letters

# Feynman's incentive prizes

**I enjoyed reading** Toni Feder's news item, "Incentive Prizes Reinvented to Solve Problems" (PHYSICS TODAY, November 2010, page 21). I was a little surprised, though, that a physics publication would omit mention of the challenges Richard Feynman put forward to the American Physical Society in a talk he presented in December 1959 titled "There's Plenty of Room at the Bottom." In that talk, he made the following challenge:

> It is my intention to offer a prize of $1,000 to the first guy who can take the information on the page of a book and put it on an area 1/25,000 smaller in linear scale in such manner that it can be read by an electron microscope.
>
> And I want to offer another prize—if I can figure out how to phrase it so that I don't get into a mess of arguments about definitions—of another $1,000 to the first guy who makes an operating electric motor—a rotating electric motor which can be controlled from the outside and, not counting the lead-in wires, is only 1/64 inch cube.
>
> I do not expect that such prizes will have to wait very long for claimants.[1]

The second prize was indeed claimed within the year, by William McLellan. But the first had to wait until 1985 when my student Tom Newman wrote the first page of *A Tale of Two Cities* in an area 6 microns square using an electron-beam column interfaced to a Mac-2 computer to write the letters in 25-nm-wide lines.[2] As Feder points out, the prize money rarely pays for the research (in Tom's case, the US Army Research Laboratory did), but it nonetheless provides incentive. Tom at first wanted to frame the check from Feynman but then settled for framing a copy and spending the cash.

## References


1. R. P. Feynman, *Eng. Sci.* **23**(2), 22 (1960), available at http://www.zyvex.com/nanotech/feynman.html.
2. T. H. Newman, K. E. Williams, R. F. W. Pease, *J. Vac. Sci. Technol. B* **5**, 88 (1987). See also J. Dietrich, *Eng. Sci.* **49**(3), 24 (1986), available at http://calteches.library.caltech.edu/597/2/Tale.pdf.



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# Scientists offer opinions about their opinions

I would like to offer another wrinkle to the idea that scientific societies should not take positions on science issues of societal consequence, an idea raised by B. K. Ridley (PHYSICS TODAY, July 2010, page 10) and discussed by Alfred Bortz (September 2010, page 9).

Because scientific societies comprise individual scientists, it may be illuminating to look at how individual scientists approach the matter. During my four years in the communications department at Woods Hole Oceanographic Institution (WHOI), I saw a small minority of scientists taking public positions on science issues of societal consequence. That low participation may have come from multiple drivers: WHOI's soft-money culture, which emphasizes the securing of funding for the next project; fear of appearing to grandstand and therefore of being one protruding nail that needs to be hammered down; or a concern for being misinterpreted or overinterpreted. Perhaps most significantly, there was a great concern that taking public positions would ultimately compromise the ability to do impartial science. As soon as you take a position, you risk becoming an advocate for something that may turn out not to be scientific truth.

The wrinkle I offer is that I discovered many scientists would be marginally comfortable offering their opinion if asked but saw it as an entirely different thing to initiate the expression of their opinion. Passive participation was OK; active was not.

A case in point was the 2004 opening of the science fantasy film *The Day After Tomorrow*, in which the cryosphere goes global in about 90 minutes. Thermodynamic impossibilities aside, at last Hollywood was using the term "paleoclimatologist," and we at WHOI had a chance to capture the public's attention, riding on science-fantasy coattails as the science fact-tellers.

I met with a handful of climate scientists before the film opened and discussed how we, as an institution, might take advantage of the moment. The scientists all wanted to run, not walk, from such foolishness. Yes, they would take calls from the media, but no, they wouldn't put their names on any statement or op-ed. It would sully their reputations and risk eye-rolling, or worse, from scientist peers: "How could so-and-so take this Hollywood drivel seriously?"

So we passed. *The Day After Tomorrow* came and went. We posted a climate change FAQ to our website and waited for the phone to ring. As I recall, it never did.

