sies, and reputations, men with their own visions of the natural world and of society—the "surprising" images of the atomic nucleus held by Berkeley chemists in the early '30s being a perfect case in point.

Concerning more specifically the Radiation Laboratory itself, the authors show clearly what made this place original in the scientific context of the time: a "fascination with hardware" and devices always bigger than the previous ones, a "lust after machinery [and] the squandering of time on mere technical improvements," an optimistic activism and a "ferocious pace in pursuit of truth," a "subordination of the individual to the group" (which appeared more and more distinctly with time), an instinct for advertising every move or "discovery," good contacts with other entrepreneurs (businessmen, for example), a preoccupation with patenting, and systematic head-hunting (which started with Lawrence, who "was hard to get and expensive to keep").

John J. Livingood and Glenn T. Seaborg "after a successful hunt. They are hurrying through Sather Gate (the south entrance to the [Berkeley] Campus) to the post office to send their latest findings to the *Physical Review*. Note the dress for the occasion." [From *Lawrence and His Laboratory*; courtesy Glenn T. Seaborg]

In fact, Heilbron and Seidel make it clear that the Rad Lab should not be regarded simply as an American equivalent of the nuclear physics laboratories in Europe at the time: the logic of its growth was not the same, its know-hows and its objectives were different. Superb at mastering machines, the Berkeley physicists contributed little to exact measurements in nuclear science before 1939 or 1940. To do that would have meant "not to be in any hurry and [to settle] down to patient, painstaking experimental study"—something rather uncongenial to Lawrence's style. Symmetrically, Rutherford appears as preoccupied only with physics questions—which led him in 1933 to get a mere 200-keV accelerator, with which he destroyed Lawrence's main scientific claim about deuteron instability. In the same way, the decisive importance at Berkeley of radiochemistry, radiobiology, and the manufacturing of radioisotopes has to be recognized. Essential for getting funds from foundations, these activities gave the Laboratory a tone markedly different from that of Joliot's or Chadwick's.

This first installment of the history of the Rad Lab closes at the advent of World War II. What is noteworthy about the situation at this juncture is, first, the decisive importance of "cyclotroneers." Being simultaneously physicists (at least those of the new generation), machine enthusiasts able to get devices to work under any conditions, and entrepreneurs used to working in multidisciplinary teams, they found themselves at the head of all major war projects. The second stunning element is the 184-inch cyclotron being pushed by Lawrence despite the warnings of theoreticians who were convinced that such a machine could never work. What strikes the reader here is the scale of the enterprise, whether in terms of money (around a million dollars), of technical means (a magnet weighing between 2000 and 5000 tons), or of the number of political and scientific allies needed for success. In a list that may seem directly inspired by Latour's sociology, Heilbron and Seidel explain the success of Lawrence's enterprise by the Nobel Prize he received in 1939, the offer of the University of Texas to hire him, the mesotron waiting to be studied by a machine, the letters in his favor received (on request) from all major European physicists, his skill at negotiation with foundations—and the war, which "also made good the shortfall in Lawrence's ideas of eluding relativity; the machine when finished in 1946 operated on a principle invented by McMillan in 1945, perhaps as a result of his wartime experience with radar."

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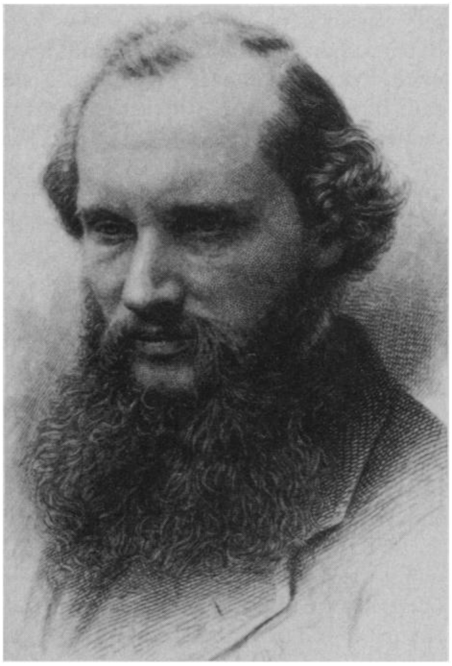
Kelvin in His Times

Energy and Empire. A Biographical Study of Lord Kelvin. CROSBIE SMITH and M. NORTON WISE. Cambridge University Press, New York, 1989. xxvi, 866 pp., illus. $89.50.

