Berry that introduced the geometric, now known as the Berry phase.

David Bohm, Yakir Aharonov, and Berry were all present. I particularly recall a plenary talk given by Bohm. In it, he expressed astonishment that certain physicists refused to accept the AB effect and even went to great lengths to try to disprove it. As he said, “It would be much more revolutionary for this effect to be wrong than for it to be right,” since it was a clear consequence of fundamental quantum mechanics.

I considered his remark very Bohmian, in that (a) no one else would have said it that way, and (b) once uttered, it was obviously true. Bohm's habit, in his soft-spoken way, of making such blindingly original statements is one of the things I most remember about this unorthodox and profound thinker.

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Batelaan and Tonomura reply: Werner Ehrenberg and Raymond Siday did propose the magnetic version of what is now called the Aharonov–Bohm (AB) effect,[1] as Peter Sturrock and Timothy Groves point out. We had included this reference in the early versions of our manuscript. However, limited space directed the focus of the paper to the “effect without a force” discussion, rather than a historic perspective.

Alexander Ershkovich notes that in the Hamilton–Jacobi formulation of classical mechanics, both the action and the Hamiltonian depend on the vector potential; he ponders whether the AB effect might have a classical manifestation. Newton's formulation of classical mechanics is equivalent to the Hamilton–Jacobi formulation. Because the absence of a field means the absence of a force in Newton's formulation, classical trajectories are unaffected. That result is not expected to change in the equivalent Hamilton–Jacobi formulation. Thus the AB effect is usually considered to be a pure quantum effect. On the other hand, we may interpret “classical mechanics” in a broader sense, such as in general relativistic classical mechanics. In electrodynamics classical trajectories are not affected by a localized magnetic field through which they do not pass. However, considering that the energy content of a current-carrying solenoid is larger than that of one without current, the trajectory is clearly affected gravitationally, at least in principle. Though not due to the AB effect, that result elucidates that a generalized description may lead to other insights. The Hamilton–Jacobi equation may be an example of a theoretical vehicle by which to explore generalizations such as relativistic effects, separation of variables, multiple particle effects, or the classical limit of the de Broglie–Bohm theory.

C. Alden Mead recollects interesting statements made by Bohm. We agree fully with Bohm's statement that “it would be much more revolutionary for this effect to be wrong than for it to be right.” Attempts to disprove the AB effect should be seen for what they are, outright attempts at finding limits to the validity of quantum mechanics itself. And although quantum mechanics is unfinished with respect to, say, decoherence theory and quantum gravity, the AB effect appears to be well within its validity range.

We do not share Bohm's astonishment that, as Mead relates, “certain physicists refused to accept the AB effect and even went to great lengths to try to disprove it.” Rather, to risk overusing a platitude, extraordinary phenomena should be exposed to extraordinary scrutiny. Failed attempts to disprove an idea often provide insight into its fundamental character. In that context, we reiterate the main message of our article. Many facets of the AB effect—for example, the electric version, the dispersionless nature, relativistic momentum conservation, the relation to the Mott–Schwinger effect, and the AB effects for other than electromagnetic gauge-invariant theories—need exploration. We predict a bright future for the AB effect, with many surprises to come.

Reference

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Tools, trophies in interactive learning

The article by Edward Prather, Alexander Rudolph, and Gina Brissenden (PHYSICS TODAY, October 2009, page 41) makes several interesting points regarding the effectiveness of interactive learning environments in introductory astronomy courses. Yet two serious points regarding the instrumentation and methodology used in the study they discuss appear to cast doubt on the results and subsequent conclusions.

The first point concerns the use of the Light and Spectroscopy Concept Inventory. The authors state that "the LSCI is capable of measuring changes in student understanding and, by extension, the effectiveness of teaching about light and spectroscopy in Astro 101" (page 45). Underpinning the pertinent discussion in their article is the assumption that increasing students' content knowledge and understanding is the goal of the Astro 101 course. However, instructors can have different educational objectives—for example, general science literacy, development of critical thinking skills, a reduction of science anxiety, or the linking of science to society and everyday life.[1] Astronomy content, rather than being the primary emphasis of the course, may be used as a vehicle to discuss science more broadly.

