Phys. perspect. 6 (2004) 477–486
1422-6944/04/040477–10
DOI 10.1007/s00016-004-0230-2
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Physics in Perspective

Book Reviews

Mary Jo Nye, ed., *The Modern Physical and Chemical Sciences.* Vol. 5. *The Cambridge History of Science*. Cambridge: Cambridge University Press, 2003, xxvii + 678 pages. $ 95.00 (cloth).

This exceedingly interesting collection is devoted to the history of the physical sciences in the nineteenth and twentieth centuries. Given the immense scope of the subject, historian of chemistry Mary Jo Nye has, quite remarkably, succeeded in providing the nonspecialist with an overview that is wide, deep, and entirely lucid.

The book is a page-turner and one could start with any of its thirty-three chapters, but there is something to be gained from consecutive reading since several of the early chapters provide connections to others later on. In the first chapter, "Theories of Scientific Method," Nancy Cartwright, Stathis Psillos, and Hasok Chang outline theoretical models and experimental traditions that resonate throughout the book--they surface, for example, in the discussion of the tension between geophysicists and geologists in "The Physics and Chemistry of the Earth," by Naomi Oreskes and Ronald E. Doel, and in James Rodger Fleming's "Global Environmental Change and the History of Science," which emphasizes the value of careful study of the past in a field whose "ideas about the global environment are changing along with the global environment itself." Frederick Gregory's fascinating essay, "Intersections of Physical Science and Western Religion," ends with a discussion of the link between religion, science, and the dearth of women (especially in physics), which segues directly to Margaret W. Rossiter's "A Twisted Tale," a history of the exclusion of women from nineteenth and twentieth-century science. The essay by Bernadette Bensaude-Vincent on the establishment and standardization of "Languages in Chemistry" from Lavoisier to the present might well be read in tandem with Hans-Werner Schütt's "Chemical Atomism and Chemical Classification." And Arthur I. Miller's "Imagery and Representation in Twentieth-Century Physics" analyzes visual imagery as a metaphor in scientific research, a problem discussed by Olivier Darrigol in "Quantum Theory and Atomic Structure, 1900–1927."

The central and largest part of the volume – eighteen chapters – is devoted to specific disciplinary areas in the overlapping categories of mathematics, chemistry and physics, atomic and molecular sciences, astronomy, and cosmology. The historical approaches are as varied as the science, and here is an almost random sampling of my favorites. I liked Sungook Hong's "Theories and Experiments on Radiation from Thomas Young to X Rays" for its multilayered history of the experiments, theories, and debates in nineteenth-century optics, spectroscopy, and electromagnetic theory. Jeff Hughes presents a transparent historical analysis of "Radioactivity and Nuclear Physics" as an interdisciplinary experimental and theoretical science at the intersection of academia, industry, and the modern state. In "Chemical Physics and Quantum Chemistry in the Twentieth Century," Ana Simões traces the application of quantum mechanics to chemistry with attention to prominent individuals, disciplinary differences, competing methodologies, and national styles. And Theodore M. Porter shows in "Statistics and Physical Theories" that statistical methods provided a cultural and intellectual link between nineteenth-century social investigations and early kinetic gas theory, raising similarly disturbing questions of causality and chance.

There's much more, but the reader's time would be better spent with the book itself. This is a fresh, intelligent, and richly textured volume that will please and enlighten anyone interested in science and its history.

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John M. Charap, *Explaining the Universe: The New Age of Physics*. Princeton: Princeton University Press, 2002, xii + 226, $19.95 (cloth).

In this book Charap presents a clear exposition of the full sweep of modern physics starting from developments in the 19th century through topics of very current interest. His presentation, virtually devoid of mathematics, is mostly accessible to nonphysicists. Charap, a distinguished researcher, writes in a lucid, engaging manner, and the book has a sufficient number of graphics to add interest and help the exposition. Perhaps the best-written chapter is the last one, where he discusses the many ways in which physics has led to a wide range of practical applications. In contrast, some of the more esoteric chapters – on string theory and cosmology – will probably be less comprehensible to some readers, but that can be said about many popular accounts of those subjects. Charap's book seems quite well-edited, with only a small number of errors. One that I happened to notice is a claim that the Hiroshima bomb used uranium-235 that was made in an accelerator while, in fact, it was made by enrichment using the gaseous-diffusion process. A second small error is the claim that neutrino-oscillation experiments measure the mass difference between different neutrinos when, in fact, they measure the difference in the squares of the masses. One peeve I have is that parts of the book tend to be repetitive, since many topics are discussed in several places. Another type of repetition is that specific facts, such as the speed of molecules in air at room temperature being 4,000 kilometers per hour, are repeated within a few pages. Notwithstanding these minor flaws, Charap has written a masterful book that any reader interested in physics should enjoy.

