# Letters

## Pseudo Skua?

For many bird watchers the class Aves may usefully be divided into two superorders, trash and nontrash birds. The South Polar skua is most assuredly a nontrasher, so most bird watchers can easily recite time, place, and weather for most, if not all of their sightings of this wonderful bird. We have observed South Polar skuas in the Pacific and Atlantic oceans, the Northern and Southern hemispheres, and in reasonable profusion in their home port of Tierra del Fuego. Never have we seen a skua resemble even remotely the bird shown on page 243 of your 20 July issue (News & Comment). The all-white breast and massive bill are completely wrong for any skua. What, then, is this bird? Assuming that the photo was taken in the vicinity of skua breeding grounds, we offer the suggestion that this bird is the kelp, or southern black-backed gull, *Larus dominicanus*.

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*Response*: The bird in the photo is a South Polar skua (*Catharacta maccormicki*). Resolution of the alleged discrepancy between the photo and the photo caption, however, is not as simple a task as it would at first appear. The photo is of a morph that relatively few bird watchers have seen, although this form of the skua is abundant. The photo was taken at Cape Crozier, Ross Island (77°S, 178°E), during late January, the place and the date both being important. In high-latitude populations of this species, that is, those populations of "continental" Antarctica, which is cold and dry, light-phase birds, such as the one pictured, make up about 50% of the population [58% at Ross Island (*1, 2*)]. Dark-phase birds make up 20%, and the remainder are intermediate-phase. Descriptions of the color phases have been published (*1, 2*); an approximation of the color patterns is shown by Harrison (*3*). Low-latitude populations reside along the Antarctic Peninsula, or "maritime" Antarctica, which is warm and wet and is the area frequented by almost all persons on nature tours. The latitude of Palmer Station, the locality where the skua controversy described in the 20 July issue of *Science* took place and near the southernmost point of most nature tours, is only 64°S. Although I have not taken a detailed survey on my five trips to the Antarctic Peninsula area, less than 5% of South Polar skuas there can be classified as light-phase. I have seen none as light as the one pictured, which is an extreme not uncommon farther south. The date of the photo is important because, in the continous light of summer, skua plumage fades, even in the Antarctic Peninsula area; by late January skua plumage has been exposed to sunlight almost continuously for 2 months at Palmer Station and continuously for 3 to 4 months at Cape Crozier. Light-phase birds become even lighter. The plumage of the bird in the *Science* photo is similar in color pattern, but perhaps even lighter than the lightest bird shown by Devillers (*2*). The darker colorations of skuas in the warmer, wetter part of their breeding range is in accord with zoogeographic patterns observed commonly in other animals.

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REFERENCES

1. D. G. Ainley, L. B. Spear, R. C. Wood, *Condor* **87**, 427 (1985).
2. P. Devillers, *Auk* **94**, 417 (1977).
3. P. Harrison, *Seabirds: An Identification Guide* (Houghton Mifflin, Boston, MA, 1985).

## Changing Science

We are grateful that Eliot Marshall (News & Comment, 6 July, p. 14) gives the National Science Foundation credit for keeping the door open to a broad range of ideas. But it is unfortunate that he keeps alive the overused literary scenario of the unappreciated maverick versus the closed-minded establishment.

For example, he neglects the effect on the science of his time of Alfred Wegener's ideas of "moving continents." Wegener was not an unheard voice crying in the wilderness. His book *Die Entstehung der Kontinente und Ozeane* ran through four editions, and journals saw no problem in publishing his articles (*1*). His effect on scientific thinking was like the tide coming in, slow but steady. The first science book I remember reading in junior high school (about 1940) (*2*), written during Wegner's lifetime, gave full credit to him and his idea of continental drift. By 1955 this idea was in the mainstream of science as a serious contender to explain the history of the earth (*3*). At the time of his tragic death, Wegener was a respected meteorologist, not an "intellectual outcast." And he was not alone in postulating boldly that continents moved (*1, 3*).

Science has changed totally several times over in the last 40 years. Most of our present scientific thinking is less than a decade or so old. Ideas "beyond the pale" crowd in and change science. I have found that scientists vote with their feet quite nimbly. The unseen hand of self-interest dominates the scientific marketplace. Any discussion of an "uphill battle for acceptance" should include such counterexamples as relativity, quantum mechanics, DNA, and the quark model, all of which gained acceptance as fast as word got around. The question could better be phrased, What are the dynamics that make some ideas take months and others take decades to find their equilibria? It is tempting to inject the drama of rejection into accounts of science [a technique perfected by some parts of the popular press (*4*)], but the scientific arguments should not be relegated to a secondary role.

