spent on clarifying issues that could have been rectified from the outset had the proponents been willing to follow up on definitive experiments rather than rushing into print. When "discoveries" with such scientific and technological importance as polywater, infinite dilution and cold fusion are made, the entire scientific community as well as the lay public will take notice. Therefore, scientists must be extremely careful that they have not been self-deluded and must be extra cautious before announcing their results publicly. Otherwise, the inevitable scientific mistakes are sorted out on the pages of newspapers and news magazines rather than in the laboratory where they should be resolved before publication and media hype.

I do not disagree with Dr. Oderwald that it is appropriate, indeed desirable, to challenge prevailing theories. However, one cannot do so with flawed experiments. It is my contention that for the three topics that I described in my article, the experiments were flawed and for that reason would not fit into any existing theoretical framework. When well-established theory, which is supported by a vast number of prior experiments, is challenged by a new experimental result, a very hard look must be taken at the new experiments. Only by proceeding with caution, exploring all possibilities and doing definitive, well-controlled experiments can the validity of the new result be assessed properly. Anything short of this is a disservice to the scientific community and a violation of the trust placed in scientists and the scientific method by the public.

I thank Drs. Czirr et al. for clarifying some of the chronology of the early days of cold fusion. The statement in the caption of Figure 1 of my article concerning extraction of usable energy from a simple apparatus is based on my belief that if fusion in an electrochemical cell were detectable, albeit at a very low level, then useful energy could in principle be extracted as the efficiency of the process is improved and the apparatus is scaled up. This viewpoint is supported by the writings of the group from BYU led by Dr. Jones. For example, in their paper in *Nature* they state: "Although the fusion rates observed so far are small, the discovery of cold fusion in condensed matter opens the possibility, at least, of a new path to fusion energy." As pointed out in their letter, I recognized and described the large difference in the magnitude and in the type of effect reported by the BYU group compared with that reported by Drs. Pons and Fleischmann. Throughout my article, the distinction between the results from these two groups was maintained.

Owing to the absence of heat and the small magnitude of the effect reported by Dr. Jones and his co-workers, the BYU group may not have received what may have been unwanted publicity had their results not appeared at the same time as the cold-fusion reports by Drs. Pons and Fleischmann. However, both groups did publish at the same time and both made the same general claim: *cold nuclear fusion had been detected*. Many other groups tried to replicate each type of experiment with decidedly disappointing results, leading to the conclusion that, although there were some unexplained phenomena, cold nuclear fusion had *not* been detected by *anyone*.

I do not wish to stifle continued and future work by Dr. Jones and his collaborators in the nascent area of the study of possible low-level nuclear reactions in condensed matter. Once their findings are sorted out, they could uncover some interesting new physics.

Throwing Rocks

To the Editors:

"Rock Varnish" (November–December 1991) by Ronald I. Dorn presents an incomplete, and consequently biased, review of rock-varnish research. Dr. Dorn suggests that rock varnish is a well-understood phenomenon and that rock-varnish dating techniques are widely accepted and reliable, in each case ignoring the increasing body of contradictory evidence. Dr. Dorn makes no mention of the large body of work generated by other active research groups or the widespread and long-standing controversy surrounding the reproducibility of his techniques, the validity of his assumptions and the veracity of his interpretations.

It appears that much of Dr. Dorn's work cannot be independently reproduced using rigorous and controlled experiments. Bierman and Gillespie (1991a) used a blind interlaboratory test to show that the chemical analyses on which many of Dr. Dorn's cation-ratio dates are based were inaccurate, a conclusion first reached by Harrington et al. (1991). Comments and replies (1992) to Bierman and Gillespie's paper further support this finding. These same replies also show that Dr. Dorn's findings have not been replicated elsewhere and that many of his blind tests are flawed, despite claims to the contrary.

