letters

become symbolic of the controversial issues of the Regents–DOE contracts and the matter of nuclear-arms control. It seems to me that distortion of history, abrogation of the 1958 regents' intent to honor Lawrence and creation of identity problems for the Lawrence Berkeley Laboratory and the Lawrence Hall of Science add up to more than enough reasons to remove the name from Livermore whether or not the university continues to operate the labs. I would welcome any suggestions or advice your readers would care to offer.

MARY B. LAWRENCE
Berkeley, California
2/86

More on Maxwell

The August issue of PHYSICS TODAY carried a review (page 66) of my book *James Clerk Maxwell: A Biography* by Daniel Siegel. The first half of this review was quite flattering, and it may seem ungracious of me to submit a rebuttal. The point is, of course, that in the second half of his review, Siegel offers what he believes to be historical criticism of my views on Maxwell's work, which I cannot let pass without comment.

Siegel implies that I have taken Maxwell's thinking out of its historical context and presented the reader "not with the real Maxwell, who was a 19th-century mechanist, but with an imagined Maxwell, who is in fact a 20th-century physicist transported into the 19th century." This, Siegel pronounces, "is not good history."

When Maxwell wrote his great papers on electromagnetism (1861–62), he introduced "molecular vortices" in the ether, with small spherical particles acting as "idle wheels" between them. Siegel's main point seems to be that Maxwell considered the former a "probable" hypothesis and the latter "awkward." This, it would appear, brands Maxwell as "a 19th-century mechanist." My failing to underline these "fine distinctions that Maxwell was so careful to make" is, apparently, the root of my historical error.

These are indeed fine distinctions and do not—in my opinion—support the weighty conclusions Siegel bases upon them. In my book I give a number of quotes from Maxwell to substantiate the opposite thesis—as when Maxwell says that he "merely [wishes] to direct the mind of the reader to mechanical phenomena which will assist him in understanding the electrical ones. All such phrases in the present paper are to be considered as illustrative, not explanatory" ("The dynamical theory of the electromagnetic field," 1862). Siegel and I could doubtless bandy properly selected Maxwell quotes back and forth *ad nauseam*, and reach no satisfactory agreement. When it comes to the uses of models and analogical thinking, I believe that Maxwell was, indeed, a generation ahead of his contemporaries (such as Lord Kelvin, who *was* a 19th-century mechanist!).

I do not feel myself entitled to pronounce on what is or is not good history, nor do I think myself privy to definitive insights into the workings of Maxwell's imaginative genius. I will, however, offer the reader—and Siegel—the following observation. Maxwell's *A Treatise on Electricity and Magnetism* was published in 1873 and contains, one must assume, his mature (final?) views on these matters. Of the 866 sections comprised by the *Treatise*, only sections 822–831, devoid of diagrams, offer a formal discussion of the hypothesis of molecular vortices. In the penultimate paragraph of section 831, referring to his 1861–62 papers, Maxwell has this to say:

> The attempt which I then made to imagine a working model of this mechanism must be taken for no more than it really is, a demonstration that mechanism may be imagined capable of producing a connexion mechanically equivalent to the actual connexion of the parts of the electromagnetic field. The problem of determining the mechanism required to establish a given species of connexion between the motions of the parts of a system always admits of an infinite number of solutions.

Caveat lector?

IVAN TOLSTOY
Knockvennie
Castle Douglas
SW Scotland
1/86

SIEGEL REPLIES: I would agree heartily with Ivan Tolstoy that one cannot decide a historical issue by "bandy[ing] properly selected . . . quotes back and forth." In particular, the issue of Maxwell's stance with respect to the mechanical world view is complex, and certainly not amenable to decisive resolution by means of a few choice quotations. My own views on the matter are developed at length in an article entitled "Mechanical image and reality in Maxwell's electromagnetic theory," in *Wranglers and Physicists: Studies on Cambridge Mathematical Physics in the Nineteenth Century* (P. M. Harman, ed., Manchester U.P., Manchester, 1985, page 180). In brief, I argue there that Maxwell began in the 1850s by using mechanical models in an analogi-

cal sense—for purposes of illustration rather than with realistic intent; that he proceeded in the early 1860s to develop a substantial commitment to the realistic status of a particular mechanical model of the electromagnetic field, based on the idea of "molecular vortices" in the medium pervading space; and that subsequently, in the mid-1860s, he began a measured retreat from his realistic commitment to the molecular-vortex model, without ever completely giving it up.

The extant record of this intellectual journey provides material that can be used to support a wide variety of historical conclusions through selective quotation. Good practice in historical writing, however, demands, above all, balance, and it is this that is lacking in Tolstoy's presentation. His reporting that Maxwell viewed one part of the molecular-vortex model as "awkward" and "provisional," while neglecting to tell the reader that Maxwell viewed another part of the model as a "probable" hypothesis, is just one example of this lack of balance. The quotation with which Tolstoy closes his letter provides another example. For balance, it should be accompanied by the preceding paragraph in Maxwell's *A Treatise on Electricity and Magnetism* (3rd edition, 1891, reprinted by Dover, New York, 1954):

> I think we have good evidence for the opinion that some phenomenon of rotation is going on in the magnetic field, that this rotation is performed by a great number of very small portions of matter, each rotating on its own axis, this axis being parallel to the direction of the magnetic force, and that the rotations of these different vortices are made to depend on one another by means of some kind of mechanism connecting them.

To quote Maxwell's reservations concerning the possibility of specifying the mechanism connecting the ether rotations, while entirely neglecting the neighboring passage expressing his continuing commitment to the basic hypothesis of rotating parcels of ether, does not make for good history. It is only when the two passages are put together that one gets a balanced picture of Maxwell's final stance with respect to the molecular-vortex model, and the general conclusion toward which the combined passages point is that Maxwell remained basically within the mechanistic camp, although with significant reservations.

DANIEL M. SIEGEL
University of Wisconsin
Madison, Wisconsin
8/86

Physics-information group?

In November 1984 the council of The American Physical Society approved the formation and operation of topical groups with the purpose of supplementing areas of physics not encompassed by one of the society's divisions. A topical group may be established by the council upon petition by 20 members of the society.

I would like to hear from APS members who might be interested in forming a physics-information and documentation topical group whose purpose would be to further the generation, organization and dissemination of physics information. Meetings and programs of the topical group would be held at least annually, possibly in conjunction with the regular APS meetings.

Those physics-information specialists, physics librarians and other APS members interested in forming a topical group should contact me at the address given below.

ALFRED J. HODINA
Sciences–Engineering Library
University of California
Santa Barbara, CA 93106
2/86

Aharanov–Bohm effect

I enjoyed very much the commendable story by Bertram Schwarzschild on the Aharanov–Bohm effect (January, page 17). Just a month earlier, in the same column, he treated us to a magnificent description of Klaus von Klitzing's discovery of the quantized Hall effect. Although the results sought after are quite different, I could not help speculating on the similarities between these two experiments:

- ▶ Both theories are built on the fundamental "flux quantum" h/e, where h is Planck's constant and e is the charge of the electron.
- ▶ Both theories predict magnetoresistance oscillations with increasing magnetic field occurring with a flux periodicity of h/e.
- ▶ The two experiments were carried out in crossed magnetic and electric fields similar in magnitude and geometry.
- ▶ Both devices may be considered "mesoscopic": One has a diameter of 1 micron while the other has an effective width of 50 microns.
- ▶ The two experiments were performed at low temperatures of the same order of magnitude.

The most remarkable comparison is that the materials and the shapes of the devices are quite dissimilar: One is a

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