Letters

Editor: In Timothy Ferris's letter of your Spring 1991 issue he gives the magnitudes of Regulus and Vega incorrectly. According to the most recent edition of the Yale Catalogue of Bright Stars, the visual magnitude of Regulus is 1.35, not –0.3; and the visual magnitude of Vega is 0.03, not 0.6.

A perhaps more likely target at 3:00 a.m. on November 2 is Capella, whose visual magnitude is 0.08, almost exactly the same as that of Vega. Also, its declination is more similar to that of Vega than is Regulus, and because of its high declination it would still be near the zenith even if two hours west of the meridian.

As Timothy Ferris says, many astronomers do not know the sky, but we do know enough to find the magnitudes of stars by going to the library and looking them up.

George Wallerstein (PhD '58)
Department of Astronomy

Editor: I applaud Timothy Ferris's efforts to determine which star the astronomers saw on the morning of November 3 (in place of the spurious Vega). When Bob Eklund first brought my attention to the discrepancy in Adams's account of the first light of the 100-inch, he theorized that it might have been Capella. So when I saw Ferris's idea that the culprit might have been Regulus, I did some digging of my own. After examining the sky position on two computer programs (EZ Cosmos and Skyglobe), I'm afraid that I have to agree with Bob Eklund. On the morning of the 3 (at 3:00 a.m. PST), Capella was nearly overhead (altitude of around 75°) while Regulus was low (30°) on the eastern horizon. And while the spectral types of Capella and Vega are not the same (G8 vs. A0), their apparent magnitudes are almost identical (0.08 vs. 0.03). Their declinations are also only 7° different, so that they would be in nearly the same place in the sky when at culmination (Regulus, with a declination of +11° would not come close to the zenith). In addition, I must disagree with his magnitude of Regulus. The lists I have show it to have an apparent magnitude of 1.35, very noticeably fainter than either Capella or Vega.

If we substitute Capella for Regulus in Ferris's argument, then, I would agree with his assessment of Adams's account. And certainly no one could argue with his conclusion about Noyes; he held very loose reins on his poetic license indeed.

Ronald Brashear
Assistant Curator (Science)
The Huntington Library

Editor: Capella does indeed seem to be the more likely Vega stand-in, and I'm grateful to Ronald Brashear for having looked into the matter. I was misled by having substituted the absolute for the visual magnitude of Regulus. (GeorgeWallerstein's impression to the contrary, I do know how to go to the library and consult a star catalog, although evidently I cannot be relied upon to read the numbers off the proper column.)

Absolute magnitude, I should explain, is defined as the visual magnitude a star would display if observed from a distance of 10 parsecs. Were Regulus 10 parsecs away from Earth the two magnitudes would be identical. Alas for my argument, Regulus is some *20* parsecs distant. Moreover, it's getting farther away all the time, receding at a rate of six kilometers per second, and thus slowly but steadily magnifying the dimension of my error. This I regard as fresh evidence of the stars' legendary indifference to human affairs.

Timothy Ferris
Professor of Journalism
UC Berkeley

Editor: The recent *E&S* obituary for Carl Anderson carried my memory back to the 1931–32 period. I was then a sophomore student of his in physics—and I also served part time as his lab assistant, funded under Roosevelt's NYA program. One day I was in the shop working on a piece of electronic equipment for him. He came up in a state of excitement to show me something; apparently none of his colleagues were around, and I was the only person available. He took me along to his research lab and showed me a cloud chamber photo, remarking that one of the tracks disproved current theory.

With youthful enthusiasm, I immediately expressed confidence that he could develop his own theory to explain the new results. He replied something like, "Oh no, I'm an experimental physicist, not a theoretical physicist." When I didn't seem to realize that there was a great distinction (he had always seemed pretty handy with theory at my level of sophistication), he remarked, "A theoretical physicist has to eat, sleep, and dream physics 18 hours a day; I'm not that dedicated." These remarks made a tremendous impression on me.

