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# LETTERS

## Rhino Conservation

Joel Berger and Carol Cunningham summarize conservation measures relating to Africa's most endangered pachyderm, the rhinoceros (Policy Forum, 4 Mar., p. 1241; Letters, 6 May, p. 757). We cannot argue with their assessment that existing legislation and enforcement have not halted the continuing decline of Africa's remaining populations of black rhinoceros (*Diceros bicornis*). We do not agree, however, with their assessment of the efficacy of rhino dehorning as a short-term measure for stemming the slaughter.

On the basis of their research in Namibia (1), the authors have determined that horns regenerate rapidly. They suggest (i) that poachers do not prefer animals with more massive horns, (ii) that poachers do not discriminate between large- and small-horned rhinos, (iii) that recently dehorned animals may not be immune from poaching, and (iv) that frequent and costly dehorning may be required (1). Their

analysis supports the assertion that poachers do not discriminate between rhino horn sizes, but it does not indicate how the horn length or mass of the smallest horn in their data set of poached horns compares with those of regrowing horns. It is thus unclear at what point a regrowing horn reaches the minimum size found within the authors' sample of poached horns and at what minimum size a middleman would pay the equivalent price for a regrown horn that he would pay for a "normal-sized" horn.

Berger and Cunningham suggest that allegations regarding the soundness of dehorning are anecdotal, but so are their assertions that hornless cows are unable to defend their calves against predators. In reports of more than 200 witnessed interactions between horned rhinos and potential predators, not a single case of predation was mentioned, nor was a single witnessed interaction between a dehorned rhino and a potential predator reported.

In discussing their observations of calf mortality in three populations within western Namibia, Berger and Cunningham offer

"a first assessment of an empirically derived relation between horns and recruitment." They point out that three calves with hornless mothers sympatric with hyenas died and conclude that hyena predation was the most likely cause of their death. There is, in fact, no evidence that we are aware of suggesting that rhino population viability in the area under discussion has been affected by predators or that three calves have died.

In considering the potential causes of calf mortality, the authors do not discuss what effect climate, forage availability, and competition with domestic stock might have had on calf survival. They observe that drought might have been a contributing factor, inducing migration of herbivores

from the area and forcing errant hyenas to turn to rhino calves as alternative prey. However, the rhinos were living in an area where they were sympatric not only with occasional hyena but also with high densities of domestic stock. Stock farmers had settled near easily accessible water points, forcing the rhino cows to travel more than 10 ki-

lometers between undisturbed water and forage. This small stock would also have provided potentially attractive prey for hyenas.

Competition with domestic stock for forage has been implicated in the recent death of a young rhino (found with horns intact) in the same region. In all, three subadult mortalities have occurred naturally in the area during the past 6 years. The deaths were caused by anthrax, possible lion predation, and starvation. All the animals that died were horned males approximately 4 years old that had left their mothers. They were found with horns intact. Field records from Save the Rhino Trust, World Wide Fund, and the Ministry of the Environment indicate that only one calf is missing in the study area. It has therefore not been possible to determine the effect of dehorning on the demographic viability of adults and their offspring.

A thorough analysis of the efficacy of existing international trade restrictions aimed at controlling rhino poaching by preventing sales of horn or horn-derived prod-



**Black rhino:** To dehorn or not to dehorn?

E. R. DEGEN/ANIMALS ANIMALS

ucts, is desperately needed. It is clear that the market is demand driven and shows no signs of decreasing. It is time to accept the fact that even effective dehorning programs, combined with successful community- or government-based antipoaching controls such as those in Namibia, cannot eliminate the demand for rhino horn products. Thus, a sustainable market based on dehorning programs (with direct sales to Asian markets, eliminating middlemen) may provide the only long-term solution for the conservation of the world's remaining rhinos. Until such a solution can be found, the international conservation community must continue to support programs, such as those in Namibia, that include dehorning combined with antipoaching patrols and community-based conservation campaigns.

