letters

Quasiperiodic pulsars

A recent Search and Discovery report on quasiperiodic oscillations from x-ray sources (October, page 17) stated, "Observations of isolated pulsars show that neutron-star magnetic fields decay in just a few million years." This idea has proven quite attractive to many astrophysicists, but it is far from being an established fact. Indeed, it stems mainly from the observation that there are few pulsars with periods longer than three seconds. Because pulsars are all slowing down, one would otherwise expect an accumulation of slow pulsars, and it was suggested early on that these slow pulsars fade from view owing to decay of their magnetic fields (as is discussed, for example, in the monograph *Pulsars* by R. N. Manchester and J. H. Taylor).

Since those early days, a number of alternative mechanisms have been identified that would also explain the lack of long-period pulsars. For example, the surface temperature of a neutron star is estimated to fall precipitously after a few million years, to name just one factor having essentially the same characteristic time. It should be emphasized that the radio emissions account for only about 10^{-5} of the apparent power output as judged from this slowing down. Thus turning off the radio emission would not necessarily change the slowing-down rate at all.

The pulsar data are very scattered; there is nothing like a "main sequence" for these objects. Indeed it is widely thought that the slowing-down torques scale essentially as the period to the fourth power, but it has not been possible to establish this exponent clearly in the data. It is curious, then, that so much confidence is invested in the detection of a first-order correction to the slowing down (magnetic-field decay) when the zeroth-order slowing down itself cannot be accurately calibrated. This rapid drop in luminosity gives a characteristic age of a few million years and a limiting period of a few seconds simply because the pulsars become too faint to be seen. Thus observational selection becomes important as well.

Theoretical estimates of the neutron-star conductivity have systematically given values too high to give such a rapid decay, except possibly in the outermost crust. The core is even thought to be superconducting.

The binary pulsating x-ray sources are thought to be old neutron stars accreting from a binary companion, yet some show line spectra suggestive of cyclotron emission in strong pulsarlike magnetic fields. The one independent observational test so far fails.

The idea that a celestial body is magnetized by flux trapping at its formation, with subsequent ohmic decay, does not work anywhere that it can be tested. It is obviously incorrect for the Earth and the Sun. To the modest degree that planetary magnetism is understood, one would even expect rotating neutron stars with superfluid cores to be generators of magnetic fields, not sinks.

It would be very important if the idea about magnetic field decay were correct because, as the article goes on to illustrate, that idea is highly constraining on evolutionary scenarios for many astrophysical objects. Gamma-ray-burst sources may also be old neutron stars, for example. Until there are reasons to be more confident in this idea, it would appear sensible to present it carefully for what it is: an idea and not an established fact.

F. Curtis Michel
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11/85

Rename Livermore

Your news item, "California Regents will renew contracts for labs" (January, page 81) prompts me to bring a relevant issue to your readers' attention: Should not the name of Ernest O. Lawrence be removed from the title of the Livermore National Laboratory? As Lawrence's widow, I have felt for some years that this government-owned facility should no longer be using his name. My reasons are moral, historical and practical.

First, I am unalterably opposed to the continuing escalation of the nuclear-arms race, and because this is now the primary activity of the Livermore laboratory—both in terms of the research conducted there and of lobby-

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ing conducted in Washington—I consider today's Livermore facility in no way a suitable memorial to my late husband.

It was only 23 days after Ernest Lawrence's death in 1958 that the regents of the University of California named the Radiation Laboratory in his honor. Their intention was undeniably to establish a lasting memorial to a distinguished member of the university family and to his important scientific achievements. At that time, the Radiation Laboratory, founded by Lawrence in 1931, included the laboratory complex on university-owned land in Berkeley and the government-owned weapons-research laboratory in Livermore, established by Lawrence in 1952 as a subsidiary of his Radiation Laboratory. (At this point, I wish to correct today's erroneous belief, held by many scientists, members of the press and individuals among the general public, that Edward Teller was the founder, or co-founder with Lawrence, of the Livermore laboratory. The co-founder version is even given out as official information by the public-relations department of the laboratory. This is simply not true, as Herbert York has attested in a letter to William Broad, science writer for *The New York Times*. York actually carried out the work of establishing Livermore under Lawrence's direction and served as its first director from 1952 to 1958. He states that "Ernest Lawrence, without any room for doubt, was the laboratory's founder, not Edward Teller.")

