



On non-equilibrium in arid and semi-arid grazing systems

GUEST
EDITORIAL

A critical comment on A. Illius and T. G. O'Connor (1999) On the relevance of non-equilibrium concepts to arid and semi-arid grazing systems. *Ecological Applications*, 9, 798–813.

A debate in ecology rages over the sources and types of dynamic behaviour driving ecological systems. Drylands have become a particular focus of this debate. In these environments extreme and unpredictable variability in rainfall are considered to confer non-equilibrium dynamics by continually disrupting the tight consumer–resource relations otherwise considered to pull a system towards equilibrium. This implies that livestock grazing in drylands, widely thought to cause degradation and 'desertification' through bad management practices leading to overstocking, might not be causing irreversible ecological change through over-use of vegetation. An article recently published in *Ecological Applications* (Illius & O'Connor, 1999), however, argues that variability in arid and semi-arid grazing systems is not the outcome of qualitatively different dynamical behaviour, and that livestock *do* cause negative change through 'normal' density-dependent relations. The authors maintain that these operate primarily in key resource areas and during drought periods.

We contest these arguments on several grounds: that key terms are poorly applied in ways which suggest inconsistencies in the internal logic of the arguments; that the paper is unjustifiably selective in the choice and interpretation of 'evidence' on which it builds; and that the authors do not engage critically with the crucial policy implications of the debate, particularly as they relate to pastoral land-use by African herders in areas under communal tenure and management. We do not suggest that degradation *never* occurs in arid and semi-arid rangelands, but that Illius and O'Connor's analysis of the mechanisms *via* which this might take place is misleading and at times, we feel, theoretically bizarre. In particular, we suggest that drought periods may be the times when density-dependent mechanisms are least likely to occur and that key resource areas exist because of ungrazeable reserves which effectively cannot be degraded, although subject to heavy grazing. We also attempt to draw out the theoretical implications of attributing change caused by biotic–abiotic effects to biotic–biotic interactions. We contend that non-equilibrium concepts remain crucial for both natural and social science approaches to understanding dryland environments and their multiple, dynamic uses by pastoralists.

Illius and O'Connor's critique of non-equilibrium concepts as a means for understanding ecosystem behaviour in African drylands seeks to address a deepening conceptual fault line separating factions of the rangeland science and livestock development community. Epitomizing one side of this divide are understandings of semi-arid and arid environments in terms of relationships between their biotic components, emphasizing the potential for grazing by domestic livestock to perturb the 'system' from a knowable and desirable 'climax' community at equilibrium. On the other, is an emerging idea of such environments as continually driven, or 'disturbed', by abiotic factors, primarily rainfall, such that systemic effects of herbivory are relatively unimportant. The two approaches have been termed 'equilibrium' and 'non-equilibrium', respectively. The debate is significant for ecological theory because viewing arid and semi-arid environments through these two lenses can influence what, and how, questions are asked about these environments, consequently affecting interpretations drawn from findings of ecological studies. However, its importance is greater than this. Equilibrium thinking in rangeland science has fostered a pervasive and self-referential narrative which holds that degradation and 'desertification' are endemic in drylands, particularly those utilized by African livestock herders under communal forms of land tenure (see, for example, Stebbings, 1935; Grainger, 1982, 1990; Dregne, 1983; Lamprey, 1983; Chapman, 1992; Seely & Jacobson, 1994; Wolters, 1994; Middleton & Thomas, 1997). This degradation narrative carries critical implications for the institution of democratic policy and planning in these areas.

The term non-equilibrium was coined by ecologist Wiens (1984) to describe the dynamics of arid and semi-arid ecosystems. As Illius & O'Connor (1999, p. 799) paraphrase, he argued that '...all ecological systems fall somewhere on a continuum from equilibrial to nonequilibrial ... the latter ... showing weak biotic coupling, independence of species, abiotic limitation rather than resource limitation, density independence and large stochastic effects'. Illius and O'Connor insist that the validity of non-equilibrium theory depends on showing that non-equilibrium environments are *qualitatively* different from equilibrial environments governed by density-dependent inter- and intraspecific interactions (Illius & O'Connor, 1999, p. 800). While admitting the failings of rangeland

management models predicated on equilibrium concepts of carrying capacity and Clementsian succession, the crux of their position is that a new paradigm for pastoral development in Africa '...cannot be predicated on the notion that grazing has a negligible impact in semi-arid systems' (Illius & O'Connor, 1999, p. 799).

