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Physics in Perspective

# Book Reviews

Graham Farmelo. *Churchill's Bomb*: *How the United States Overtook Britain in the First Nuclear Arms Race*. New York: Basic Books, 2013, 554 pages. $29.99

Compared with histories of the Manhattan Project, the literature devoted to British wartime and postwar work on nuclear energy, both weapons and reactors, is less extensive yet nearly equally definitive. Margaret Gowing's official three-volume history of the British nuclear program, *Britain and Atomic Energy*, has not been surpassed in scholarly depth or in breadth of subject matter and resources since the last volume appeared forty years ago.[1]

British writer Graham Farmelo, recently the author of a well-known biography of Paul Dirac,[2] has now entered the realm of British atomic energy with his latest work, *Churchill's Bomb*. Even more than does his Dirac biography, Farmelo's new book rests upon extensive archival research, including some resources not available to Gowing, but his purpose is quite different from Gowing's. As a science writer for general audiences, his aim is to bring a fascinating slice of British atomic history—that surrounding Prime Minister Winston Churchill—to as wide an audience as possible and in a style that intrigues as much as it informs. In order to accomplish this, however, his historical insights do not extend beyond those found in Gowing's volumes or in Richard Rhodes's *The Making of the Atomic Bomb*,[3] and he chose to leave out much of the science and technology. In addition, the archival resources serve mainly to provide inside information about such novelistic details as arrangements at meetings, personal impressions of the main characters, and even the PM's poodle. Because of this, the chance is missed to utilize the archives for significant new historical insights after forty years, and to utilize this story to educate the general public more fully on nuclear fission and its uses.

Nevertheless, what Farmelo does do he does well. Not only is the book a good read, approaching novelistic quality, but it also recounts an interesting story about Churchill, Britain, the United States, and nuclear energy for general audiences who are not likely to encounter Gowing or Rhodes. The content of the book, however, is *not* well-captured by its overly dramatic subtitle, "how the United States overtook Britain in the first nuclear arms race." There was no nuclear arms race between Britain and the United States, nor did the United States overtake Britain; rather, Britain fell behind for its own internal reasons. What happened to Britain and why constitute the content of this book.

To those familiar with the early nuclear age, Farmelo's story is well known, except for his new details, and it does emphasize two ironic twists. The first derives

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from the circumstance that, during the early years of the war, both Britain and Germany were ahead of the United States in practical fission, and Britain in radar as well. Yet by 1942 both had fallen way behind in fission as the US surged ahead, propelled ironically by a British memo prepared in March 1940 by two German-Jewish refugees, Otto Frisch and Rudolf Peierls, a memo that British leaders essentially chose to ignore. The memo provided an outline of the atomic bomb and a calculation showing that only a small critical mass of Uranium-235 was required. A bomb was feasible, and within reach during this war. That memo went to the secret scientific MAUD Committee in Britain, which excluded Frisch and Peierls as "enemy aliens," but it agreed with the aliens in its final report of August 1941. It urged immediate work in Britain on the bomb lest the Germans (or the Nazis, as Farmelo prefers) should build the bomb first and drop it on London. While Churchill and his aides debated the recommendation of the MAUD Report, their representatives shared the report with their US colleagues, along with a cavity magnetron, an essential piece of hardware that spurred US radar research. Thanks to tireless lobbying by Vannevar Bush and James B. Conant, the MAUD Report likewise fueled a US program to build the bomb that soon culminated in the Manhattan Project. In October 1941, President Franklin D. Roosevelt invited Churchill to the US to discuss joint US-UK cooperation on a project to build the bomb. Churchill did not respond for seven weeks and then never mentioned the project in a meeting with FDR in December, the same month as the Pearl Harbor attack. With Churchill seemingly uninterested, FDR ordered a crash program in the US to build the bomb alone. By mid-1942 Bush and the Americans had shut out the British from any collaboration or exchange of information. "By that time," writes Farmelo, "the United States had entered the war and was gearing up to begin its gargantuan Manhattan Project, which it pursued with a self-interest so ruthless that it left Churchill floundering." (7)

Not until 1943 did Churchill fully realize the strategic significance of British participation in bomb development, not just for this war but for British postwar influence on American policy. Only after the Québec agreement between FDR and Churchill in August 1943 did a small, but valuable, contingent of British scientists finally join the Manhattan Project. One of their greatest contributions was Peierls's theoretical solution of plutonium implosion. Although Farmelo characterizes General Leslie Groves as continuing to restrict information from the British, Peierls writes in his memoir that, at least in his dealings with a New York supply company, "Gen. Groves had given instructions that we were to be given all the information we needed."[4] Yet without any policy influence on the Americans, Churchill saw the work of the British scientists mainly as preparation for the possible postwar construction of a British nuclear weapon that would help bolster British prestige and diplomatic influence.

