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COMMENTS ON PDN PAPERS 29A (ABEL AND BLAIKIE 1990) AND 28B (SCOONES 1989)

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* Abel, N O J and P M Blaikie (1990) 'Land degradation, stocking rates and conservation policies in the communal rangelands of Botswana and Zimbabwe' PDN paper 29a ODI, London

* Scoones, I (1989) 'Patch use by cattle in dryland Zimbabwe: farmer knowledge and ecological theory' PDN paper 28b ODI, London

COMMENTS ON PDN PAPERS 29A (ABEL AND BLAIKIE 1990) AND 28B (SCOONES 1989)

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Comments on PDN paper 29a 'Land degradation, stocking rates and conservation policies in the communal rangelands of Botswana and Zimbabwe' by N O J Abel and P M Blaikie (May 1990)

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These comments on Abel and Blaikie's paper are made following a recent study (Mokoro 1990) of Botswana's Western Region, namely Kgalagadi and Ghanzi districts, which together cover about 40% of the country, albeit the drier part. The study focused on resource use - wildlife management and utilisation, pastoralism and dryland cropping - and the measures required to bring about a more equitable allocation of resources and more sustainable land use.

On the basis of research findings, Abel and Blaikie criticise government authorities who recommend standard stocking rates, fencing of paddocks and destocking, etc in the Communal Areas. They conclude that the adoption of such advice would reduce the viability of small enterprises. They demonstrate that rangeland (and hence its carrying capacity) is intrinsically unstable and that allocated grazing territories in Communal Areas can rarely, if ever, be large enough for long term self-sufficiency. They argue for a more efficient 'tracking' strategy which would require rapid destocking in times of drought and accelerated restocking afterwards and a system of land use planning or 'ecological fencing', which takes account of seasonal and inter-year variation.

Our recommendations emerged from an analysis of the different pastoral systems in Western Region (commercial ranches, TLGP ranches and cattle-posts in the Communal Areas). They recognise the need to reduce the risks and uncertainties faced by the great majority of stock owners, namely the small ones. Interestingly, the conclusions of the Western Region Study, which attempted to take technical as well as complex socio-economic issues into account, were in several ways consistent with those of Abel and Blaikie, especially with respect to the promotion of grazing systems for the Communal Areas.

Both studies open the door on two important policy issues: drought-related marketing (emergency destocking and accelerated restocking) and land use planning for the Communal Areas. In Botswana, progress on livestock marketing has been easier than land use planning, despite major efforts in this direction. At the national level, market infrastructure in Botswana has been steadily improving to the point where it should be able to cope with an emergency. However, much detailed work has to be done to formulate an action plan.

Abel and Blaikie's criticism of the inadequacies of current farm-level range management advice in the Communal Areas is valid. Both farmers and advisers have long recognised that it is unrealistic and that it is largely ignored, even by the officials themselves in managing their own herds. Why is it that such unhelpful advice is perpetuated, despite the evidence of its inappropriateness? Much of it is based on management practices on the commercial ranches. These generally have much more flexibility in terms of resources, especially land, and follow a different production strategy than producers in Communal Areas. Above all, commercial producers are able to buy in concentrates and supplements to sustain their breeding animals in times of drought (an option which was overlooked by Abel and Blaikie - *see foot of page 3*).

In putting together a revised policy along the lines proposed by Abel and Blaikie, there is a danger of once again recommending the unattainable for the Communal Areas. After all, the authors' 'tracking strategy', the continuous matching of stock numbers to available feed resources, is the stock-in-trade of commercial producers. Their path is a difficult one to follow and no longer feasible at present densities of human population and intensities of land use in the Communal Areas. It is not helpful to hold up private ranchers as models of good husbandry for part-time cattle-post operators. However, the positive management lessons are there to be learned and applied at the regional rather than the farm level:

- in times of drought, to destock all animals surplus to breeding requirements
- diversify sources of income, herds and flocks
- to maintain fall back areas for times of drought; to rehabilitate overgrazed areas through bush clearing
- fine-tune the emergency marketing and stockfeed supply operations
- spread the grazing pressure more evenly over the available range through more effective land use planning and groundwater development

With respect to the last, even if there was the political will to effect a more equitable allocation of range resources, which there does not appear to be, the land use planning task required is not feasible given the current level of technical resources allocated to it. And this is in Botswana, which has made more progress in this area than any other country in Africa.

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Some remarks concerning the importance of soil erosion as a land degradation process

I endorse the main thrust of Abel and Blaikie's analysis which lays bare a few common myths concerning the state of sub-Saharan Africa's environment and land development policies: communal rangeland is less productive than fenced-off grazing systems; levels of land degradation under communal rangeland conditions are unacceptable; fenced-off grazing systems are an effective means of increasing short and medium-term rangeland productivity as well as protecting the sustainability of the range; rangeland conservation pays for itself - see also Biot *et al.* 1990 for a similar discussion concerning soil conservation. The authors thus demystify the 'desertification' literature which is so often referred to in support of apocalypse-type analyses of the state of the African environment. They de-mystify the usually expatriate 'expert' who seems to 'know' so much on the basis of insufficient data, and demonstrate that traditional management was, after all, not as unproductive as has been claimed.

It might be worth investigating the causes of the exaggerated statements made about the environment and development by former colonisers and today's aid organisations, and why such statements are given so much credibility - an analysis which I leave to those who master the right tools to undertake such analyses. I can help, though, to identify the origin of some of the common (often erroneous) statements issued regarding soil erosion.

First, there is a problem with the perception of the problem and its causes. In the case of Botswana's rangeland, it has been stated that 'there is little data available on soil erosion', but that 'it is an important problem ... '. This interesting contradiction finds, I believe, its roots in the exaggerated importance given by people who come from areas of permanent green pastures to patches of bare soil (especially during periods of drought), gullies and what is increasingly referred to as 'road erosion'. Of these three indices of water erosion, only the third can be directly ascribed to human activity. Bare soil is a common feature during periods of drought and around areas of high population densities (villages, boreholes); gullies are often natural phenomena. The nature of the 'expert's' work routine bringing her/him most frequently in association with such areas which exhibit erosional features tends to create a set of associations which it is difficult to dispel when prompted to make statements about environment and development.

A second, more sophisticated but often equally mistaken, basis for the identification of soil erosion as a major threat to soil productivity is the use of rates of erosion calculated for plots of short lengths to estimate rates of overall landscape denudation - a mistake which, I must confess, I have made myself. Indeed, rates of erosion are usually calculated/measured for slope lengths of 20 to 30 m. These rates, however, are much higher than net soil losses from a catchment of, say, 1,000 ha. Much of the soil lost from short slope segments accumulates elsewhere, and only a fraction of the total is lost. Abel and Stocking (1987) estimate the net rate of soil loss from the landscape in the Sandveld of Botswana to be only 20% to 25% of the rate of erosion derived from 30 m long erosion plots - a factor which I found to tally with the difference between runoff from a typical 1 ha plot in the rangeland and stream-flow in the main rivers (Biot 1990); see also Dye (1984) for conditions in Zimbabwe.

Thus, soil is not so much lost from, but redistributed within, catchments. Conversely, it could be hypothesised that soil productivity is not lost from but redistributed within the landscape. There is little research on this subject worldwide, apart from Australia (Walker, *personal communication*), but interesting circumstantial evidence. Bush encroachment in the areas where it does occur is mainly along drainage channels in Botswana (Bland 1985, and own observations).