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REFERENCES AND NOTES

1. See, for example, M. Schwartzbach, *Alfred Wegener: The Father of Continental Drift*, C. Love, transl. (Science Tech, Madison, WI, 1987).
2. W. M. Reed, *The Earth for Sam: The Story of Mountains, Rivers, Dinosaurs and Men* (Harcourt Brace, New York, 1930), pp. 45–47.
3. G. A. Barnard, as quoted in N. I. Fisher, T. Lewis, B. J. J. Embleton, *Statistical Analysis of Spherical Data* (Cambridge Univ. Press, New York, 1987), pp. 12–13; P. M. S. Blackett, *Lectures on Rock Magnetism* (Weizmann Science Press, Jerusalem, 1956), pp. 31–37.
4. See D. Hofstadter's discussion of the *National Enquirer* [*Sci. Am.* **246**, 18 (February 1982)].

## Boron Neutron Capture Therapy: The Role of Peer Review

I write to comment on the 13 April article by Mark Crawford, "Pork in a medical wrapping" (News & Comment, p. 156) and the subsequent letter (20 July, p. 227) by Ronald V. Dorn III *et al.* of the Idaho National Engineering Laboratory's (INEL's) Power Burst Facility's (PBF's) boron neutron capture therapy project. The most important issue raised by Crawford's article is whether the decision-making process on funding of scientific research by the federal government should be permitted to bypass or (more accurately in this case), to ignore, peer review. The INEL group has used and is using political clout to obtain large amounts of funding, contrary to the recommendations of well-qualified peer-review panels, for their boron neutron capture therapy (BNCT) project. In previous years, this funding has exceeded that for peer-reviewed and recommended BNCT projects in other

laboratories by about an order of magnitude. Currently INEL is again seeking even more funding, $13.5 million for fiscal year 1991, by direct congressional action and against the specific April 1990 recommendations of the Department of Energy's Health and Environmental Research Advisory Committee (HERAC) (*1*).

Dorn's letter mentions that the INEL BNCT project has undergone several peer reviews, including the April 1990 review by the HERAC panel (*1*). The impression may therefore be conveyed that the HERAC review and other outside reviews, such as that of the 1988 National Cancer Institute (NCI) review panel (*2*), approved the INEL project. In fact, the NCI review panel recommended against approval of the INEL project. The recent HERAC review panel recommended that "Conversion of the reactor (PBF) for medical use should not occur and it should not be maintained in a standby state for medical purposes."

Funding of INEL's BNCT program against peer-review recommendations undermines the important principle that support should be based on objectively determined merit. It is also clear that funding obtained through use of political muscle results in decreased funding for meritorious peer-reviewed projects, since there is not enough funding available for support of all good research. Undermining the effectiveness of our country's science management infrastructure is also a cost of science support determined politically rather than on the basis of merit.

A few comments on the necessary conditions for clinical trials for BNCT are appropriate because Dorn's letter provides a one-dimensional and largely self-serving discussion of the requirements for BNCT therapy. To develop a complex treatment modality, such as BNCT for brain tumors, a number of important capabilities in addition to a good neutron beam must be present. Of critical importance are the first-class credentials of the investigators and the quality of the supporting scientific and medical infrastructures. Also critical is the proximity and participation by a major tertiary medical center with skilled radiation oncologists, neurosurgeons, radiologists, and top expertise in physics support. Finally, neutron beam facilities such as those at the Massachusetts Institute of Technology (MIT) and Brookhaven National Laboratory (BNL) that have been *experimentally* demonstrated—not just calculated (as is the case with INEL's PBF beam)—to be suitable for the irradiation of patients from safety and efficacy considerations, are essential.

The message in Dorn's statements that the epithermal beam, which could be built at the PBF (for "a few tens of millions") (*3*) is essential for the development of BNCT and that other currently available neutron beams, such as those at BNL and MIT, are not adequate is incorrect. The capabilities of the existing BNL and MIT therapy beams are adequate for clinical trials based on objective criteria of safety and efficacy. These beams are discussed in the peer-reviewed papers in (*4*) as well as in other scientific reports.

Ultimately, the judgment concerning the capability of any group to carry out BNCT trials should be made by independent and expert review panels. These decisions should be insulated from parochial interests and from political pressure of any type. Such an approach will ensure the best chance of success for BNCT and the most effective use of public resources.

