bet she can't fly a helicopter!"

May we all excise inconsiderate talk from our physics vocabulary.

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Until I read Matt Landreman's Opinion piece, I thought the affliction he described was specific to computer scientists. When asked to explain any particular topic, a computer scientist invariably begins with "Basically, . . ." and then fills several chalkboards with detailed set-theory equations. It's good to know that physicists are also on the cutting edge when it comes to belittling the masses!

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Having taught physics at Swarthmore College from 1955 to 1958, I suspect I know something of Matt Landreman's experience there. I had some very good students, but unfortunately for them and me, I don't believe any of them made it to Oxford University on a Rhodes scholarship. To the litany of trivial stories I can add mine from when I took Philip Morse's Methods of Theoretical Physics course at MIT. When Morse explained how he got the answer to some problem, I complained, "That was a trick!" He replied, "A trick that works twice is a method."

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I agree with the spirit of Matt Landreman's Opinion but not with all of its substance. Words such as "trivial" and "easy" are sometimes used in a patronizing manner, but I think they are more often intended in the spirit of a hint. If an author tells me that a derivation is "easy," I take it to mean that if I get bogged down in some messy equations I am probably doing it wrong and should back up and try again. That hint can save me from flailing away needlessly on the wrong path. I would urge that such adjectives be used with discretion and care rather than eliminated altogether.

Rio Beckwith
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Landreman replies: The use of "easy" and its synonyms described by Rio Beckwith is indeed a standard one. We utter these words to convey that a calculation is not analytically impossible, that it does not require the years of monastic toil required to prove the Last Theorem of Fermat, or that the solution is immediately comprehended by the speaker—who, unlike his audience, has regularly thought about the topic for the past 10 years. But the English language provides other words that more aptly express what we mean: "possible," "feasible," "soluble," "practicable." There is nothing inherently wrong with an instructor's hinting that a student's derivation involving 17-term expressions and elliptic integrals is probably going awry. However, you can be a much more effective communicator

Albert Einstein to George Ellery Hale[1]

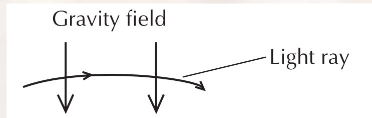
Translated and annotated by Bertram Schwarzschild

Einstein writes to Hale (1868–1938), director of the Mount Wilson Observatory near Los Angeles, seeking advice about the observability of the gravitational bending of light he had recently deduced from the equivalence principle.[2] Einstein's 1913 prediction is only half the deflection predicted by the full general theory of relativity, completed two years later.

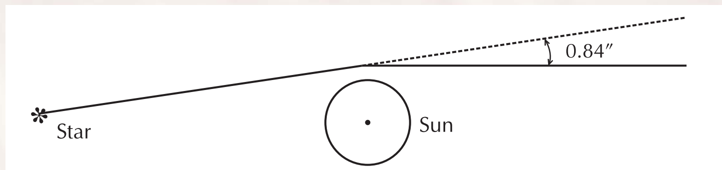
Zurich, 14 October 1913

Highly honored colleague,

A simple theoretical consideration makes it plausible to assume that light rays in a gravitational field experience bending.



At the edge of the Sun, the total deviation should be 0.84 arcseconds, and it should fall off like $1/R$ (R being the ray's [closest] distance from the Sun's center).



It would therefore be of the greatest interest to know how close to the Sun fixed stars could be seen *in daylight* with the strongest magnification.

On the advice of my colleague, Professor [Julius] Maurer, I therefore ask you to let me know what you—with your rich experience in these things—take to be achievable with the best modern instruments.

Yours very respectfully,

A. Einstein

Technische Hochschule Zürich

Hale responded that "there is no possibility of detecting the effect in full sunlight." But he did pronounce the alternative of exploiting a solar eclipse "very promising."[3]

The rest of the story has become Einstein lore: A German team set out to measure the effect in Russia during an upcoming 1914 eclipse. But the outbreak of war intervened. In a sense, that was fortunate, because the team would have been comparing the measurement with Einstein's first, incorrect *prediction. By the time Arthur Eddington's eclipse expedition set out in 1919, the predicted effect had doubled and the war was over. Eddington's confirmation of the general-relativistic bending of light, albeit with a large observational uncertainty, made Einstein instantaneously famous.*

References

1. *The Collected Papers of Albert Einstein*, vol. 5, M. J. Klein, A. J. Fox, R. Schulmann, eds., Princeton U. Press, Princeton, NJ (1993), p. 559.
2. A. Einstein, proceedings of *Schweizerische Naturforschende Gesellschaft* **96**, part 2, 137 (1913), in *The Collected Papers of Albert Einstein*, vol. 4, M. J. Klein, A. J. Fox, J. Renn, R. Schulmann, eds., Princeton U. Press, Princeton, NJ (1995), p. 475.
3. *The Collected Papers of Albert Einstein*, vol. 5, M. J. Klein, A. J. Fox, R. Schulmann, eds., Princeton U. Press, Princeton, NJ (1993), p. 566.

and educator by stating these suggestions directly—for example, "The question can be answered without integration"—rather than falling into the bad habit of using emotionally laden words like "simple," "obvious," or "trivial." These adjectives unnecessarily impugn your students' competence and make them feel defensive.

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An Early Step Toward Asymptotic Freedom

I read with appreciation Bertram Schwarzschild's report on the richly deserved Nobel Prize won by David Gross, David Politzer, and Frank Wilczek for the discovery of asymptotic freedom (PHYSICS TODAY, December 2004, page 21). I am writing to note significant events that preceded this discovery, relating both to Murray Gell-Mann's current algebra and to scaling.

The first sum rule to test current algebra, which depended only on the commutator of axial-vector charges, together with the partially conserved axial current (PCAC) hypothesis, was the Adler–Weisberger sum rule, derived independently by William Weisberger and me in 1965.[1] The sum rule, which related the nucleon axial-vector beta-decay coupling g_A to pion–nucleon scattering cross sections, was in good accord with experiment and gave great encouragement to the current-algebra program. Many people entered the field, and various experimentally verified current-algebra PCAC soft-pion theorems were found. In other work on the g_A sum rule, I noted that by using my earlier observation that forward neutrino reactions couple only to the divergences of weak currents, the PCAC assumption could be eliminated. This led to relations involving cross sections for neutrino scattering with a forward-going lepton. During a visit to CERN in the summer of 1965, Gell-Mann asked me whether I could make some comparable statement about the local current algebra.

After considerable hard algebra, I discovered a sum rule[2] involving structure functions in deep inelastic neutrino scattering that directly tested the local Gell-Mann algebra. This sum rule for neutrino scattering was soon converted into an inequality for deep inelastic electron scattering by James Bjorken.

Although not directly tested until many years later, the neutrino sum rule had important conceptual implications that figured prominently in later developments. First, it gave the earliest indication that deep inelastic lepton scattering could provide information about the local properties of currents, a fact that initially seemed astonishing, but which turned out to have important extensions. Second, as noted by Geoffrey Chew in remarks at the 1967 Solvay Conference and in a letter[3] published shortly afterward, the closure property tested in my sum rule would, if verified, rule out the then-popular "bootstrap" hadron models, in which all strongly interacting particles were asserted to be equivalent ("nuclear democracy"). In a similar vein, Bjorken argued in his 1967 Varenna lectures that the neutrino sum rule strongly suggested the presence of hadronic constituents.

Those conceptual developments left undetermined the mechanism by which the neutrino sum rule could be saturated. In a 1966 analysis of