Thirdly, there is the misconception that 'change' is synonymous with 'degradation', a link which is logical for beings who find more security in a known situation than in a future unknown environment, but not necessarily true; see the erosion productivity debate (Stocking and Peake 1985).

The second and third points are worth taking a little further. We should, by now, have an image of rangeland composed of areas which lose soil (and productivity) - runoff areas - and others which gain soil (and productivity?) - runon areas - which, together with other causes of landscape/soil heterogeneity, strengthens Abel and Blaikie's description of rangeland in the Hardveld of Botswana. The table below illustrates for the 1986/87 season the difference in vegetation (hence carrying capacity) and stocking densities between landscape/soil positions within a given area of rangeland similar to the one described by the authors.

The same is true for the Sahel: Penning de Vries and Djiteye (1982) claim that the presence of runoff and runon areas are a *conditio sine qua non* for the successful utilisation of a semi-arid ecosystem. Indeed, more often than not rainfall is insufficient for adequate grass growth, and runoff is needed to supplement supplies in selected runon areas where grass growth is possible. The inclusion of 'different' landscape positions within grazing territories is therefore *crucial* to successful long term rangeland management. There is, as yet, little research done to identify proper guidelines for this, but the concept of ‘catchment’ is probably a good starting point. Not only will we find within catchments areas of runoff and runon, we will also have an area from which the overall loss in productivity because of soil erosion is probably minimal.

I do not want to end without stating that in writing these comments I have deliberately taken a provocative viewpoint. I do this to propose an antithesis to those who claim through a careful selection of ‘case studies’ that Africa is slowly turning into a desert. One of the main points of Abel and Blaikie’s paper is that policy should be based on an analysis of *local* circumstances and evidence (which can be important enough to warrant drastic intervention - see, for instance, the Kondoa Experience (Ostley 1986) - rather than blanket statements which invariably follow a ‘communal rangeland ... tragedy of the commons ... desertification’ logic.

If there is a generally applicable strategy which emerges from Abel and Blaikie’s paper, as well as from the review by Warren and Agnew (1988), it is that safeguarding tomorrow’s production involves safeguarding diversity and making room for a dynamic nature - two principles which might well be applicable in other areas of analysis of ‘sustainability’, but are difficult to streamline. Maybe they shouldn’t be.

Vegetation characteristics and stocking densities in four different soil/landscape units in Central District, Botswana

	Soil/Landscape unit 1	2	3	4
Trees				
Density (/ha)	2230	1404	634	2165
Canopy cover (%)	43	17	9	71
Mean height (m)	1.4	1.1	0.8	2.2
Herbaceous layer				
Density (/ha)				
grass	30	88	58	74
forbs	25	21	102	23
seedlings	17	81	108	18
Grass basal cover (%)	18	44	5.2	1.5
Bare soil (%)	76	66	62	75
Standing litter (kg/ha)	2233	538	434	931
Grass standing biomass (kg/ha)	228	698	346	192
Forbs standing biomass (kg/ha)	128	98	127	126
Annual grasses (%)	22	14	6	59
Stocking density				
Dung (/100m)	5.6	3.6	118	0
Cattle tracks (/100m)	1.5	1.6	2.2	2.0

Notes:

1 Upper and mid pediment slopes - granites
2 Upper and mid pediment slopes - granites/dolerites
3 Wide and shallow, sandy depressions
4 Levees

Characteristics of the herbaceous layer: mean for 2 samples (December and March) of 1986/87 season

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I have discussed the paper with two colleagues, Dr Silitshena, himself from a pastoral people, and Dr J de Queiroz, who has extensive experience of NE Brazil and the western USA rangelands.

My own feeling is that all Abel and Blaikie are suggesting really is a return to purely traditional practices, of moving cattle to where it has rained and therefore good forage is available ('tracking'), and adapting numbers to the season, multiply up in good years, reduce in bad. These practices worked when ample range was available, and in a social system where traditional discipline prevailed. Now there is little room for much movement, and there is little cohesive discipline. Like it or not, the range is degrading, and something must be done - management is a must, but the precise methods are proving elusive. We don't think this paper helps much in the search.

I append verbatim remarks from my colleagues:

'The authors are overly biassed against the proposition that over-grazing causes range degradation. Thus they dismiss evidence supporting the overgrazing-degradation link, and suggest equally weak evidence supporting non-degradation.'

'They describe degradation as an irreversible change, and assume that reversible changes are acceptable. However, while the range is reversibly degraded it is producing less than it could.'

'They assume most degradation is reversible - in other words not really degrading. It is my opinion that irreversible bush encroachment is very common, and only reversible at expense of great time and money.'

'The authors assume that many households, each with a few cattle, are responsible for large animal numbers. Thus they reason, relatively poor people cannot reduce herd size. In Botswana that is not the case, for few households own most of the cattle, and could easily reduce numbers without starving. They do not do so for social reasons.'

'They give the impression that output/ha increases with stocking rate. It does up to a point, and that point is a threshold beyond which a crash occurs, as Van Vegten showed (Van Vegten 1981).'

'Their suggested low price = high rainfall: low rainfall = high price pricing, would in fact ensure that the range was always heavily stocked, and would result in a beef shortage in good rainfall years.'

'The paper hardly takes into account why Africans keep large cattle numbers if they can. They are a major source of cash, especially in Botswana where arable agriculture is too risky and unprofitable. In both Botswana and Zimbabwe grazing land in Communal areas is in short supply, and some form of sustainable management that takes note of this major constraint is urgently necessary'.

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Grazing territories and variability of resources

The paper correctly emphasises variability of forage and water resources and of rainfall as a critical issue. Many problems arise from the current design of grazing schemes and systems: territories which are often too small to encompass the required diversity of resources, fencelines which do not take account of ecological variation, and recommended management systems which ignore farmer knowledge and practice. The full implications of this perspective for design of alternative schemes have not been fully explored by anyone yet, but this is surely the most pressing need for research and policy.

Although in general the point about the problems of self-sufficiency is well made, some of the paper's specific assertions can be queried. What, for instance, is the basis for asserting that 'critical size' will vary enormously? In some of the Zimbabwean grazing schemes I am familiar with, and which are much smaller than 6,000 ha, there appears to be *no* movement off the scheme to obtain forage and water, even in drought years. Chamatamba Scheme in Mondoro, for example, includes about 1,200 ha of grazing for 500 Livestock Units, and is bounded by two perennial rivers. It is true that *some* communities use fences to exclude outsiders from their grazing while simultaneously moving their own cattle onto outsiders' grazing, (Chiwundura Scheme in the Midlands is a good example), but this is by no means the dominant pattern. Where a scheme fences off a relatively large grazing area the most common form of inter-community conflict is the attempt by less well-endowed neighbours to gain access to forage resources in paddocks.

It is important to remember that the fenced paddocks constitute only part of the total forage-producing territory, since arable, fallow and commons outside the paddocks are extensively used by livestock as well, and these fall within a community's whole territory. Where paddocks enclose only some resources farmers often herd animals through non-enclosed areas such as vleis as well, even during the cropping season when the labour-saving benefits of fencing are most important. This is the case in Mutakwa Scheme in Zimuto, Masvingo Province, an area where the contrast between government-planned schemes (which take little account of ecological realities) and farmer practice (oriented to 'key resources' such as vleis) is dramatic. If the whole territory is taken into account then self-sufficiency becomes less problematic.