The consequent conclusion drawn by Illius and O'Connor is that non-equilibrium dynamics are less relevant, for both rangeland ecology and policy formulation, than argued in many recent studies (for example, Sandford, 1983; Coughenour *et al.*, 1985; Caughley *et al.*, 1987; Homewood & Rodgers, 1987; Ellis & Swift, 1988; Warren & Agnew, 1988; Westoby *et al.*, 1989; Boonzaier *et al.*, 1990; Biot *et al.*, 1992; Behnke *et al.*, 1993; Shackleton, 1993; Thomas & Middleton, 1994; Scoones, 1995; Sullivan, 1996; Mortimore, 1998; Oba *et al.*, 2000a). Illius and O'Connor's primary contention is that '...despite the apparent lack of equilibrium, animal numbers are regulated in a density-dependent manner by the limited forage available in "key resource" areas, which are utilized in the dry season' (1999, p. 809). In other words, '...strong equilibrium forces exist over a limited part of the system, with the animal population being virtually uncoupled from resources elsewhere in the system', such that '...spatially and temporally, the whole system is heterogenous in the strength of the forces tending to equilibrium' (Illius & O'Connor, 1999, p. 809).

We acknowledge that present understandings of non-equilibrium environments are as much conceptual as empirical, and argue that further site-specific studies across a variety of dryland areas should be understood within their own contexts in order that the theory can be applied critically by decision-makers in the interests of social, economic and environmental 'sustainability'. What follows is our attempt to promote a critical debate by analysing three crucial components of their paper: namely, a review of their interpretation of key terms; a reconsideration of the material they present as 'evidence'; and an engagement with the policy implications of their arguments.

Since Illius and O'Connor's paper was published in 1999, it has been cited as providing a serious challenge to a non-equilibrium view of arid and semi-arid rangelands (e.g. Cowling, 2000). We thereby offer our comments here in the spirit of intellectual engagement and discussion regarding issues critical for both the discipline and 'real world' of ecology, and for the livelihoods of those who utilize dryland environments.

REVIEW OF KEY TERMS

Level and scale

We fully accept Illius & O'Connor's (1999, p. 799) arguments that non-equilibrium dynamics are likely to be important at small spatial and temporal scales of observation, but that, despite unpredictable extinction and recruitment events (i.e. 'instability') characterizing transient patches, some sort of 'stability' emerges at larger scales (cf. Allen & Starr, 1982; Connell & Sousa, 1983, p. 792; Wiens, 1984, p. 454; Caughley, 1987, p. 165; DeAngelis & Waterhouse, 1987, pp. 2–3; Costanza *et al.*, 1993, p. 545, 548, 553; Collins, 1995, p. 96). Such observations have emphasized that spatial and temporal scale affect conclusions regarding ecosystem behaviour (Auerbach, 1984, pp. 414–415) and highlight the danger of extrapolating findings from small-scale studies to larger spatial and temporal scales (cf. Sandford, 1983; Warren & Agnew, 1988, p. 7, 12; Perkins & Thomas, 1993; Stocking, 1996, pp. 148–151; Rohde, 1997a; Sullivan & Konstant, 1997; Sullivan, 1999).

What we query is Illius & O'Connor's (1999, p. 799) extrapolation that stability at larger scales implies a 'stable equilibrium state'. Instead, we consider that a more accurate term would be that of 'qualitative persistence', i.e. a term which accepts that variability in a *quantitative* sense occurs at all times and in all directions in living 'systems', but that the emergent *qualitative* character of these 'systems' may remain essentially the same, i.e. persist over larger temporal and spatial scales. Importantly, if short-lived or transient patch dynamics observed at smaller scales are crucial elements in the persistence of systems at higher hierarchical levels (DeAngelis & Waterhouse, 1987, p. 2; Costanza *et al.*, 1993, p. 545, 548, 553), then logically it may follow that local variability arising as a result of varying intensities of resource utilization by people and livestock may be more correctly interpreted as 'positive' in terms of conferring long-term persistence to the qualitative character of the wider ecosystem. This is particularly relevant for the African situation where pastoralist populations have been creating, maintaining and utilizing arid and semi-arid rangelands for millennia; although in many contexts patterns of use have been radically affected during this century through policy interventions, particularly in land tenure, the distribution of water-points, and the intruding demands of a capitalist market economy.

