The Value of Einstein's Mistakes

Steven Weinberg's love for and knowledge of history inform his instructive sampling of Albert Einstein's mistakes (PHYSICS TODAY, November 2005, page 31). One mistake, or at least one tantalizing omission, seems worth adding to the collection. In a May 1905 letter to Conrad Habicht, Einstein wrote that he thought his revolutionary contribution was the hypothesis that light consists of particles.[1]

Consider his lifelong passion for unification, as in his resolution of the clash between Isaac Newton's mechanics and James Clerk Maxwell's electrodynamics (with the special theory of relativity modifying the former). It is hard to believe that Einstein did not worry about reconciling the well-established wave aspects of light with his new particle hypothesis. If he had pursued that connection, he could have developed one-photon quantum mechanics in 1905 or shortly afterward, by combining the Poynting-vector expression for the power intensity of light with his own relation between frequency and energy of a particle to obtain the photon-number intensity of a light beam. The wave equation is the Maxwell equations, and the probability interpretation pops up immediately.

Many observers have said that general relativity was one advance that would have taken a very long time without Einstein, but we have no direct test for that statement. However, if you accept my argument that Einstein could have developed the first true quantum mechanics, then we can say exactly how long it took the physics community to catch up—20 years for Heisenberg's matrix mechanics and Schrödinger's mathematically equivalent wave mechanics.

Letters and opinions are encouraged and should be sent to Letters, PHYSICS TODAY, American Center for Physics, One Physics Ellipse, College Park, MD 20740-3842 or by e-mail to ptletter@aip.org (using your surname as "Subject"). Please include your affiliation, mailing address, and daytime phone number. We reserve the right to edit submissions.

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As Steven Weinberg points out, it's a good thing for people to understand that even the greatest scientists make mistakes. However, I think Weinberg grossly understates the issue. Maybe his article should have been titled "Einstein's Published Mistakes."

The practice of science, as PHYSICS TODAY readers surely know, involves making mistakes, realizations, corrections, and more mistakes. Trial and error is a fundamental part of the process. I think that point deserves emphasizing. Too many of our schoolchildren learn to avoid invention and new thinking because they have been convinced that making mistakes is shameful.

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In his thoughtful and timely article, Steven Weinberg analyzes some of Einstein's mistakes and notes some others. Another fundamental conceptual mistake is hidden in Einstein's celebrated 1905 paper on relativity.

In a lengthy discussion in the first part of that paper, Einstein showed that the speed of light can be made constant by adopting a clock synchronization based on two-way light signals. With that synchronization, measurements of the one-way speed of light become logically circular, and Einstein later declared that the constancy of the speed of light was "neither *a supposition nor a hypothesis* about the physical nature of light, but a *stipulation* which I can make at my free discretion to arrive at a definition of simultaneity."[1]

However, Einstein overlooked that the validity of Newton's laws at low speeds in each reference frame permits the use of simple mechanical methods of synchronization, such as slow clock transport or sound signals. Einstein's synchronization procedure with light signals is thus superfluous—it plays no fundamental role and is merely the most convenient of several possible synchronization procedures. Furthermore, if clocks are synchronized by slow clock transport or by some other mechanical procedure, then measurements of the one-way speed of light are not logically circular, and those measurements provide an unambiguous experimental test of the constancy of this speed. In fact, clock transport has been used in such experimental tests.[2,3] Einstein should have considered the implications of alternative synchronization procedures for the conceptual foundations of relativity, and he should have recognized that the constancy of the speed of light had to be established by experiment, not by stipulation.

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Steven Weinberg writes, "Einstein rejected the notion that the laws of physics could deal with probabilities, famously decreeing that God does not play dice with the cosmos. But history gave its verdict against Einstein—quantum mechanics went on from success to success, leaving Einstein on the sidelines."