(Interestingly, one of the case studies used by the authors to compare rangeland condition inside and outside grazing schemes also involves extensive use of grazing outside of fenced paddocks. Ndambani Scheme in Bikita won the National Conservation Competition in 1988 for its five paddocks of grass in excellent condition and 3 ha plot of fine-stem stylo. The records of rotations kept by the Grazing Scheme Committee showed a regular pattern of seven days in each paddock right through the summer. Yet the five paddocks together are only 88.5 ha in extent, and the 42 households in the Scheme hold a total of 163 LU. The conundrum of stands of chest-high grass at a stocking rate of less than 0.5 ha/LU was explained when a herdboy revealed that in fact the community's herds had not entered the paddocks for six months, and that herding took place on the surrounding hills and in and around arable fields instead [and possibly on neighbouring villages' grazing as well - this was not possible to determine]. The paddocks were apparently intended to be used as a winter grazing reserve. The well-kept records appear to have been a highly successful ruse to win a competition and the rolls of barbed wire, the ploughs and wheelbarrows that come as prizes.)

The question of the qualitative aspects of forage resources and the possibility of missing trace elements etc is well taken, but these may affect levels of productivity rather than mortality. The point about trade-offs that the paper makes in relation to degradation can be applied here too.

Farmer practice appears to take variability much more into account than conventional grazing scheme design does; how can their practice be improved upon? The authors' suggestion that 'ecological fencing' be used to separate land types is intriguing, but as in the case of Scoones' (1989) suggestions with regard to 'key resource grazing schemes', the idea is not further developed. Proposals for a particular resource management group operating on a specific resource base would allow evaluation of the potential of this kind of approach. Even better would be implementation of such proposals on a trial basis.

Stocking rates and degradation

The notion of a 'socially-acceptable rate of range degradation assessed in terms of trade-offs' and a correspondingly socially-acceptable stocking rate is attractive in its recognition of the centrality of farmer perceptions and choices to the development of viable policies. The notion may also approximate how farmers themselves understand these issues.

Evidence of farmer perceptions has been gathered in interviews in a large number of grazing scheme communities in Zimbabwe and reported in Cousins (1988). The interviews showed that farmers reject the imposition of what they perceive to be arbitrary and inflexible stocking rates, and hope for *increases* in livestock numbers with the advent of the grazing scheme. Some said that the basis for this increase would be better management of the grazing, through resting etc, while others stressed the exclusion of outsiders' cattle. (This desire stands in stark contrast with extension plans for stabilising or even decreasing stock numbers in grazing schemes.) There also appeared to be an acceptance of relatively high rates of mortality when drought occurs, given the capacity of both grazing land and herds themselves to 'bounce back' when seasons of better rains re-occur. There was also a recognition that an upper limit to stocking rate increases exists, and that this was apparent in sharply declining levels of forage production. As one respondent in Chikowore Scheme in Musana commented: 'Of course there is a limit to how many animals we could keep ... . If we kept all our cattle in one paddock all the time then soon there would be a desert there and the cattle would die'.

Agronomic research in Zimbabwe has now accepted that the long-standing farmer practice of applying basal fertilizers *after* the emergence of a crop is well founded and does not result in yield losses (Low, *personal communication*) - after decades of futile extension efforts to persuade farmers to drop this practice. Perhaps science and indigenous knowledge are beginning to converge in the sticky area of stocking rate policy as well?

Social and institutional issues

Although not the primary focus of the paper, the policy implications spelled out at the end do touch upon some of the social and institutional issues which are so crucial to improved rangeland management. Following through the suggestions will mean, in my view, confronting the question of *democracy* at three different levels:

a) Research into the political dynamics of grazing schemes reveals that inequality and heterogenous economic strategies can undermine attempts at institutional innovation, or alternatively that rural élites are capable of manipulating institutional rules for their own benefit (Cousins 1990). Collective action is thus likely to be effective in the desired direction only when the majority in a community are empowered and are able to apply sanctions to the implementation of democratic decisions. 'Internalising costs and benefits' to specific groups of people *is* a prerequisite for rangeland conservation, but the existing structure of the distribution of costs and benefits within those groups will have to be confronted. This probably implies some attempt at a redistribution of wealth within rural communities, so that the ‘communal’ component of ‘communal tenure’ achieves a greater degree of reality than at present.

b) Empowerment of the majorities within rural communities is unlikely to take place without sympathetic state and/or ruling party policies aimed at facilitating such a political dynamic. Even a research/extension orientation which gives more weight to farmer views and practices (in such matters as the design of ecologically appropriate grazing schemes) is unlikely to become the norm if state policies remain premised on the marginalisation of the poor and rural. Rangeland conservation may thus depend critically on the further extension and development of political democracy.

c) One of the reasons why grazing schemes are planned on the basis of an inadequate territorial base is because they take as their starting point the existing distribution of populations and resources within national agricultural systems. In theory Zimbabwe’s resettlement programme complements ‘internal reorganisation’ of the Communal Lands and land use planning exercises, but in practice there has been little relief from the pressures of population growth. Yet in Zimbabwe the bulk of high quality land is still held by a small group of large scale commercial farmers. This politically influential group produces high value crops and livestock, and earns significant amounts of foreign exchange, but does not necessarily utilise all its land in an ecologically or economically optimal manner. Greatly increased resettlement, an expanded small farm development programme, and national policies aimed at supporting the middle and poor peasantry (ie a revitalised agrarian reform programme) may also constitute the only realistic basis for the technical and institutional innovations which Abel and Blaikie propose.

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In this brief commentary on Abel and Blaikie's paper I will attempt to elaborate on a series of statements made in the paper. Comments are based largely on my research work in southern Zimbabwe on communal area livestock production (Scoones 1990).

Range condition 'improves' under short duration grazing

Despite the considerable amount of research carried out on different grazing management systems in southern Africa, there has been no conclusive evidence presented in favour of rotational systems (Gammon 1978, O'Connor 1985). Establishing range improvement due to management system requires the controlling of a range of factors that influence vegetation condition. Most studies conclude that stocking rate is the dominant influence and that differences due to management are difficult to detect, especially in the short term.

In this work there is no evidence presented that the stocking rates are equivalent inside and outside the schemes studied. Indeed the authors comment that the grazing schemes are only used for part of the year and that schemes are often used by communities with larger grazing territories to protect them from grazing by outsiders. As mentioned, stock owners on schemes may also move their animals outside the scheme area to take advantage of the available communal grazing before retreating inside the scheme when resources become scarce.

If any of these conditions apply, the result will be that grazing pressure inside a scheme will be lower than that experienced outside, resulting in an expected lower biomass and 'poorer' species composition outside due to the effect of stocking rate. The findings will therefore not represent support for the short duration grazing (SDG) system, but merely reflect differences in stocking rate. Without substantiated evidence for stocking rate equality inside and outside the schemes (a difficult task under uncontrolled conditions), the stated conclusion of the vegetation studies presented cannot be fully accepted.

Short Duration Grazing is not always managed formally

In the discussion of Zimbabwean grazing systems, there was no information given on the form of management actually practised in the grazing schemes studied. It was assumed that the communities were following the recommended SDG system. Actually this may not happen. Grazing practices evolved by members of grazing schemes often involve adaptations of pre-scheme local grazing practices (eg deferred grazing, use of vlei/river bank resources) or the initiation of new ones (eg use of reserve grazing along fence lines and adjacent to roads). Formalised paddock rotations as encouraged by extension agents seem to be rarely adhered to (see Cousins's commentary). Without evidence that the schemes are actually carrying out recommended SDG practices, the data cannot be taken as support for the SDG management system.

