Letters to the Editor

Letters (~300 words) discuss material published in *Science* in the previous 6 months or issues of general interest. They can be submitted by e-mail (science_letters@aaas.org), the Web (www.letter2science.org), or regular mail (1200 New York Ave., NW, Washington, DC 20005, USA). Letters are not acknowledged upon receipt, nor are authors generally consulted before publication. Whether published in full or in part, letters are subject to editing for clarity and space.

Retraction

AN AD HOC INVESTIGATORY COMMITTEE AT the University of North Carolina at Chapel Hill has found that the last author (S.A.L.) of the paper "BRCA1 required for transcription-coupled repair of oxidative DNA damage" (*1*) has "fabricated and falsified research findings," and this paper has been implicated. The last author disputes this finding of the ad hoc committee. Although some findings in the paper, notably those in Fig. 4, are valid, the overall integrity of the paper cannot be supported, and it should be retracted. The first four authors of the paper have not been implicated in any way. L.C.G., A.M.L., and B.H.K. can certify the quality of the cells used in all of these experiments and are willing to provide them to other investigators who would like to address the role of BRCA1 in transcription-coupled repair.

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Reference

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Larvae Retention: Genes or Oceanography?

IN THE RECENT REPORT BY M. S. TAYLOR AND M. E. Hellberg ("Genetic evidence for local retention of pelagic larvae in a Caribbean reef fish," 3 Jan., p. 107), the "local retention" of larvae of the goby fish *Elacatinus evelynae* could be alternately, and more accurately, interpreted as "limited dispersal extending to zoogeographic barriers." The geographic distribution of *E. evelynae* color forms (Fig. 1 in the Report), largely based on my published (*1*) and unpublished work (*2*) on the genus (used without acknowledgment), shows their broad distribution (hundreds of km) with abrupt junctures between color forms. Similar patterns exist among other color forms and species (*1*, *2*). The amount of genetic interchange documented across islands within a color form is consistent with the known biology of these fishes. However, the abrupt break in genetic flow between the white and blue forms of this fish at the Mona Channel is almost certainly the result of a marine zoogeographic barrier between Puerto Rico and Hispanola. This "Mona barrier" exists for a number of other reef fishes with relatively short larval lives (*2*). A spectacular example is *Gramma melacara*, the most common reef fish below 40 m depth in the western Caribbean and Bahamas (*3*), which does not occur east of the Mona Channel. Similar barriers exist in the Yucatan channel, the southwest Caribbean, and central-northern Bahamas (*2*). Although these barriers filter out only a small percentage of species, they are important for considerations of fisheries management and connectivity in the overall region. The distributions of some reef fishes, such as *Elacatinus atronasum* in the northern Bahamas (*4*) and *Elacatinus lori* in the Gulf of Honduras (*5*), are coincident with barriers associated with distinct current systems.

The blue form of the goby fish *Elacatinus evelynae*.

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It is not easy for fish larvae hatched in shelf waters to become part of oceanic zooplankton, much less cross interisland open water barriers. Many simply remain in or near shelf waters during the few weeks of their larval life but may not play any active role in doing so. In their Perspective on the Taylor and Hellberg Report ("Why gobies are like hobbits," 3 Jan., p. 51), S. R. Palumbi and R. R. Warner invoke a "hidden level of drift control" as the only possible explanation for Taylor and Hellberg's results, but it is not necessary to provide any behavioral explanation to account for distributions and genetic interchange. "Simple" oceanography will do.

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Response

WE DEEPLY REGRET OUR FAILURE TO acknowledge Colin. He generously provided us with a copy of his unpublished manuscript on *Elacatinus* zoogeography that, in combination with his cited book (*1*), provided most of the distribution records for Fig. 1 of our Report.

In his Letter, Colin states that zoogeographic barriers exist at the Mona Channel (between Puerto Rico and Hispaniola) and in the central Bahamas. Although the range boundaries of a few *Elacatinus* species and color forms occur at the Mona Channel and at the southern end of Exuma Sound, other *Elacatinus* species span these regions (*1*). We agree that such a barrier, even for a subset of species, would have important consequences, and are currently pursuing the genetic data from other species that will be needed to evaluate the generality of the Mona Barrier.

Colin suggests that the phylogeographic structure observed within blue and white color forms of *Elacatinus evelynae* is consistent with the known biology of these gobies and that simple oceanography predicts the patterns of interisland differentiation we found. We disagree. Although Colin does not specify the aspects of reef fish biology to which he refers, the one most commonly discussed is pelagic larval duration. If simple oceanography ruled, then genetic differences among islands would inversely correlate with the time larvae spend developing in the water column. However, other Caribbean reef fish with demersal eggs and pelagic larval durations identical to *E. evelynae* showed widely differing patterns of genetic structure among some of the same localities we sampled (*2*). The assumption that species with a lengthy pelagic larval duration disperse to distant populations is overly simplistic. Whether larvae fail to entrain in oceanic currents by physical or behavioral means (or some combination of these), our results suggest the larvae of *E. evelynae* fail to successfully recruit far from their natal populations.

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Response

WE AGREE THAT OCEANOGRAPHIC PHYSICAL barriers may be sufficient in some cases to limit gene flow between marine populations, but it certainly is not clear that "simple" oceanography will always be sufficient to isolate populations over the distances shown by Taylor and Hellberg in their Report. There is strong population genetic structure among islands between different color groups. But as Taylor and Hellberg show, there is also strong genetic differentiation within color groups over relatively short distances. This differentiation in the absence of color-based selection strongly suggests that dispersal is limited beyond what would be expected from simple oceanography. What Colin offers as fact—that shore-spawned pelagic larvae do not enter oceanic waters and stay close to home—is actually one side of a lively ongoing controversy in marine ecology. Although opinions on this subject may be changing (this is why articles like Taylor and Hellberg's are noteworthy), we are by no means at a point where we can identify the mechanisms for limited dispersal. It is certainly premature to dismiss larval behavior as an important factor. In the end, this discussion points to the need for more detailed studies of dispersal, an exciting and vitally important area of focus in marine ecology.