Einstein did not reject quantum theory merely because it is probabilistic. He wrote: "There is no doubt that quantum mechanics has seized hold of a beautiful element of truth, and that it will be a test stone for any future theoretical basis."[1] Nor was Einstein unilaterally opposed to God playing dice. He expected God to either play dice all the way or not at all. If individual events were totally undetermined, then the overall events should also be undetermined, and not display remarkable regularity. "In for the penny, in for the pound," he wrote. Thus, a more accurate quote from Einstein about God and dice playing is the following:

"That the Lord should play with dice, all right; but that He should gamble according to definite rules, that is beyond me."[1]

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I enjoyed Steven Weinberg's article except for the not-so-subtle knock on religion at the beginning, where he refers to "other supposed paths to truth," and the subhead, "Science sets itself apart from other paths to truth by recognizing that even its greatest practitioners sometimes err." If the point of the article is to show the superiority of science over other "supposed paths," Weinberg confuses the issue by ending with the claim that Einstein "made no mistakes" in his decisions about "great public issues," including his opposition to militarism, his refusal to support the Stalinist Soviet Union, and his enthusiastic Zionism. Since none of those public issues are ones in which science alone can provide answers, how did Einstein achieve such infallible knowledge about them without relying on paths to truth other than science? With all due respect for his undoubted genius in science, I think Weinberg's hostility to religion is blinding him to errors in elementary logic.

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How unfortunate that Steven Weinberg chose to insert a criticism of religion—"other supposed paths to truth"—in his article. That Einstein was not infallible seems to have little relevance to the question of whether the prophets of various religions are infallible, and the latter question seems to have little place in a piece about Einstein.

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While I very much enjoyed Steven Weinberg's article "Einstein's Mistakes," I am puzzled by the author's statement about quantum mechanics: "The difficulty is not that quantum mechanics is probabilistic—that is something we apparently have to live with. The real difficulty is that it is also deterministic, or more precisely, that it combines a probabilistic interpretation with deterministic dynamics."

Quantum mechanics is an acausal deterministic theory in the sense that a physical system's state (mathematically described by a state vector) at a given initial time determines its state at a specified later time, but its state is not in one-to-one correspondence with sharp values of all its dynamical variables; that correspondence is probabilistic. Therefore events, identified by sharp values of those variables at one spacetime point, are not causally connected with other events. That is something we have to live with.

Why does the combination of these two attributes—acausality and determinism—constitute a special difficulty? Weinberg asks, "So where do the probabilistic rules of the Copenhagen interpretation come from?" Why do they have to come from anywhere other than from human brains? Nature exists out there, independent of human thought, but its mathematical description surely is a human construction rather than an immutable law given to us on a stone tablet.

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Einstein should be allowed his mistakes, like the rest of us, and Steven Weinberg understandably points out only the most newsworthy. I write to point out another misunderstanding—mistake, if you will—in Einstein's work only because it is often found in the literature today.

Einstein described diffusion as the motion of neutral particles on atomic (Brownian) length and time scales. He used a stochastic differential equation—a Langevin equation—in the high-friction limit to describe diffusive trajectories. Einstein did not discuss how his treatment could accommodate macroscopic boundary conditions or produce macroscopic flow, which is, after all, what Fick's law of diffusion is all about.

Langevin equations, in the spirit of Einstein's work, are widely used today to describe the motion and fluctuations of density of charged particles in, for example, aqueous solutions. The electric force in those equations is usually described by a steady function. Fluctuations in number density of charged particles are allowed in Einstein's treatment but fluctuations in net charge and electric potential are not. Traditional Langevin equations of Brownian motion seem inconsistent with the idea that charge creates electric force and so are unlikely to be helpful, at least in my view. It is hard to imagine systems in which the number density of ions can fluctuate while the number density of charge does not.

I believe Einstein's description of Brownian motion must be coupled to equations describing the electric field when the diffusing particles have significant charge. An equation is needed to show how the charge on one particle creates force on another. The ink particles studied by Robert Brown were surely charged. The fluctuating electric field and stochastic flow can be computed from the density of ink particles, ions, and solvent molecules by solving Poisson's or Maxwell's equations together with flow equations. (Spatially inhomogeneous boundary conditions are needed to force the macroscopic flow described by Fick's law.)

This so-called self-consistent treatment of diffusion and the electric field is used in computational electronics to design the transistors and integrated circuits of our electronic technology.[1] Diffusion and the electric field have not been treated self-consistently in most of computational chemistry and biology—for example, in simulations of molecular dynamics of ions or proteins—although such treatments are found in analyses of ionic motion through protein channels.[2–5]

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The fascinating article recounting Einstein's mistakes at different stages of his career goes beyond the usual focus on the cosmological constant and quantum mechanics. In particular, the discussion of Kaluza–Klein theory examines Einstein's later attempts at a unification theory. But in the course of developing general relativity, Einstein made another assumption, which he later tried to revisit—one that future generations may come to regard as Einstein's greatest "mistake."