That possibility became a reality in 1946 when the US passed the "brutally self-interested" McMahon Act, (8) which made it illegal for US scientists to share nuclear secrets with any other nation. As British scientists left Los Alamos, Britain established its own nuclear program. After Britain detonated its first fission bomb in 1952 and its first fusion bomb in 1957, the United States suddenly welcomed it back as a close partner and ally in nuclear matters. Churchill's postwar strategy had paid off.

But what happened during the war? Why did Britain squander its lead and fall so far behind the Americans? The answer leads to the second irony in Farmelo's story. Apparently no other major politician or public figure of that time displayed such an early interest in and knowledge of atomic physics as did Winston Churchill. In the aftermath of gas warfare during the Great War, Churchill foresaw as early as 1925 the possibility of scientific weapons of mass destruction that could well bring about the demise of humanity. In 1931, he lamented that political leaders were not up to the challenge of new weapons from science; in 1935, before the discovery of fission, Churchill and H. G. Wells privately debated the grim prospect of nuclear weapons in future wars.

As that prospect approached reality during the early years of the war, Churchill was already well aware of the dangers; he was also one of the ablest politicians of the time, and, as the Battle of Britain raged overhead, his steadfast optimism and determination helped save his nation during its "darkest hour." Farmelo points out the irony that "even he—aware at the beginning of the conflict that the nuclear age was in prospect—struggled to deal effectively with the coming of the Bomb… his handling of the technology when it arrived was not one of his great achievements as a politician." (457) In addition to noting the distractions of a life-or-death air war raging over Britain, Farmelo, like others, puts the blame on Churchill's advisors and in part on Churchill's own limited conception of good scientific advice. From 1921 on, Churchill relied for scientific advice almost exclusively upon just one man, the physicist Frederick Lindemann, a descendent of German immigrants whose expertise and, most importantly, judgment were widely questioned in scientific circles—but not at 10 Downing Street. Churchill placed undue weight on the opinions of the "clever but supercilious Lindemann," who was joined later during the war by the "unimaginative" Sir John Anderson. (207) In addition, Farmelo points out, Churchill failed to understand that the authority of science arises not from individuals, however brilliant or impressive they may be, but from the scientific community as a whole.

Churchill's confidant Lindemann proceeded to exclude from leading advisory positions those able physicists against whom he harbored personal grudges, and, despite the MAUD Report, he continued to overestimate the difficulty of making an atomic bomb and to underestimate the ability of the Americans to do so. The results led to the crucial turning points that left Britain at a standstill even as the Americans rushed ahead. These included Churchill's failure to respond to FDR's invitation in 1941 to collaborate as equal partners on bomb development; his failure to recognize the significance of the bomb until 1943; and his failure, like most other politicians at the time, to prepare for the dangerous postwar political landscape that the atomic bomb would bring. The latter was poignantly displayed by Niels Bohr's failed mission in 1944 to persuade Churchill to help head off a postwar nuclear arms race by informing Soviet leader Joseph Stalin of the new weapon and by sharing Allied nuclear technology with him. (Stalin, as it turned out, already knew about the bomb.) The failure of Bohr's mission also reflected Churchill's overriding concern about Britain's diminished world power as Stalin prepared to dominate much of postwar Europe.

