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**Responses to RANGE DEGRADATION IN BOTSWANA:
MYTH OR REALITY? (Paper 35b)**
by Joao S. de Queiroz

**CARRYING CAPACITY, RANGELAND DEGRADATION AND
LIVESTOCK DEVELOPMENT FOR THE COMMUNAL
RANGELANDS OF BOTSWANA**
Nick Abel

**THE DEGRADATION DEBATE:
IS CLARIFICATION POSSIBLE?**
Annika Dahlberg

COMMENTS
Richard White

THE DEGRADATION DEBATE: IS CLARIFICATION POSSIBLE?

Annika Dahlberg

The stated objective of de Queiroz's paper - 'to dispel some of the confusion surrounding the application of the range degradation concept in Botswana' - is important, especially in the light of recent Botswana government policies on range management. However, if the issue is one of clarification, then some of the arguments put forward by de Queiroz, and some of the data sources he utilises, deserve comment.

The paper by de Queiroz was written in reaction to a recent publication by White (1992) which criticised past and present Government interventions in rangeland management (see e.g. Republic of Botswana 1975, Ministry of Agriculture 1991). Although de Queiroz agreed that much of this criticism was justified, he disagreed with what he perceived as White's main line of argument; i.e. his 'categorical dismissal of the notion that Botswana's rangelands are degraded' (de Queiroz 1993). However, in the detailed criticism presented by White this was not his main argument. Instead White's major concern was with the fact that earlier, and very similar, rangeland programmes have generally failed. White examined the assumptions underlying these programmes and concluded that most of them were false. *One* of these assumptions was that the rangelands of Botswana, especially in communal areas, are *severely* overgrazed and degraded. White disputed this assumption on the basis of his own observations (White 1992) and the results of recent studies in Botswana. However, he dealt with the issue quite briefly, and his own opinion is not totally clear as he also criticised planned interventions on the grounds that they will 'increase the rate and severity of range degradation' (White 1992).

The main reason for White's paper being considered controversial in Botswana is not dependent on his standpoint on degradation, but rather because of the negative socio-economic effects he believes will occur if the new policies are implemented, and because past experience has not shown that similar programmes have alleviated environmental problems, however serious they may or may not be.

De Queiroz used two examples from Southern Africa in order to show that the 'state-and-transition model', as described by Westoby et al. (1989), is of relevance to this region. However, in using these examples his purpose was unclear, since the applicability of this model in itself says nothing about the present condition of the environment, at least not in terms of degradation levels. To show that the environment has passed from one 'stage to another', even one less desirable from a specific management point of view, does not imply that it has reached a state of degradation. According to the model suggested by

Westoby, natural and man-induced disturbances will cause the transition of the environment from one stage to another - and back again. Only if the environment reaches a negative stage, for a specified management purpose, which will not revert back again over a given time period without costly interventions, can one talk about degradation. Furthermore the environment has to be defined spatially.

Summing up his two examples, de Queiroz stated that they indicate that the changes described were triggered by heavy grazing, with or without drought, and that some of the described changes may imply 'a clear manifestation of irreversible degradation'. The first example described measurements undertaken in a semi-arid savanna in South Africa. Changes along designated degradation gradients on land grazed by sheep were described (Fuls 1992). Soil and plant characteristics were measured at one point in time, and on-site precipitation was recorded for 30 days prior to sampling. A number of physical degradation factors were compiled in order to place the patches in different categories of degradation. However, nowhere was degradation defined according to management objectives or degree of reversibility. What is even more important is that the changes observed, and cited by de Queiroz, did not constitute change over time, but instead differences in space, and over very small-scale space at that. Thus, these data say little concerning the process of degradation over time, and even less in terms of explaining possible causes. Further conclusions made from the same study area were that lightly-grazed patches withstood drought better than patches subjected to heavy grazing (Fuls and Bosch 1991). What is not mentioned by de Queiroz is that the area was only studied for one (low rainfall) year, i.e. measurements were taken before and after the area had been closed to grazing for a 12 month period. Since the time period was short, and no data were given which explained the condition of the range prior to its enclosure, it is, impossible to be sure if degradation had occurred, in an environment which may be intrinsically unstable (Behnke and Scoones 1993). A much longer time period, and measurements over years with different amounts of annual rainfall, and more detailed data on intra-annual distribution of rainfall would be necessary in order to show whether observed changes were reversible.