To me, the moral of the story is that most scientists not only have few incentives to take positions on how society might act based on science findings, they have abundant disincentives to do so, mainly in the form of peer censure and risk to credibility. In the end, if one can't move the individuals on an issue, it will be difficult to move the professional societies they constitute.


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**If it really is hubris** "for a committee, however distinguished its membership, to pontificate on scientific matters,'' as B. K Ridley asserts, then it is

hubris with a long history. A cursory examination of the history of the Royal Society of London makes it clear how involved the society was throughout the 18th and early 19th centuries in offering advice on scientific matters to official commissions. The British Association for the Advancement of Science (BAAS), founded in 1831, frequently offered advice to the British Parliament in areas in which the Royal Society was thought to be failing. Such counsel is arguably an important element in building the general culture of society.[1]

Would the 1860 clash over *On the Origin of Species* between Thomas Huxley and Samuel Wilberforce at a meeting of the BAAS have had anything like the resonance that it did if the role of the BAAS in scientific matters had not been generally recognized? The outcome, by showing clearly how imperfect Wilberforce's understanding was, compromised the position of the established church in matters in which it had been regarded as authoritative.

## Reference


1. See, for example, J. Morrell, A. Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science*, Oxford U. Press, New York (1981).



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# On the value of author indices

Letter writer J. Richard Gott III (PHYSICS TODAY, November 2010, page 12) and PHYSICS TODAY readers should be relieved to learn that Albert Einstein's h-index as given by the Institute for Scientific Information (ISI) is 50, not 27 as Gott reported; hence it is higher than Gott's 46. Although Einstein's h-index is substantially lower than Edward Witten's 125, remember that Einstein published between 1901 and 1955, when the entire body of scientific literature was very much smaller than in Witten's publishing lifetime, 1976–present. To get an idea of the scaling involved, in the first six months of 1948, *Physical Review* had a total of 1476 pages, whereas in the first six months of 1998, *Physical Review A–E* had 39 141 pages—many more papers with many more references. Werner Marx, Lutz Bornmann, and Manuel Cardona have published an exhaustive analysis that clearly illustrates the point about different citation impacts of papers published in different time periods.[1]

I cannot think of any other physicist publishing in Einstein's lifetime who would have an h-index comparable to his 50. For example, ISI gives an h-index of 26 for Erwin Schrödinger (1914–61), 30 for Werner Heisenberg (1921–76), 32 for Enrico Fermi (1922–55), and 44 for Paul Dirac (1924–85). Cardona and Marx give an h-index of 41 for Max Born (1900–70) and 45 for Heisenberg, corrected for some ISI omissions.[2] I know of no physicist publishing before 1956 who has an h-index higher than 50. Thus Einstein's h-index today properly reflects his stature relative to his contemporaries, and comparing h-indices of physicists living in vastly different time periods, as Gott does in his letter, doesn't make sense.[1]

Of course, the h-indices of Einstein and of his eminent contemporaries have increased because of many posthumous citations. Einstein's h-index at the time of his death in 1955 was a "measly" 20. I would not be surprised if some of his physicist contemporaries whom we know little about today had higher h-indices than Einstein's then and much lower ones than his today. Similarly, some physicists today with h-indices higher than those of their contemporaries may have their fortunes reversed decades after they are gone. Just as with good wine, the h-index's reflection of merit, when properly used, gets better with time.

## References


1. W. Marx, L. Bornmann, M. Cardona, *J. Am. Soc. Inf. Sci. Tech.* **61**, 2061 (2010).
2. M. Cardona, W. Marx, *Ann. Phys.* **17**, 497 (2008).



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**In the November 2010** issue of PHYSICS TODAY (page 12), J. Richard Gott III criticizes the h-index as an inaccurate measure of the impact of a given author's work; he recommends its replacement by an index he calls the E-index in honor of Albert Einstein. The h-index was proposed in 2005 by Jorge Hirsch,[1] in a paper that has been cited about 660 times in five years, a phenomenal impact for work on bibliometrics. The main reason for the large impact is the h-index's simplicity and transparency and the ease of obtaining the index using the Web of Science. At least 37 variants of Hirsch's h-index have been proposed since, and an E-index already exists.[2]