By the same token, however, on extremely large temporal scales (e.g. geological/evolutionary) the qualitative character of an environment might be catastrophically transformed in nonlinear and unpredictable ways (e.g. through climate change, meteor impact, etc.), or, in the somewhat shorter term, due to economic and/or political factors. What makes the acceptance of non-equilibrium thinking particularly relevant for drylands is that dramatic variability in productivity in these environments occurs *in the timescales that are crucial for management decisions* enabling the sustenance of livelihoods based on multifaceted pastoral production. This is particularly true for environments at the more arid end of the spectrum because of extreme interannual variability in rainfall and the overwhelming constraints of this on primary productivity.

Arid and semi-arid

Two relationships between biological productivity and moisture availability are important in drylands. First, productivity is primarily (but not exclusively) limited by plant available moisture as opposed to plant available nutrients. Secondly, that above an absolute minimum amount of precipitation (the zero-yield threshold) productivity increases linearly with rainfall (Walter, 1939; Noy-Meir, 1973; Seely, 1978; Rutherford, 1980; Seely & Louw, 1980, pp. 32–37; Hadley & Szarek, 1981, pp. 747–748; Deshmukh, 1986; Le Houérou, 1984; Caughley, 1987, pp. 159–160; Hoffman *et al.*, 1990, p. 463; Günster, 1994, p. 152; O'Connor & Roux, 1995, pp. 619–621). Given this significance of precipitation for primary productivity, a third pattern is extremely important; namely, that there is a positive relationship between aridity and interannual variability of rainfall. In other words, productivity is both more moisture-limited and more variable the drier the region.

Illius & O'Connor (1999, p. 800) draw attention to the importance of drought frequency and interannual variations in rainfall in their references to Caughley *et al.* (1987), Ellis & Swift (1988), Coppock (1993) and Ellis (1994). But assessing the relevance of their case-study findings is problematic because no mention is made of the rainfall regimes affecting the examples drawn on in their paper. We would suggest that such an omission gives rise to a rather misleading juxtaposition of case material. This is because there may be some qualitative differences between a very arid area such as Turkana, where some areas receive 150 mm yr⁻¹ (cf. Ellis & Swift, 1988), and a semi-arid area such as southern Zimbabwe, where wetter areas receive in the region of 800 mm yr⁻¹ (cf. Scoones, 1993). Turkana is considered to be driven by non-equilibrium dynamics because of its correspondingly high *interannual* variability of rain *not* because of its 'extreme seasonality' (Illius & O'Connor, 1999, p. 800) which all arid and semi-arid environments share. Differences in moisture regime between arid and semi-arid environments *may* mirror a continuum between non-equilibrium and equilibrium dynamics, as suggested by Wiens (1984, see above), such that arid environments are affected by unpredictable abiotically driven dynamics to a greater extent than wetter, semi-arid environments. As outlined above, however, spatial and temporal scale-effects also are important in considering the relative importance of these dynamical processes.

Density dependence

Arguments for equilibrium vs. non-equilibrium dynamics in rangelands rest on implicit assumptions regarding plant : herbivore interactions. The difference between the two approaches is akin to the proverbial chicken and egg. Those adhering to equilibrium arguments hold that livestock populations are tightly coupled to the availability of forage and can thereby impact adversely on vegetation when concentrations of herbivores exceed forage productivity. Non-equilibrium arguments maintain that, while livestock clearly require forage, the availability of forage is driven by (or coupled more strongly with) overriding abiotic factors, primarily rainfall but also fire and substrate conditions: it is these abiotic constraints on primary productivity that drive animal populations and thereby weaken any deterministic coupling between plants and animals. For both theories, declines in primary and secondary productivity are focused during droughts. From an equilibrium perspective, these are the periods when animals degrade vegetation by exceeding the population that can be supported without harm to the wider environment; thus, livestock impact '...in drought years will be accentuated by the reduced abundance of vegetation relative to animals' (Illius & O'Connor, 1999, p. 801, 802). Thus, when animals die during or following drought it is because they have effectively 'eaten themselves out of house and home', such that density-dependent mortality is the inevitable result. A simple question follows, however, that if drought reduces forage availability, then how can animal populations have an *increased* impact on vegetation that is not there, as is the case during extreme events in grassland

savannas? Management strategies incorporating mobility as well as diet-switching to browse and pods are employed at these times precisely to attenuate the lack of alternative forage, thereby further shifting attention from species vulnerable to lack of rainfall (e.g. Homewood & Rodgers, 1991; Oba & Post, 1999; Oba *et al.*, 2000a, p. 36, 2000b).