The second example presented by de Queiroz dealt with data collected in southern Botswana which showed that 'the potential for rapid grazing-induced changes in vegetation composition from perennial grasses to annual grasses and forbs is real' (de Queiroz 1993). On the one hand it can be argued that changes in vegetation cover and composition are not of any great importance when looking at rangeland degradation (Biot 1988, Abel 1992). On the other hand, for many areas the only data going back in time are data on vegetation characteristics. It is doubtful whether the data used by de Queiroz are of sufficient quality to make a judgement concerning specific causes for observed

changes. The range monitoring programme established by the Range Ecology Unit of the Ministry of Agriculture in Botswana (described in Hendzel 1981), has been heavily criticised on a number of points, and the data collected in the southern part of the country are considered to be especially unreliable (Prince 1982, GRM 1987). I do not want to imply that this data, going back over 20 years, is of no interest (in fact I am doing a similar analysis for sites in the north eastern part of Botswana), but I do think that one has to be much more careful in what one thinks the data can prove, and that the uncertainties and unreliability of the sampling procedures and statistical analysis should be fully described. Furthermore, considering the high spatial variability of rainfall in Botswana, it would be of interest to know how far the sampling points were from the rainfall station in Gaborone. To make correlations between rainfall and other factors, such as grazing and relative cover of grasses, it is important to have access to local rainfall figures.

De Queiroz's third objective was to present recent research results which support the view that Botswana's livestock industry is responsible for ecological degradation in the Kalahari. It seems strange to use the temporal changes in pan soil and grass phosphorous levels, which are as yet unmeasured, as evidence for claiming that Botswana's rangelands are degraded. De Queiroz himself, earlier in the paper, discussed the importance of using an appropriate definition of land degradation, and he described two lines of arguments. One was to measure degradation in terms of production. Thus, if the land was to be used by wild ungulates which depended on the pans, and if the pans shifted to a new, permanent state of less value to the animals, then such a change might have constituted degradation. However, in the case of Botswana the main production system is based on livestock, and so changes to the environment should be judged in the context of how they affect future incomes from this sector. According to de Queiroz cattle do not even use the pans, so changes here are not likely to affect the cattle sector.

I find this line of argument slightly too narrow. It does not take into consideration the possibility of future changes in land use. I therefore agree with de Queiroz that degradation should also be measured in terms of effects on 'fundamental ecosystem processes' whether these are directly relevant to the present management system or not. However, as recent studies of semi-arid environments have shown, it is often very difficult to be sure of what constitutes 'fundamental' processes in any specified ecosystem. Even if phosphorous levels should drop, due to declining numbers of wild ungulates, this is only part of what makes the pans important environmental pockets. Soils are not fertile, nor grasses nutritious, solely because phosphorous levels are high. Other factors are probably just as important, and there seems to be no reason to believe that the remaining wild animals would stop utilising the special environment of the pans simply because of declining phosphorous levels.

In addition, in order to view the changes anticipated by de Queiroz as being part of a 'fundamental ecosystem process', a much broader understanding of the whole ecosystem, in space and over time, is necessary. The number of wild ungulates has always fluctuated, due to drought and other factors, e.g. the outbreak of rinderpest at the turn of the century. This might cause phosphorous levels, and a number of other environmental factors, to fluctuate, or even change on a more permanent basis. Within the definition of degradation used here, such changes would be negative if they pushed the whole system over a threshold, so that wild animals, in this particular case, could no longer adapt to the environment as a whole.

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COMMENTS

Richard White

I wish, briefly, to take issue with a number of statements made by Dr. de Queiroz in his paper, where he criticises my work (White 1992a)¹.

He describes my assertion that '*...research has shown that lowering the stocking rate raises productivity per animal, but it does so at the price of reducing productivity per hectare*' as unqualified. This statement is based on the results of research on stocking rates carried out by APRU between 1982 and 1986 and analysed by Abel & Blaikie (1989). There is a simple scientific explanation as to why this should be - that one of the primary plant responses to herbivory is to grow faster - so increased grazing pressure stimulates primary production leading to an increase in secondary production. He offers no suggestion as to what qualifications might be appropriate.

De Queiroz suggests that my paper (White 1992a) implies that the optimum strategy for the communal rangeland is to stock them as densely as possible.

This is not quite correct. In another paper, (White 1992b) I show that the rangelands are protected by biological and economic feed-back mechanisms, which ensure that livestock numbers on communal land are maintained at a level well below K (or biological carrying capacity) in all but drought years. In fact, stocking rates on communal land in Botswana are controlled by economic forces and are maintained at a low enough level so that significant drought induced mortality only occurs in severe droughts, which recur on average every 20 years.

There is no argument that intense grazing pressure and drought lead to marked changes in species composition in Botswana's rangelands. What is in doubt is whether these changes constitute degradation or are undesirable. Abel & Blaikie (1989) show that there is no clear link between change in grassland species composition and livestock productivity. Water is a scarce and expensive resource in Botswana. It is inevitable that any water source will be intensively used and that trampling and grazing in its vicinity will cause marked vegetation changes. These changes are as much part of the price of developing a water point as the financial cost of drilling and equipping a borehole.

De Queiroz has quoted me selectively on the soil erosion issue. I state: '*Actual measurement of soil loss in Botswana shows that even in the most vulnerable areas, the rate of soil loss due to overgrazing is only slightly*

¹This work has also been published as *Livestock Development and Pastoral Production on Communal Rangeland in Botswana* by The Botswana Society, P.O. Box 71, Gaborone, Botswana, 1993 and is available through the Botswana Society or the PDN - see note in this newsletter.