The lesson of this story for today's general audiences is a valuable one: Scientific advice to politicians, and politicians' understanding of what constitutes good scientific advice, can be significant contributors to the success or failure of a nation's scientific and technological development. And in this case, to quote Farmelo: "It was ultimately the narrowness of [Lindemann's] advice on these matters that led Churchill to make such an uncharacteristically flat-footed response to the most powerful explosive his scientists devised." (146)

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Dan Ch. Christensen, *Hans Christian Ørsted*: *Reading Nature's Mind*. Oxford: Oxford University Press, 2013, 768 pages. $69.96

Although famous for his epoch-making discovery of electromagnetism in 1820, the Danish physicist, chemist, and natural philosopher H. C. Ørsted is relatively little known in international history of science. Indeed, scholarly studies on Ørsted are limited and, in most cases, focused on the great discovery with which he name is firmly associated. Only recently has there been increased interest in his scientific achievements and philosophical ideas, with the result that they have been more carefully studied and disseminated to the international community of historians and philosophers of science. For example, in 1998 his *Selected Scientific Works* were published by Princeton University Press, in 2007 an anthology on *Hans Christian Ørsted and the Romantic Legacy in Science* appeared from Springer, and in 2010 *The Travel Letters of H. C. Ørsted* were published by The Royal Danish Academy of Sciences and Letters (of which Ørsted served as a powerful secretary between 1815 and 1851). These and other works on Ørsted are dwarfed by Dan Christensen's magisterial and impressively detailed biography, the first complete and thoroughly contextual work on the Danish polymath and natural philosopher. The book is an abridged and revised version of an even longer two-volume work that Christensen published in Danish in 2009 (*Naturens Tankelæser: En Biografi om Hans Christian Ørsted*).

Christensen's biography deals with all conceivable aspects of Ørsted's life and far from only with his scientific work. This was very important to Ørsted, but he was also greatly interested in, for example, literature, philosophy, and art. Christensen pays full attention to these areas and to Ørsted's wide-ranging impact on cultural life in Denmark in general. Several of the chapters in the book deal with subjects that are explored here for the first time. Examples include Ørsted's role as a freemason, his religious views and disagreements with clerical authorities, and his deep interest in musical aesthetics. He was truly a man of two cultures. Of course Christensen also paints a picture of Ørsted as a human being, realizing that it may be mistaken for hagiography. But, he asserts, "it is hard to find examples of unpleasantness or accusations of selfishness being levelled at Ørsted, and I have searched meticulously for them in the archives" (11). Christensen briefly, and sensibly, compares Ørsted with Niels Bohr, noting that they both "had something boyish about them, some trait of straightforward innocence and plain modesty" (14).

In recent historiography, Ørsted has been depicted as both a Kantian and an adherent of Friedrich Schelling's *Naturphilosophie* (the natural philosophy associated with the Romantic movement). Christensen deals critically and quite sharply with scholars advocating the latter view, in particular Kenneth Caneva, Andrew Wilson, and Michael Friedman. They are wrong, he concludes, for Ørsted was not at all a friend of Schelling's idealistic philosophy and only marginally influenced by it. Ørsted explicitly rejected Schelling's claim that *Naturphilosophie* ranked higher than science and speculation higher than experiment. But if Ørsted's life-long project of reforming physics and chemistry on a dynamical basis owed little to Schelling, it owed much to Kant's dualistic philosophy. The latter, Christensen asserts, was scientifically fruitful, whereas natural philosophy à la Schelling never led to any scientific discovery. Although stressing the role of Kantian ideas in Ørsted's work, Christensen also points out that the discovery of electromagnetism was original to Ørsted and not simply a product of his adherence to Kant's philosophy. Christensen further dismisses the myth, still to be found in some physics textbooks, that Ørsted stumbled over the discovery by chance: during a lecture he happened to notice the deflection of a compass needle in the presence of a wire connected to a battery. Voila! The myth goes back a long time and was propagated as early as in the 1820s by leading physicists such as Ludwig

Gilbert and Christoph H. Pfaff. But it is no less wrong for that. Far from being a lucky accident, the discovery was the result of his long-held belief in the unity of the forces of nature. As Louis Pasteur later said, referring specifically to Ørsted's discovery, chance favors the prepared mind.

Although the discovery of electromagnetism came to be of enormous technological and economic importance, Ørsted himself showed no interest in these potential applications. The same was the case a few years later, when he made another remarkable discovery by isolating the metal aluminum that Humphry Davy unsuccessfully had tried to extract from clay by means of electrolysis. Uninterested in the use of the silver-gray metal, he handed over the recipe to Friedrich Wöhler. Of course, our modern world relies crucially on aluminum and electrical technologies. While Christensen is understandably impressed by Ørsted's achievements in science and culture, it seems to me that he is also a bit uncritical when it comes to his weaknesses. At least in two areas his view of the physical sciences turned out to be counterproductive, to be blind alleys. As a convinced Kantian dynamicist, Ørsted never accepted John Dalton's seminal idea of indivisible and ponderable atoms, which he considered to be an expression of pure materialism. Moreover, lack of mathematical training made him foreign to the increasing mathematical orientation of science, maintaining that experiment was the central or even the only basis of knowledge in physics. His pervasive influence on Danish science had the unfortunate effect that Newtonian mechanics and other areas of theoretical physics only became accepted many years after his death.