When the regents separated the two parts of the Lawrence Radiation Laboratory in 1971, with all classified work assigned to Livermore, the Lawrence name was retained at both sites. It was not until the very end of 1979 that Congress, in one short paragraph of a lengthy Military Authorization Act, added the word "National" to Livermore's designation, thus fixing the entire name in Federal law.

Since that time, the Livermore laboratory has become a very different kind of institution from what it was in Lawrence's day. Its development has been such that it now completely overshadows its parent laboratory in Berkeley and is no longer an appropriate memorial to any individual, and certainly not to Lawrence. Of course the Livermore people enjoy the prestige and the aura of scientific expertise and sanctity that accrue to them through the use of two famous names, that of a great university and that of a great scientist. They are understandably eager to hang onto both. However, I see the present use of Lawrence's name as contributing to a distortion of scientific history and as a sort of *de facto* abrogation of the 1958 regents' intent to honor him.

It is time to put Lawrence's defense work in proper perspective with his contributions to basic scientific knowledge, peaceful applications and worldwide dissemination of such knowledge. These contributions should be the basis for his place in scientific history but the use of his name at Livermore serves only to distort the picture. The public now recognizes the name as that of someone who "helped to develop the atom bomb and started the Livermore lab," work that occupied the latter part of his career, when his reputation as a leading American scientist had already been established. Some even believe that he was awarded the Nobel Prize for his defense work. Surely it is time to set the record straight and it would certainly help for the Livermore lab to be designated by its place name only, as Los Alamos and Oak Ridge are.

From the practical point of view, the existence of three major institutions in the same California county, all bearing the Lawrence name—the Berkeley and Livermore laboratories and the Hall of Science—has become an administrative headache. It is a source of endless confusion, even among otherwise well-informed people—a confusion all too frequently reinforced by errors in the press and on television. By now, I have lost track of the number of times I have read or heard references to "the Lawrence Livermore Laboratory in Berkeley." As recently as September 1985, an Associated Press release about the same regents' vote reported in your January issue included this very error.

This sort of thing is very damaging to the Lawrence Berkeley Laboratory, the direct descendant of the original Radiation Laboratory and Lawrence's true and proper memorial. What's worse, even the Lawrence Hall of Science, a fine educational institute created to "raise the scientific literacy of the nation," is now often confused with the Livermore weapons facility. It is certainly high time to clear up this confusion; a change of Livermore's name would help a lot.

In March 1983, I formally asked the university's regents to drop the Lawrence name from Livermore; I was told this would require an act of Congress. On 26 October 1983, my Congressional Representative, Ronald Dellums, introduced a bill to change the name. Unfortunately, despite Dellums's efforts, it was allowed to "die" in committee at the close of the 98th Congress in 1984. On 7 February 1985, Dellums introduced an identical bill (now known as HR 1036) but again it is languishing in the House Committee on Armed Services.

I am told the problem in getting action is that the name change has

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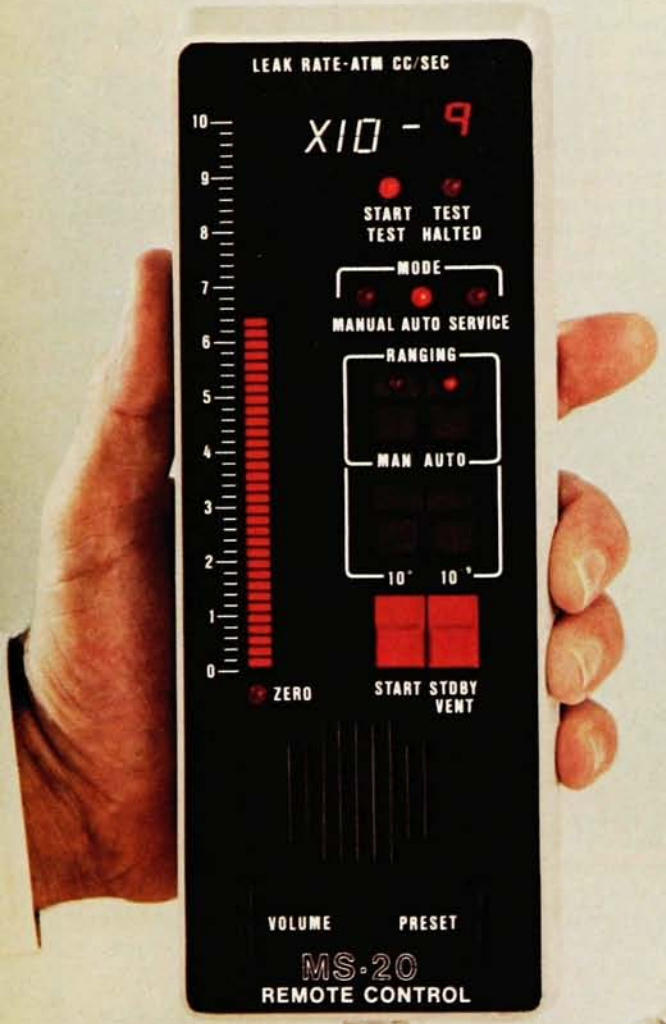