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Clarifying Standards for Using Placebos

IN HER ARTICLE "ETHICS GROUP GIVES QUALIFIED nod to placebos" (News of the Week, 14 Feb., p. 995), Gretchen Vogel notes that the European Group on Ethics in Science and New Technologies has judged that "placebo-controlled trials can sometimes be justified in developing countries, even when proven treatments are available in wealthy countries." As she correctly points out, this is contrary to the Declaration of Helsinki, a declaration of the World Medical Association that gives worldwide standards for clinical research ethics, as it was revised in 2000. However, the World Medical Association issued a clarification in 2001 and, with minor revisions, incorporated this clarification into its 2002 revision of the Declaration (*1*). The relevant passage in this clarification states: "[A] placebo-controlled trial may be ethically acceptable, even if proven therapy is available… [w]here for compelling and scientifically sound methodological reasons its use is necessary to determine the efficacy or safety of a prophylactic, diagnostic or therapeutic method…" The statement of the European Group is not clearly incompatible with the current Declaration of Helsinki. This is because the recent clarification is not very clear. Indeed, the many critics of the 2000 version of the Declaration who protested that its position on placebo use was overly restrictive are now concerned that the clarified version may be interpreted as being overly permissive.

We prefer the much clearer guidance on this matter provided in the Council of International Organizations of Medical Sciences (CIOMS) International Ethical Guidelines for Biomedical Research Involving Human Subjects (revised version issued in 2002) (*2*). The relevant passage is the following:

"An exception to the general rule (requiring established effective treatment as the control condition) is applicable in some studies designed to develop a therapeutic… intervention for use in a country… in which an established effective intervention is not available and unlikely in the foreseeable future to become available, usually for economic or logistic reasons. The purpose of such a study is to make available to the population of the country… an effective alternative to an established effective intervention that is locally unavailable. Accordingly, the proposed investigational intervention must be responsive to the health needs of the population from which the research subjects are recruited and there must be assurance that, if it proves to be safe and effective, it will be made reasonably available to that population. Also, the scientific and ethical review committees must be satisfied that the established effective intervention cannot be used as comparator because its use would not yield scientifically reliable results that would be relevant to the health needs of the study population. In these circumstances an ethical review committee can approve a clinical trial in which the comparator is other than an established effective intervention, such as placebo or no treatment or a local remedy."

The guidelines issued by the European Group are generally compatible with, albeit not as specific as, the CIOMS statement. Each of these ethics statements now seems to require a protocol-by-protocol judgment of the ethical issues, rather than a categorical rejection of the use of placebo where standard treatment exists. We have recently suggested guidelines for exercising such judgment when evaluating proposals to use placebo-controlled studies, using as an illustration the evaluation of treatments for schizophrenia (*3*).

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References

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CORRECTIONS AND CLARIFICATIONS

Reports: "Eye-specific retinogeniculate segregation independent of normal neuronal activity" by A. D. Huberman *et al*. (9 May, p. 994). In the reference list, references (*2–16*) and (*18–27*) had incorrect volume and page numbers and a wrong year. The corrected reference list is shown here.

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Random Samples: "Lost in the Shuffle" (2 May, p. 735). Iraqi microbiologist Huda Salih Mahdi Ammash received a master's degree from Texas Woman's University in Denton, and not from the University of North Texas, as reported.

News Focus: "From bioweapons backwater to main attraction" by J. Bohannon (18 Apr., p. 414). The organizer of the 5th International Conference on Anthrax should have been identified. It was the Pasteur Institute.

Special Section on Biological Imaging, News: "Quantum dots get wet" by C. Seydel (4 Apr., p. 80). The image displayed on p. 80 was provided by Xingyong Wu of Quantum Dot Corporation.

News Focus: "How much are human lives and health worth?" by J. Kaiser (21 Mar., p. 1836). The acronym QALYs should have been described as Quality Adjusted Life Years.

Editor's Choice: "X-rays hit the nanospot" (14 Mar., p. 1629). The note incorrectly described the nanostructured lenses as having been fabricated in a diamond substrate. The substrate was, in fact, silicon.

TECHNICAL COMMENT ABSTRACTS

COMMENT ON "Separate Evolutionary Origins of Teeth from Evidence in Fossil Jawed Vertebrates"

Carole J. Burrow

Smith and Johanson (Reports, 28 February 2003, p. 1235) argued that the presence of regular rows of denticles, increasing in height and composed of dentine, indicate that eubrachythoracids have "real" teeth that arose separately from those of other vertebrates. This conclusion may be flawed, because these features are also seen in the dentition of basal placoderms and in tubercles on dermal plates in several placoderm groups.

Full text at www.sciencemag.org/cgi/content/full/300/5626/1661b

RESPONSE TO COMMENT ON "Separate Evolutionary Origins of Teeth from Evidence in Fossil Jawed Vertebrates"

M. M. Smith and Z. Johanson

Basal placoderms do not show the detailed dentition pattern of derived forms, nor do any of the examples proposed by Burrow show a prepattern for ordered rows of teeth. We cite new evidence to support an endodermal origin for dentition patterning as part of the visceral-skeleton in jawed vertebrates. These data reinforce our argument for convergent evolution of teeth in placoderms.

Full text at www.sciencemag.org/cgi/content/full/300/5626/1661c