Christensen's recommendable biography of Ørsted is a major contribution to the cultural history of science in the first half of the nineteenth century. Although much of it focuses on Denmark, it also contains descriptions of Ørsted's many travels abroad through which we get to know contemporary scientists such as Jöns Berzelius in Sweden, Christopher Hansteen in Norway, John Herschel in England, Johann Ritter and Thomas Seebeck in Germany, and Joseph Fourier in France. My only complaint is the lack of a subject index, which makes it almost impossible to look up specific topics and events. This is more than just an insignificant flaw in a book of nearly 750 pages.

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Bruce Cameron Reed, *The History and Science of the Manhattan Project*. Berlin: Springer-Verlag, 2014, 472 pages. $59.99

By now, the historiography of the Manhattan Project is so huge that it is difficult to discover anything about it that hasn't already been explored, poked, and analyzed to within a gnat's eyelash. And yet, here comes a new volume, *The History and Science of the Manhattan Project,* which manages to earn a new place in the project's canon. The book is presented as a text for an advanced undergraduate course although the author, Bruce Cameron Reed, does state that it could "also be accessible to non-students and non-specialists who wish to learn about the Project." Reed, a physics professor at Alma College, a small college in Michigan, has been teaching such a course there for a number of years. He has clearly mastered the voluminous material available as primary and secondary sources in the open literature, much of it declassified from various secrecy levels.

Problems and answers are included, so that the book really does have the feel of being a textbook. Yet the writing style, and the broad scope of the material, makes it as readable as a novel. And in fact the project itself is so vast and ambitious that it really does read like science fiction. It's easy to run out of superlatives in describing the history of the Manhattan Project, from the initial phase, which was pure physics and chemistry, to the early efforts to understand and control nuclear fission, then to the development of the actual Manhattan Project with all of its aspects, then the denouement—Hiroshima and Nagasaki—and finally to its aftermath, bringing us up to date on its heritage.

The history and its science are presented chronologically, with some exceptions to enable a more logical presentation where necessary. The book contains nine chapters, which I list here: Introduction and overview; a short history of nuclear physics in the mid-1930s; the discovery and interpretation of nuclear fission; organizing the Manhattan Project, 1939–1943; Oak Ridge, CP-1 and Clinton Engineering Works; the Hanford Engineering Works; Los Alamos, *Trinity*, and Tinian; Hiroshima and Nagasaki; and the legacy of Manhattan. You can see that Reed covers every base, just about. (Some of the minor, though important, aspects are not included). In particular, the three principal sites of the Manhattan Project are covered in rich detail: Los Alamos, Oak Ridge, and Hanford.

Los Alamos, a community built essentially from scratch, apart from some residential quarters, was the scientific center of the project. Here the basic physics and chemistry for the underlying science, the design, and the ultimate construction of the bombs was carried out. It has been the site most thoroughly examined over the years, for a variety of reasons. It was, after all, the place where many of the world's leading physicists had assembled, and where the bombs were actually designed and constructed. It has acquired you might say, a romantic aura. Oak Ridge, the sprawling establishment, also built from scratch in the hills of Tennessee, was the center mainly for the production of fission-quality uranium during the war and has become since the war a thriving government-sponsored research center. It has also attracted considerable scrutiny by historians, although not as much as has Los Alamos. The Hanford Engineering Works, built from scratch as well on the banks of the Columbia River in the state of Washington, has not received as much attention, perhaps because its purpose was you might say a strictly engineering one—to construct a plant for the manufacture of plutonium, using nuclear reactors for the transmutation of ordinary uranium—on the face of it, not a particularly romantic enterprise. But in the book the latter two sites have not received short shrift, and their histories, their countless problems and challenges, and their ultimate successes, are described in as much detail as is Los Alamos. In fact, as Reed, with perhaps a bit of hindsight, reveals that the engineering challenges at Oak Ridge and Hanford were even more daunting that those of Los Alamos, where the underlying physics of nuclear fission was already known. The separation of $U^{235}$ (Oak Ridge) from $U^{238}$ and the production of $Pu^{239}$ (Hanford) on the scale needed for actual bombs was far from certain at the time. The ingenuity and persistence of the principals at both those centers were as remarkable as the successes at Los Alamos. They confronted harrowing engineering challenges—innumerable roadblocks, dead ends, and the unprecedented demands of precision specifications, radiation hazards, manpower needs, and more, on a gigantic scale. Massive unknowns had to be addressed, each as they arose.

