evolution, as they go against the dogma of eye evolution that can be found in most textbooks.

We not only presented sequence comparisons, but also found the conservation of splice sites that argue strongly in favor of the hypothesis that *eyeless* in *Drosophila*, *Small eyes* in the mouse, and *Aniridia* in humans are true homologs. We can now extend this list to the *Pax*-6 genes of squid, ascidians, nemertines, nematodes, and plathelmints. However, the much stronger argument for true functional homology comes from the fact that we can induce ectopic eyes with the mouse gene in *Drosophila*. Meanwhile, we have shown the same for the squid and ascidian genes. Evidence of this kind is not easy to obtain and is entirely new. Already, on the basis of our first paper, Stephen J. Gould has proposed (3) that our finding challenges the traditional model of eye evolution, which assumed that primitive eyes evolved separately in more than 40 different phyla (4) and that the prototypic eye might have evolved only once in evolution. We were holding back on this interpretation until we had carried out the crucial experiment, which was to induce ectopic eyes with both the *Drosophila* and the mouse gene. On the basis of these experiments, we are proposing now that the prototypic eye arose only once in evolution and that subsequent convergent evolution gave rise to the image-forming eyes of vertebrates and cephalopods, whereas the compound eyes of insects resulted from divergent evolution. The main difference from the "traditional" view is the assumption of a single, rather than more than 40, prototypic eyes. Our hypothesis is much more compatible with Darwin's theory, because the prototypic eye evolved before the time when selection was effective as a driving force, as stated by Darwin himself.

We have not implied that *eyeless* only functions in eye morphogenesis. To the contrary, we stated clearly (2, p. 1791)

> In addition to eye morphogenesis, *ey* controls other functions in the developing nervous system, because null mutations are lethal, and the loss of eye structures alone is not the cause of lethality.

The reason for proposing a new type of master control gene comes from the observation that the loss-of-function mutation leads to a loss of eye structures rather than a switch in cell determination, as in the previously described homeotic mutations.

We do not think that we have overstated the conclusions drawn from our experimental data. Of course, it is difficult to prove an evolutionary hypothesis, but we continue to accumulate evidence in favor of our admittedly revolutionary idea.

Walter J. Gehring
Biozentrum, Universität Basel,
CH-4056 Basel, Switzerland

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NIH Regional Primate Centers

Of particular interest in Jon Cohen's News & Comment articles about changes in AIDS research control at the National Institutes of Health (NIH) (2 Feb., p. 590; 15 Mar., p. 1491) were statements relating to AIDS research at the seven NIH Regional Primate Research Centers (RPRCs).

As the former director of the RPRC program I addressed two subgroups of Office of AIDS Research Director William Paul's advisory committee on the subject of usage of the RPRCs by AIDS researchers. At that

time (a year ago), not a single "outside" scientist had been refused animals and expertise at the centers for a peer-reviewed, funded project. I believe that is still true today. Some in the audience objected, stating they knew this had occurred, but none could provide a specific example of such a refusal. My impression was that some investigators, lacking peer-reviewed project support, believed the RPRCs should provide animals and expertise "gratis." The perception among these investigators appears to be that there is not "equal access to nonhuman primate models." Such is not, and has not been, the case.

It is ironic that, with this increased interest in nonhuman primate models relative to AIDS, the RPRCs have been reported to have received less than a cost-of-living budgetary allowance during the current year. One would hope that if greater research were needed, it would be reflected in a more positive increase in support funds to these valuable research resources.

There would, however, be great value in a review of the total RPRC program, as the committee suggests. Each RPRC is reviewed extensively every 5 years, but a total review of the entire program has not been conducted for more than 15 years and is overdue. Two years ago, the plans for such a review were initiated, and NIH received an outstanding planning report by a blue-ribbon committee (which included AIDS researchers). Unfortunately, this report did not receive the approval of an NIH committee. One hopes that the research efforts relating to AIDS will not only build on the work that the RPRCs have done but, through appropriate review and evaluation, will further strengthen future research on this dread disease.

W. *Richard Dukelow*
Endocrine Research Center,
Michigan State University,
East Lansing, MI 48821–1225, USA

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Going to Sea

I strongly disagree with the suggestion, quoted by Jeffrey Mervis ("A fleet too good to afford?", News & Comment, 15 Mar., p. 1486), that academic research ought to be performed on ships provided lowest bids. This would be the worst of all possible outcomes. The great bulk of the nation's oceanographic research is done on ships of the University–National Oceanographic Laboratory System's (UNOLS's) fleet. The science operations conducted at sea represent the spectrum of the work done in our nation's premier scientific laboratories. This work may range from deploying large instruments such as remotely operated vehicles and deep-sea moorings, to probing the atmosphere with laser-based instruments, to studying trace elements under clean room conditions. The ships and their crew play a critical, and constantly changing, role in this work by properly handling and deploying instruments, station-keeping, and providing ship services that range from highly regulated electrical power for sensitive instrumentation to safe areas for research with radioactive isotopes. This type of experience is not developed elsewhere in the commercial shipping industry, and the crews of the ships in the UNOLS fleet represent a remarkable asset that has grown from within by long experience. Any ship that cannot excel at this spectrum of work will not remain competitive in the fleet. In an age when success rates for ocean science proposals are running as low as 5 to 10%, it would be a gross disservice to the science community to send researchers to sea on a vessel that could not be counted on.

Kenneth S. Johnson*
Moss Landing Marine Laboratories,
Moss Landing, CA 95039, USA

*Chair, UNOLS.