always uncertain.

Simplicity (which may seem elegant) is valued because it sometimes leads to truth. At least, if an assertion is simple, then testing it is a more straightforward procedure. I have to confess here that my philosophy of what is real science comes via Karl Popper, who is unfashionable. Even so, he was reliable in his understanding of good science.

Unfortunately, these home truths about what is desirable in science play an ever-diminishing role in much of modern physics. When theory is totally divorced from experiment, as it so often is in current publications in our physics journals, how do we judge the value of what is contained therein? Hence the rise of this concept of elegance as a value judgment. We should be asking not "Is it elegant?" but "Is it true?" or "Is it falsifiable?"

Clever readers can surely judge for themselves how best to apply this criterion to contemporary fashions in theoretical physics, like string theory, or indeed as Mermin suggests, quantum computing. These subjects will contribute to science not their elegance, but rather only that part of them that is testable in real experiments. Like surgery, this is often messy, but it is certainly what we are about as physicists.

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MERMIN REPLIES: I'm delighted to learn from Peter Drummond the reason why, as noted in the author blurb accompanying my column, Boltzmann's "Elegance is for tailors" is so often misattributed to Einstein. This is a spectacular example of the Matthew effect operating even among the giants of our profession. Who would have thought that the great Boltzmann could have been a victim? (My own low grade victimization was reported in PHYSICS TODAY, April 1981, page 53.) I'm also ashamed to have wasted months in a fruitless search for the true source of that quotation, when all along the answer lay unnoticed on my bookshelf in the preface to the very book by Einstein that inspired Bruno Latour's infamous essay on relativity, my commentary on which got me into so much hot water in these Letters pages just a few years ago (April, 1998, page 15).

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## Proper Credit Given for Early Cancer Work

I was dismayed that your announcement (PHYSICS TODAY, May, page 9) of the very significant result achieved in our laboratory, demonstrating the ability of light scattering spectroscopy to find precancerous dysplastic lesions without removing tissue, failed to credit the creators of the work.

The idea was conceived by Lev Perelman, a principal research scientist, and Vadim Backman, a graduate student, in the Harvard–MIT Division of Health Sciences and Technology. Working closely with their principal clinical collaborator, gastroenterologist Michael Wallace, they demonstrated the validity of the concept by analyzing an extensive series of clinical measurements and comparing them with biopsies taken from the same areas.

For these reasons, their names appear first on each of the initial technical publications.[1,2,3] Working with other collaborators, they were able to demonstrate the applicability to many other organs.

It is true that the work involved many collaborators, and because of his position as director of the laboratory, Michael Feld played an important leadership role. However, we physicists make so few important breakthroughs in our working life that I feel it is important to get the credit right.

My role? I helped create the apparatus with which the data were taken, and have reviewed the progress of the project continuously from its inception.

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## The Neutrino Road Not Taken

This letter is in response to Allan Franklin's article, "The Road to the Neutrino" (PHYSICS TODAY, February, page 22). The hypothesis that a neutral particle is emitted in beta-decay processes, proposed by Wolfgang Pauli in 1930 (and later called the neutrino by Enrico Fermi), was made with two purposes: to save the principles of conservation of energy and momentum and to explain observed difficulties in relation to the statistics (Bose or Fermi) of nuclei and conservation of spin. In his article, Franklin refers, for a reason, to the former purpose, but unfortunately he does not even mention the latter.

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FRANKLIN REPLIES: Luciano Blanco is correct that a neutrino in the nucleus model would solve the spin-statistics problem for nuclei. Such a model, however, would still require electrons in the nucleus, which would result in large unobserved nuclear magnetic moments. Perhaps this is why Pauli did not mention it in his letter that originally proposed the neutrino. The nuclear problem was soon solved by James Chadwick's discovery of the neutron.

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## Trouble for Quantized Hubble

The possible bunching of galactic redshifts that Maurice T. Raiford notes in his letter in the February issue of PHYSICS TODAY (page 75) could be important. However, the apparent numerical agreement between the putative quantum of recessional velocity, $v = 72$ km/s, and the Hubble constant results from the convention of writing that constant in units of kilometers per second per megaparsec instead of inverse seconds, and is without any deep significance. If $H_0$ is expressed as $2.3 \times 10^{-18}$ $s^{-1}$ instead of 71 km/(s Mpc) then there is nothing to suggest a direct proportionality between $v$ and $H_0$.

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