The Uncertainty of Data and Dehorning Black Rhinos

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Wild black rhinos are approaching extinction faster perhaps than any other African mammal. In Namibia's deserts they occur at low densities, have home ranges up to 2000 km^2, and have birth intervals that exceed 30 months. Clearly, they are difficult to study. Practical constraints will always be placed on sample sizes which, in turn, limit the robustness of generalizations. Nevertheless, the international conservation community has not written off this flagship species, and the luxury of awaiting the results of 10-year studies is something that neither CITES nor concerned groups can afford. Loutit and Montgomery (1994) suggest that dehorning may be needed to develop a sustainable horn harvest to save the world's remaining wild rhinos, and they challenge a number of points reported in Berger et al. (1993). We do not take issue with the possibility that government harvesting and sale of rhino horn might possibly help eliminate the poaching industry; however, Loutit and Montgomery do not offer a rational consideration of issues associated with the efficacy of dehorning. Instead, their response is tainted by a political agenda to discredit our findings coupled with the goal of stockpiling dehorned horn for later trade. For instance, although we reported that one of us was convicted for felony rhino poaching (Berger et al. 1993:922), Loutit and Montgomery (1994) feel the need to drive home the point "One of the authors ... was arrested and convicted." What they fail to say is that because our coauthor, Archie Gawuseb, was employed by them when the poaching occurred, he knew the locations of horned and dehorned rhinos, opting for the former.

Loutit and Montgomery seek clarification about our estimates of horn growth. Although methods were provided (Berger et al. 1993) and additional explanation is offered elsewhere (Berger & Cunningham 1994*a*), a brief comment may help remove ambiguity about the limitations of our data. We derived *annual* growth by dividing the total size of regrown horn (converted subsequently to mass) by time since removal. These are of course estimates only, as measurement error will always exist. Does it really matter whether male horns regrow at 5.3 or 5.5 cm per year? Loutit and Montgomery also indicate that to assess the economic efficacy of dehorning the risk of capture (for poachers) must be balanced against the incentive to poach. Our lack of data on prices received by middlemen should not detract from

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our major assertions: horns grow fast and are worth a good deal of money. Perhaps horns could be used sustainably. But we are surprised that Loutit and Montgomery omitted citing the work of Milner-Gulland et al. (1993) which, using middlemen prices, suggests dehorning is not as worthwhile as other conservation actions. The remainder of our comment concerns two matters, the viability of dehorned black rhinos and alternative hypotheses for the mortality of juveniles with dehorned mothers.

The issue of whether human intervention has enhanced the viability of dehorned rhinos is tricky. Loutit and Montgomery argue that there was interchange between populations thereby assuming a larger subpopulation than really exists. We studied discrete dehorned populations in the Springbok and Ugab River regions and found no evidence of interchange.

Data we collated from records of unmistakable individual rhinos from 1988 to 1993 show that our dehorned study population dropped 16%, from 25 to 21 animals; this includes 5 surviving calves, 9 deaths, and 4 transfers (3 were subadult females—25% of the cohort of potentially breeding females). To the north of a country-long veterinary cordon fence we focused on a subset of the larger population (about 90) of exclusively horned animals. Our small sample increased 17%, from 10 to 12, over the same 6-year period. Because the disjunct, dehorned population to the south had (1) no female immigration, (2) potentially breeding females removed, and (3) a mortality rate that exceeded natality, there is no apparent need to modify the statement that "a high degree of uncertainty about their long-term population viability exists" (Berger et al. 1993:923).

Numerous hypotheses exist for why juvenile mortality was restricted to areas where dehorned rhinos and spotted hyenas overlapped. We have already addressed four possibilities—predation, maternal age, year of dehorning, and nutritional stress (Berger and Cunningham 1994*a*). However, the claim that ecological conditions among our study areas may have been affected differentially by domestic stock warrants attention. If livestock changed the dynamics of the area where calves died, then it would be difficult to argue that our experimental design was balanced. We considered this possibility indirectly by suggesting that if food availability differed among study sites, then rhinos from nutritionally poor areas should have been in worse condition than other rhinos. We contrasted body condition and found that it did not differ among areas (Berger & Cunningham 1994*a*). But, it would be fruitful to explore the idea of ecological similarity among sites further.

