letters

Aptly named Aharonov–Bohm effect has classical analogue, long history

Writing about what is usually termed the Aharonov–Bohm (AB) effect, Peter Sturrock and Timothy Groves argue (PHYSICS TODAY, April 2010, page 8) that the same physics was discovered a decade earlier[1] and should rightly be called the Ehrenberg–Siday effect. I agree that Werner Ehrenberg and Raymond Siday deserve recognition for their anticipation of AB. Indeed, in recent talks celebrating the 50th anniversary of AB, I began by describing the unfairly neglected paper by Ehrenberg and Siday. Nevertheless, I have come to a different conclusion from Sturrock and Groves: The expression "Aharonov–Bohm effect" is justified, for two reasons.

First, although there is no doubt that the work by Ehrenberg and Siday anticipated how inaccessible magnetic flux can influence electron interference, that was as a curiosity, at the end of a paper whose main emphasis was the Hamiltonian analysis of electron optics. By contrast, Yakir Aharonov and David Bohm emphasized from the start, as an essential and general aspect of quantum mechanics, the physical influence of inaccessible fields that act nonlocally through the vector potential.

Second, Ehrenberg and Siday's semiclassical approximation—essentially applying the Dirac magnetic phase factor to electrons traveling on either side of the flux—implies a wavefunction that is multivalued and therefore not the correct solution of Schrödinger's equation. The lack of a single-valued wavefunction leaves their prediction open to doubt. By contrast, Aharonov and Bohm derived the exact single-valued solution for waves scattered by a flux line. Recently, it was shown that the Ehrenberg–Siday approximation corresponds to the first terms in a "many-whirls" representation that treats the exact AB wavefunction as a topological sum over paths circling the flux.[2]

Attribution of credit is a delicate matter. It tends to excite strong feelings, and I write about it reluctantly. But the Ehrenberg–Siday paper does seem to exemplify the unfortunate phenomenon identified by Alfred North Whitehead in a 1916 address to the British Association for the Advancement of Science: "Everything of importance has been said before, by someone who did not discover it."

In a companion letter in the April 2010 issue, Alexander Ershkovich correctly points out that the AB effect is present in classical Hamiltonian mechanics, even though remote magnetic fields cannot influence Newtonian trajectories. He advocates a "search for experiments that might prove . . . a classical analogue of the Aharonov–Bohm effect." Such an experiment exists already. In the classical physics of waves on a moving medium, the flow velocity acts like the vector potential in quantum mechanics, so the flow vorticity acts like the magnetic field; the analogy is precise. Fine details of the AB wavefunction were observed in ripples on the surface of water swirling irrotationally into a bathtub vortex,[3] whose core is the analogue of inaccessible magnetic flux.

References

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I am an author of the 1961 paper that first called the Aharonov–Bohm effect by that name.[1] The term has since appeared in more than 1000 published papers. However, it has been suggested a few times, most recently by Peter Sturrock and Timothy Groves, that the name does not do justice to Werner Ehrenberg and Raymond Siday, who found in 1949 that the motion of electrons can be influenced by magnetic fields confined to regions that the electrons do not enter.[2] I argue that the name Aharonov–Bohm effect is appropriate, although for a reason that I did not fully appreciate in 1961.

The 1959 Aharonov–Bohm paper profoundly changed the way we think about electromagnetic fields in quantum mechanics.[3] The gauge-invariant part of the vector potential was promoted to a real physical field, not just a convenient device for summarizing certain information about the electric and magnetic fields. In the words of C. N. Yang, "The electromagnetic field strength $f_{\mu\nu}$ in quantum mechanics underdescribes electromagnetism, as the Bohm–Aharonov effect demonstrates . . . information about the phase factor $\exp[ie\oint(\mathbf{A}\cdot d\boldsymbol{x})/\hbar_c]$ for all closed loops correctly describes electromagnetism."[4]

A century earlier, James Clerk Maxwell changed the way we think about action at a distance by identifying what we now call the Maxwell fields as real physical things that contain energy and momentum and that enable microscopic conservation laws; they are not just mathematical functions that summarize the necessary information about the past motions of charges.

Later those physical fields had to be quantized. Yakir Aharonov and David Bohm found that in quantum mechanics the Maxwell fields in a multiply connected region do not contain all the physics; the vector potential must also be endowed with reality to make sense of the subtler interactions in quantum mechanics.