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Giuliano Pancaldi, *Volta: Science and Culture in the Age of Enlightenment*. Princeton: Princeton University Press, 2003, xv + 384 pages. $35.00 (cloth).

Uncommonly deep and absorbing, Giuliano Pancaldi's new book represents a timely and valuable addition to the existing literature on Volta and the Enlightenment.

The book is clearly the scholarly product of a deep mind well trained in the history and methodology of science and reflects the author's exposure to the rich and ancient cultural tradition of both Bologna and Oxford, two institutions that rank among the truly historic learning centers in the world.

An elegant jacket illustration, "The Triumph of Science," a fresco by Nicoló Barabino in 1876 encapsulates the inspiring theme of the book. Quoting from that illustration:

> The scene depicts a heroic vision of the scientific and industrial revolutions, with the symbolic, female figure of Science, swathed in light and holding a geometry book, vanquishing the dark, writhing figure at her feet, representing ignorance and obscurantism. Volta himself is seen standing on the right, demonstrating his electric battery to his assembled peers – an assortment of natural philosophers from earlier ages, as well as scientists and engineers from the age of steam and electricity.

The publication of the book is timely in a double sense. First, not many people today, outside the circle of scientists and historians, know much about Volta. If this sounds preposterous, consider that in my experience as a physics teacher I find that most undergraduates taking an introductory course in electricity and magnetism are unable to associate the unit of electromotive force with the name of Alessandro Volta. They are simply unaware of the existence of a historic figure responding to the name of Volta.

More to the point, Pancaldi's book is also timely, and valuable, because its focal point is neither the biography of Volta nor the study of the Age of Reason *per se*, but the conjunction of both, as the title implies, in a unique perspective that the author explores from many angles and in great depth. This timely combination of two themes, cleverly intertwined against the historical backdrop of Napoleon's meteoric rise (and fall), is the most critical and original aspect of the book. Overall, I think that Pancaldi has written a serious piece of scholarly work, aimed at the *cognoscenti* of the history of science, taking the life of Volta almost as a pretext for a masterful analysis of Enlightenment science and its intercourse with other prevalent social, political, and cultural factors that characterize Europe in the late eighteenth century.

In all fairness, this positive assessment of the book comes with a qualification, or warning for some readers, about its potential downside. Keeping in mind that all literary judgments are inherently subjective, the book may not appeal to the widest audience because of its style. This book is not bedtime reading. Rather, it is a ponderous book that requires dedicated time and undivided attention to grasp the meaning conveyed by some of its dense pages. The reader in search of lively entertaining prose, perhaps full of colorful biographical or historical anecdotes, may well be disappointed. The closest that Pancaldi comes to telling amusing anecdotes, in my opinion, is a brief passage (page 154) extracted from a letter by Count Giambattista Giovio, a lifelong friend and traveling companion of Volta, in one of his well-described excursions across the Alps:

> My Volta is always busy. What an industrious scholar he is! When he is not paying visits to museums or learned men, he devotes himself to experiments. He touches, investigates, reflects, takes notes on everything. I regret to say that everywhere, inside the coach as on any desk, I am faced with his handkerchief, which he uses to wipe indifferently his hands, nose and instruments.

However, while the book is not easy to read, for those who persist the reward is a unique and insightful appraisal of Volta's ambition to transform himself, gradually but methodically, from a relatively poorly educated young man in the provincial setting of a Jesuit school in Como into a mature self-made natural philosopher of international stature and one of the foremost experimenters of his time in the field of weak electricity.

If that was the ambition of the man during his lifetime, what is Volta's standing among the "natural philosophers" of science two hundred years after the epochal invention of the voltaic pile? I must admit that Pancaldi's book has had the consequence, perhaps unintended, of altering my view of Volta's scientific legacy.

