijo on one point; his assertion that Segrè lost his whole family in the Holocaust is untrue.

Though Magueijo's statements about Segrè may bother me personally, I find the ones about Fermi much more disturbing since they are at the core of the comparison to other physicists that Magueijo attempts to make and of course because Fermi was a far greater physicist than my uncle. Fermi is described as *riddled by an inferiority complex* and elsewhere as *lacking imagination*. In another section we read how the *adult Fermi could be truly oleaginous*. *His wife Laura wrote an unctuous biography*, *which accidentally reveals what an overbearing person he was. He shouted orders at everyone around him like a Fascist expecting obedience.* Magueijo also accuses Fermi and Amaldi of publishing in 1933 work that Majorana had done earlier without even acknowledging him. This is an unrecognizable portrait of Fermi. In fact, we might not even be crediting Majorana for his insights on neutrinos had Fermi not generously taken it upon himself to write up that piece of research and publish it, under Majorana's name, a gesture Magueijo fails to mention.

Nor are questionable descriptions limited to Italians. Heisenberg was certainly not known for a *hedonistic life style* nor would Bohr or Born say, as Magueijo does, that *Heisenberg invariably woke up late and worked only in the afternoon*. Bohr's well known aphorism that the opposite of a great truth may also be a great truth is attributed to Dirac, a statement one can scarcely imagine the ever precise Dirac making, but these are admittedly quibbles.

There are also some mistakes that one finds hard to believe were written by a physicist. Rutherford, who spent most of his career as an experimentalist studying the scattering of alpha particles, including discovering the atomic nucleus by these means, would surely be surprised to read that alpha radiation can *easily be stopped by a sheet of paper* while gamma radiation is *by far the most powerful form of radiation.*

To his credit, Magueijo writes well, in an unusually lively prose and has done a good deal of research into Majorana's life, including interviewing some of the by now very elderly people who knew him. The story moves along very briskly, often having some of the features of a good mystery novel. I can easily imagine a reader unencumbered by my reservations finding this an interesting and often quite thrilling read, superior to the more prosaic scientific biographies. But the pity is that Magueijo could have retained the same excitement without attempting to reshape the course of events as much as he has.

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