As far as grazing scheme members are concerned, the advantages of fenced grazing schemes seem to lie mainly in the ability to exclude others and retain grazing lands for local use combined with reduced herding labour resulting from fencing. Additional advantages may lie in the opening of access to development assistance in other priority areas (such as water development) through cooperation with government or donor agencies, both of whom are keen promoters of grazing schemes (see Cousins's commentary).

People seem less keen on adopting a formalised rotational management scheme with the perceived hidden agendas of external control and destocking attached. The reasons for community acceptance of schemes, issues of conflict within and between communities and local management systems that evolve are all of interest here, but not addressed in the paper.

Rangeland varies in its ability to tolerate grazing

The paper's general discussion rightly takes a disaggregated view to differences in the resilience properties of different rangeland types. There are significant differences in the productivity, stability and resilience of clay soil savannas and sandy soil savanna areas. Sand veld areas appear to be quite resilient and with less variable production, while clay veld areas may have high potential production, but are more unstable and possibly less resilient (Frost *et al.* 1986). The implications for rangeland assessment are important, as different indicators will be required depending on the availability of different rangeland types for livestock use.

A differentiated approach to rangeland types is also of relevance to the development of the vegetation cover-soil loss model developed in the paper. The extrapolation of a relationship developed for Botswana 'hard veld' to Zimbabwe sand veld may result in significant errors of interpretation.

In the same model the estimation of soil losses using a plot based estimator (SLEMSA) may also result in misinterpretation. Soil loss from a single plot (30 m) says nothing about how soil is lost from the landscape as a whole. Degradation processes may involve redistribution of soil with reduction in productivity in one area, but increases elsewhere. Again this argues for a landscape approach to range types and soil processes.

There is not necessarily a clear link between changes in grassland species composition and livestock productivity

Choices of indicators for range assessment are reliant on the demonstration of a link between the indicator and livestock production. As mentioned in the paper, the presence of annual grasses may not necessarily be construed as an indicator of 'poor' range condition, as they may provide better quality and more timely fodder for cattle. In the same way, 'bush encroachment' may, in certain circumstances, be advantageous. In the context of communal area farming systems, 'bush' may be vital for dry season browse, small stock fodder and human use. This may not apply in a commercial beef ranch, where 'bush' as a competitor to grass, is regarded as a problem. Therefore the indicator used needs to be related to the objectives of the production system and a close understanding of the functional relationship between it and productivity.

Rangeland degradation does not begin or cease either side of some arbitrary 'carrying capacity'

The heuristic model developed in the paper to illustrate the importance of considering the trade-off between stocking rate and soil erosion demonstrates a number of very important points of relevance to policy makers. There is a clear need for time discounted economic assessments of the relative costs and benefits of different livestock policy options. This is an important start.

However I would argue that to have utility the argument needs to be taken one stage further and the relative costs of degradation and benefits to livestock production of different components of the landscape need to be considered. Certain land resources may be critical to sustaining livestock production and the costs of soil loss in such areas may be far higher than elsewhere. Such areas can be termed 'key resources' and may include valley bottomlands, riverine strips etc (Scoones 1989). A disaggregated approach to land degradation would focus on the question of which parts of the landscape are key areas for production, hold a higher 'environmental premium' and this should be the focus for management.

This paper focuses upon the technical basis of current rangeland policy

Perhaps the major (and partially acknowledged) gap in the paper is the failure to address the role of local level perspectives in grazing management policy systems around them. As argued in Scoones (1989), it is local people who can best identify 'key resource' areas and possibly organise territorial grazing management systems around them. The paper suggests the development of grazing management systems (with improved technical design of 'ecological fencing') and the development of policy options for opportunistic 'tracking' strategies. However these are argued for without an analysis of what local strategies and adaptations already exist and what are the implications of alternative management systems for local institutions and political forces.

In parallel with a technical critique of range management policy, it is vital to develop an analysis of the range of socio-economic, political and institutional factors that influence actual practice and potentials for intervention. Without this the technical preoccupations of land-use planners in southern Africa will continue to dominate and the failures of previous attempts at imposed land management (eg the Native Land Husbandry Act of the 1950s and 1960s, and the grazing scheme promotion that followed this in Zimbabwe) will be repeated.

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Abel and Blaikie's paper provides an excellent argument in favour of variable animal stocking rates based on a tracking strategy. The issues raised are of fundamental importance for pastoral development strategies throughout dryland Africa, and it is within this broader context that my comments are presented here.

On the technical side I find little to disagree with the authors. Their conclusions suggest a programme of necessary research which should keep us all busy for many years. For example, it is clearly important to have some idea about the position of the point of inflexion on the curve relating vegetative cover to soil loss (Figure 6). It is at levels of vegetative cover below this point of inflexion that rates of soil erosion start to rise more sharply, leading to the collapse of pasture productivity. But how much time and effort can we put into defining this point in practice, given the variability between sites? And how well-defined should it be if it is to be of practical value to pasture and herd managers? Can we use herders' knowledge of their own local environment to help identify changes in patterns of plant cover that would indicate we had neared this point of inflexion?

Establishing an effective tracking strategy is clearly the best option but it requires a number of essential support measures, outlined by Abel and Blaikie, which are themselves costly. First, what kind of assessment needs to be made of pasture productivity, at what time of year, to enable decisions to be taken about levels of stocking and subsequent movements of stock into/out of the grazing area? Second, measures to support animal prices and help with drought recovery programmes are not likely to be self-financing, as the authors admit. While Botswana may be able to subsidise the livestock sector from mineral export earnings, most Sahelian states would find it much harder to finance such price support programmes. Third, what kind of support (both administrative and technical) would be available in practice to local grazing associations to enable them to pursue an effective tracking strategy? Who would carry out the grassland productivity survey? Would local government be able to help administer de-stocking measures within the association, and help resolve conflicts within the association and between local herd owners and outsiders? Fourth, a fee inversely related to grazing availability in a particular year is evidently sensible in providing incentives to reduce stocking levels when grazing is in short supply. But what impact will this fee have on poorer households, particularly those with little access to cash incomes? If the re-stocking and drought recovery programme worked well, the position of poor households might not be adversely affected by the imposition of a fee, but in practice such programmes never do in fact work well. Instead, they tend to favour the better-off, well-placed households who know how the system works and who have easy access to the people who run the programme. Thus, I have my doubts about the equity of such a system, how reductions in stocking rates would be administered in practice and about the effects on equity of imposing a fee on different households within the association. One way around this might be by the imposition of a fee (inversely related to pasture availability) once some minimum level of animal holding had been reached.

The authors note that the governments of Botswana and Zimbabwe are both committed to the transfer of power and control over local resources to local communities. In the western Sahelian region, there is also widespread commitment at the level of government rhetoric to making farmers and herders responsible for the management of their own resources, yet little evidence of this happening in practice. Will governments really support local communities' rights to control access by outsiders to valuable resources, especially where these outsiders have strong government links? Will governments be ready to allow politically marginal herding groups the power and authority to determine who can water their animals and have access to grazing in a particular region? On paper, the commitment is there, yet in practice governments are both unwilling to permit and unable to support effectively this transfer of power to local groups.