"Insofar as it is a biography, this is a biography in context," says Pancaldi in the Introduction, the specific context being the last quarter of the eighteenth century, a period of European history characterized by political and cultural turbulence. Chapter 2 is a scholarly gem that must have cost the author endless hours of painstaking work, as it presents a detailed study of the Italian scientific community from 1770 to 1795, based on a survey of seventy-four natural philosophers active south of the Alps. His assessment, not entirely flattering for the Italian community of that period, is made through the documented testimony of insiders as well as foreigners and partially justifies Volta's distinct inclination to seek contacts with researchers on the other side of the Alps. His admiration for England is especially noteworthy – he was literally struck by the ferment of new ideas and industrial inventiveness of that nation – and perhaps explains why his most important scientific communications were addressed to the Royal Society of London, including his epoch-making communication (in French) outlining his discovery of the voltaic pile. Scientifically, the last quarter of the eighteenth century marks the branching out of natural philosophy, a notion that consolidated gradually in the course of the seventeenth century into the more specialized disciplines of physics and chemistry that were firmly established in the course of the nineteenth century. Undoubtedly, Volta represents a key figure in that cultural transition just as the discovery of the voltaic pile is emblematic of the passage, at the threshold of the nineteenth century, from practically useless electrostatic instruments, at most capable of generating sparks by friction, to the much more versatile electrochemical devices that made possible the extraordinary advances in chemistry and the unification of electricity and magnetism, perhaps the crowning achievement of nineteenth-century physics.

Out of this background the portrait of Volta sifts through the pages of Pancaldi's book, almost by a process of literary osmosis. The characterization of Volta, the man, may seem rather diffuse, or even lost to some readers. As mentioned earlier, I believe that this is by design, and I concur with Pancaldi's choice that the big picture, namely, the "biography in context" is more important than the factual, or strictly personal biography of the man. Thus, the life of Volta is sketched in the first chapter, a relatively short one given the length of the book, where we are told that Volta was born into a family of lesser nobility in Como, a town within the Duchy of Lombardy then under the "enlightened" rule of the Austrian empire. The thread that connects this biographical chapter to the rest of the book dealing with the "context," is that young Alessandro, after a religious phase when he contemplated joining the order of Saint Ignatius, probably under the influence of his Jesuit tutor, eventually and wholeheartedly embraced the whole philosophical package of the Age of Enlightenment. This personal intellectual turnaround, in a broader social context, was consistent with, and probably influenced by the political and cultural directives from Vienna. Those directives were to reform the public administration of the province, starting with the secularization and modernization of the system of public education. Volta fully endorsed the idea and the whole reforming process, in which he played a very substantial role throughout his life as a teacher and public servant, eventually ascending through the educational ranks to the position of superintendent of the secondary school system of Como and then to a full professorship at the prestigious University of Pavia.

How did young Volta achieve that remarkable feat given that his formal training was far from impressive? A keen intellect and a deep curiosity about the intriguing but rather mysterious electrostatic phenomena, combined with an uncommon dexterity at conceiving and performing actual experiments are, without a doubt, the main ingredients of Volta's success. These qualities transpire from Volta's extensive laboratory notes and other sources painstakingly analyzed by Pancaldi, with true scholarly zeal, in his comprehensive bibliography. However, Pancaldi also tells us that one of Volta's great assets, besides his solid experimental work, was his mastery of the multiform art of networking, mostly through correspondence and carefully arranged travels to the many cultural centers of Europe. The network included personal contacts with just about everyone, from clerics and merchants to members of the interconnected European nobility; from influential public administrators close to the centers of power to established scientists on both sides of the Alps; from inventors of new electrical equipment, technicians, and machinists, to performers of "magic shows" involving the most spectacular electrical devices available at the time. Summing up, I get the distinct impression that had Volta lived in the age of radio and television (to the invention of which he partially contributed, albeit very indirectly) he probably would be on many popular "talk shows," as his reputation as a great communicator and demonstrator of the arcane but impressive manifestations of static electricity was legendary. In all fairness to Pancaldi (and to Volta) we are also told that this networking activity was not just a matter of opportunism but had an intellectual side to it linked to the Enlightenment ideal of a supernational Republic of Letters to which most European natural philosophers felt they belonged. I like to think that the true and unique internationalization of the contemporary scientific community is the evolutionary end point of that lofty ideal.