Many of Dr. Dorn's fundamental assumptions are unsupported by published evidence and appear to be untenable. Reneau and Raymond (1991) summarize thousands of varnish analyses suggesting that significant cation leaching—the basis of Dr. Dorn's cation-ratio dating—does not occur in rock varnish. Bierman and Gillespie (1991b) show that Dr. Dorn's assumption, that the micron-thick varnish and the underlying substrate remain stable for hundreds of thousands to millions of years, is commonly not valid. Reneau et al. (1991) point out the many unsupported and questionable assumptions inherent in the radiocarbon dating of varnish.

Dr. Dorn's interpretations are not robust. Wells and McFadden (1987) point out significant flaws in Dr. Dorn's sampling strategies and his geological interpretation of alluvial-fan deposition. Lanteigne (1989, 1991) used simple statistical tests to show that many of Dr. Dorn's conclusions are not supported by his own data.

We believe that Dr. Dorn's article presents a narrow viewpoint, one that misrepresents, by omission, the spectrum of ideas and evidence that have been published by a variety of active but unmentioned research groups. We urge readers seeking to balance their understanding of rock-varnish research to review critically both Dr. Dorn's work and the papers we have cited, because it is impossible for us to justify or explain fully our findings in a short letter such as this.

Bierman, P., and A. Gillespie. 1991a. Accuracy of rock varnish chemical analyses: implications for cation ratio dating. *Geology* 19:196–199 (subsequent comment and reply, *Geology* 20).

Bierman, P., and A. Gillespie. 1991b. Range fires: a significant factor in exposure-age determination and geomorphic surface evolution. *Geology* 19:641–644 (subsequent comment and reply, *Geology* 20).

Harrington, C. D., D. J. Krier, R. Raymond and S. L. Reneau. 1991. Barium concentrations in rock varnish: implications for calibrated rock-varnish dating curves. *Scanning Microscopy* 5:55–62.

Harrington, C. D., and J. W. Whitney. 1987. Scanning electron microscope method of rock varnish dating. *Geology* 15:967–970.

Lanteigne, M. 1989. Comment on Age determinations for rock varnish formation within petroglyphs. *Rock Art Research* 6(2):145–149.

Lanteigne, M. 1991. Comment on Age determinations for rock varnish formation within petroglyphs. *Rock Art Research* 8(2):127–130.

Reneau, S., and R. Raymond. 1991. Cation ratio dating of rock varnish: why does it work? *Geology* 19:937–940.

Reneau, S. L., T. M. Oberlander and C. D. Harrington. 1991. Comment on Accelerator mass spectrometry radiocarbon dating of rock varnish. *Geological Society of American Bulletin* 103:310–314.

Wells, S. G., and L. D. McFadden. 1987. Comment on isotopic evidence for climatic influence on alluvial-fan development in Death Valley, California. *Geology* 15:1178.

Paul Bierman and Alan Gillespie
University of Washington

Charles Harrington, Robert Raymond and Steven Reneau
Los Alamos National Laboratory

Leslie McFadden
University of New Mexico

Steven Wells
University of California at Riverside

Dr. Dorn replies:

In a newly emergent field, such as the use of rock coatings as a dating method or tool to understand past environments, it is important to determine where a disagreement takes place. The *first* step is basic research on how rock varnish forms and how its characteristics vary. Here, there is overall agreement in rock-varnish research. The *second* step is to decide whether methods generally work—for example, whether layers of rock varnish that are low in manganese form in more alkaline conditions. Here again, there is overall agreement. Even in the most contentious aspect of rock-varnish research, cation-ratio dating, Harrington and Whitney (1987) replicated my findings that cation-ratios decline over time. A. F. Glazovskiy from the Academy of Sciences in the former U.S.S.R. similarly reported (1985 issue of *Data on Glaciological Studies*) a decline in cation-ratios with time. W. B. Bull of the University of Arizona reported the same trend in his 1991 book *Geomorphic Responses to Climatic Change.* The *third* step in assessing this disagreement on rock-varnish research is the details of the methods, and the generation of specific results. Here I have and will continue to ruffle the feathers of individuals who do not like my specific findings.

