in small-science disciplines, graduate students in high-energy physics plan small experiments (as part of routine data taking or of constructing, debugging and testing apparatus), build the necessary equipment and analyze the data as part of their thesis work. Frequently this is done in small groups, comparable to those in other branches of physics. In addition graduate training offers students involved in big science opportunities to learn from professionals in other fields and to develop the ability to work in large groups, which can be highly beneficial in their postgraduate careers.

Finally, I am puzzled by Kadanoff's views on basic-research funding. He states, "I do not think that the nation's or the government's budget for research or R&D is too small," but presents no supporting evidence. He himself writes that "over the last seven years, we have seen attrition in support for small-group efforts." During the same period the funding level for construction of new facilities in high-energy physics was about one-third (when corrected for inflation) of what it was around 1970. The fraction of GNP that is devoted to basic research in the US is smaller than in Japan and many Western European countries. Kadanoff refers to "billions for the Superconducting Super Collider, but inadequate funds to even run the neutron sources" mentioned above. I, too, deplore the underutilization of our scientific facilities for lack of money. However, I should point out that the $3.2 billion (in 1988 dollars) for construction of the SSC is the result of an integration over ten years and is, so far, strictly virtual: Not a penny has been appropriated. The associated R&D program receives less than one-third of the amount of funding that was projected as necessary four years ago.

The worth of a scientific project cannot be judged *a priori* on the basis of its size. Rather, we should determine if it represents intellectually and technically valuable research and then press for the resources necessary to carry it out.

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3/88

Nuclear Test Estimates Yield Criticism

The article "Yields of US and Soviet Nuclear Tests" by Jack F. Evernden

and Gerald E. Marsh (August 1987, page 36) claims that studies by Evernden and his coworkers in the early 1970s demonstrated the existence and size of a significant bias (compared with the Nevada Test Site) in magnitude estimates for Soviet explosions. It further claims that the US government refused to accept these conclusions and therefore has been making "incorrect" yield estimates. The article then reviews various lines of evidence for this bias and adds a variety of nonseismological arguments to draw sweeping conclusions about Soviet nuclear testing practices and weapons systems. We do not wish to comment on these larger issues, but we think it is important to point out that the article considers only a portion of the extensive evidence that has been exhaustively studied by government, industry and university seismologists who have been struggling with the m_b bias question for years. Virtually every paragraph in the sections on bias estimation and procedures (pages 38–40) contains one or more of the following distortions: a magnitude–yield relationship that is different from what nearly all other seismologists have deduced from the data, failure to acknowledge key observational data, and conclusions selected from one extreme of a range of possibilities.

This is a complex subject, but we can point out some revealing problems with the article. For example, the article makes much of the comparison of independent yield estimates based on the magnitudes m_b and M_S, but sweeps aside evidence that M_S underestimates Soviet yields because it is biased by an out-of-phase contribution of tectonic strain energy release (that is, nonexplosion energy) to the signals. The authors' assertion that "there is no evidence for such a perturbation" ignores extensive research that establishes its reality beyond doubt. Indeed, this nonexplosion contribution is larger than the contribution from the explosion itself for many signals. Correction for this effect significantly reduces the bias estimate obtained by comparing M_b and M_S. Another indication of the selectivity displayed by Evernden and Marsh is that they remark that the "ideal and direct technique [for measuring the m_b bias] would be to have amplitude data for P waves of distant earthquakes or explosions as recorded at or very near the Soviet test sites," without reporting that a very thorough experiment of exactly this type was conducted some years ago to determine the m_b bias between the Nevada Test Site and sites in eastern North America. The latter sites were selected to provide an upper bound for the m_b bias for the Soviet test site. However, the measured m_b bias turned out to be small, and large, semiscientific, *ex post facto* "corrections" are required if one wishes to make the results of this experiment consistent with a bias even as large as 0.25 for these "upper bound" sites. Evernden and Marsh propose a bias of 0.45, and since magnitude bias is in logarithmic units, a 0.2 difference converts to a 58% difference in the yield estimate. Actually, we do not believe experiments of this kind provide conclusive evidence about bias, but the results should not be ignored because they fail to confirm the authors' model. The seismological literature contains many other examples of contradicting evidence on seismological aspects of yield estimation (for example, see the review by Bache in the December 1982 *Bulletin of the Seismological Society of America*, page S131, which lists many references).

The truth is that there is a large uncertainty in yield estimates for test sites for which there is no independent calibration information (in essence, explosions of measured yield). Without this calibration information, it is necessary to estimate with great precision the effects of geophysical phenomena of which seismologists now have only a qualitative understanding. It is certainly possible to begin with a conclusion (for example, that the largest Soviet tests since 1976 have yields of 150 kilotons) and selectively collect evidence consistent with this conclusion, but the same can be done for many other hypotheses about Soviet testing. What has not yet been done by anyone is to construct a consistent and complete geophysical model that can be confirmed by normal scientific hypothesis testing and that leads to yield estimates of high accuracy. Absent this satisfactory scientific model, seismologists must consider all relevant evidence and strike a balance among the contradictions that plague the subject. Evernden and Marsh have not done this, and there is much less confidence in their conclusions than they claim.