Be that as it may, combine all of the above ingredients with a self-serving cautious respect for authority, both political and religious, and one gets a picture of Volta (at least I do) as a consummate survivor (unlike Luigi Galvani) adept at the game of turning to his own advantage the shifting winds of power sweeping across Europe in the latter part of the eighteenth century. In a more strict scientific sense, these qualities enabled Volta to excel in the "imitation-competition game" typical of the Enlightenment culture of the time. By that expression Pancaldi means the art of reproducing older experiments by adding a new spin, almost as a "variation on a theme" by an earlier electrician and in competition with similar work by other active researchers, often with unintended and far-reaching consequences. This seems to be the case with the well-known electrophorous, still in use today in one form or another, as a classroom demonstration of electrostatic effects to the amazement and delight of students around the world. Volta's invention of the electrophorous, an improved version of a similar device previously introduced by Aepinus, properly advertised through Volta's networking channels, propelled him to notoriety within the circles of inventors and scientists working at the "cutting edge of research" in the embryonic science of electrical phenomena. The same applies to Volta's sensitive *condensatore* and, to a certain extent it seems, to his crowning achievement, the electrical battery. This stunningly simple device, a "magnificent instrument of philosophic research" in the words of Michael Faraday, was the end product of the famous Volta-Galvani debate on the nature of "animal electricity." That debate spread like wildfire across Europe and eventually earned Volta the highly-coveted Copley Medal of the Royal Society of London (its highest award) in 1794.

Against the background of that historic debate on "galvanism," as reported by Pancaldi, I wish to express my ambivalence about the decisive role that Volta had in the debate itself and the not-so-decisive role that he had in the subsequent history of electromagnetism. If one looks for a sound bite that encapsulates Volta's most significant attribute as a man of science, none seems more effective than "a man with his brain in his fingertips," attributed by Pancaldi to D.J. de Solla Price (page 203). To my mind, that description underscores the contrast, perceived by many of Volta's contemporary peers, between his uncanny ability in the laboratory and his constant struggle to provide a sound theoretical, albeit qualitative interpretation of his innovative results. From here, playing the devil's advocate, I am led to query: Was Volta, at best, a brilliant inventor of impressive electrical instruments? In a way, as I ponder this question I also express admiration for Pancaldi's work since it represents a puzzle that is still unresolved in my mind after reading Pancaldi's compelling narrative, especially his account of the twists and turns leading to the discovery of the electrical battery.

The "seat of electromotive force" is universally regarded as Volta's everlasting legacy in the history of science and one of the most "useful" discoveries in the history of technology. However, this is an *a posteriori* assessment based on the collective judgment of scores of physicists, chemists, and historians about the role of the battery in the subsequent development of the science of electrochemistry and, even more important in my view, in the unification of electricity and magnetism into one of the "fundamental" forces of Nature. The indisputable point of fact is that Volta never set out to purposefully design and build a device capable of generating a constant "pressure" for driving a continuous flow of "electric fluid." While Volta recognized the clear advantage of having at one's disposal a continuous and controllable flow of electricity, as opposed to the transient and unpractical flashes produced by all previous electrostatic machines, he seemed to be blind to the electrochemical nature of the new device, a point that was quickly grasped, it seems, by William Nicholson and Anthony Carlisle in the brief interval of time between the two consecutive communications announcing the new discovery to the Royal Society of London. Whether Volta neglected this window of opportunity by prejudice or lack of interest for the new phenomenological evidence is still not clear to me.

More understandable is that Volta did not anticipate the possibility of connecting electricity with magnetism, which was, historically speaking, just around the corner. Somewhat paradoxically, I note that Volta did have in mind a sort of "unification" but of a totally different kind. The ultimate objective of Volta's research was to dismantle Galvani's idea of "animal electricity" as a "life force" *distinct* from conventional electricity. While I admit prior ignorance about the historical details of that controversy, Pancaldi's account in chapter 6 is intriguing and captivating enough that I could not put the book down. From his account it transpires that Volta arrived at the invention, or discovery, of the battery in an attempt to reproduce, by electrical procedures and techniques known to electricians of the time, the same type of discharge caused by some living organisms such as the torpedo, or electric eel. Accomplishing this would bring the debate on galvanism to an end, as it did, proving conclusively that animal electricity was just another manifestation of ordinary electricity and not the manifestation of a new force inherent in all living organisms.

I may add, incidentally and not incorrectly I hope, that a similar feat of unification was accomplished decades earlier by Benjamin Franklin, another recipient of the Copley Medal, who demonstrated conclusively with his world-famous experiments that the spectacular but poorly understood phenomenon of lightning was, in fact, reducible to just another manifestation of the electrical force. The difference, I guess, is that in Franklin's case there was no debate, as far as I know, at least not of "galvanic proportions," since there was no alternative explanation of lightning in terms of a mysterious new force, say an "atmospheric force."

