

# Letters

*Editor:* It seems that every time I have an article in *E&S* I get into trouble. Last time, I offended Professor Paul Bellan by saying *magnetic confinement* when I meant *magnetic mirror confinement*. This time I've made an even worse error since it is in my own field of expertise.

One of my esteemed colleagues, who prefers not to have his name dragged into this tawdry affair, has written to point out that I said sound waves are *adiabatic*, when I should have said they are *isentropic*. The distinction between adiabatic and isentropic is well known to all readers of *Engineering & Science*, so I should have known better than to try to get away with such sloppy writing.

He goes on to point out that I said sound waves are pushed along by heating and cooling, whereas I should have said in a sound wave, the temperature rises and falls, because there is no heating or cooling involved. (In other words, sound waves are adiabatic.)

Yet another distinguished reader, Dr. Robert Glaser, has written to point out that William Summerlin worked not at the Sloan-Kettering Institute in Minnesota as I said, but rather at the Memorial Sloan-Kettering Cancer Center in New York. He is perfectly correct.

I have carefully considered the significance of these misstatements and decided that, while they do amount to serious scientific error, no misconduct or fraud is involved, and they do not alter the main conclusions of the article. The article therefore does not have to be retracted, and this letter may serve to correct the scientific record (murky as that is).

David L. Goodstein

*Editor:* David Goodstein's article on scientific fraud laments the growing interference from the sponsor's watchdogs, but fails to give any suggestions for how the scientific establishment might better police itself. Based on the well-known cases of fraud in science and my own experience, I would like to offer the following suggestions for dealing with the problem, which will only grow as the scientific research profession becomes increasingly competitive.

1. Authors relying on experiment or observation should be prepared to make copies of their raw data available for any legitimate request, whether for the purpose of repeating the experiment or for further developing the technique.
2. Incentives should be given for the usually thankless task of verification of the results of others. The disproof of the cold fusion claims is an outstanding example of the importance of verification, but simple verifications are usually not even publishable.
3. To avoid repeating fiascos like the Baltimore affair, senior people must accept responsibility for papers that bear their names and for the honesty of protégés whom they support and sponsor. After all, it is their prestige that provides the edge in winning supporting grants and assures rapid acceptance of results submitted for publication.

Peter Gottlieb (BS '56)

*Editor:* I just read "First Lights," and really enjoyed it, especially your delightful investigation of what happened on the 100-inch first light night. Firing up a planetarium program and running it for the night of November 2, 1917 (until it crashed my aging computer), I found that Jupiter, the Moon, and Saturn were all near the zenith (or, more correctly, the meridian) soon after sunset that night. So we may assume that Hale and the rest were testing the telescope on near-zenith objects, as is standard practice. (Perhaps as Adams recalled, "the telescope was swung over to the eastward" to see Jupiter, but unless it was still dusk they didn't have to swing it over all that far.) If, then, they returned at 3 a.m. and chose a bright star near the zenith, which star was it? Not Vega, which as you note was below the horizon. *Regulus*, however, was quite close to the Los Angeles zenith at that hour. The visual magnitude of *Regulus* is -0.3, quite close to the 0.6 mag of the then-subterranean Vega, and its spectral class is B7, close enough to Vega's A0 to make the two indistinguishable in color to all but an experienced visual observer. (Which most professional astronomers are not; indeed, in my experience, many astronomers don't know the sky well enough to find their way out of the woods.)

I hypothesize, therefore, 1) that Adams's account of the evening is substantially accurate, except that his memory substituted one blue-white, zenith-achieving, first-magnitude star for another, and 2) that Noyes like the others saw a poor image of Jupiter, but decided (after learning that the telescope worked) to substitute what he *should* have seen for what he actually *did* see. Such prettifying is the stock in trade of clichéd and hackneyed poets, who with "bated breath" as Noyes puts it steadfastly pursue the cosmic yalp as they anticipate it to be, without letting the facts trip them up.

Timothy Ferris