The book can serve as a general resource for anyone interested in that period of history. It is so thorough in its coverage, so interesting, and so well-written that I was really quite overwhelmed. Reed's teaching of this course for many years has certainly helped considerably.

More than once, Reed made a particular observation that I found extremely telling. He points out that, despite the massive investment in money, materials, and (most of all) precious manpower during the height of the Second World War under conditions of intense secrecy, there never occurred even a hint of skullduggery or scandal of any kind, anywhere in the project at any of its sites. (I must unfortunately exclude the espionage activities of Klaus Fuchs, Theodore Hall, and David Greenglass.) To me, this was a reflection of an even more impressive aspect of that period. This was a time during which all the people of the US seemed to work in unison. The "greatest generation," immortalized by Tom Brokaw to characterize the American soldiers and sailors of the time, could really be generalized to include the entire population. Not long before Pearl Harbor, there was plenty of disagreement on how to deal with the war in Europe, where Hitler was having a field day. A quite strong isolationist movement attracted people on both the left and the right. On the left, "the Yanks are not coming" was a favorite slogan, at least before Hitler invaded the USSR in June 1941; on the right, Charles Lindberg personified the not very veiled sympathy for the Nazis. But all that vanished overnight on December 7, 1941. Reed's description of the Manhattan Project demonstrated America's unity after Pearl Harbor in as a dramatic fashion as one could imagine. The contrast to the mood in America today is depressingly different.

Now let me address a few specific details related to the book itself. I applaud the author's inclusion of what he calls "exercises," which appear in virtually every chapter. These are on a level that can be handled by a fairly well-prepared reader, and are actually quite a lot of fun (I tried a number of them myself). The only scientific error I was able to find was the statement concerning the detonation timing for ignition of the implosion bomb. The timing tolerance on the simultaneity of the detonation switches was not "down to several hundredths of a microsecond." This was not quite state of the art at the time, I believe. It should have read "several microseconds." I was pleased to see that the considerable contributions by British scientists to the project were clearly described.

The book suffers from a few production problems. First, there is no index—a serious deficiency considering the density of information the book contains. (Reed promises one in a later edition.) I found the many interesting photographs spread throughout the volume to be crudely reproduced, of poor quality, and occasionally difficult to decipher. On the other hand, I was impressed by the quality and quantity of the references. In particular, the reader has a chance to pursue many important primary sources by following the references to various websites. Many of these still are marked Top Secret (mostly, but not always crossed out!). In this age of information democracy, the reader can follow the same trails as the author himself. This is truly a huge advantage to the interested reader.

Another item worth mentioning: We have been accustomed to view the Manhattan Project as having had two essential leaders—J. Robert Oppenheimer and General Leslie R. Groves, of opposite personality and character, though both essential to its success. Oppenheimer, of course, was lionized after the war for his scientific leadership of the project, and Groves was admired for his management abilities. But I did not fully appreciate how important Groves really was. He seemed to be everywhere when important decisions were to be made. Despite his reputed hard-nosed character he was quite able when occasion merited to show flexibility, as he famously did by personally ordering Oppenheimer be granted secret clearance despite personal reservations. He was willing to take considerable risk on occasion. Reed cites many examples, not just at Los Alamos but at Oak Ridge and Hanford, where the project's success on occasion hung in the balance.

Also, I have to say, after reading of the various physics and engineering roadblocks that developed along the way (for example, the near-fatal roles played by contamination bv unwanted neutron-absorbing isotopes, as well as the spoiling effects of spontaneous nuclear reactions) and also of the never ending ballooning of its size, I began to think differently about the German nuclear weapons program. Only America, at that time, could have organized the resources at the scale needed for the fission bomb. Despite its original head start, Germany simply could not have devoted the resources required. The race was pretty much one-sided. But, of course, I am also exhibiting here a bit of hindsight of my own.