The objections noted in the letter from Dr. Gillespie and his colleagues relate to details and applications of cation-ratio dating and to an older, less-accurate approach to radiocarbon dating than the one reported on in my article. The methodological debate over cation-ratio dating is one small aspect of rock-varnish research; to treat the topic properly would have doubled the size of my paper. The authors of the letter have already written comments and I have replied to those comments in the refereed journals noted in my bibliography. However, there is also an uglier reality involved in my decision not to cite their works: I simply did not want to dirty the bath water. One of the key papers (Bierman and Gillespie 1991a) supporting their objections involves their claim of a "blind interlaboratory test." Tom Cahill of the Crocker Nuclear Laboratory of the University of California at Davis comments on the work of Drs. Bierman and Gillespie in a forthcoming paper in *Geology*: "The data in B&G's [Bierman and Gillespie 1991a] Table 1, described as 'PIXE UCD' [proton-induced X-ray emission from the University of California at Davis] did not in fact come from us.... The data in Table 1 are a faulty construct, and were known to be incorrect by both UC Davis and B&G well prior to publication. Readers of *Geology* must totally discount the data reported to come from 'PIXE UCD' in B&G's Table 1." This is an extreme example of misrepresentation. A slight one is the claim that cation leaching "does not occur." The approach outlined by myself and David Krinsley in the November 1991 issue of *Geology* matches scanning-electron micrographs with quantitative electron-microprobe data to assess whether leaching occurs. It does.

It is important not to lose perspective even on the details of cation-ratio dating. I have stated in all my previous work that this method is the weakest in the baggage of rock-varnish techniques, but not for the reasons listed by Dr. Gillespie and his colleagues. The inherently problematic aspect of the technique is that it is based on chemical changes that are dependent on a variety of environmental factors, other than time, that need to be controlled. For those readers interested in the nitty-gritty details of different approaches to cation-ratio dating, I refer them to a forthcoming book edited by Charlotte Beck, *Dating in Surface Context* (University of New Mexico Press), where I do extensively quote and analyze the works of the authors of this letter in a chapter on dating rock varnish.

It is an exciting time for those interested in exploring rock coatings as an interpretive tool in archaeology and earth science. As our basic understanding of these coatings grows, new tools are developed. It is only through open discourse and further basic research that methods are refined, and the good ones can be separated from the bad.

Super Steamship

To The Editors:

In his interesting article on the economics of steamships (November–December 1991), Henry Petroski notes that in the early crossings of April 1838, the *Great Western* almost caught up with the *Sirius*. Professor Petroski wrote: "Capable of gaining on the *Sirius* at the rate of two knots per hour, the *Great Western* was able to make the crossing in just over 15 days...."

As a former Navy person, I am struck by the degree to which history might have changed if the *Great Western* had lived up to her potential. A knot is by definition a speed of one nautical mile per hour. Two knots per hour represents a rather gentle acceleration of about 3.18 feet per second per hour. Had the *Great Western* maintained this steady acceleration, she would have started very slowly from Land's End, and would have barely had steerage-way after an hour of steaming. Things would have picked up after that. Somewhere in the mid-Atlantic, she would have reached her "hull speed" of perhaps 20 knots, and would thereafter start hydroplaning. By the time she arrived off the coast of New York, about 2,800 nautical miles away, she would have been whistling along at over 100 knots, and would have established a new record of just two days for the crossing.

What a sight she would have made, planing in with her side wheels screaming like banshees and a huge rooster tail in her wake! It would have been enough to make the grog-shop regulars swear off drinks for a whole afternoon.

Please tell Professor Petroski that I enjoyed his article, but just couldn't resist making these calculations.

K. Dexter Miller, Jr.
Houston, Texas