To assess whether herbivore populations followed similar patterns, we estimated biomass in each area believing that herbivores track in a general sense the available vegetation. Annual data on the frequency of springbok, kudu, giraffe, ostrich, gemsbok, and mountain zebra were compiled from 126 transects that covered 5106 km during wet and dry seasons. Data were normalized by transformation using $\log(x + 1)$, which avoids the problem of having zeroes in which the log is negative infinity. The same pattern of seasonal fluctuations in herbivore biomass characterized each site (Fig. 1). Seasonal

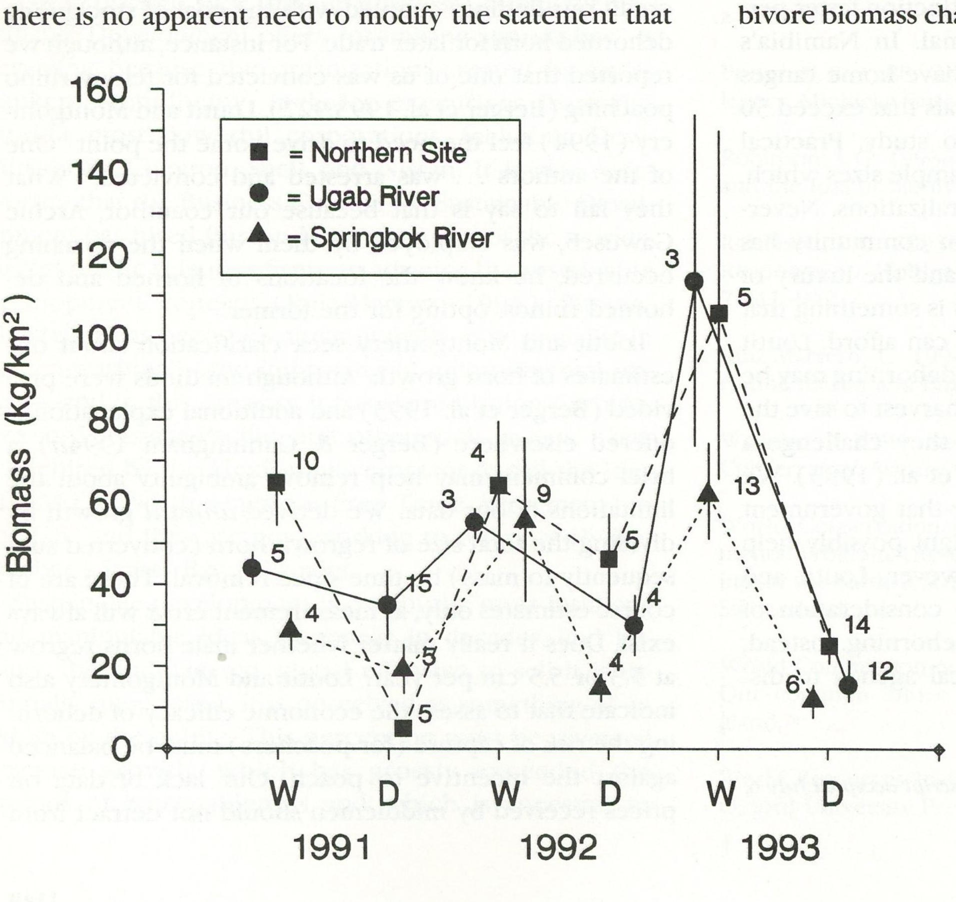

Figure 1. Relationships between large herbivore biomass (mean kg/km^2) (SE) and year (1991–1993) during wet (W) and dry (D) seasons at three northern Namib Desert areas (size in km^2): Northern Site (1858 km^2), the Springbok River (and an undisclosed site combined) (1710 km^2), and the Ugab River (3418 km^2). Species and mean masses (kg) as follows: springbok (37), gemsbok (210), kudu (157), mountain zebra (276), giraffe (850), and ostrich (111). Values on the figure are number of transects.

influences were highly significant ($F_{1,72} = 9.74$, $p < 0.01$) but yearly ($F_{2,72} = 0.09$) or site ($F_{1,72} = 1.40$) effects were not. This indirect assay fails to suggest obvious ecological differences in patterns of herbivore biomass and it reinforces the idea that rhinos were exposed to similar conditions.