I was absent from Caltech for the 1932–33 academic year and somehow missed the announcement of Anderson's discovery of the positron. When he later received the Nobel Prize, I thought back to the above incident. But something about it didn't seem to fit: I'm not sure whether it was the date (a bit too early, perhaps), or that my vague recollection of the particular cloud chamber feature involved suggested it was not the reversed-curvature track of a positron. Also, there was his remark that the special track disproved current theory; finding the positron was, on the other hand, confirmation of a theoretical prediction. Thus for a long time I have had the feeling that I had been a witness to an earlier event, a precursor to the big one. But since the November 1981 article on Anderson in *Physics Today*, I have wondered if I had not indeed been there at the actual discovery.

Herbert S. Ribner (BS '35)
Professor emeritus
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University of Toronto

Editor: When I carried the Spring 1991 issue home with me and read it this summer, I was quite surprised. An article, "Caltech's *Other* Rocket Project: Personal Recollections," really struck "home." I live in sight of the China Lake Naval Weapons Center, earlier known as Inyokern NOTS. I have always wondered who decided to put a Navy facility in the barren desert of the Indian Wells Valley.

Very few of the local residents know Caltech's place in the area's history. They do not realize that if Charles Lauritsen had chosen a different site for his rocket experiments, a city of 28,000 would not exist.

I almost felt like part of the story. In 1943, those scientists from Caltech came to the Indian Wells Valley. Nearly 50 years later, I left the Indian Wells Valley to come to Caltech in order to become a scientist. It looks like a cycle.

The Caltechers set China Lake on the right track for rocketry in the forties. In the nineties, we need some Caltechers to put China Lake on the right track for chemistry. I recently read that China Lake chemists are experimenting with cold fusion!

David M. Krum
Sophomore

Editor: I enjoyed Hans Bethe's article (v. 54, no. 3) on nuclear power. Although I agree that nuclear power is relatively safe and that high-level nuclear waste repositories will probably be quite clean, the article omits some damaging information about backfill material and probably understates the overall environmental impact of nuclear power. In addition, it seems reasonable to question whether disposal of "spent" fuel is wise.

Bethe talked about the backfill material, which would be placed between the metallic waste canister and the rock. According to most designs, this material would consist largely of bentonite clay, which swells dramatically on contact with water and is supposed to form a seal around the canister. Bethe called this "the most important [component] of all," and noted that "reasonable people have estimated that this backfill may easily last 100,000 years."

In fact, bentonite is a rather unstable material, and its swelling properties are strongly dependent on the chemical and thermal environment. It was recently discovered that its ability to swell in water is severely degraded by just a few hours' exposure to steam at 150° to 250° C. The effect of steam would have been a severe design problem for the proposed repository at Hanford. This repository was to have been located several hundred meters below the groundwater table, with projected temperatures far in excess of 200° C for the backfill material.

Fortunately, there are ways around the problem, and repository designers are well aware of the effect of steam on bentonite. The repository at Yucca Mountain will not use bentonite. The Swedish repository and the Canadian repository are likely to use bentonite, but will accept waste only after it has cooled considerably.

There is one fundamental question about nuclear waste disposal that is seldom considered. That is whether it makes sense to dispose of the transuranic elements. "Spent" fuel is not spent at all, but is merely contaminated with fission products and slightly depleted of fissionable U^{235}. The U^{238} remains and can be converted to fissionable Pu. Most of the U^{235} also remains. Thus, most of the energy content of the fuel remains. On a planet that has limited resources, disposing permanently of an energy source of this magnitude seems irresponsible.

Bethe correctly pointed out the serious environmental problems caused by coal and oil, and compared them to the rather negligible accident rate and negligible environmental contamination caused by nuclear power. Unfortunately, the vast quantities of tailings and other waste that are produced by mining and processing of uranium ore were not mentioned. Although this problem may not pose as great a threat to the planet as coal and oil, it is by far the biggest source of pollution from nuclear power. Consequently, nuclear power may not be as clean as it appears from Bethe's article. Significantly, if nuclear fuel were reprocessed and reused, far less mining waste would be produced, and possibly the overall impact would be reduced.

Although nuclear energy is an excellent source of power, no source is nonpolluting, except perhaps conservation and solar power. One thing is perfectly clear: our national energy future is guided by emotional and aimless public debate rather than by rational considerations.

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I am preparing a biography of Linus Pauling—with Dr. Pauling's cooperation—and would appreciate hearing from former students and colleagues who have anecdotes or insights about his years at Caltech. Please contact Tom Hager, 3015 Friendly Street, Eugene, OR 97405.