We feel, therefore, that Illius & O'Connor (1990, p. 801) model interrelationships between rainfall, primary productivity and livestock populations in a rather misleading way. In this, herbivore population dynamics are linked in a density-dependent manner to vegetation growth but random variation (at CV = 50%) in the rate of herbivore mortality is incorporated in order to '...prevent the system from reaching the theoretical equilibrium'. The authors' intention is to demonstrate that '...stochastic variation leads to dynamics which might be confused with a nonequilibrium regime', when density dependence in fact 'is an explicit component of this equilibrium model'. Their justification for this model is a reported claim by Ellis & Swift (1988) that in the non-equilibrium pastoral system of Turkana *per capita* animal mortality rates are random with respect to vegetation (Illius & O'Connor, 1999, p. 801). We can find no claims to this effect, however. Instead, Ellis and Swift's (1988, pp. 453, 455–456) paper is infused with statements describing drought-induced livestock mortality, the close tracking of rainfall-driven seasonal dynamics of plant productivity by livestock nutritional status and productivity, and density-independent regulation of livestock populations by drought perturbations. Be that as it may, the output of Illius and O'Connor's simulation, not surprisingly, is that '[r]andom variation in (animal) mortality rate prevents the system from reaching the theoretical equilibrium' (Illius & O'Connor, 1999, p. 801). In the first instance, this might beg the question of whether the theoretical equilibrium has any conceptual worth for a model this dynamic, or empirical worth for the 'real world' situations it is intended to portray (see below). More importantly, however, the model bears little relation at all to non-equilibrium ideas as currently formulated for arid and semi-arid environments, where animal populations are not driven by 'random variation in mortality', but by stochastic variation in rainfall-driven primary productivity, i.e. animal mortality is *not* random, is very much related to the availability of forage, but the incorporation of stochastic abiotic phenomena driving primary productivity greatly ameliorates the usefulness of models and ideas of strict density-dependent (biotic–biotic) relationships.

Illius & O'Connor (1999, p. 802) nevertheless assert that '[t]he existence of density dependence shows that animal population dynamics are coupled to vegetation, and that animals are at least having a sufficient impact on the vegetation to experience intraspecific competition'. At this stage in their paper it seems to us that the existence of density-dependent interactions and consequent effects on animal population dynamics have not been demonstrated at all convincingly. Whenever reductions in animal populations or vegetation biomass have been referred to it has been in the context of drought-induced limitations; abiotic factors, as opposed to density-dependent interactions, thus seem more robust in explanatory terms.

We would further question Illius and O'Connor's assertion that if animal populations are uncoupled from vegetation resources through intervention, e.g. through supplementary feeding, this will '...present the greatest risk ... of high stocking rates resulting in catastrophic impacts on vegetation and soils' (Illius & O'Connor, 1999, p. 802). Surely in some cases any intervention which allowed sustained high stocking rates would do so precisely by removing the constraints on animal populations imposed by rainfall-driven primary productivity. In other words, feed inputs which support high production rates tend to decrease pressure on forage resources thus reducing, *not* strengthening, biotic–biotic interactions (Perevolotsky & Seligman, 1998). On the other hand, by encouraging high concentrations of livestock through their decoupling from climatically constrained forage availability, change might occur due to the effects on soils of focusing livestock populations in localized areas, i.e. via biotic–abiotic interactions (although see *Soil erosion* below). These circumstances characterize situations such as refugee settlements and the overcrowded and artificially created 'homelands' of southern Africa, where herders have been compelled to survive within inappropriately small areas of range and frequently are dependent on drought feed and other supplements in order to do so. Our point here, however, is that biotic–abiotic effects should be distinguished theoretically from ideas of tightened consumer–resource relations and density-dependent (biotic–biotic) interactions (Sullivan, 1996).

