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.

Adélie Penguins and Environmental Change

In their Review "Environmental change and Antarctic seabird populations," (Special Issue on Polar Science, 30 Aug., p. 1510), J. P. Croxall *et al.* describe paradoxes, many of which exist only because of a limited review of available information. Stating that the paleoecological record shows close correspondence between ice cover and the presence of Adélie penguins is correct (*1*), but claiming this species' absence from Antarctica until after the Last Glacial Maximum (LGM) is not supported, particularly by their cited references [(*9*, *10*) in the Review]. These references indicate only that the species spread north to the tip of the Antarctic Peninsula during the Little Ice Age (responding to more persistent sea-ice cover) and has since been retreating, especially during this era of rapid warming (*2*). Adélie penguin genetics indicate strains that likely resulted from two refugial populations surviving the LGM (*3*).

Croxall *et al.* mislead by stating that, as a general rule, reduction in sea-ice extent would cause population decline in three pagophilic bird species. This is true, but in the case of Adélie penguins, the best studied of the three, only along the northwestern Antarctic Peninsula (~5% of the Antarctic coast) has the population noticeably declined in the past 50 years (*2*). Elsewhere during this era of alleged widespread decline in sea-ice extent, this species has been increasing (*4*). Moreover, once ice persistence decreases, extensive coastlines are available to be colonized, even recolonized– about half the Antarctic circumference (*5*). Given the recent rapid disappearance of ice shelves in the Antarctic Peninsula region (*6*), exposing low-lying coastal terrain, new colonies should be forming.

Croxall *et al.* repeatedly claim a widespread decline in the modern presence of sea ice. There are no supporting data for this claim, except for the northern Bellingshausen and Amundsen seas (*7*–*9*). A claim exists that the positions of whaling vessels, as affected by sea-ice extent, indicate a large (2.8° latitude), widespread retreat of sea-ice extent just before the satellite, remote-sensing era began (*10*). Unfortunately, those data are muddled by the economic realities of whaling; i.e., depletion of open-water species required a shift to the pagophilic minke whale (*Balaenoptera bonarensis*) during the key time period (*11*), thus bringing ships closer to the pack ice. Comparison of ship positions to satellite-sensed ice edges during summer (the whaling season) shows consistently further north positions of the ice edge (*12*). Applying this correction to ice edges derived from presatellite era data explains the discrepancy inferred from the ship positions (*13*). In addition, the current winter sea-ice boundary is within the spatial variation evident during the Holocene, as judged by prevalence of pack-ice–related diatoms in deep-sea cores (*14*).

Finally, Croxall *et al.* claim repeatedly that the Adélie penguin depends on Antarctic krill (*Euphausia superba*). If true, arguments later in their paper would apply because *E. superba* prevalence has been affected inversely by winter sea-ice cover, although only in the northern Bellingshausen Sea (*15*) where, as noted above, ice persistence has become sporadic. However, such dependence occurs only in that region during summer. Elsewhere, this penguin's summer diet combines *E. superba*, *E. crystallorophias*, and, especially, the fish *Pleuragramma antarcticum*; in 11 studies quantifying summer diet, the contribution of *E. superba* averages 40% (range 0 to 99%) (*1*). In the only study of winter diet [(*8*) in the Review], which they cite in a misleading way, *E. superba* contributed just 28% (the remainder: fish and squid). Therefore, the species adjusts its diet depending on prey availability, and diet should not be part of the discussion.

Croxall *et al.* end by asking for a fuller understanding of the climatic and environmental processes affecting the Antarctic fauna. We fully agree, but this requires full appreciation of the available information.

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Response

Throughout our Review, our primary concern was to achieve an overview of the complex processes influencing environment-prey-seabird interactions in the Southern Ocean around Antarctica. We did not have space to rehearse many of the species and site-specific details of the jigsaw; therefore, other researchers may well differ in the aspects and interpretations highlighted. Although we find some of the detailed perspectives of Ainley *et al.*'s Letter interesting, they focus on only one species of the three we discussed and do not, to us, seem to offer any substantial alternative insights.

In terms of the Last Glacial Maximum, we indicated the "virtual" absence of Adélie penguins on Antarctica, a statement not dissimilar to that of the senior author of the Letter: "land-nesting penguins (Antarctic genus *Pygoscelis*) could not have nested on the Antarctic continent, or at best at just a few localities" (*1*, p. 2670). Space precluded our addressing recent genetic studies, but the existence or otherwise of refugia has limited relevance to our main thesis.

We are twice criticized for making general statements concerning widespread declines in modern sea ice and consequent population declines. Nowhere do we say this; we are at pains to present the evidence for regional variation and for complex causes and consequences of population change. We did not address issues of potential colonization/recolonization of emerging ice-free land, partly because of space and partly because increased availability of breeding sites may be of limited relevance if feeding grounds in close proximity to them are absent. Predicting the location of feeding grounds is very difficult, given that regional climate change may also alter the general thermohaline circulation.

We did not reference the interesting paper by de la Mare [(*10*) in the Letter] in our Review. We are not aware of any primary literature publication properly addressing some of the issues raised by Ainley *et al.* about the analyses presented by de la Mare.

Although we referred (and not "misleadingly") to studies of winter diet, we focused on the substantial dependence on krill—still clearly the main dietary constituent from most studies at most sites—because only for this prey species are there published data on environmental influences and effects.