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REFERENCES

1. "Review of the Office of Health and Environmental Research Program, Boron Neutron Capture Therapy" (Office of Energy Research, Department of Energy, Washington, DC, 1990).
2. "National Cancer Institute review of Power Burst Facility Boron Neutron Capture Therapy Program, site visit, 9 February 1988" (National Cancer Institute, Bethesda, MD, 1988).
3. Senator J. A. McClure (R–ID), testimony before the Subcommittee on Energy and Water Development, Committee on Appropriations, U.S. House of Representatives, 3 April 1990.
4. *Neutron Beam Design, Development and Performance for Neutron Capture Therapy, March 29–31 1989; Proceedings of an International Workshop at the Massachusetts Institute of Technology*, O. K. Harling, J. A. Bernard, R. G. Zamenhof, Eds. (Plenum, New York, in press).

## Long-Term Potentiation

I enjoyed Marcia Barinaga's account of the current excitement among neuroscientists studying the phenomenon of long-term potentiation ("The tide of memory, turning," Research News, 29 June, p. 1603), but there is one point on which I would like to set the record straight. Long-term potentiation (LTP) was discovered not, as Barinaga states, in my lab at the National Institute for Medical Research, but by Terje Lømo, working in Per Andersen's lab at the Institute of Neurophysiology, University of Oslo, in Norway. Lømo published a brief account of his discovery in 1966 (*1*), several years before the first detailed descriptions appeared in 1973 (*2*). The Oslo lab has continued to make important contributions to LTP and to hippocampal neurophysiology in general, not least by introducing the transverse hippocampal slice preparation used both by Bekkers and Stevens and by Malinow and Tsien for the quantal analysis of LTP described in Barinaga's article.

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REFERENCES

1. T. Lømo, *Acta Physiol. Scand.* **68** (Suppl. 277), 128 (1966).
2. T. V. P. Bliss and T. Lømo, *J. Physiol.* **232**, 331 (1973); T. V. P. Bliss and A. R. Gardner-Medwin, *ibid.*, p. 357.

*Erratum*: In the report "Polarity and velocity of sliding filaments: Control of direction by actin and of speed by myosin" by J. R. Sellers and B. Kachar (27 July, p. 406), figure 4 (p. 407) was incorrectly printed. The correct figure appears below.

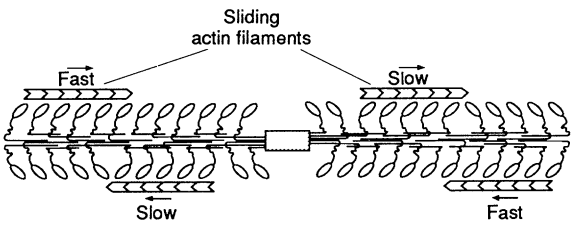


**Fig. 4.** Schematic diagram showing the allowed sliding interactions of the polar actin filaments with the bipolar myosin filament. The arrows indicate the direction of movement. The myosin heads are schematically shown at the ends of their power strokes. The "reverse chevrons" concept for the myosin heads contacting actin moving away from the center of the myosin filament is taken from Reedy *et al.* (*10*). The crosshatched area represents the bare zone.

*Erratum*: In the News & Comment article "Bloch leaves NSF in mainstream" (24 Aug., p. 848), the bar graph on the right in the illustration on page 850 was incorrect. The colors blue (representing "research and related activities") and green (representing "centers") were interchanged. Therefore funding for centers indeed accounts for less than 10% of the National Science Foundation's total budget.

*Erratum*: On the map accompanying the News & Comment article "Eastern Europe: Missing an opportunity" (6 Apr., p. 20), the number of scientists and engineers working in research and development in Poland was incorrectly given. The number of scientists only is 76,000, but more than 160,000 scientists, engineers, and others work in research and development in Poland's universities and industries.

*Erratum*: The affiliation of Richard C. Ogden, the fourth author of the Research Article "De novo design, expression, and characterization of Felix: A four-helix bundle protein of native-like sequence" by M. H. Hecht *et al.* (24 Aug., p. 884) should have read, "Agouron Institute, 505 Coast Boulevard South, La Jolla, CA 92037."

*Erratum*: The name of the reviewer of *Apprenticeship in Thinking* [Barbara Rogoff (Oxford University Press, New York, 1990)] (10 Aug., p. 684) was incorrectly printed. It should have been James V. *Wertsch*, not James V. Wertsh.

*Erratum*: The price of the book *Science and the Navy: The History of the Office of Naval Research* by Harvey Sapolsky, reviewed in the 17 August issue (p. 808) was listed incorrectly. The correct price is $24.95.