Loutit and Montgomery (1994) claim that "When the calves died, approximately 500 goats were brought to the area," a statement that is nonsense. When the calves died remains unknown. There was, at a minimum, an 8-month period between the birth of two calves and the discovery that they perished. Additionally, their meaning of "the area" as well as the correct number of domestic stock is ambiguous. We visited our 1710-km^2 Springbok River study area seven times in 1992–1993 and *never* found evidence of domestic stock prior to the period when calves disappeared, although goats and cattle are found about 20 km to the east of water frequented by dehorned mothers. What seems more likely is that Loutit and Montgomery misinterpreted a report of a total of 493 livestock, "408 head of cattle and 85 goats" in the Ugab River (Morkel 1992) and claimed there were 500 goats at that river site. The Ugab region served primarily as our hyena-free site with dehorned mothers. Thus, there is much uncertainty about the timing of mortality, duration and location of livestock presence (if any), and discrete effects.

Most serious perhaps is that Loutit and Montgomery indicate they have knowledge of only two calf deaths. The issue of whether two or three calves died is not pivotal to conclusions already presented in Berger and Cunningham (1994*a*). It is, however, critical in understanding our reluctance to accept assertions about the certainty of data compiled by Save the Rhino Trust (SRT), a Namibian nongovernment organization where Loutit and Montgomery serve as directors. Our assessment of calf mortality has been conservative and we deliberately minimize chances to inflate calf mortality. For instance, Loutit (in Vigne 1989) claims that "the first dehorned rhino (May–June, 1989 [our insertion]) was Tammy, a pregnant cow." Neither we, nor any one else with whom we talked, have any knowledge about the fate of that pregnancy. This female is one of four that has been sympatric with spotted hyenas since at least 1989. We discounted the possibility that this female gave birth to a 1989–1990 calf that later died. We did however record a mortality after a 1992 calf born to that female disappeared between July 1992 and February 1993. Hence, in assessing post-dehorning patterns of calf production and survival, we ignored Loutit's claim