Key resource areas

Illius and O'Connor's critique of non-equilibrium conceptions of dryland environments rests crucially on the significance of 'key resource areas', i.e. patches of vegetation where productivity is relatively

stable and where density-dependent interactions apparently are more important than abiotically induced fluctuations. In other words, of limited patches of forage constrained by equilibrium dynamics in an otherwise non-equilibrium landscape of uncoupled plant : herbivore relationships (Illius & O'Connor, 1999, p. 798, 802).

We wish to highlight three points in this regard. First, the conditions which make possible the concentration of forage in these 'limited' key resource areas are presumably abiotic, including soil and substrate factors that allow the concentration of moisture (primarily) and nutrients. These areas, therefore, must be (and are) also subject to abiotic fluctuations which, by affecting the availability of forage, contribute to fluctuations in animal populations. As Illius & O'Connor (1999, p. 802) themselves state, '...variation in key resources cannot be negligible or they would support more stable animal populations than is generally observed'.

A second aspect of this is that key resource areas are likely to be comprised of species with considerable 'ungrazeable reserves' (cf. Walker & Noy-Meir, 1982; Deshmukh, 1986, p. 120). We agree that heavy grazing can have an effect on population processes, especially on species which do not reproduce vegetatively. However, woody vegetation concentrated along drainage lines and creeping perennial grasses of *vleis/dambos* and swamps, for example, both have reserves of biomass (trunk and roots, and underground storage organs, respectively) that livestock are unable to consume under any stocking levels and which allow plant survival and future productivity under a range of abiotic conditions (Homewood & Rodgers, 1987, p. 117). In other words, in key resource areas both abiotic and autecological factors might conspire against the incidence of density-dependent (and deleterious) impacts by livestock (also observed in Oba *et al.*, 2001). An example is the *Cynodon* Rich. and *Echinochloa* P. Beauv. swamp grasslands of the southern end of Lake Baringo, Kenya, an area which official statements throughout this century describe as an 'overgrazing endpoint' or worse (as documented in Homewood & Rodgers, 1987, pp. 121–122). Livestock concentrate on these grasslands during dry periods when, because of the lack of rainfall, there is little forage available elsewhere in the wider landscape. Despite this concentration, however, the creeping and underwater habits of the swamp grassland species make it impossible for livestock to graze out these key resources, leading to situations where livestock starve to death even while the swamp sward remains effectively undamaged and productive (Homewood & Rodgers, 1987, p. 122; Homewood, 1994).

Finally, the significance of 'key resource areas' in Illius and O'Connor's arguments rests heavily on Scoones's work in six communal areas of southern Zimbabwe (Scoones, 1993, 1995; Scoones & Cousins, 1994), where low-lying *vleis/dambos* constitute important dry season grazing. Illius & O'Connor (1999, p. 801) assert that '[t]he scarcity of these key resources obviously implies tight density-dependent coupling of animal population dynamics with these vegetation components' and that 'Scoones ... found clear evidence of density dependence in fecundity and mortality ... directly negating one assumption of non-equilibrium dynamics, namely that of weak biotic coupling'. Several qualifying comments are appropriate here. First, Scoones (1993, p. 64, 70, 72) suggests that any density-dependent regulation in runs of non-stress, i.e. wetter years, is rather weak and that mortality rates during these periods also are significantly correlated with, and possibly explained by, rainfall. Secondly, it is unclear exactly what processes maintain population levels during low stress periods. The assumption by Illius and O'Connor is that they are caused by density-dependent regulation through the coupling of animal populations to forage availability, particularly in key resource areas. Scoones (1993, pp. 74–75), however, highlights the ability of management by herders both to expand the spatial range of forage use through migration, and to enhance the use by cattle of spatially (and temporally) dispersed key resources. In other words, opportunistic management decisions act to reduce the coupling of animals to specific key resource areas. Thirdly, and as also noted for northern Kenya by Oba (2001), cattle mortality rates in 'stress' years from 1923 to 1986 were completely unrelated to density (Scoones, 1993, p. 70): given that stress periods dominate livestock population dynamics for these areas, it would seem that non-equilibrium concepts have a greater explanatory and practical usefulness. Fourthly, differences in the quality of sward found in low-lying *dambos* under different grazing regimes raise questions regarding apparent impacts of herds on *dambo* vegetation. Scoones & Cousins (1994, p. 589) note, for example, that *dambos* within large-scale commercial farms under freehold tenure and with low stocking rates are dominated by poor-quality grass species whereas vegetation in communal area *dambos*, which are regularly grazed at high stocking rates, have higher quality forage and productive potential. Finally, it should be mentioned that the area analysed by Scoones (1993, p. 62) receives in the region of 600–800 mm a⁻¹, i.e. it is at the somewhat wetter end of the spectrum by any definition of semi-arid lands (cf. Mortimore, 1998, pp. 9–15). As stated above, very arid areas in which productivity is predominantly moisture-limited and where moisture