William Thomson, who became Lord Kelvin ("Baron Kelvin of Largs") in 1892, was during most of his long life widely regarded as the most important physicist in the English-speaking world. Born in Belfast in 1824, he was educated at Glasgow and Cambridge universities. By the time he graduated from Cambridge in 1845 he had already established himself as an authority on mathematical physics through several publications on Fourier's theory of heat conduction. The next year he was appointed professor of natural philosophy at Glasgow University, a position he held until his retirement in 1899. He was knighted in 1867 for making possible the establishment of telegraphic communications between Britain and America through the Atlantic Cable. He outlived his century and its physics, dying in 1907 after leading a losing battle against Ernest Rutherford's theory of radioactivity.

Among Kelvin's many other contributions to science and technology (for some of which he shared the credit with others) were the establishment of the absolute ("Kelvin") temperature scale, the laws of thermodynamics, and the principle of irreversible dissipation of energy; estimation of the sizes of individual atoms; mathematical formulations of the theories of electric and magnetic fields; and invention of the mirror galvanometer and other electrical and magnetic instruments. In the 20th century his reputation has been somewhat diminished by the ascendancy of James Clerk Maxwell's electromagnetic theory, which Kelvin vigorously opposed in favor of more elaborate and less successful ether models; and by the triumph of multibillion-year time scales for the earth and sun, making him seem foolishly dogmatic for trying to impose a much shorter time scale on geology and astronomy. The late-Victorian debate on the age of the earth is often mentioned (misleadingly) as a battle between Kelvin and Charles Darwin, posthumously settled in favor of Darwin.

Although Kelvin's life and work have been frequently discussed by historians of science, the new biography by Crosbie Smith and Norton Wise is the first to offer a comprehensive treatment of his scientific research and engineering projects. Smith and Wise have gleaned much information,



William Thompson, later Baron Kelvin of Largs, aged 52. [Reproduced on the dust jacket of *Energy and Empire* from *Nature* **14**, facing p. 385 (1876)]

some of it previously unpublished or known only to specialists, from careful study of the extensive holdings of letters and manuscripts at Cambridge and Glasgow. They also provide fascinating details about Kelvin's family and tell the sad story of Sabina Smith, who declined Kelvin's marriage proposal three times and spent the rest of her life regretting it. They have incorporated enough background material on the political history of Britain and Ireland to provide a useful context for Kelvin's own political opinions and activities; in particular they are able to make a plausible argument that Kelvin's elevation to the peerage was due not so much to his scientific achievements as to his role in organizing Scottish opposition to Irish home rule.

But the most important feature of this magnificent book is its technical analysis of Kelvin's work in physics. For example, the authors' explanation of how Kelvin developed his ideas about electricity and magnetism not only shows why Kelvin and other physicists resisted Maxwell's theory but provides valuable insight into the concepts presented axiomatically in modern textbooks. Physics teachers as well as historians can learn much from the chapters on the kinematics and dynamics of field theory.

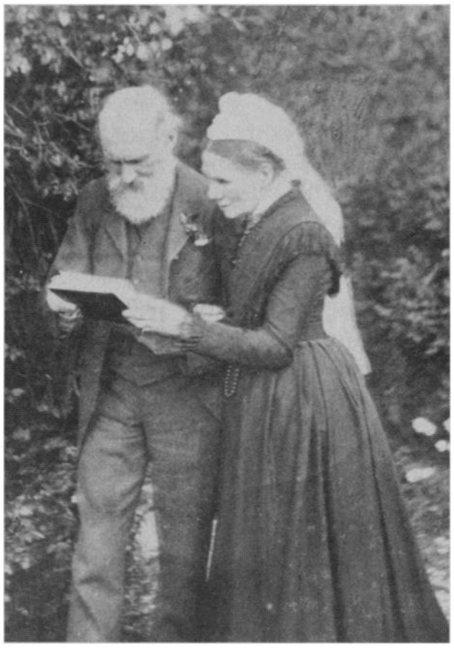
As the title of their book suggests, Smith and Wise see a connection between Kelvin's development of the energy concept in physics and his support for British imperialism. They even suggest that the decline of his mechanistic viewpoint in mathematical physics is a symbol of the "decline of the industrial spirit in Britain" (p. 491). But this is not a book about social or economic influences on science; scientific research itself, as conceived and executed by Kelvin, is at the center of the stage most of the time. Nor do the authors pay much attention to possible psychological factors; they ignore the significance of being a second-born son (discussed by Frank Sulloway at this year's AAAS meeting), and they do not suggest any connection between Kelvin's public declaration in 1852 that the world is inevitably running down and his private feelings about his final rejection by Sabina Smith in that year. It is also somewhat disappointing that they have turned up little new information about Kelvin's life before he went to Cambridge in 1841.