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## Clinical Trials: Subgroup Analyses

We refer to Rachel Nowak's Special News Report article of 10 June, "Problems in clinical trials go far beyond misconduct" (p. 1538). While we agree with a number of points in this article, in particular, the need for more education of clinicians in the conduct and evaluation of clinical trials, we would like to set the record straight regarding the analysis of AIDS Clinical Trial Group (ACTG) 155 by Margaret Fischl and her collaborators. Nowak's article implies that the ACTG-155 team scanned through many different subgroups looking for a treatment difference. In fact, the only subgroups analyzed were those formed by pretreatment CD4 cell count and the three stratification factors (human immunodeficiency virus status, time on prior zidovudine, and *Pneumocystis carinii* pneumonia prophylaxis). The subgroup analysis by pretreatment CD4 cell count was planned and was first specified by the study chairs in June 1992, before the study's results were revealed to them in March 1993. Analysis by subgroups formed by the stratification factors was also planned before June 1992 and is common practice in clinical trials.

Because of documented associations between pretreatment CD4 cell counts and treatment outcome, for example, the follow-up study of ACTG 019, which showed that the extent of benefit of zidovudine compared with that of a placebo depended on the pretreatment CD4 cell count, it is difficult to imagine interpreting the results of ACTG 155 without taking the pretreatment CD4 cell count into account. A trend analysis of ACTG 155 looked at the association between clinical progression and pretreatment CD4 cell count and showed lower progression rates for combination therapy relative to those for zidovudine therapy as the pretreatment CD4 cell count increased ( $p = 0.027$ ), which is consistent with other published data.

The ACTG-155 team did not overstate the results of the trial, clearly indicating in presentations that no overall differences were noted among the three treatment groups. Data analyses were reviewed at multiple levels within the ACTG before public presentations. A major focus of the protest, as expressed to Fischl after her talk, was the slow progress toward effective therapy. We are well aware that there are some investigators who adopt the position that all subgroup analyses should be avoided, regardless of previous plans and corroborating information. In the view of many, including ourselves, researchers should maintain a conservative approach in the evaluation of their study. But they also need to thoroughly evaluate their data in light of the evolving literature, clearly specifying what was done and which findings should be viewed as exploratory.

We were disappointed that Nowak makes little distinction in her article between valid scientific debate and misconduct. This problem was compounded by the article's provocative text and title. Our study appears to have been selected for public criticism without an adequate determination of whether the criticism was valid.

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**Response:** My article did not simply imply that the Fischl team scanned treatment subgroups looking for statistical differences; it stated explicitly that, according to Stanley, the subgroup analyses were planned ahead of time. But it also clearly stated that some biostatisticians and AIDS activists were critical of the team's subgroup analyses and of the team's high-

lighting the results of one subgroup analysis at a presentation at last year's ninth international AIDS conference in Berlin. The article also explained that, rather than being completely out of bounds, subgroup analysis is acceptable under certain constraints; for example, to be valid the number of subgroups analyzed must be few, and the subgroups must be defined before the study begins. Even then, the evidential (as against the exploratory) value of such analyses is debatable.

I disagree with the charge that my article did not adequately distinguish "between valid scientific debate and misconduct." On the contrary, the article clearly differentiated between erroneous and merely debatable clinical trial design and analysis. Perhaps the confusion arises because I also reported the opinion of at least one clinical trialist—namely, Richard Peto of Oxford University—that making errors in clinical trial design and analysis through ignorance is tantamount to scientific misconduct because of the devastating consequences wrong results can have for public health.

—Rachel Nowak

## A Coincidental Move

As a not-young University of California (UC) faculty member, I found Marcia Barinaga's News & Comment article "Early retirement program cuts deep into UC faculties" (20 May, p. 1074) to give, on the whole, a fair portrayal of the difficulties and opportunities created at UC by the voluntary early retirement incentive program (VERIP). However, Barinaga's account links the decision of Nobel-laureate chemist Yuan T. Lee to leave Berkeley to head Academia Sinica in Taiwan with a quote from a UC San Diego administrator that top faculty would be enticed to "cash in on retirement and leave and go to some other institution." This characterization does not apply at all to Yuan Lee, who is a national hero in Taiwan. Taiwan's efforts to recruit him and his own commitment to return someday to lead his native land's endeavors in science and technology have been well known among Chinese-Americans for many years. When his predecessor, Ta-You Wu, decided to retire, it was natural that Yuan Lee should replace him as president of Academia Sinica. The timing of these events in Taiwan with the commencement of VERIP-3 at UC was a coincidence.

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