I would make one suggestion for an additional reference to the next edition. In the last paragraph of the book, Reed discusses the future preservation of important Manhattan Project sites and mentions the effort being made right now to have Congress designate parts of these locations as a National Historic Park. This effort is being led by an organization called the Atomic Heritage Foundation. Its website contains a veritable treasure-trove of Manhattan Project material, including primary source references and innumerable oral histories, well worth exploring by anyone interested in the project.

If you are looking for a source where (almost) the entire Manhattan Project can be accessed in one readable, comprehensive and comprehensible place, this is for you.

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Eric Scerri, *A Tale of Seven Elements*. New York: Oxford University Press, 2013, 270 pages. $19.95

Many stories of chemists and physicists, mixed in the crosscurrents of history, lie behind the highly organized periodic chart of the chemical elements. The marvelous adventures, bitter cross claims, missed opportunities, and disputes arising from national pride—the humanity of science—are hidden from students who stare at or ignore the chart and sometimes try to use it.

Eric Scerri, a philosopher of science and long-time periodic table scholar, spins many interwoven tales in this well-researched book. He accomplishes the difficult task by bringing together material that interests non-specialists as well as experienced chemists or physicists. The more technical passages are so artfully interwoven that the reader may glide past them with little twinge of reader's regret. Scerri's stories of these infra-uranium elements lead us from the 1917 discovery of protactinium (Pa) through technetium (Tc), hafnium (Hf), rhenium (Re), astatine (At), and francium (Fr) to promethium (Pm) in 1945.

The introduction wends its way through several helpful topics, including sociology of science (Robert Merton), what it means to discover an element, and philosophy of science (Karl Popper and Thomas Kuhn). Scerri ends with the difficult personal, scientific, and nationalistic issues over questions of priority in discovery. He points out what most scientists but few laymen understand: scientific knowledge benefits greatly "from the fierce scrutiny given to new claims. What matters is progress of overall human knowledge."

Chapter 1 lays out the development of modern chemistry from Dalton to the discovery of the periodic system. Even chemists familiar with Dalton and

Mendeleev will find new insights. Scerri presents the sharp disagreements by some well-known chemists (e.g. Berzelius and Dumas) on whether atoms are real entities or "only" philosophical concepts. In this way, chemistry differs from biology and, to some extent, physics. Chemists do not see or handle the objects of their thinking, the atoms and molecules. Scerri points out that, in some ways, this issue pervades much of the discovery of the seven elements in the book title. No less than seven scientists' efforts are portrayed in the story of the development of the periodic law and chart. Scerri ascribes most credit to Dmitri Mendeleev because of his skilled use of predicting elements yet to be discovered (including their predicted properties). He ends with the conceptual challenge arising by the discovery (Ramsay and Rayleigh) of the (chemically inert?) noble gases.

In Chapter 2, Scerri tweaks chemists a bit with "The Invasion of the Periodic Table by Physicists." Though more technical material is presented here, Scerri's tale-telling talents shine through as he marks a trail through the work of at least thirteen physicists. Notable signposts include the discovery of the electron and radioactivity as well as the development of the nuclear model of the atom and quantum theory. Although the word "atom" loses its basic meaning (uncuttable) as applied to the fundamental particles, the name persists because of its long association with chemical properties. The path leads on to X-ray discovery and X-ray spectroscopy, a major development for element identification. Soddy's coining the term isotope (*same place* in the periodic table) is seen as a significant clarification.

Chapter 3 (29 pages) tells the story of "Element 91—Protactinium," predicted by Mendeleev (1871) and discovered by Lise Meitner (1917). The protactinium saga sets the pattern for the remaining six elements. Scerri emphasizes the uncertainties in these ensuing element discovery stories: nature of radioactivity, meaning of electronic structure, and chemical similarity based on families or horizontal relationships. Some investigators even questioned the validity of the periodic law for such large atomic weights. Scerri weaves such history of science ideas into each chapter. In the process of isolating protactinium, Meitner and Hahn also discovered nuclear fission, a completely new process unanticipated by earlier work on radioactivity. We are led thorough the rivalries of war and racial discrimination (Meitner, a German Jew who was forced to leave Germany) that invaded the objectivity of science. In the end, though, only Otto Hahn received the Nobel Prize.