In addition to describing and interpreting Kelvin's own work, the authors add to our understanding of several of the other scientists with whom he interacted: George Gabriel Stokes, William Hopkins, Peter Guthrie Tait, and his brother James Thomson. (The reader who is interested in this topic should also consult David B. Wilson's *Kelvin and Stokes* [Hilger, 1987].) Again, I wish they could have found out even more about these interactions, especially with Hopkins (his tutor at Cambridge), whose work on physical geology was so clearly relevant to Kelvin's interest in the age of the earth that they must have had some intense discussions, still undocumented, on this topic.

In keeping with current fashions in the historiography of science, the authors discuss Kelvin's ideas and discoveries from a contextual (19th-century Britain) viewpoint, usually omitting any assessment from a presentist (20th-century) viewpoint. They avoid the practice (still followed by many scientists) of continually telling the reader whether a particular theory held by a scientist in the past is now considered right or wrong. Indeed, they have progressed so far beyond the presentist view, sometimes called the "whig interpretation of the history of science," that they can use the term "whig" in a completely different sense to describe Kelvin's physics and politics. But their contextualism is so narrow that we learn very little about Kelvin's influence on modern science or even on younger scientists active during his lifetime. This is not a criticism of the book under review, which is already long enough and filled with material that I would not want to have omitted, but a reminder that there is a need for yet another book to explore in comparable depth other important aspects of Kelvin's role in the development of modern science.

Contextualism seems to assume that there is an objective truth about the past—a history "as it actually happened" which, once discovered, does not have to be revised by later historians in the light of subsequent events. But whether or not such a history exists, we choose to study certain people and events at least partly because of their signifi-

"Uniting the Empire; labour, as well as capital, engineering, and electrical science, was of necessity employed in the submarine telegraph enterprises which linked Britain to her Empire. Here a stage in the vital Indo-European telegraph, which would employ patents such as Thomson's siphon recorder, involved landing the cable in the mud at Fao, Persian Gulf, in mid-1865." [Reproduced in *Energy and Empire* from *Illustrated London News*]



Kelvin "discussing a problem with his sister, Elizabeth." [Reproduced in *Energy and Empire* from A. G. King, *Kelvin the Man* (London, 1925)]

cance to us, not only because of their significance to their own time. (Thus American and British historians publish more books about 19th-century British physicists than about 6th-century Chinese emperors.)

In our century Lord Kelvin has come to represent a certain kind of thinking about science summed up in his two famous statements: (i) I can't really understand something unless I can make a mechanical model of it; (ii) if you can't measure something in numbers, your knowledge of it is not really scientific. Smith and Wise discuss the contexts of these statements but leave it to the reader to supply their consequences.

More recently Kelvin has been dragged into the creation-evolution controversy as an example of a great scientist who was also a creationist. (The creationists have difficulty finding any respectable living scientists who support their views.) When history is abused in this way it is up to the historian to point out that, while Kelvin thought natural selection was inadequate to explain evolution, he also rejected creationism, "never aligning himself with biblical literalists and anti-evolutionists" (Smith and Wise, p. 634). In fact Kelvin's cosmology was evolutionary in the broad sense, while allowing a place for divine guidance.

I am grateful for the intellectual feast provided by *Energy and Empire*; and I am hungry for more.