In addition, light and spectra make up one small portion of the Astro 101 course, and not all instructors may allot equal classroom time or cover the topic in equal detail, so students' opportunities to learn the concepts assessed by LSCI may vary greatly. The authors initially acknowledge some limitations of the LSCI, but the discussion quickly moves to teaching effectiveness in general.

The second point concerns the survey that is used to determine the Interactive Assessment Score (IAS), a measure of the "nominal percentage of time . . . spent on interactive learning strategies during the term" (page 45). The survey provided by the authors[2] relies on instructor-reported data on typical classes and term averages. As a result, it is not clear whether the split the authors make in figure 4 of their PHYSICS TODAY article between an IAS less than 25% and an IAS greater than or equal to 25% indicates a substantially different level of interaction in the classroom and whether the split actually occurs when light and spectra are discussed.

Simply moving the split point to around 30% creates more even sample sizes and still yields a statistically significant result (according to the data in figure 4) but dramatically reduces the strength of the relationship, as indicated by Cohen's d, for example, a measure of effect size. One is left to wonder how much of the authors' conclusion is thus due to artifacts in the data rather than to real effects.

In addition, the authors mention that they removed from the sample all classes with fewer than 25 students because they "believe that the teaching and learning in classes with a very small number of students can be a special case, bordering on personalized instruction" (page 45). They offer no research evidence for that belief, nor do they explain why the limit was set at 25 students; it is not clear whether the removal of those data is warranted. Arguably, smaller classes may have more opportunities for interactive elements—for example, class discussions—both among the students and between students and instructor. Supposedly, such classes would be in the higher end of the self-reported IAS range. Inclusion of the smaller classes thus has the potential to strengthen or weaken the authors' result.

Although the educational literature leaves little doubt about the benefits of interactive elements in class and the benefits faculty receive from having access to professional development opportunities, this study does not present a strong enough case, given the uncertainties in and assumptions of the instrumentation used.

References

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2. E. E. Prather, A. L. Rudolph, G. Brissenden, W. M. Schlingman, *Am. J. Phys.* **77**, 320 (2009).

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In the late 1940s, my father, Toivo E. Rine, professor of mathematics at Illinois State University, asserted that interactive learning strategies could help students better understand basic concepts in applied astrophysics and astronomy. Rine used his interactive method of teaching astronomy, navigation, and survey instruments to motivate undergraduate nonastronomy majors. An additional higher-level course showed students how to do surveying and navigation using astronomy methods; that course was a module of a much more extensive one Rine pioneered for the US Navy on the Illinois State campus and for navy personnel entering World War II. Many of Rine's students who entered the war easily applied his interactive techniques to learn navigation and instrumentation.

His teaching strategies evolved over the years until shortly before his death in April 1964. Afterward, the Illinois chapter of the National Council of Teachers of Mathematics established the T. E. Rine Award, presented annually to a teacher who applies the motivational teaching techniques he pioneered.

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Learning too well from Wheeler

Tony Zee's poignant recollection of his association with John Wheeler (PHYSICS TODAY, October 2009, page 10) included the comment that he learned to "never calculate without first knowing the answer." Perhaps, like me, he learned that lesson too well—we both missed the opportunity to calculate the beta functions for non-abelian gauge theories.

At the time, the beta functions for all known field theories had a positive sign. It was also known, however, that the scaling observed in deep inelastic scattering would be comprehensible if the field theory of the strong interaction, then still unknown, had a negative beta function.

My thesis adviser, Tom Appelquist, specifically suggested the calculation for non-abelian theories to me more than a year before it was done by the Nobel Prize–winning trio of Frank Wilczek, David Politzer, and David Gross (PHYSICS TODAY, December 2004, page 21). Having had an excellent graduate education at Harvard University, I responded immediately by asking, "Is there any reason to believe that it has the opposite sign?" Of course, no one had any idea then, and unfortunately, it took me about a decade to come up with the physical argument that implies the correct answer without explicit calculation.[1]

Tony has probably suffered even more for having calculated the beta function for every other known case even earlier. So it would seem that at least occasionally, there is also a good argument for calculating even when you don't know the answer—particularly when there is a discovery waiting to be made.

Reference

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