Chapter 4 recounts the discovery of hafnium (atomic number 72) by George Hevesy, which was heavily dependent on Henry Moseley's X-ray spectroscopy method. The discovery "sparked off one of the most bitter and acrimonious priority disputes in twentieth-century science," pitting French and English scientists on one side, Danish and Hungarian scientists on the other. Scerri makes excellent use of his command of the contemporary literature with many quotations from periodicals including *Comptes Rendus*, *Chemical News*, *The Times* of London, and *Nature* magazine. He even makes an excursion into the philosophy of chemistry with a quotation from Karl Popper, "reduction of chemistry to physics was a reduction in principle only."

Chapter 5 on rhenium (number 75), pays tribute to the excellent laboratory skills of Ida Noddack, who isolated 1 gram of rhenium from 660 kg of molybdenite. As in several other chapters, careful attention is given to the significant role of women. Rhenium, named after the Latin word for the river Rhine, is today the most widely used of the seven elements. It possesses the widest range of oxidation states (−1 to +7), forms a metal–metal quadruple bond, and is part of very important catalysts for organic synthesis.

The story of technetium (number 43) in chapter 6 occupies 29 pages because of its uniqueness in element discovery. Perhaps Mendeleev's predictions for technetium, which has the smallest atomic number among the "missing" elements, inspired a long list of pursuers. All earlier attempts failed largely because of technetium's scarcity in the earth's crust, there being perhaps only about $2 \times 10^{-10}$ grams per kilogram of pitchblende ore. Emilio Segrè is credited with "creating" technetium in a cyclotron in 1937, isolating it from molybdenum foil. In 1947, he named it after the Greek word τεχνητός—artificial. Another tantalizing Scerri sidelight is the description of the natural Oklo nuclear reactor in African Gabon. Technetium (as Tc-99m isotope) accounts for about 85% of all medical imaging diagnostic tests.

The stories of francium (number 87) and astatine (85), the last of the known alkali metals and the halogens, occupy chapters 7 and 8. They were discovered, respectively, by Marguerite Perey in 1939 and by Dale Corson, Alexander MacKensie, and Emilio Segrè in 1940. Scerri points us to a possible unexpected role for francium in the understanding of the anapole, a concept associated with the electroweak force, another intriguing side story in these chemical discussions. Scerri carefully describes "the nationalistic prejudices that persist to this day in many respects and that the identity of the 'discoverer' of the element (astatine) very much depends on the nationality of textbook which one might consult." (165) The ultimate credit, according to Scerri, goes to the Segrè team who bombarded a target of bismuth with alpha particles.

Chapter 9 describes the last of the rare earth elements, promethium (number 61), which is very difficult to isolate because of its extreme chemical similarity to other rare earths. Scerri quotes Clarence Murphy as saying that the element was named for "Prometheus who store fire from the Gods" to embody the "a warning that atomic energy could be the savior or the destroyer of humankind." (182) The development of ion exchange chromatography, one side benefit of the Manhattan Project, was absolutely necessary for the isolation of promethium. J. A. Merinsky, credited with the discovery (isolation), used the now well-known sulphonated phenol–formaldehyde type chromatographic column. Scerri discusses the electronic configuration of the rare earths (the lanthanide contraction) as a way of understanding their greater horizontal rather than family similarity within the periodic table.

In the last chapter, "From Missing Elements to Synthetic Elements," Scerri weaves more tales together while discussing issues related to element synthesis, electron configuration, and the controversial prospect for further element discovery (synthesis). One note readers might find intriguing is his discussion of the possible role that relativity theory might play in the color of the element gold.

Overall, this book is a genuinely interesting read. Scerri joins the stories smoothly together with the more technical material. I found few typographical errors, though they do not cause problems in reading: $\alpha$ for $\beta$ (67); (85) atomic number instead of atomic weight (180); Ni for Hf in Figure 9.7 (191); b for $\beta$ (193). I do differ with the author in one point: the rare earths actually existed before they were isolated and discovered (175). I highly recommend this book for a wide range of readers, including specialists and non-specialists, advanced high school science majors, college chemistry and physics majors, and anyone with a general interest in the history of science. Scerri writes in such a way that missing some points in one chapter does not negatively impact reading subsequent chapters. This book, a useful collection of information, is much more an intriguing human story of science.

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