Curvature of spacetime is, of course, related by general relativity to the presence of mass-energy. This curvature, though it plays out in the arena of four-dimensional spacetime, corresponds to our intuitive understanding of geometric curvature in three dimensions. General relativity also makes a crucial assumption that another geometric object, called the torsion, vanishes. That is not the only assumption that could have been made, however, and as Einstein explored extensions of general relativity after 1915, he reevaluated his initial assumption.

In the 1920s and 1930s, Einstein collaborated[1] with the eminent French mathematician Elie Cartan, who was responsible for much of the foundation of 20th-century differential geometry. As early as 1922, Cartan tried to explain to Einstein that a different type of curvature, which could be called a total curvature and which contains the traditional curvature as a piece, vanishes. With this condition, called teleparallelism (TP), the torsion need not vanish. Einstein and Cartan explored the implications of TP for generalizing general relativity beyond the gravitational field, but ultimately abandoned that route. Unfortunately, the tools Cartan himself offered to differential geometry were insufficiently mature at that stage to be exploited by Einstein even if the physicist had been able to fully understand them.[1]

Teleparallelism does offer advantages, including a greater mathematical richness than general relativity and a potential resolution of mathematical issues related to the nature of conservation laws in general relativity.[2,3] Wielding the methods of modern differential geometry that Cartan first introduced, physicists in the past couple of decades have elaborated unified theories with TP as an important component.[3,4] For instance, TP and another geometric ingredient[5] lead to the “natural” incorporation of electromagnetism in one such theory, fully within the tradition of the geometrical paradigm of Einstein.[3]

TP may ultimately prove to be a better assumption for a geometric theory. If so, it would still be an extreme excess of Whiggery, to use Weinberg’s wonderful phrase, for those future generations to fault Einstein for his choice in general relativity. The very mathematical concepts, let alone the tools, behind TP did not even exist in 1915 when general relativity was unveiled to the world.

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Weinberg replies: I thank the writers of these letters for their thoughtful remarks. Alfred Goldhaber offers a fascinating speculation, that Einstein might have developed modern quantum mechanics by building on his 1905 introduction of the quantum of light. However, there would have been an obstacle in his path: a shortage of relevant data. By concentrating on atoms rather than photons, de Broglie, Bohr, Heisenberg, and Schrödinger were able to find guidance and confirmation from the huge amount of spectroscopic data already available to them. I can’t think of any way that the quantum theory of light itself could have found similar quantitative support from experimental data in the 1900s or 1910s.

Tom Cornsweet wisely reminds us that the published literature gives only a limited insight into the work of scientists. Real historians, unlike me, try to go deeper by studying diaries, letters, and personal reminiscences, but some aspects of the past can never be recovered.

As far as I have thought about the matter, I agree with Hans Ohanian about the synchronization of clocks. I have not emphasized this point when I have taught relativity theory, preferring instead to take Lorentz invariance as a starting point.

I do not know of any evidence that Einstein would have been content for God to play dice all the way, as suggested by Ravi Gomatam. Einstein did acknowledge the many successes of quantum mechanics, but as far as I know he always hoped that those successes could be explained on the basis of a thoroughly deterministic theory.

Ron Larson takes me to task for my “not-so-subtle knock on religion.” I certainly never intended my remark to be subtle. The reason that I did not mention religion is that I intended to knock reliance on *any* supposedly infallible authority—in other words, not only the attribution of infallibility to the Bible or Koran, but also to *Das Kapital*, *Mein Kampf*, or Mao’s little red book. I did not say that science gave Einstein guidance on public issues. The reason I said Einstein made no mistake on the issues I mentioned is not that I thought he was infallible, but that I thought he was right.

It is of course true, as Brian Hall says, that Einstein’s fallibility does not in itself show that religious prophets are fallible. My point was that, in recognizing that even Einstein was not infallible, we physicists set a good example. While it doesn't prove anything, our example may have some beneficial moral influence. As to whether this sort of remark belongs in an article about Einstein, it seems to me that part of the justification of pure scientific research lies in the impact it has on the culture of our times. Anyway, some of us unpaid contributors to PHYSICS TODAY take our compensation in the opportunity that publication gives us to express our personal views on one thing or another.