Be that as it may, the undisputed facts are, first, that Volta was not consciously searching for a source of constant electrical current and, second, that the seminal idea of reproducing in the laboratory, by physical means, the "life force" of an electric eel did not germinate in Volta's mind. There is evidence, in Pancaldi's compelling account, that "something" triggered a spark (electrical?) in Volta's mind (the pun is intentional here, as I ponder what Volta and Galvani would have made of the discovery of synapses and neurons as the pathway of electrical signals in the human brain). As suspenseful as a good thriller can be, Pancaldi identifies that "something" with an excerpt from a paper by William Nicholson (quoted on page 199) published in the November 1797 issue of his *Journal of Natural Philosophy, Chemistry and the Arts*. The gist of the paper was a bold and fairly detailed proposal that a mechanical and electric machine could be built to produce a shock like the electric eel's. From there, it was a matter of disciplined (and masterful) laboratory work for Volta to follow up on his theory of "contact electricity" and come up with that stockpile of different metals and brine-soaked pasteboard discs that eventually became, in the able hands of Humphry Davy and Michael Faraday among many others, a truly "magnificent instrument of philosophic research."

Pancaldi's assessment of Volta's scientific legacy, essentially tied up with the invention of the battery, is discussed in the last chapter of the book and is the only point that finds me in relative disagreement (without taking anything away from his penetrating analysis). As in the case of Volta's biography, the assessment of his legacy is "contextual" rather than individual. In fact, the subtitle of this chapter 9 is " Enlightenment Legacies" rather than "Volta's Legacy." In this contextual perspective, the motivation of Volta's research as a controversy on the nature of animal electricity is pushed into the background, while the monumental contributions that followed, at the hands of others who brilliantly and effectively used the battery in fields that Volta did not anticipate, are pulled into the foreground. Pancaldi sees this as the result of a cultural "diversity," typical of the Age of Enlightenment along with its emphasis on "quantification" and "useful science" – this whole complex of factors leading to "chance in the form of unintended consequences." This argument presumably explains how "the public career of the battery followed paths that the inventor himself was unable to anticipate and control." This line of reasoning is all very well and applies, I believe, to all human endeavors, great and small, as the "context" is always there and cannot be obliterated. However, I find the argument somewhat misleading and definitely unfair. Volta certainly was a true and lucky son of the Enlightenment, and his contribution must be seen within the social and cultural "boundary conditions" under which his life unfolded. However, he received the Copley Medal because of his *decisive and personal role* in resolving the dispute on galvanism. From there he was led to the invention of the battery in the attempt to simulate, in a laboratory setting, the shock of an "electric fish." Why not simply admit that Volta's *individual* contribution ends there and that he received plenty of recognition for it during his lifetime. The rest is history and history hands down judgments when the ephemeral dust of the day settles down. True, after two hundred years, the discovery of the battery is deemed to be "epochal" in a larger context than the life of a single man. Probably, in the end, we all agree that the invention of the voltaic pile is an epoch-making one not because Volta managed to simulate the shock of a torpedo, nor for bringing to a close an ineffectual and long-forgotten academic debate, but because of its irreplaceable role in establishing a new "world system," well beyond Volta's wildest dreams, that effectively altered the course of our civilization.

I like to think, because I can understand the human side of it, that Volta's decision, conscious or subconscious, not to be a part of that new world system was simply because by 1800 he had already reached the apex of his career. Shunned by his more established peers in the early stages of his scientific development, Volta turned out to be one of the most revered electricians of his time with a long list of tangible honors bestowed upon him by scientific peers as well as by politicians and rulers, chief among them the First Consul and then Emperor Bonaparte. With such an illustrious career to look back to, it may be difficult to look ahead for new challenges, especially during turbulent political times. I also think that the posthumous accolade in 1881, the year in which the unit of electromotive force was named after him, is the one that probably would have given him the greatest satisfaction. Pity that most of our undergraduate students know nothing about it!

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Peter Pesic, *Abel's Proof: An Essay on the Sources and Meaning of Mathematical Unsolvability*. Cambridge, Mass: The MIT Press, 2003, viii + 213 pages. $24.95 (cloth).

The greatest legacy the ancient Greeks left us in the field of mathematics was the idea that to be recognized as a valid theorem, a mathematical proposition required a solid, ironclad proof. Many earlier civilizations, such as the Chinese, Indian, Babylonian, and Egyptian, had produced correct and important mathematical statements without ever seeing the need for proving them. Even in the twentieth century, the enormously creative and productive Indian mathematician, Srinivasa Ramanujan, who grew up without a Western education, produced a large number of important and very complicated results without ever proving them. (After his death it took a huge effort on the part of other mathematicians to provide the proofs; only a few of them were found to be incorrect.)