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Persuasion at a Distance

Shaping Written Knowledge. The Genre and Activity of the Experimental Article in Science. CHARLES BAZERMAN. University of Wisconsin Press, Madison, 1988. xii, 356 pp. $40; paper, $17.50. Rhetoric of the Human Sciences.

This is an old-fashioned book best described using compliments that have been devalued. It is about rhetoric in a positive sense of the word: about eloquent and persuasive writing in the experimental sciences. Eloquence doesn't refer to flowery embroidery but to effective composition. Scientific rhetoric has become a trendy topic, replete with well-funded conferences. Much of that activity is skeptically anti-scientistic, intended to reveal and undermine the sources of scientific authority. Charles Bazerman is well informed about current "social construction of scientific facts" schools of philosophical sociology of science, but he is himself an English teacher—in the best sense of that label, for he tries to teach people how to write well and has published manuals and studies of reading and writing. In fact, he concludes his present book with a chapter called "Writing well, scientifically and rhetorically," but this is not a manual. It is a study of how the criteria for good writing of experimental science came into being. It includes for analysis samples of fine writing, Compton on his effect, for example. But they are not used as models, but to discover how the writing persuades. What, in a field, at a time, enables the writer to succeed, especially when readers are asked to believe new facts, even those contrary to expectations?

Eloquence is not a timeless relation between reader, writer, and content. The scientific magazine came into being at a definite moment—most of us think of the *Philosophical Transactions of the Royal Society of London*, 1665, although there are rivals. The same is true of specialist journals, closely associated with professional societies or subgroups and hence part of the history of professionalization. In each type of publication styles of writing have evolved. That means that standards of excellence have changed.

How did styles and standards evolve, why, and in response to what interests? Bazerman keeps a good balance between two violently opposed pictures. One presents the writing up of an experimental result as a final, necessary, and perhaps tedious job for the record or for professional advancement. The other locates all the real work of experimental science in the transformation of inscriptions from the initial scribbles in a notebook, tracings of a marker on graph paper, or the first printouts, through the transparencies at the talk, via preprint, to published paper, the author all the while embracing allies and overwhelming any opposition; scientific writing should be seen as a political act. Bazerman is well informed about the second picture, which goes along with ideas of "social construction of scientific facts," but he is addressing readers who incline to the first picture without realizing the extent to which scientific writing is a collection of specific styles, with an instructive history, whose forms are essential to the growth of knowledge.

Bazerman proceeds in two ways, case studies and literature surveys. The surveys are happily free of phony methodology. He reads every fifth volume of *Phil. Trans.* up to 1800. He gets a sense of the publication practices of memorable scientists (book or periodical?) by skimming the A's and B's in the *Dictionary of Scientific Biography*. In modern times, 1893–1980, a subject is chosen, spectroscopy, a premier journal, *Physical Review*, and articles and even sentences are selected in the same unpretentious way. These surveys will, I think, confirm the subjective opinions of people familiar with the field, although as rhetorician Bazerman focuses on unexpected or seemingly trifling aspects of articles. It is the case studies that motivate a study of those aspects, and for many readers they will carry the book.

Why should anyone believe what the experimenter reports? The truth must be transparent, beyond question. A kind of writing empowered with authority had to be created. In the beginning experiments were demonstrated at a society meeting. The very rank of the witnesses was important to credibility: it helped to have a king in the company. This model entered crisis with what Bazerman regards as the first truly important experimental report in *Phil. Trans.* (1672), Newton's demonstration of "the phenomena of the colours." These are ill suited to public demonstration, requiring a large pitch-dark room and a point source of light. Newton casually talks of letting in a ray of light from the sun through his shades, but a successful result is (to almost everyone's surprise) powerfully difficult to achieve. Notebooks and lecture notes confirm that Newton had deep theoretical concerns, but he insisted he was only reporting what he saw. He wanted us to be "as it were" in the rooms of the Royal Society seeing it all—but we weren't. His critics, especially the tenacious Hooke, distressed him. He angrily replied, but never again published in a journal (aside from a squib on temperature), reserving himself for the *Optiks* (1704). Bazerman gives a new direction to this well-known story. The experimenter must forge new ways of being convincing, given that