The authors argue that an opportunistic stocking rate strategy is rational, even at the cost of massive stock losses in years of drought, since these losses are more than compensated for by gains during years of good rainfall. It would be interesting to see whether this argument also holds within the Sahelian context, given the frequency of drought, the damage inflicted on soils and vegetation during drought periods from heavy stock levels, and the possible existence of a feedback mechanism between rainfall and vegetative cover, whereby the frequency of drought increases as ground cover falls. (However, the latter feedback mechanism is yet to be firmly established.)

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I have been greatly stimulated by the comments received and thank the authors for their time and trouble. I respond under the headings below.

The existence of degradation

Our paper seems to have given Cooke, Silitshena and de Queiroz the impression that we do not think that grazing can cause rangeland degradation. That was not our intention. There *is* degradation but it needs to be assessed in terms of its rate, spatial distribution, magnitude and importance. Our paper attempts to do that. Cooke *et al.* state 'like it or not the range is degrading and something must be done'. But this assumes, against the background of a literature on Botswana almost devoid of evidence, that rates and seriousness have been assessed and found to be unacceptable.

In the case of soil erosion, much has been said and written in Botswana about the high rates of soil loss from the 'hardveld', but almost no research has been done. Yet the rainfall is not very erosive and the slopes are mainly gentle, so that a low rate of loss is predictable (Abel and Stocking 1987). About the only solid research on rate of loss to date is Biot (1988, 1990), and that shows very small losses from rangeland that is both heavily grazed and, by Botswana standards, steeply sloping. The onus to demonstrate that current rates of loss are unacceptable, even in the long term, is upon those who claim that there is an ecological crisis.

In Zimbabwe rates of soil loss from communal rangeland are undoubtedly greater than in Botswana. But here too there is a need to establish the seriousness of this loss. It is not rational to assume that 'something must be done' in every case of land degradation, for each country has limited resources and a set of priorities. In my opinion one of our jobs as scientists is to contribute to the establishment of these priorities, rather than to preach a gospel of conservation fundamentalism.

The reversibility of vegetation change

Much of the concern about rangeland degradation in Botswana and Zimbabwe stems from vegetation change. It is quite clear the gross changes have occurred this century. Given the increase in livestock numbers and the intrinsic instability of most semi-arid rangeland, it would be astonishing if such changes had not happened. We stressed that vegetation change is not degradation if the process is reversible. We also emphasised the great resilience of semi-arid rangeland. Cooke *et al.* argued that much shrub encroachment is effectively irreversible, and therefore constitutes degradation. A review of a number of recent articles obliges me to agree that much shrub encroachment and other changes in the structure and species composition of rangeland are indeed for practical purposes not reversible. I would now, given the chance, modify the emphasis in our paper on the ease with which vegetation change can be reversed. I shall next therefore discuss briefly some recent rethinking of theory of vegetation change.

Succession is the theory on which conventional range management is built. This assumes a smooth transition, in either direction, between a 'pioneer' plant community colonising bare ground, and a climax one representing the ultimate in diversity of structure and species composition that the climate and soil can support. Degradation is the process of reducing a plant community to a lower successional stage. The management ideal is thought to be a successional stage as close to the climax as can be achieved while using the land for economic gain. Add herbivores, introduce a drought or burn, and the climax is reduced to a lower stage, perhaps even to the pioneer phase if enough herbivores are added, or the drought persists. Remove herbivores or bring back the rain and succession proceeds again towards the climax. Equilibrium between herbivores and vegetation is achieved when progression towards the climax is exactly balanced by feeding pressure from herbivores or burning or both. Under varying rainfall this can only be achieved by adjusting the density of herbivores or the timing or frequency of burning or grazing. These are therefore the primary tools of range management.

The successional model does not work well in practice, nor do management strategies based upon it. In an extensive review of the literature Westoby *et al.* 1989 found many examples which falsify the theory. When livestock have been removed from rangeland the vegetation has often not changed at all, has not proceeded towards the climax as predicted, or has increased in quantity with no change in species composition. Perennial grasslands, converted under grazing pressure to annual pasture, have not reverted to perennial status under lighter stocking. Sometimes the stability of the vegetation in its new state can be explained by soil changes, but very often there are other reasons. One example is positive feedback between vegetation and fire. Grasses support fire and are themselves supported by fire. Woody plants are susceptible to fire, particularly in the seedling stage. Woody plants, once established, suppress grasses. These relationships can lead to the existence of alternative stable states, one grass-dominated, the other wooded. Other explanations include, following Westoby *et al.*

(1989):

- **demographic inertia** Some plants require the chance concurrence of rare events in order to become established. Examples might be a particular sequence of fire, drought and flood. Once established such plants may persist for very long periods

- **the existence of more than one stable equilibrium between herbivores and vegetation** Noy Meir (1975), among others, has shown how this can occur

- **alternative stable states depending on the initial abundance of species** When there are competitive relationships between plants, those which are initially abundant may be able to establish dominance and determine the nature of the community. Any change in the initial abundance obviously affects the outcome

Bartolome (1984) makes the crucial point that changes in vegetation, and recovery from those changes are often different processes, and that recovery can follow several different pathways. This follows logically from the existence of the processes of vegetation change outlined above. Although grazing and browsing play a part in these processes, it is quite often a minor one. Vegetation changes induced by climatic fluctuations have in some cases so overridden any effects of herbivory that Bartolome (1984) wrote that:

> Grazing by large herbivores is of little importance in the process of vegetation change. Environmental factors associated with the site are usually the major determinants of the species mix (p919).

He argues that site factors are so important in determining the nature of both initial change and recovery that it is not possible to generalise about the effects of reduced stocking on the recovery of vegetation. That destocking can fail to produce the desired change has already been stated, but Westoby *et al.* 1989 ask us to:

> ... drop the assumption that inaction or conservative grazing is safe. In many situations moderate grazing leads to range deterioration. Sometimes very heavy grazing is a constructive thing to do ... . Where possible, regulation should focus not on stocking rates but on changes in the actual state of the land or the vegetation (p273).

This leaves little reason for continuing to apply the conventional succession model to range management problems. Westoby *et al.* (1989) have offered a ‘state-and-transition’ model as an alternative. This recognises the existence of a set of more-or-less stable states for each rangeland plant community. The management objective should be to shift to or maintain a favourable state. The desirability of a particular state is determined by the requirements of the land user rather than in relation to an ecological ideal, as in the case of succession theory. Management and research seek to discover how natural events and management action effect transitions between states. Management actions must be applied at a time and in an amount which works with and not against environmental processes.