The issue of proof in mathematics was raised poignantly in John Barrow's book, *Pi in the Sky: Counting, Thinking and Being* (Oxford: Carendon Press, 1992), in which he envisioned NASA having finally made contact with an alien, highly developed civilization. It turned out that, though the alien mathematicians were extremely learned and knew much of what our mathematicians knew and more, they had never adopted the requirement of a proof; using their powerful computers, they simply subjected each proposed theorem to many stringent tests in special cases and were satisfied when the theorem passed them all. Peter Pesic convincingly demonstrates, by implication, that Barrow's fable is very unlikely to correspond to reality. His book shows the great fruitfulness of proofs in mathematics, far beyond their immediate aim of establishing the validity of particular theorems. The most useful and admired proofs open up entirely new vistas and lead to progress in unanticipated directions. Without them, a large collection of valid propositions would lack the depth and richness of mathematics as it exists now and the field would not be where it is. The premise of Barrow's tale, the existence of an alien civilization that has produced a highly advanced field of mathematics without use of proofs, is thus extremely implausible.

The first half of Pesic's book recounts the history of irrational numbers and the gradual development of the field of algebra in its struggle against mathematicians for whom only geometrical reasoning, grounded on and modelled after Euclid, fulfilled the ideal they were striving after. He points out that Isaac Newton's opposition to both Descartes and Leibniz were ultimately based on his preference for geometrical reasoning as against the more algebraic arguments of the latter two. (We are the inheritors of the algebraic notation in the calculus introduced by Leibniz and of the Cartesian algebraization of geometry as well. Newton's geometric demonstrations in the *Principia* are very foreign to our way of thinking today.) The specific issue discussed in detail is the effort of many mathematicians to solve nonlinear algebraic equations. If the solution of quadratic equations was a simple matter, the cubic and quartic became much more difficult, but the quintic raised a stone wall.

For the reader who is not a professional mathematician it may be necessary to explain what is meant by the solvability of these equations. It is not a question of the existence of roots of an *n*th-order polynomial, or of their calculation by means of well-known approximation procedures (such as Newton's); indeed in the nineteenth century machines were constructed – a picture of one of them is shown in the book – for their computation. An algebraic equation was regarded as solvable, and this is what the subtitle of the book refers to, if its solution could be expressed in terms of its coefficients by means of fractions and radicals. In this specific sense, equations of order 2, 3, and 4 had been found solvable. However, the equation of order 5 resisted the most stenuous efforts, and mathematicians gradually became convinced that it was impossible, though the reason for this was not understood.

Niels Henrik Abel, a young Norwegian, ill of health and living in poverty, was determined to solve this most challenging problem in the field of algebra at the time, and before he died in 1829 of tuberculosis at the age of 26, he managed to do it. His proof that the roots of a general polynomial of degree 5 cannot be written in terms of ratios and radicals – the quintic equation is not solvable – made use of an entirely new argument based on expressing each root as a function, or transformation, of another. In this manner he forms a set of relations that we now call a group, a concept that did not exist at the time, and he shows that only those equations are solvable for which the order in which the transformations are applied does not matter, that is, the elements of the group commute. (Such groups are now called abelian.) In contrast to the equations of lower order, the corresponding group for the general fifth order of polynomial is nonabelian, so the general quintic equation is not solvable. Q.E.D.

Evariste Galois, nine years younger than Abel, carried the torch farther and founded the branch of mathematics now called group theory, setting the tone for much of modern physics as well as mathematics by attention to symmetry properties. He too died young, in a duel at the age of 20. The legacy of Abel's proof is thus visible in all of our most fundamental theories of the structure and development of the universe. Pesic tells the story well, in a readable and eloquent style. However, the book will appeal only to readers with a keen curiosity about mathematics. Though the author relegates all equations to specially marked boxes that can be skipped, he describes the mathematical developments in great detail, which will be fascinating to some but more than those without an intense math interest will appreciate. I can certainly strongly recommend *Abel's Proof* to physicists and mathematicians. This little book is a fascinating read.

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Susan Haack, *Defending Science – within reason*. Amherst, New York: Prometheus Books, 2003, 411 pages, $28.00 (cloth).