availability is extremely variable might be expected to demonstrate particularly close relationships between rainfall and productivity from year to year. Scoones' work suggests that, while livestock populations in rather wetter areas might exhibit dynamics which can be described as density-dependent for periods when there is a run of good rain years, these are by no means the dominant limiting factors for these areas.

Non-equilibrium vs. equilibrium

Illius & O'Connor (1999, p. 799) agree that there is growing evidence that abiotically induced non-equilibrium can '...promote coexistence of species at community scale', thus providing the beginnings of a mechanistic understanding of the consequences of non-equilibrium. They contend, however, that definitions of non-equilibrium systems as '...not presently or usually at equilibrium' are meaningless because they tell us little or nothing about the effects of non-equilibrium on population or community phenomena in such systems (Illius & O'Connor, 1999, p. 799). Apart from suggesting that non-equilibrium ideas are highly explanatory because they make explicit the tight links which exist between unpredictable abiotic phenomena and biological productivity, this begs the question as to whether definitions of dryland grazing systems based on density-dependent equilibrium dynamics tell us more.

Key to this issue is the statement that '[v]irtually all natural systems are to some extent nonequilibrium ... in permanent orbit around a moving attractor, never coming to rest at the equilibrium it would achieve under constant conditions' (Illius & O'Connor, 1999, p. 800). To exemplify this contradictory situation of a theoretical equilibrium which a grazing system in the 'real world' never actually reaches, Illius & O'Connor (1999, p. 800) draw on a model of kangaroo population dynamics in arid south-east Australia by Caughley (1987) which demonstrates stable vegetation biomass and kangaroo densities *under simulated constant rainfall conditions*. Our response to this is that if system equilibrium is only possible under purely hypothetical 'constant conditions', then what is the value of shoe-horning analyses of 'real world' environments into a conceptual framework which has the idea of equilibrium as its cornerstone?

This question is particularly relevant for environments that are unpredictably variable within the time-scales important for human production complexes and decision making. For these environments, the statement that '[d]isruption of the equilibrium of systems *that would be stable under constant conditions* commonly occurs as a result of stochastic, abiotic effects' (Illius & O'Connor, 1999, p. 800 emphasis added) is curious and patently academic. Caughley (1987, pp. 160, 167–168, 184), in fact, additionally states that the rate of kangaroo population increase '...is predicted best by pasture biomass which is predicted best by rainfall', that the environment overall has '...one driving variable, rainfall, which is unpredictable', and that the explanatory strength of rainfall in relation to animal population dynamics increases with increasing aridity. The study concludes with a damning statement regarding the validity for such an environment of carrying capacity concepts based on assumed density-dependent relationships, i.e. equilibrium dynamics: '[w]e do not argue that the notion [of carrying capacity] is invalid for an arid-zone grazing system. Our objection to it is stronger: that it cramps our understanding of that system' (Shepherd & Caughley, 1987, p. 194; also see Norbury *et al.*, 1994; McLeod, 1997).

System

It is arguable that dominant ideas of density dependence and equilibrium system-dynamics in ecology are an artefact of the broader modernist project of reductionist and mechanistic analytical science, and thus can only be fully understood when set within their social, historical and political contexts. In other words, they are part of a particular approach to knowledge which understands the 'natural' environment as conceptually separable from society (cf. Latour, 1993), and which emerged in a relatively constant and predictable temperate climate (Oba *et al.*, 2000a, p. 35). Crucial in this regard is the influence in mathematical ecology of tractable models developed to describe closed thermodynamic systems, for which deflection from normal system behaviour (or equilibrium) is seen as random variation or 'noise'. Systems thinking (including General Systems Theory, the theoretical field of cybernetics and energetic analyses) thus draws on an ideal of bounded systems, emphasizing identification of negative feedback relationships between the system's components as the mechanisms which preserve stability of desirable system states (cf. critique in Jantsch, 1980, p. 56; Moran, 1984, p. 6). More complicated equilibrium models such as stable limit cycles (cf. Coppock, 1993) or multiple stable states separated by transition events (cf. Westoby *et al.*, 1989; Milton & Hoffman, 1994), integrate the

possibility of discontinuous shifts between states. The dominant focus, however, remains on ‘...the ability of these systems either *to return to equilibrium following disturbance* (resilience), or *to retain equilibrium in the face of disturbance* (resistance)’ (Sullivan, 1996, p. 3).