To answer Roger Newton, the difficulty that I find with quantum mechanics is that its rules tell us how to use the wavefunction to calculate the probabilities of various values of dynamical variables, but the apparatus that we use to measure these variables—and we ourselves—are described by a wavefunction that evolves deterministically. So there is a missing element in quantum mechanics: a demonstration that the deterministic evolution of the wavefunction of the apparatus and observer leads to the usual probabilistic rules.

Did Robert Brown study the motion of ink particles, and did they carry a significant electric charge, as Bob Eisenberg says? I thought that Brown chiefly studied pollen grains and dust particles, but whatever they were, I suppose the particles may have been charged, and if so, then the effect of electric forces on Brownian motion should be examined.

I may be missing the point of Robert Becker's remarks, but I have never understood what is so important physically about the possibility of torsion in differential geometry. The difference between an affine connection with torsion and the usual torsion-free Christoffel symbol is just a tensor, and of course general relativity in itself does not constrain the tensors that might be added to any dynamical theory. What difference does it make whether one says that a theory has torsion, or that the affine connection is the Christoffel symbol but happens to be accompanied in the equations of the theory by a certain tensor? The first alternative may offer the opportunity of a different geometrical interpretation of the theory, but it is still the same theory.

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Unintended Impact of Author Impact Factor

The letters in the March 2005 issue of PHYSICS TODAY (page 12) in response to Mohamed Gad-el-Hak's Opinion piece (March 2004, page 61) on citation rates and impact factors show how important these criteria have become for hiring, tenure, and promotion, and suggest some models that may result in undesirable, unintended consequences. In particular, the suggestion by Loc Vu-Quoc that multiple-author publications be divided in some fashion according to the number of authors might result in having nervous faculty members delete students and important support staff as coauthors and relegate them to acknowledgments.

The notion that all coauthors are equally responsible for content is not valid in many fields; in solid-state physics, crystal growers, with or without PhD degrees, are not technicians but highly skilled collaborators of equal standing, and students may often play a more important role in that field than in theoretical physics. When I was at Bell Labs (1966–72), no one thought that Howard Guggenheim or Joe Remeika should be responsible for the detailed theoretical analyses of data on their superb crystals, but it would have been unethical not to list them as coauthors; they had grown the world's best specimens of new materials.

The law of unintended consequences has many examples in life; one such story, albeit apocryphal, is that of rat extermination in Singapore. According to the anecdote, a bounty of, say, a few cents was offered for each dead rat turned in to the authorities. Within days numerous rats were delivered, and the numbers dropped quickly as the extermination neared completion. Surprisingly, however, after two weeks the numbers suddenly shot up. Young boys were breeding rats! In a similar vein, if the formulas Vu-Quoc proposes were implemented, we might see a sudden explosion in the number of short, single-author publications by untenured faculty members. Probably these would have about the same value as the rats in Singapore.

We must be careful what we recommend.

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Vu-Quoc replies: James Scott describes a knee-jerk reaction of short-sighted authors who focus on getting more credit for a single paper but lose sight of the bigger picture.

Ethical guidelines such as those of the American Chemical Society[1] clearly state that "the coauthors of a paper should be all those persons who have made significant scientific contributions to the work." Most authors would follow these guidelines and share the credit—and sometimes the blame—for the work.[2,3]

Many journals already require each author of a paper to state his or her contribution.[4] Coauthors are sometimes listed for ethically questionable reasons.[5] Inflated authorship, like inflated grades, devalues authentic authorship, does not contribute to good education, and misleads potential employers. The author impact factor (AIF) is a statistical average over a collection of papers. Its unintended consequence is to promote effective and genuine collaboration, good collaborative work, and adherence to the ethical guidelines for authors.[1]

Instead of such narrow issues as, for example, trying to get more credit for a paper, the AIF concept, with its robustness against database errors, addresses much more broadly the challenges of ranking the publication impact (reputation) of heterogeneous groups of researchers—for example, for use in the ranking of doctoral programs.

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