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become symbolic of the controversial issues of the Regents–DOE contracts and the matter of nuclear-arms control. It seems to me that distortion of history, abrogation of the 1958 regents' intent to honor Lawrence and creation of identity problems for the Lawrence Berkeley Laboratory and the Lawrence Hall of Science add up to more than enough reasons to remove the name from Livermore whether or not the university continues to operate the labs. I would welcome any suggestions or advice your readers would care to offer.

MARY B. LAWRENCE
Berkeley, California
2/86

More on Maxwell

The August issue of PHYSICS TODAY carried a review (page 66) of my book *James Clerk Maxwell: A Biography* by Daniel Siegel. The first half of this review was quite flattering, and it may seem ungracious of me to submit a rebuttal. The point is, of course, that in the second half of his review, Siegel offers what he believes to be historical criticism of my views on Maxwell's work, which I cannot let pass without comment.

Siegel implies that I have taken Maxwell's thinking out of its historical context and presented the reader "not with the real Maxwell, who was a 19th-century mechanist, but with an imagined Maxwell, who is in fact a 20th-century physicist transported into the 19th century." This, Siegel pronounces, "is not good history."

When Maxwell wrote his great papers on electromagnetism (1861–62), he introduced "molecular vortices" in the ether, with small spherical particles acting as "idle wheels" between them. Siegel's main point seems to be that Maxwell considered the former a "probable" hypothesis and the latter "awkward." This, it would appear, brands Maxwell as "a 19th-century mechanist." My failing to underline these "fine distinctions that Maxwell was so careful to make" is, apparently, the root of my historical error.

These are indeed fine distinctions and do not—in my opinion—support the weighty conclusions Siegel bases upon them. In my book I give a number of quotes from Maxwell to substantiate the opposite thesis—as when Maxwell says that he "merely [wishes] to direct the mind of the reader to mechanical phenomena which will assist him in understanding the electrical ones. All such phrases in the present paper are to be considered as illustrative, not explanatory" ("The dynamical theory of the electromagnetic field," 1862). Siegel and I could doubtless bandy properly selected Maxwell quotes back and forth *ad nauseam*, and reach no satisfactory agreement. When it comes to the uses of models and analogical thinking, I believe that Maxwell was, indeed, a generation ahead of his contemporaries (such as Lord Kelvin, who *was* a 19th-century mechanist!).

I do not feel myself entitled to pronounce on what is or is not good history, nor do I think myself privy to definitive insights into the workings of Maxwell's imaginative genius. I will, however, offer the reader—and Siegel—the following observation. Maxwell's *A Treatise on Electricity and Magnetism* was published in 1873 and contains, one must assume, his mature (final?) views on these matters. Of the 866 sections comprised by the *Treatise*, only sections 822–831, devoid of diagrams, offer a formal discussion of the hypothesis of molecular vortices. In the penultimate paragraph of section 831, referring to his 1861–62 papers, Maxwell has this to say:

> The attempt which I then made to imagine a working model of this mechanism must be taken for no more than it really is, a demonstration that mechanism may be imagined capable of producing a connexion mechanically equivalent to the actual connexion of the parts of the electromagnetic field. The problem of determining the mechanism required to establish a given species of connexion between the motions of the parts of a system always admits of an infinite number of solutions.

Caveat lector?

IVAN TOLSTOY
Knockvennie
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1/86

SIEGEL REPLIES: I would agree heartily with Ivan Tolstoy that one cannot decide a historical issue by "bandy[ing] properly selected . . . quotes back and forth." In particular, the issue of Maxwell's stance with respect to the mechanical world view is complex, and certainly not amenable to decisive resolution by means of a few choice quotations. My own views on the matter are developed at length in an article entitled "Mechanical image and reality in Maxwell's electromagnetic theory," in *Wranglers and Physicists: Studies on Cambridge Mathematical Physics in the Nineteenth Century* (P. M. Harman, ed., Manchester U.P., Manchester, 1985, page 180). In brief, I argue there that Maxwell began in the 1850s by using mechanical models in an analogi-