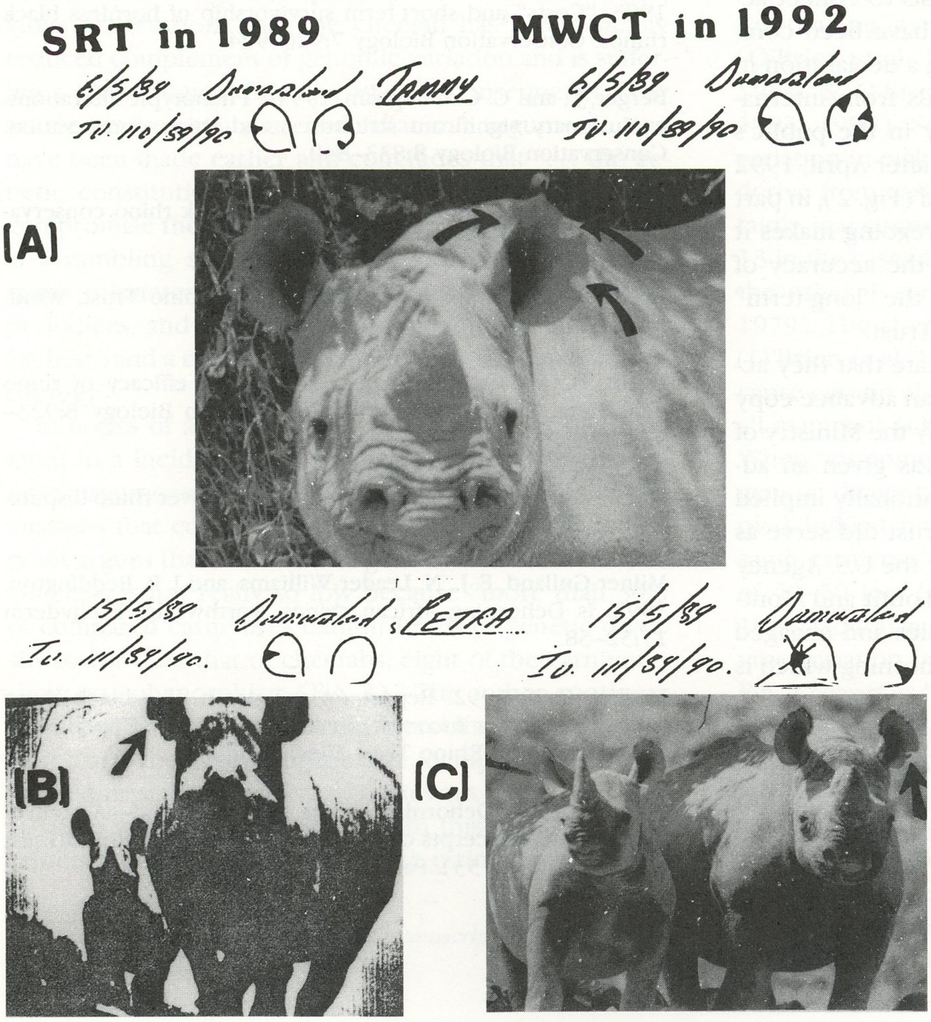

Figure 2. Examples of misidentification of rhinos by Save the Rhino Trust (SRT) (1989 errors shown with 1992 corrections): Photograph of Tammy, drawings of ears and associated notch patterns assigned by SRT and corrected notch locations by Namibian Ministry of Wildlife, Conservation, and Tourism (MWCT) after examination of the photograph (A); photocopy of a reversed April, 1990 SRT photograph of Petra and calf and SRT 1989 and MWCT 1992 identifications of Petra's ear notches (B). The actual photograph of Petra and calf (C) taken in February, 1992. Arrows indicate conspicuous ear notches or tears. Photographs by Berger and Cunningham, handwritten names and drawings of ear notch patterns and corrections are photocopies from SRT and MWCT files obtained with permission in 1991 and 1992, respectively.

of pregnancy and it is possible that a calf was never born. However, we must point out that when efforts are not made to discover missing infants or if census intervals are too widely separated, then it will be impossible to detect the presence of calves and mortalities will always remain unknown.

Loutit (in Vigne 1989:47–48) also stated that "we have been monitoring rhinos in Damaraland for nearly ten years, and believe that we know enough of their habits to warrant the results of removing their horns," and Loutit and Montgomery (1994) maintain that their records "are kept continuously updated through population monitoring." We disagree with each contention.

Assessment of the successes or failures of the dehorning strategy hinges on accurate knowledge of individuals. The latter task is challenging because without horns rhinos look very similar. This problem is avoidable by using ear tears or notches as identifying markers and keeping accurate records (Fig. 2). Thus, it is unconscionable that 50% (8) of the dehorned animals that remained in 1989 were not correctly identified by Save the Rhino Trust (Fig. 2). These mistakes went uncorrected for three years, despite the 1990 claim made subsequent to dehorning that "Each individual rhino is monitored by SRT and Ministry of Wildlife, Conservation, and Tourism staff on a regular basis to collect accurate information for dossiers which have been compiled of each rhino . . ." (Loutit 1990:5), a declaration in the SRT color brochure soliciting funds from international donors. Accountability is clearly in the public's best interest. However, it was not until after April, 1992 that identification errors were corrected (Fig. 2), in part due to assistance we rendered. The foregoing makes it difficult for us to place much faith in the accuracy of claims by Loutit and Montgomery or in the "long-term" monitoring records of Save the Rhino Trust.

Finally, Loutit and Montgomery indicate that they accorded us neither sponsorship nor saw an advance copy of our manuscript. They are correct; only the Ministry of Wildlife, Conservation, and Tourism was given an advance copy. We apologize if we unintentionally implied an endorsement. But Save the Rhino Trust did serve as one of Berger's two official conduits for the U.S. Agency of International Development support. Loutit and Montgomery also provided access to their files and engaged in conversation about the efficacy of dehorning which is why we acknowledged them.

Our intent has been to clarify claims made by Loutit and Montgomery about the efficacy of dehorning black rhinos, not to suggest that our work is without its own shortcomings. They raised some interesting points and have done a first rate job at bringing Namibia's desert pachyderm issues to the public's eye. Nevertheless, murky records, proclaimed support for policy action (e.g., horn trade), and a hazy view of biological principles are poor stepping stones for building a foundation upon which to conserve endangered species. It may be too early to tell if dehorning is effective or not. But, to be circumspect, to err on the side of prudence, and to use empirically based data in decision-making seems far wiser than plowing ahead with a tactic that will never be fully tested, at least in Namibia where foreign researchers are discouraged (Berger & Cunningham 1994*b;* Macilwain, 1994). No matter how well intentioned, nongovernment organizations that deal with environmental conservation should be guided by biological counsel if not by formal scientific advisory boards. Life at the forefront of public relations and fund raising will always necessitate trade-offs with the time spent in on-the-ground management. Save the Rhino Trust exemplifies the challenges therein. Wishing to present a persuasive program of action in an attractive light can either help facilitate or block valid conservation agendas. Science should play a role in rhino conservation.

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