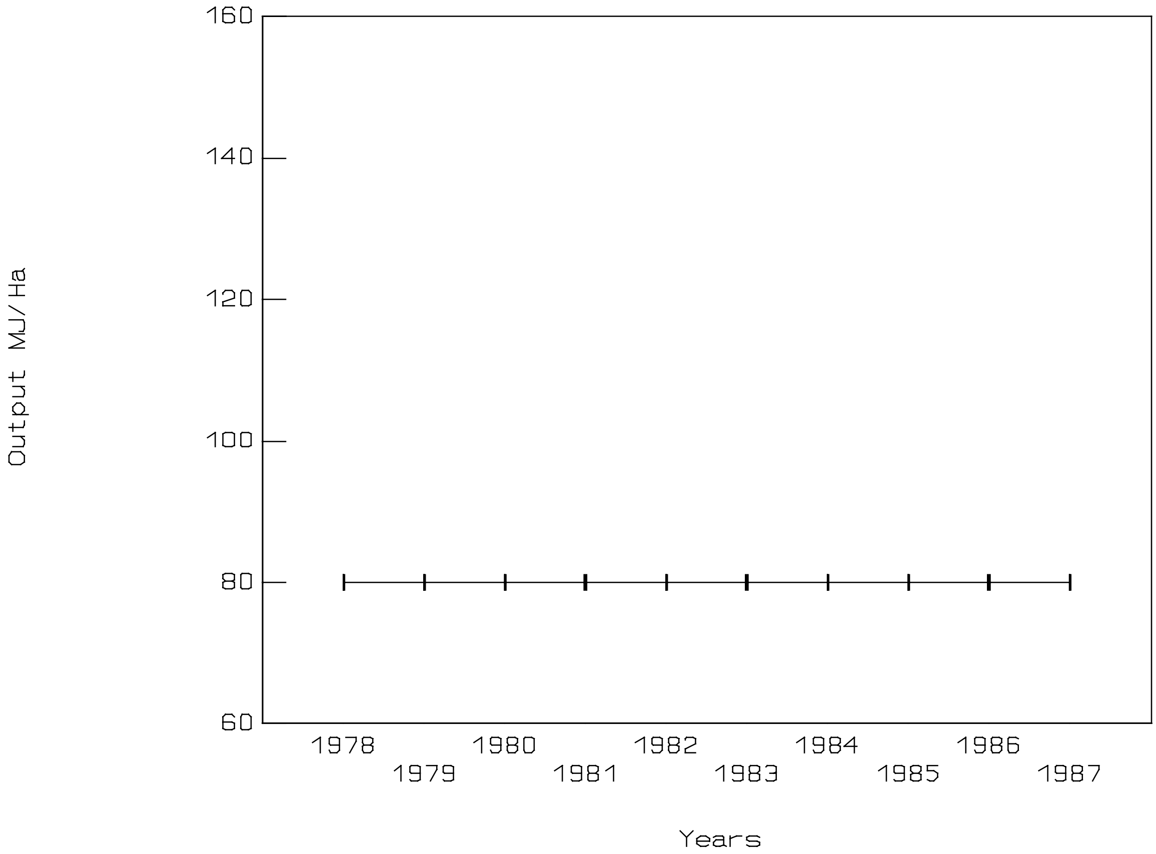
Management action should not be applied at those times when environmental processes make it ineffective because they are working in the opposite direction. Thus managers cannot achieve favourable changes of state at will, but only when environmental conditions permit. This is an opportunistic management strategy.

Succession theory, on which recommendations promoting destocking of communal rangelands has been based, is not a useful basis for management. Current thinking advocates site-specific and opportunistic management. Opportunistic management is already the practice in the communal areas. It does not follow that communal area producers have found the best available opportunistic strategy. But current strategy does produce the required outputs in far greater quantity than could be produced under a regime of conservative management. Destocking would not only reduce welfare in the communal areas, but, since it is based on theory that is demonstrably wrong, it is unlikely to produce the vegetation changes which conservationists tell us we want.

The effects of vegetation change on livestock productivity

Cooke *et al.* argue that while range is 'reversibly degraded' it is producing less than it could. Much of our paper was devoted to refuting that claim, based on work by Behnke (1985a), and de Ridder and Wagenaar (1984, 1986). I have since modelled output at different stocking rates using the Agricultural Statistics for Southern Region (CSO 1978 to 1987), milk output and weight gain in the Integrated Farming Pilot Project (IFPP) study area (Flint 1986), and ranch productivity data from the Animal Production and Range Research Unit (APRU 1978 to 1988). I used three stocking levels in the model; a conservative one of 12 ha/livestock unit, an opportunistic one based on a 1983 stocking density of 6 ha/LU, and an intermediate one of 8.5 ha/LU. I assumed that each strategy had the same objective - the production of milk and draught power, and growth and replacement of the herd. The productivity index was the energy content in Mega Joules of the annual output of milk, draught power and 12 month old calves. I used growth and reproductive data from APRU ranches for the conservative strategy, plus an estimated amount for milk production. The opportunistic model was based on reproductive rates from the regional agricultural statistics, and growth and milk yield data from IFPP. The intermediate strategy was, not surprisingly, set between the two extremes. The assumptions were highly favourable towards the conservative strategy and would have reflected any supposed benefit to be gained from improved vegetation conditions under reduced stocking. The results are summarised in the graph below.

Total output per hectare
(from southern region, milk, ox-power and calves combined)



I conclude that for destocking to produce an increase in output per hectare, the increase in the productivity per LU would have to be much greater than has yet been achieved under the capital-intensive conditions of the APRU ranches. So, this not being achievable, I disagree with the assertion of Cooke *et al.* that 'reversibly degraded' land has a lower secondary productivity.

Cooke *et al.* do also state that there must be an optimum stocking density above which output per hectare falls. Certainly, but a figure for Botswana quoted in our paper suggests that this density is substantially higher than the densities so far achieved in the communal areas.

The redistribution of soil in the landscape

Biot and Scoones both emphasise the importance of scale in looking at soil loss, making the point that the redistribution of soil in a landscape implies a similar redistribution of primary productivity. Abel and Stocking (1987) modelled soil loss from catchments in the IFPP study area and predicted only small exports of sediment. In my view this should now become a major area of study, linking to the work already initiated in Australia (Pickup 1985).

The self-sufficiency of communal grazing schemes

Cousins questions the large size of communal scheme mentioned in the paper as necessary for self-sufficiency. I was surprised to learn how small one of the apparently self-sufficient Zimbabwean schemes is. By contrast De Leeuw talks about the insufficiency of ranch-sized schemes in Maasailand. Certainly in Australia, as in Botswana, there is considerable inter-regional movement of livestock during severe drought. The paradox is that while seasonal movement is technically efficient, the trend, determined by political-economic pressures, is towards increasingly formal tenure and subdivision, making movement harder (De Leeuw again, and Behnke 1985b).

Spatial variation in the resilience of the landscape

Toulmin points to the practical and economic difficulties of management measures designed to cope with spatial variation in resilience. Cousins encourages us to operationalise the concept of ‘ecological fencing’. Both are right in noting the difficulties of applying management measures to a heterogeneous landscape. Westoby *et al.* 1989 have with their ‘state-and-transition’ model begun the process of finding practical solutions. What is clear is that bureaucracies with their craving for standardisation are unsuited to the management of such variable resources.

Social and institutional issues

Cooke *et al.* point out that ownership of livestock in Botswana is highly skewed, so that if owners of large herds shed animals, destocking could be achieved without the further impoverishment of numerous poor households. This is true for the country as a whole, and for the Kalahari sandveld, but it applies much less to the traditional ‘hardveld’ communal areas which were the focus of our paper. Here ownership, although inequitable, is less skewed, and even the owners of the larger herds are not rich except in comparison to those with few or no animals.

Cousins takes up the issue of inequality of livestock ownership, regarding it as a major hindrance to the formation of institutions for the control of communal grazing in Zimbabwe. The development of communal activities in Botswana is similarly handicapped. Toulmin warns against the inequitable effects of implementing a subsidised tracking strategy, because from experience elsewhere it is the wealthier households that tend to use such schemes. A progressive management fee such as she proposes was also proposed by Abel *et al.* 1987. I wonder if it would be acceptable?