We are seismologists who have specialized in nuclear test monitoring for many years. One of us (Romney) has been continuously involved since 1949 and was a member of the 1958 Geneva Conference of Experts, which established the scientific basis for the inital nuclear test ban treaty negotiations. Another (Ryall) was a member of the Department of Defense panels that thoroughly reviewed seismic yield estimation methods and uncertainty during the years from 1982 to 1986. He is also a past president of the Seismological Society of America. Bache was an early organizer and participant in those panels, and he has published numerous papers on technical aspects of the seismic yield estimation problem.

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EVERNDEN AND MARSH REPLY: It is unfortunate that Thomas C. Bache, Carl F. Romney and Alan S. Ryall chose to not address the "sweeping conclusions" and "larger issues" raised by our article. As it is, they have avoided our main thrust, limiting themselves to criticisms that are easily rebutted.

We will respond to their criticisms in order:

Disagreements relative to the appropriate m_b magnitude versus yield curve for Nevada Test Site explosions in tuff and granite are significant only at yields of 50 kilotons and less, not in the range of relevance to our paper.

The government, industry and university studies ignored by us remain a mystery. If they mean the US study they mention later in their letter, see below.

To illustrate how "extreme" our views are:

▷ Three technical panels convened by various agencies of the Department of Defense in the past two years or so have specifically rejected the positions of Bache and Romney, while concluding that the yield versus magnitude and bias relationships were adequately known to allow the conclusion that there was no evidence of Soviet cheating. Such conclusions required acceptance of a bias value at least as high as 0.35.

▷ The CIA and the seismological group at Lawrence Livermore National Laboratory have stated that there is no evidence of Soviet noncompliance, and LLNL has repeatedly so stated to Congress. These positions require acceptance of a high bias value.

▷ We know of no technical conference that has reached different conclusions.

▷ Several eminent American seis-

mologists have testified to Congress on these matters. Lynn Sykes and Paul Richards (Columbia University) have given opinions agreeing with ours and thus accepting a large m_b bias between NTS and Semipalatinsk. Charles Archambeau (University of Colorado) has given similar testimony. Though the details of their analyses may differ slightly from ours, they reach similar conclusions as to bias and yields of Soviet tests.

Our position appears to be "extreme" only in that it disagrees with that of Bache, Romney and Ryall, not in that it disagrees with that of nearly all other investigators. Even this disagreement is nearly nonexistent, as Bache and his coauthors appear to accept bias values of 0.35 for Novaya Zemlya (see below) and thus at least equivalent values for Semipalatinsk. At no place do the authors suggest a proper value of bias between NTS and Semipalatinsk.

There have been a variety of problems in interpretation of the M_S values of the "150-kiloton" events at Semipalatinsk. What we pointed out was not the detailed agreement of yield estimates for these events by m_b and M_S (these estimates average 175 and 115, respectively, as one can read from our figure 3d using an m_b bias of 0.45), but the agreement on average of these two modes of estimating yields at Semipalatinsk when using a 0.45 m_b bias. Numerous explosions at Semipalatinsk show little tectonic release, and it has been documented since at least 1976 that the discrepancies between estimates of yield based on M_S and uncorrected m_b values are as great at nearly all Soviet PNE (peaceful nuclear explosion) sites as at Semipalatinsk and Novaya Zemlya. Consistent estimates of yield for these explosions are obtained based on m_b and M_S via use of large m_b bias corrections controlled by the phenomena described in our article, thus establishing that resort to tectonic release as a truly confusing factor in yield estimation is out of the question. Bache and his coauthors make no comments about Novaya Zemlya, as there never has been evidence of pronounced stress release there, giving them no recourse but to accept our conclusions. Thus their comments relative to tectonic release are irrelevant. It would appear that when the smoke screens are dispersed, the only issue between them and ourselves is simply whether to use 0.35 or 0.45 as the m_b bias for Semipalatinsk.

The authors don't believe the results of the US study of the m_b bias between the eastern United States and western Nevada are pertinent to Semipalatinsk. We couldn't agree more, and thus saw no reason for mentioning the study. All discussion of the pertinence of this old exercise to Semipalatinsk is now moot as Kenneth Priestly and coauthors, using Natural Resources Defense Council data obtained from stations in the general area of Semipalatinsk, report an m_b bias between western Nevada and the Semipalatinsk area of 0.45 ± 0.05!

It is true that we ignored numerous references. Ours was a short review article, not an original contribution, and we cited what we deemed the most pertinent references.

Though these authors ignore our nonseismological arguments bearing on the proper value of the m_b bias, we wish to reiterate that we feel those arguments are both pertinent and powerful.

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3/88

High-Altitude Physicists

It is well known that physicists and mountains have long had an affinity. Many of us have trekked and climbed, with great pleasure, in Nepal. On my last trip—in the spring—to that beautiful country, I made the acquaintance of the Nepal Physical Society. This entity publishes a small journal and generally reflects the interests of Nepalese physicists and physics teachers. While in Nepal on this last trip, I gave a lecture to a group of teachers and students on, of all things, the width of the Z^0 and the number of neutrino flavors, a subject that had been discussed in the journal. I had an audience of some 50 people. I am writing to encourage all of you who plan to go to Nepal to contact the Physical Society and perhaps also give such a lecture. (The address is Nepal Physical Society, Department of Physics, Tri-Chandra Campus, Ghantaghar, Kathmandu, Nepal.) Nepal is a very poor country, so do not expect any material rewards. However, it will add greatly to your *sönam*—"merit"—and, apart from what this may do for you in the next life, you will meet some delightful people and have a more meaningful, less touristic visit to that country.

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