Susan Haack's *Defending Science* is an attempt to steer a middle course in the discussion of science between the narrow, logicist view of what she calls the "Old Deferentialism," typified by Karl Popper and Carl Hempel, and the "New Cynics," whose membership includes Bruno Latour and Harry Collins. The old view tended to privilege scientific knowledge, whereas the new cynics tend to deny the possibility of such knowledge. She describes her own view, "critical common-sensism," as follows:

> Scientific knowledge is not the only knowledge, nor scientific inquiry the only legitimate kind of inquiry; the rationality of the scientific enterprise cannot be completely captured by any narrowly logical model; scientific claims and theories are fallible and revisable; scientists are seldom if ever wholly objective, impartial observers; and the social context in which scientific work is conducted can affect not only what questions are thought worthy of investigation, and not only what solutions come to mind, but even, sometimes, what results are arrived at. But it doesn't follow that there is no such thing as objectively stronger or weaker evidence, objectively better- and worse-conducted inquiry, objectively true or false scientific claims and theories….
>
> And allow me to say one last time, also: a claim is true if things are as it says, false if they are not; evidence is better or worse depending on how supportive it is, how independently secure, and how comprehensive; and investigation is better or worse conducted depending on how scrupulous, how honest, how imaginative, how thorough it is (pp. 338–339).

She also has chapters that critically examine and argue persuasively against social constructivism, some of the more radical aspects of the rhetoric of science movement, and other forms of postmodernism.

Does Haack succeed in establishing her "critical common-sensism" about science? My answer is a qualified yes.

Part of my difficulty is that I am not sure who is the intended audience for this book. At times it seems to be intended for other philosophers of science. Her chapter, "Nail Soup: A Brief, Opinionated History of the Old Deferentialism," is really too brief to be of value to readers not already familiar with the authors she discusses. She also gives a rather detailed discussion of both the "grue" problem and the "raven paradox," topics that are unlikely to be of great interest to anyone outside the field. I will discuss only the "grue" problem, recognizing that my audience is mostly scientists. "Grue" is a property of any object such that if it is examined before some time *t* in the future, say 2:30 this afternoon, it is green, otherwise it is blue. Thus, observing a green emerald this morning confirms both "All emeralds are green" and "All emeralds are grue." If one observes a green emerald after 2:30 one has witnessed an interesting phenomenon: emeralds have changed from grue to bleen. There are solutions to this problem, notably a Bayesian one, although that is not available to Haack because she rejects Bayesianism, the use of the probability calculus in discussing the confirmation of hypotheses. (Because we have never observed a grue object, the prior probability of the grue hypothesis is very low and even though the observation does support it, it never becomes very probable.)

Philosophically one might also criticize Haack because although her reasonable view of science needs good evidence to warrant belief in scientific hypotheses and to justify scientific knowledge, she never quite specifies how one determines what constitutes such good evidence, not does she have any way of distinguishing important from unimportant evidence. Her discussion of this issue is really too vague to bear that weight. She does argue that good evidence is that which fits in well with what is already accepted as scientific knowledge. Her metaphor here is that science is like a large crossword puzzle in which some parts are quite filled in and others are empty and one can judge how well the proposed evidence fits with what has already been filled in. I note that the crossword puzzle analogy, if carried to its logical conclusion, really doesn't fit in with Haack's fallibilist science. A puzzle has a unique solution, something I don't think Haack would support.

At other times it seems that Haack's intended audience is scientists, other academics outside the sciences and philosophy of science, or perhaps the general public. None of the comments on the book jacket are by philosophers of science, and three of the fours are by authors who have participated in the recent "Science Wars." The chapters on the sociology of science and the rhetoric of science hint at that combat. The chapters on "Science in the Law," "Science and Religion," and "The Values of Science" are of value to both specialists and nonspecialists.

There is another problem with this book. Except for one early and fascinating discussion of the evidence for Watson's and Crick's double helix, there is virtually no discussion of examples from the history of science. I believe that the book would have been better if more illustrations from the history of science had been included.

Despite these criticisms I believe this is a good book and one worth reading. Haack's "critical common-sensism" view of science is quite reasonable and she presents good, although not completely persuasive, arguments for it. Although I would have preferred somewhat lengthier treatments of the issues discussed in the chapters for nonspecialists or the general reader, they are still worth reading. I just haven't been able to figure out under whose Christmas tree to put it. I'll just keep it for myself.

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Steven Weinberg, *The Discovery of Subatomic Particles*. Cambridge: Cambridge University Press, 2003, xvi + 206. $25.00 (cloth).