We seem in our paper to have given the impression that we were advocating a return to traditional forms of livestock management in advocating a ‘tracking strategy’ (Cooke *et al.*). This is in fact Sandford’s (1977) idea. Tracking involves measures to promote stocking rates that follow variations in carrying capacity in time, rather than in space. The aim is to reduce drought losses and pressure on pastures when carrying capacity falls. Traditional methods emphasised rapid build-up between and accepted catastrophic loss during droughts. Cooke *et al.* point out that such a strategy would lead to low sales in good years, which could be a problem.

Local knowledge and local production strategies

Scoones and Cousins both emphasise the need to learn about local production strategies before promoting any type of intervention, and I accept this unreservedly. It should in my view be the role of governments to promote policies which *enable* rather than coerce agropastoralists to use their land resources wisely. For too long the aims of the two governments and of people in the communal areas have been in opposition because livestock development policy, built on succession theory, has always carried the threat of de-stocking. Newly emerging ecological theory provides at least a technical basis for an approach towards the development of livestock in the communal areas that is more in line with the opportunistic methods of the agropastoralists.

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Comments on PDN paper 28b 'Patch use by cattle in dryland Zimbabwe: farmer knowledge and ecological theory' by Ian Scoones (August 1989)

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I have comments of two kinds: general ones concerning the relative statuses of local technical knowledge and range science, and specific ones concerning the interpretation of ecological processes in the case study. I am in full agreement with Scoones on the utility of local knowledge as a provider of ecological information, and on the need to use local production strategies as a framework to guide research and development in the communal areas. I will be harder than he has been (in this particular paper) on the contribution of range science to the understanding of ecological processes in the communal areas. But when I look at the subjects of 'patch use' and 'key resources' I find myself defending range science, as it has been using such concepts for a long time. It may be that because of the complete failure of communication between range scientists and local people the former fail to learn about crucial processes from the latter. But range science is changing from within, and the type now taught and practised will, I hope and predict, be fundamentally different in the emerging paradigm. I expect the theories of the new paradigm to vindicate many of the strategies of African pastoralists. I expand slightly on these ideas below.

The awakening of academics and development agencies to the depth and sophistication of local technical knowledge (LTK) began during the late 1970s, although there are instances of much earlier use of LTK by employees of colonial governments such as Allan (1965). The use of LTK in developing countries, Zimbabwe included, is not established to any significant extent within state or international development agencies, and may for political reasons never become so. Nevertheless the notion that LTK is valid and useful has become intellectually respectable (Chambers 1983, Richards 1985), and there is no need in this response for further advocacy. There is, however, a need to examine the role and contribution of range science to the development of the communal areas in Zimbabwe.

Range science originated in the US during the 1890s at a time when large, growing and spreading livestock populations were interacting with drought to produce crisis in the livestock industry (Stoddart *et al.* 1975, Dwyer *et al.* 1984). It was introduced to Zimbabwe in the 1960s by people like Riney and Dasmann. Although the science is so recently established it has become the dominant influence on the bureaucratic approach to the management of the communal area rangelands and the formulation of policy in Zimbabwe.

Range science is thus very new both in the world and in Zimbabwe. It is concerned with the study and management of ecosystems that are inherently variable in space, and over time. It has had only a brief period in which to accumulate data needed to test and develop its theories. It has nonetheless been applied in a confident and prescriptive manner in the traditions of 'scientism' (Abel and Stocking 1981) as if it were based on established scientific 'laws' and 'facts'.

The main theories of conventional range science are concerned with changes in plant species composition and associated changes in livestock productivity under various stocking densities. These theories are now crumbling under the accumulation of contradicting evidence from the heartland of range science in the western US (Bartolome 1984), from Australia (Westoby *et al.* 1989), and from Africa (Behnke 1985). Yet in Zimbabwe its theories continue to dominate the formulation of policies such as the National Conservation Strategy, and form the technical basis for the establishment of grazing schemes. Graduate range scientists continue to be trained in conventional range science methods, and local knowledge continues to be ignored.

One reason for using local knowledge in the development of communal rangelands is that, unlike formal range science, it is based on a long period of observation of the phenomena with which it is concerned. Another is this: one of the greatest failings in conventional range science is its inability to generalise from specific studies of rangeland because of the unique behaviour of individual sites (Bartolome 1984). Local knowledge is based on the shared experiences of many observers of a multiplicity of sites, so there is less need to extrapolate widely from a limited number of cases.

Turning now to the specific case of Scoones's 'key resource' areas, in fairness to range science this concept has been well established for decades (Stoddart and Smith 1955). Its neglect in the estimation of grazing capacity in Zimbabwe brings into question the technical competence of practitioners, but not in this instance range science itself. This does however raise an important difference about the relationship between range scientists in the US and in Africa.

In the US there is reasonable (although far from perfect) congruence between the aims and methods of range scientists, and those of the producers (ranchers). The scientists and ranchers are from the same culture and tend to share similar values. Thus in the US LTK and formal range science interlock and influence each other. In Africa, at least until recently, range scientists have mainly been expatriate or of European origin. They have been applying methods and assumptions derived from ranching to the analysis of communal area production systems. Such systems have a totally different rationale, and there is no congruence at all between range science and communal area production strategies. In such circumstances it is easy to see how the lack of communication and understanding between range scientist and communal area farmer could lead to the former failing to appreciate the significance of key resources.

I have already said that many of the tenets of range science are crumbling. These are mainly to do with the stability and resilience of rangeland, the relative impacts of climate and of grazing animals on range condition, and on the effects of reducing grazing pressure on range condition (Bartolome 1984, Westoby 1989). The current tendency is to move away from grand theory as the basis of uniform management responses to change, and towards more flexible and short term responses to environmental variation in time and space. It seems that the flexible and opportunistic production strategies of African pastoralists are about to be vindicated in a new paradigm of range science.

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From the perspective of Australian rangelands science, Scoones (1989) is fascinating for its parallels with findings in the very different pastoral systems of this ‘developed’ continent. This comment aims to discuss two of these parallels as support for his arguments, but also remark on some lessons which have been learned from these issues in Australia.

Firstly it must be observed that the capitalistic, low population density pastoralism of Australia (see, for example, Harrington *et al.*, 1984) is very different to the system practised in Zimbabwe, and indeed that I have relatively little experience of the latter. Typical property sizes in Australia range from 10,000 ha to 30,000 km^2 carrying free-ranging sheep or cattle within the confines typically of 10 to 40 fenced paddocks. As an indication of population density, a single property might support 1 to 10 families. The effect of poor years is one of financial losses and possible bankruptcy rather than famine and starvation.

Key production areas

Scoones (1989) emphasises the importance of spatial heterogeneity in the landscape at both regional and local scales. Regional differences between dystrophic and eutrophic soil types and consequent vegetation differences are vital to the large scale patterns of seasonal land use for Zimbabwean pastoralists. More locally, areas of richer productivity within these zones are responsible for the majority of animal production.

Spatial heterogeneity plays an equally important role in the Australian arid zone, both in the functioning of natural ecosystems (Stafford Smith and Morton 1990), and in animal production in areas where these systems are tuned to pastoral management. For example, about 60% of the production of a 200 km^2 arid zone cattle paddock came from about 30% of the area (Foran 1984). This type of result is due to the uneven use of a heterogeneous environment by sheep (Stafford Smith 1988) and cattle (Pickup and Chewings 1988). Their uneven distribution is driven partly by their dependence on water in dry times, and partly by their preferences for particular vegetation types, as well as shade and camp-site locations.