Neither Aharonov and Bohm nor Ehrenberg and Siday were the first to observe that magnetic fields in places where a charged particle's wave function vanishes may influence the motion of that particle.[5] Paul Dirac, in his 1931 paper on magnetic monopoles, noted that the electron's wavefunction must vanish on singular flux lines but said nothing about that raising any question

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of nonlocality. Fritz London pointed out in the 1930s that the motion of electrons in a superconducting ring depends on an external magnetic flux through the hole in the ring, where the electrons cannot go. He wrote in his 1937 paper, "The most stable state of a ring has no current, unless an external magnetic field is applied." Like Dirac, he said nothing about a nonlocal action of the magnetic field being surprising or unusual. Of course, London, like Dirac, was focusing on something else.

Ehrenberg and Siday were also focusing on something else—electron optics—when they found in 1949 that the motion of an electron can depend on the magnetic field in a region from which the electron is excluded. They chose not to mention that curious phenomenon in the abstract of their paper, although they did explicitly say elsewhere that it was "curious."

None of those earlier authors went on to conclude that such a phenomenon implies that the vector potential has to be seen as a real physical field in quantum mechanics. Only Aharonov and Bohm did that.

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Higher standards combat culture shock in medical physics

I can sympathize with Gregory Davis, who laments the new requirements for entering medical physics (PHYSICS TODAY, March 2010, page 10), but there is another side to the story. I suffered culture shock when I entered the field from "pure" physics 20 years ago. I went from a world where the language of advanced mathematics was understood to one where few people knew what a cosine was and many (not the physicists, but most of the other hospital staff) had to struggle to recall the Pythagorean theorem. Conversely, my new colleagues talked with ease about anatomy, medical instruments, and medical procedure, while I felt lost and inept. It took the better part of a decade for me to really feel that I was in command of my subject.

The new requirements are an attempt to reduce that transition shock and to better prepare new entrants to the field. They may not be a perfect fix, but at least they are a start. The fact is, medical physics is far more medical than physics, and it will continue to move in that direction. I can see a day when medical physics will be considered a medical specialty and not a physics specialty at all.

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As a board certified practicing medical physicist I was disappointed with Gregory Davis's remarks regarding the changing requirements to practice in my field. His assertion that the bar is being raised in order to limit practitioner numbers and thereby raise salaries for current practitioners is farfetched. The bar is being raised to bring the training of medical physicists in line with that of other practitioners represented by the American Board of Medical Specialties. Medical physicists are one of the few nonphysician groups represented.

When I finished a medical physics graduate program accredited by the Commission on Accreditation of Medical Physics Educational Programs in the early 1990s, I was considered a medical physicist, but I was in no position to function independently in a clinical environment. I was fortunate to work in a consulting group with a mentor who made the time and had the patience to properly train me while providing me with employment. The responsibilities of the clinical medical physicist in a therapy setting include ensuring the absolute calibration of a linear accelerator capable of delivering lethal amounts of radiation, consulting with radiation oncologists on the development of optimal treatment plans, and measuring the equipment's radiation characteristics for sophisticated computerized modeling to generate accurate representations of delivered dose. In a nutshell, medical physicists are solely responsible for the safe and optimal use of the equipment and the accurate and precise delivery of the prescribed amount of radiation to the patient. Davis's assertion that a physics degree is versatile is correct; however, it alone is not sufficient to prepare a person for clinical responsibilities.

As to Davis's claim that there is no evidence of threats to public safety, several newspaper articles by Walt Bogdanich that have appeared in the *New York Times* this year tell a different story about the consequences of medical physicists' errors.[1] In my opinion, the best way to minimize those errors is to standardize the education and training of medical physicists: Uniform graduate education, residency, and board-certification requirements will help ensure a candidate's competence for independent practice. Radiation oncologists, medical doctors who define the volume to be treated and prescribe the quantity of radiation to be delivered, are already expected to meet those requirements.

If I or a family member needed radiation therapy, I would want a board-certified medical physicist to review the treatment plans and calculations and to calibrate the equipment. The medical physicist is the sole individual in the clinic to attest to accurate and precise delivery of radiation treatments.

For more information see the position statement at the American Association of Physicists in Medicine website, http://www.aapm.org/publicgeneral/StatementBeforeCongress.asp.

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A fine point on topological insulators

Although I found the article "The Quantum Spin Hall Effect and Topological Insulators" by Xiao-Liang Qi and Shou-Cheng Zhang very interesting (PHYSICS TODAY, January 2010, page 33), I was disturbed to read on the second page that "cadmium telluride . . . has a similar lattice constant but much weaker spin–orbit coupling" than mercury telluride. The authors then attribute to this erroneous statement the *s–p* gap inversion of HgTe. Because of the rather topical nature of topological insulators, and to prevent propagation of the error, I believe it should be corrected. I also want to set the record straight concerning Steven Groves and his thesis adviser, William Paul, whose discovery of the inverted gap of α-tin,[1]