We prepared a Review, based on the published literature, in which we tried to credit the work that the Letter writers have done. Inevitably, we will have interpreted some of it differently than they might have. Equally, we were trying to set it in a broader context, to emphasize the likelihood that many factors contribute to observed changes in population size and extent and that unitary explanations are unlikely. The fact that they take issue with, in essence, a few matters of detail concerning one species, we regard as largely a vindication of our approach and conclusions. Indeed, we are pleased that our Review has stimulated some of the data holders to evaluate their data in ways analogous to our overview of the important issues.

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Developing Drugs for Parasitic Diseases

THE INITIATIVE DESCRIBED BY DENNIS Normile in his article "Norvartis kicks off institute for neglected diseases" (News Focus, 7 Feb., p. 811) is a welcome development, but it is a drop in the bucket compared with the huge burden of parasitic diseases confronting the developing world. Current drugs for treating these diseases are dogged by recurring problems of toxicity and development of drug-resistant parasites; this is in spite of strategies pointing to science's ability to make a difference (*1*).

Malaria, for example, is still one of the most lethal infections in history. It is estimated to kill about 2.7 million people a year, one million of them children, to say nothing of the related serious economic consequences for developing countries. The genetic code of the malaria parasite has been cracked. Understanding the genetics of *Plasmodium falciparum*, the parasite that causes the disease, and *Anopheles gambiae*, the mosquito that carries it, is a big step toward revealing novel drug targets that could effectively contain the spread of malaria in subtropical regions. But the obstacles to progress are significant.

An *Anopheles gambiae* mosquito, which carries the malaria parasite.

In an age of genomics, how can this be true? Of the more than 1200 drugs brought to the market between 1975 and 1997, only 1% are indicated for the treatment of parasitic diseases. The reason for this dearth of tuberculosis and antimalarial drugs is the cost of development, which rules out interest by big pharmaceutical companies. Other hurdles include the lack of adequate testing resources and animal models, the lack of scientific consensus on the critical path of drug development, and the importance of sustained clinical testing support in endemic areas.

The lack of interest and investment by commercial companies has led to a loss of expertise in industry and to a similar loss in research universities, where, as I can attest from my institutional experience and other studies, the number of academic scientists interested in parasitic diseases has been steadily declining.

The bottom line is that developing new drugs for neglected diseases of the developing world would require substantial

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investments from governments of developed nations and private scientific foundations and innovative partnering with private industry. This fact has been known for a long time. If that were to happen, the burden of parasites and neglected diseases in general would be exponentially reduced (*2*).

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Using Microbicides to Fight the Spread of HIV

WE WELCOME THE INCREASING INTEREST IN the use of microbicides to fight HIV ("Raising new barriers against HIV infection," R. S. Trager, News Focus, 3 Jan., p. 39). The need for a female-controlled method of HIV prevention to supplement male options (condoms and male circumcision) has been apparent for 20 years. Sadly, we have to agree with the judgment expressed in Trager's article by Zeda Rosenberg, the CEO of the new International Partnership for Microbicides, that if current strategies are followed, then a microbicide is unlikely to be approved "until after 2010." Moreover, it would still take several more years to achieve an epidemiologically significant level of use.

If we are to prevent the AIDS pandemic from overwhelming developing countries in the next decade, we desperately need to develop new methods for preventing the sexual transmission of HIV. These must be available, acceptable, and affordable to that quarter of the world's population eking out a living on less than $2 a day.

Perhaps the developed world is ill equipped to respond to such a challenge? Patenting, confidentiality, profit motives, and a goal of high efficacy with virtually zero risk may not be appropriate in the face of a disease that has the potential to kill more than half the population over 15 years of age in some countries (*1*). Ideally, money and know-how should be transferred to the frontline researchers in those countries most affected by AIDS.

We cannot afford the luxury of evaluating 50 candidate microbicides in the hope of finding one that might meet developed-world standards of acceptability sometime after 2010. A less-than-optimum microbicide available now will save more lives than an almost perfect one in a decade's time.

There are a number of promising leads that could be evaluated immediately. Intravaginal lemon juice has been used as an effective spermicide since time immemorial (*2*); we are currently studying its effects on the vagina of monkeys (*3*). Another possibility is that topical vaginal estrogen, by thickening and keratinizing the human vaginal epithelium, could protect women against HIV infection (*4*). The combined oral contraceptive pill, if given vaginally, is an effective contraceptive (*5*), and its estrogen might protect by thickening the vagina.

HIV is going to kill more people than died as civilians and combatants in World War II. The rapidity with which a microbicide reaches widespread use is a life-or-death issue for literally millions of women, nearly all living in the developing world. The international community must be more imaginative and courageous in attempting to develop simple, acceptable, low-cost solutions to this crisis.

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CORRECTIONS AND CLARIFICATIONS:

Letters: "A risky forest policy in the Amazon?" by F. D. Merry *et al.* (21 March, p. 1843). Two coauthors were not listed: Benno Pokorny of the center for International Forestry Research, Belém, Brazil, and Imme Scholz of the German Development Institute, Bonn, Germany.

Reports: "Detection and monitoring of ongoing aseismic slip in the Tokai region, central Japan" by S. Ozawa *et al.* (1 Nov., p. 1009). In reference (*9*), three of the equations had errors. In line 16, the equation should be

$$\log[Z \int \exp[-(\bar{v}_0 - v_0)^T V_0^{-1} (\bar{v}_0 - v_0)] dv_0].$$

In line 19, the equation should be

$$-\alpha^2 \bar{v}_0^T G \bar{v}_0.$$

In line 21, the equation should be

$$\propto \exp[-(v_0 - \bar{v}_0)^T V_0^{-1} (v_0 - \bar{v}_0)].$$