This selection can be said to occur at both spatial scales noted by Scoones (1989), since some poor soil types with their associated vegetation alliances such as the spinifex covered sand plains are highly unpreferred, but local selection for riverine strips and calcareous country is also very strong. Furthermore some managers certainly practise some deliberate transfers of animals between paddocks dominated by vegetation types with different responses to seasonal conditions. Finally, agistment, whereby animals are transferred out of a droughted area, is common in dry times especially in recent years as transport facilities have improved. Although this movement is normally either to another property owned by the same company, or in return for payment, it is actually not uncommon to hear of pastoralists with major excesses of feed helping out neighbours who happen to have missed out on storms, for relatively trivial payments.

Scoones (1989) has similar findings to these in relation to pastoralists in Zimbabwe, although the patterns of movement of their animals are doubtless more defined by the behaviour of the herders rather than of their animals, at least at the more critical times of year. He rightly remarks that the management of the productive capacity of the richer or more reliable components of the landscape becomes a prerequisite once their importance is recognised.

It is important to highlight two different types of units in this regard. Some landscape units are vital because they persist in dry times (whether through a dry season or over dry years) but are not particularly preferred in wet times. In paddocks with some heterogeneity this is often true of Australia’s mitchell grasslands, and it sounds as if it is also true of Zimbabwe’s sandier soils. These units are naturally rested, and may therefore be naturally resilient. Other units are in fact preferred at all times. In Australia, with its generally very poor soils, areas where nutrients are concentrated such as riverine strips and calcareous soils are examples, and the same might be true in Zimbabwe. These units will always be grazed preferentially as long as they exist - even after apparently being grazed bare, such minor growth as appears may be preferred to dry material elsewhere. In general it is in these units that the most significant land degradation has occurred in Australia. Thus it is vital to identify the different functioning of these two land types and manage them accordingly. (This is a factor of significance to the conclusions of Abel and Blaikie, 1989.)

It has been noted that the levels of soil nutrients in rangeland Australia may generally be below that of even the dystrophic soils of southern Africa. Thus animals forced to eat, for example, most spinifex grasslands rapidly lose condition and die (giving rise to the apocryphal research station story of experimental cattle greatly preferring to graze the cardboard cartons in which equipment was delivered over any of the local vegetation). This means that the loss of the better land units of the Australian rangelands may have more dire consequences for net pastoral production than is suggested by Abel and Blaikie’s (1989) analysis for Zimbabwe.

It is clear that the pastoralists of Zimbabwe are close observers of the use of the land by their stock, and the types of findings discussed above would not come as any surprise to them. Although early pastoral production in Australia was often herded, most animals have been allowed to range freely within their paddocks for nearly a century now. This absence of active herding may well have led to the importance of critical areas being overlooked by many Australian pastoralists, who have tended in the past to stock their paddocks as if they would be grazed almost evenly. Today a better appreciation of the spatial variability of grazing pressure is rapidly developing, with associated efforts to design paddocks to maximise the evenness of grazing, or to target it sensitively at the best parts of the range (eg Lange *et al.* 1984, Purvis 1986).

The importance of pastoralists’ perceptions

A second vital issue that Scoones (1989) raises is that of the importance of obtaining information from land managers. Poorly targeted research is always a problem, and the more diffuse the problem and the ‘owners’ of the problem, the more poorly targeted it usually seems to be. Thus Scoones’s call for such an approach deserves endorsement (eg Stafford Smith and Foran 1990).

Two additional points should however be made. Firstly, although research should always take advantage of managers’ experiences, these should not, of course, be taken as the final words. Industry surveys in northern Australia, for example, determinedly return the belief that cow mortalities average only 6.1% (GRM 1987).

When herd models are run with such rates, however, huge surpluses of young animals are produced (Foran *et al., submitted*), and real rates must be around 15%. Managers are both failing to allow for the effects of variable years in estimating their 'average' mortalities (in fact they are quoting the mortalities in good years), and secondly, many are keeping inadequate records to obtain accurate estimates anyway. Managers' experiences and ideas can therefore be used as an intelligent starting point, which in many but not all cases may turn out to be as good as research's end point.

Secondly, tying research to management at the start of a project is one thing, but making the connection during and at the end of a research project is equally essential. Some research efforts are badly targeted because a simple exercise in sensitivity analysis would have indicated that the management system is not sensitive to particular outcomes (for example, Scoones rightly points to the importance of understanding the functioning of the key areas, before spending too much time on the rest of the landscape). Other research, of course, is ineffectual because it is never transmitted on to potential users at the end. I would argue that one of the major positive outcomes of taking Scoones's (1989) approach of talking with pastoralists at the start of projects is that it becomes clear whether or not an extension route is likely to exist at the end of a project, and a social connection to enable this extension to take place may actually be formed. The need for researchers to be open to methodologies such as those outlined in Abel *et al.* (1989) has never been more clear.

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Comments on both PDN paper 29a (Abel and Blaikie 1990) and PDN paper 28b (Scoones 1989)

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The paper by Ian Scoones touches on spatial heterogeneity and the importance of key grazing areas for the survival and/or productivity of herds. Many examples of 'niche' exploitation by livestock (and their herders) exist, a few of which are given in the paper on carrying capacity (PDN 29a 1990). This spatial heterogeneity and its effect on the utilisation of grazing resources by stock is usually overlooked when estimating carrying capacity or safe stocking rates. I guess most range managers/scientists are aware of this complication, but do not know how to deal with it and even Scoones does not indicate how to adjust carrying capacity to spatial variability. Neither do we deal adequately with the 'temporal' heterogeneity or the ups and downs in the forage supply, although Le Houérou has published a couple of papers on the relationships between the variability in annual rainfall and in forage supplies (see *J Arid Env* 1988, 15: 1-18; 1984, 7: 1-35).

The importance of browse is another complicating element in the assessment of carrying capacity. In the paper on carrying capacity (PDN 29c), woody cover is treated as a negative factor leading to downward adjustments of carrying capacity. The Zimbabwe data (see Figure 8, Tables 7 and 8) are the highest browsing percentage for cattle I have ever seen. I wonder whether these are isolated events, lasting a few days or whether cattle continued this behaviour over any length of time?

The paper by Abel and Blaikie raises a number of issues that are equally relevant to the management and productivity of pastoral herds in Maasai group ranches. The main issue of destocking to reduce perceived overstocking was part and parcel of the development philosophy underpinning the justification of creating group ranches. As we know now, nothing really happened and cycles of under- and over-stocking usually followed the ups and downs in seasonal (and annual) rainfall. Although permanent settling increased, grazing territories coincided with group ranch area only in good years and became more unstable during dry seasons/years, while chaotic mobility occurred during the serious drought in 1984 (as described by Grandin *et al.* 1987, 1990), implying 'lack of self-sufficiency' of group ranch areas (Abel and Blaikie, p17/3). However, despite this, individuation is rapidly progressing and many group ranches have been subdivided in recent years. How Maasai producers are coping with this drastic reduction of their grazing orbits is not known, but it is an issue worrying the administration and in general a decline in livestock productivity is predicted.

Proper stocking rates is another issue discussed in the paper and the situation in Maasailand is similar to that described by the authors. Heavy pressure had led to bush encroachment and an increase in the proportion of annual species, which colonise bare ground. Many of those are ephemeral forbs and grasses adapted to low seasonal rainfall and short growing seasons. Recolonisation of bare ground by perennial grasses is a very slow process except for rhizomatous ones like *Cynodon spp.* and *Bothriochloa*. B H Walker made some relevant comments on this process (see *Australian Rangelands Journal* 10:2 1988 p73).