Steven Weinberg's small book is a second edition, slightly revised, of the book of the same title published in 1983 as part of the Scientific American Library. Like Weinberg's best-selling predecessor dealing with theories of the early universe, *The First Three Minutes* (New York: Basic Books, 1977), the text contains no equations of the usual sort and the mathematics employed in the appendixes is at the level of beginning high-school algebra. However, some of the physical arguments in the text, being as rigorous as is possible without mathematics, are not easy to follow without intense concentration. Weinberg treats the discoveries of the electron, proton, and neutron in some detail, while other particles (photon, neutrino, positron, etc., all the way to quarks and gluons) are discussed briefly. Strange particles, for example, get only a short paragraph.

The chapter entitled "The Discovery of the Electron" comprises about a third of the book (pp. 9–66) and carries through the plan of the book to inspire the reader's interest in the marvelous world of subatomic physics by a combination of historical and anecdotal (in a good sense) material, together with physical arguments. The chapter aims to provide the motivation, description, and analysis of J.J. Thomson's experiments of 1897, which showed that cathode rays are a stream of negatively charged particles and determined the particle's charge e and mass m, especially the experiment that measured deflections in an electric and a magnetic field to determine e/m.

The reason for the length of the chapter on the electron discovery is that, writing for the reader who has never studied physics or who has forgotten it all, Weinberg introduces a number of "flashbacks" to instruct said reader in those elements needed to understand the experiments and to lead that person through the analysis. The flashbacks in the electron chapter are: The Nature of Electricity, Newton's Laws of Motion, Electric Forces, Magnetic Forces, and Energy. Obviously this includes a fair portion of an elementary physics course, all without equations! (Actually there are a very few equations in the text, but they are expressed in words, not symbols.) The approach to these topics is historical, but a compact "logical" presentation, with algebraic equations, is given in a set of appendixes, which also contain tables of units, physical constants, the chemical elements, and notes for further reading.

After the electron chapter there are shorter chapters on The Atomic Scale, with flashbacks on atomic weights and electrolysis (so, a brief history of chemistry), on The Nucleus (radioactivity, the nucleus, the proton, and the neutron), and finally on More Particles (photons, neutrinos, … gluons). This last chapter appears as something of an afterthought, as any discussion of quantum mechanics, spin, quantum statistics, etc., is considered to be far beyond the scope of this book. Incidentally, although the new edition is called "revised," it contains hardly anything new. The "notes for further reading" are identical to those in the first edition and do not list any of the excellent popular books that have appeared in the last twenty-five years. Though of somewhat smaller physical dimensions than the first edition, the book is attractively packaged and illustrated with well-chosen photographs.

To the readers of this journal who may wonder for what audience Weinberg's book could be recommended, I might mention that many years ago as a high-school student, before I had any idea of becoming a physicist, I was captivated by reading books intended for the "intelligent layman" by eminent scientists like Arthur Eddington, James Jeans, and Bertrand Russell (*The ABC of Relativity*). In the preface to Max Born's *Einstein's Theory of Relativity* (New York: Dover, 1962), the author said that he followed "a quasi-historical method of presentation" and restricted himself to mathematics "contained in the syllabus of the lower forms of high school," hoping that the reader "might really obtain an understanding of modern scientific thinking." In Weinberg's preface (p. xii) he writes that it seems to him "a tragedy that so many otherwise well-educated people are cut off from this part of our culture by a lack of familiarity with the basics of science." His effort to help rectify this lack is certainly commendable, even if it does not constitute the "radical revision in the way that science is brought to nonscientists," as he suggests (p. xiii). The effort required to understand the very compact version of the basics of elementary physics as sketched in the appendixes may well lead the reader to consult the explanations in some of the fine elementary physics textbooks available, billiard-ball collisions and all. Weinberg's book is based on part of a core-curriculum course on modern physics that he gave at Harvard and the University of Texas, and its best use would probably be as supplemental material for such a course.

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How to Hit a Telephone Pole

Werner Heisenberg recalled that once, on a visit to Niels Bohr's cottage when a chance event occurred in a game, he remembered the following episode:

> Bohr's tendency to philosophical generalizations was often stimulated by very simple games. Once, when on a lonely road I threw a stone at a distant telegraph post, and contrary to all expectations the stone hit, he said, "to aim at such a distant object and hit it, is of course impossible. But if one has the impudence to throw in that direction without aiming, and in addition to imagine something so absurd as that one might hit it, yes, then perhaps it can happen. The idea that something perhaps could happen can be stronger than practice and will."

Werner Heisenberg, "Quantum theory and Its Interpretation," in S. Rozental, ed., *Niels Bohr: His life and work as seen by his friends and colleagues* (Amsterdam: North-Holland and New York: Wiley, 1967), pp. 96–97.