world without nuclear weapons. "Demilitarization of fissile materials," as urged by Alexander DeVolpi, would be one of the important measures requiring cooperative enforcement.

As to Jonathan Katz's concerns about potential dangers in a world without nuclear weapons, I am also gravely concerned that with the spread of nuclear know-how, materials, and weapons, we face a growing danger of their falling into the hands of rogue states or terrorist organizations and enabling mass murder on an unprecedented scale. In today's world, relying on nuclear weapons for deterrence is becoming increasingly hazardous and decreasingly effective. Achieving a world of zero nuclear weapons cannot mean returning to the world of pre-1945. Knowledge of nuclear weapons will still exist, as will the capability to reconstitute them.

Getting to zero will take hard work on policy and technical issues. It will require an international consensus on strict compliance-monitoring procedures that are considerably more intrusive than achieved so far. A framework for increasing mutual trust and transparency was established by the comprehensive data exchanges and on-site verification measures negotiated in the New START treaty, and I am optimistic that detection of reconstitution efforts will be possible, as is essential by the time zero is achieved.

Those who see hope for a safer world without nuclear weapons need to get to work on eliminating them. My article is a call to action to meet that goal.

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Cold fusion and reproducibility

In response to Bernard J. Feldman's review (PHYSICS TODAY, July 2010, page 50) of David Goodstein's book, *On Fact and Fraud: Cautionary Tales from the Front Lines of Science*, I offer a note on reproducibility and cold fusion. High-temperature superconductors were initially very difficult to reproduce, and many obscure results were noted but not regularly reproduced. If the researchers had sat on the results until they were totally reproducible, the field would have taken years longer to develop. The cold-fusion results suffered from actually being reproducible—so long as the experiment was flawed in the same way as the original. I think Feldman has way overstated the importance of reproducibility to first publishing. Besides, as much as it felt like we had been foolishly led astray in the end, wasn't it fun to examine the possibility of cold fusion and those very odd and interesting electrochemical effects?

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Bernard Feldman is to be commended for his thoughtful review of David Goodstein's new book. However, Feldman has seriously misrepresented the scientific facts about the contentious topic of cold fusion. In particular, he suggests that Goodstein's "sympathetic view toward scientists working in cold fusion" is misguided because cold fusion is "a prime example of a field characterized by unverified results." Although the vast majority of the early attempts to reproduce the associated effects failed, not only were the effects reproduced, but with time, the reasons for the difficulties that were encountered have become well understood.

An important source of confusion is that Martin Fleischmann and Stanley Pons did not discover a colder version of conventional fusion. They discovered something else: a new form of aneutronic nuclear fusion, involving a two-deuteron reaction in which helium-4 is created without the emission of high-energy particles or gamma rays.[1–3] It is not altogether surprising that Feldman is unaware of that. Effectively, mainstream scientific journals have maintained an embargo against cold-fusion papers that report positive findings. That failure has, in fact, become a topic in the mainstream ethics-in-science literature.[4] Twenty-one years after cold fusion was first announced, a more "normal" dialog about the subject[5] is badly needed.

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Feldman replies: In response to Fred McGalliard, it is instructive to compare the American Physical Society meeting right after the first reports of high-temperature superconductivity with the one right after reports of cold fusion. From what I remember, more than 100 physicists reported the observation of high-temperature superconductivity; in the meeting following cold fusion, the majority of the talks were sharply critical of the initial claim. That is an excellent example of the essential role of reproducibility in a well-functioning physics enterprise. I must also comment on a statement McGalliard made about physicists having fun with cold fusion. I wonder if he investigated whether graduate students who did their theses on cold fusion had fun finding their next position or having a productive scientific career.

My response to Scott Chubb is simple: I'm from the show-me state. If cold fusion is to be accepted as valid by physicists like me, it must demonstrate the same level of reproducibility that high-temperature superconductivity has. So far, it has not come remotely close.

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A new index for measuring scientists' output

The online SAO/NASA Astrophysics Data System (ADS) has a search mechanism that makes it easy to look up citation statistics for astronomy and physics. The h-index has been proposed for rating individuals' output.[1] As examples, Edward Witten currently has an h-index of 125 because he has 125 papers receiving 125 or more citations each, and Albert Einstein has an h-index of only 27. I, by comparison, have an h-index of 46. Any index on which I score higher than Einstein is not optimal!

The h-index is nonlinear and doesn't proportionately reward individuals for their most important paper, however influential it may be. Furthermore, by using total citations, the h-index unfairly favors people in large collaborations, because it effectively treats all authors in a multiauthor paper as if each had written the entire paper alone. Total citations are fine for ranking papers, but for ranking individuals, citations for a paper must be allocated among its authors. There are two easy ways to do that: Either use citations that acknowledge first-author status (for example, ^Einstein, A.) and thus recognize leadership, or use fractions and split the citations equally among authors (normalized citations).

What if we include both measures and take their average? Still, Einstein does not do that well. It's not that people are no longer using his papers; rather, he has become so famous that people no longer bother to cite the original references. Can we find those hidden citations? Yes. Einstein's name is often mentioned in the abstracts and titles of papers. Those name citations, which can also be found on the ADS, are just as important as direct citations to his papers and are arguably even harder to get. Eponymous citations count: If you do something important, people will name things after you—the Einstein ring, the Hubble constant, Feynman diagrams, and the like.

I propose an E-index citation count, proportional to total output. In the E-index, the total count C would equal ½ first-author citations + ½ normalized citations + last-name citations in abstracts + last-name citations in titles. Using ADS as of 1 January 2010, Einstein has C = 71 444 citations. (If you suspect your candidate shares a last name and is not responsible for all the abstract or title citations, then rank the most recent 3000 of them by citation count to get the most important ones, and look at the top 10. If 8 of the top 10 refer to your candidate, give your candidate 80% of those abstract or title citations.)

A convenient citation unit is the milli-Einstein (mE) = 71.4 citations. Some who did well include Fermi (1277 mE), Einstein (1000), Hubble (815), Landau (657), Witten (641), Anderson (561), Schrödinger (502), Weinberg (457), Heisenberg (417), Planck (374), Hawking (323), and Feynman (313).

Every automatic method of ranking will have a few outliers who do either much better or much worse than expected, but the E-index should be an improvement over the h-index for evaluating 20th- and 21st-century astronomers and physicists.

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Remembering Howard Voss

I was saddened to learn of the death of my teaching mentor, Howard Voss (PHYSICS TODAY, July 2010, page 61). I was a graduate teaching assistant at Arizona State University from 1969 through 1972, and although I later earned a teaching certificate in Pennsylvania, most of what I know about teaching I learned as a teaching assistant.

Howard Voss had more confidence in me than I did in myself when I started handling recitation classes, and his trust and confidence slowly paid off. Later on he trusted me and another teaching assistant with a significant revision of the introductory physics lab program. As part of that task we made some bold steps to help students be more creative in learning physics. That was my first experience in seeing my ideas create positive impacts for students.

Possibly the most important thing I learned from that project was the value of trusting people who want to do well. Trust, for me, worked in two directions. I found students and professors trusting me, while I learned about the advantages of trusting my students.

I have known few people who work as well with others as did Howard. In three years the only time I recall seeing him angry was when a fellow grad student of mine somehow dislodged the plutonium–beryllium source from its seat in a neutron howitzer. Even then, with a mirror on a stick in one hand and a contrived tool for picking up the radioactive source in the other, the professor was equal to the task.

I take some comfort from having seen Howard at several American Association of Physics Teachers events in recent years and having taken the time to talk with him about the old days.

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