Given contextual influences on the construction of what amounts to an ‘...equilibrium-centred view of constant nature’ (Holling, 1986, p. 310, 313), it is perhaps instructive to speculate on what an ecology rooted concept in an alternative environment might look like: particularly if, as in tropical drylands, this environment is continually and unpredictably variable in the time-scales of immediate relevance to human survival. It is likely that the ‘norms’ of such an ecology, i.e. its reference points and ‘key signifiers’ in metalanguage terms (Eco, 1984; Fowler, 1991; Stott, 1997, 1998), would embrace abiotic change and autecological variability, opportunism, creativity, uncertainty, flexibility, fluidity and exchange. The abiotic ‘stresses’ considered external to an idealized biotic and somehow bounded system, what Illius & O’Connor (1999, p. 803) describe tellingly as the ‘...rare, extreme events such as drought or fire’, might be conceived instead as ‘...the essence of things’ (Stott, 1997, p. 208), such that ‘...the system itself is a moving target’ (Holling, 1998, p. 4). Indeed, integrative approaches to complex (living) systems characterized by openness, system–environment interactions, non-equilibrium, multiple levels or scales, and internal reinforcement of fluctuations (cf. Jantsch, 1980, p. 24; Kauffman, 1993; Cilliers, 1998) emphasize all of these, and have important and recognized implications for ecology (Allen & Starr, 1982; Holling, 1998). The term ‘system’ itself thus is problematic, implying easily defined boundaries and masking usually unquestioned assumptions regarding the source/s and desirability of dynamical behaviour. Our contention here is that a culturally and historically located notion of the term ‘system’ lies at the root of the conceptual tools at the core of the biophysical sciences, and that a reconsideration – even deconstruction – of the ways in which these terms emerged and evolved is *central* to understanding the significance of the debate we are attempting to extend in this editorial (cf. Foucault, 1970 (1966); Escobar, 1996; Worster, 1997; Anker, 1999).

CRITIQUE OF EVIDENCE

This is not the place to indulge in a comprehensive review of studies of land-use and ecology in arid and semi-arid grazing systems. Instead, we aim to highlight incidences of what we feel to be unconvincing ‘evidence’ in Illius and O’Connor’s paper, and to introduce some alternative analyses and interpretations. Oba *et al.* (2000a) review a large, and in many cases, complementary range of studies which, in general, support and add to our observations here. We follow the headings used by Illius and O’Connor with the *proviso* that study findings are in most cases interrelated.

Species composition

Illius & O’Connor (1999, p. 803) review a number of studies which indicate that plant species composition is affected by grazing over and above the effects of rainfall. Our first observation is that there seems to be something of a contradiction in a broad conclusion drawn in this section. It is stated that ‘...the effect of grazing on species composition appears to be greatest at low rainfall sites’ [Illius & O’Connor, 1999, p. 803 following O’Connor (1985) and Abel (1993)], again, due to the effects of drought in ‘...focusing herbivory on a depleted number and biomass of surviving plants’ (Illius & O’Connor, 1999, p. 804). But given the known positive relationship between amount of rainfall and primary productivity as discussed above, these should be the *least productive sites*; and this conclusion thus seems counter to the statement earlier in this section that ‘[c]hanges in the species composition of the vegetation were found to be related to the duration of continuous heavy grazing, *especially at more productive sites*’ (Illius & O’Connor, 1999, p. 803 following Milchunas & Lauenroth, 1993). Interestingly, Milchunas *et al.* (1989) elsewhere found that slope position accounted for ‘...a greater proportion of the variation in herbaceous community structure and species composition than did grazing effects’ (reported in Parsons *et al.*, 1997, p. 325). Apart from the contradictions between the two sets of findings, we find problematic the idea of a deterministic relationship such that the lowest rainfall areas will be the more susceptible to herbivore impact. A recent study (Sullivan, 1998, pp. 253–278), which assessed herbaceous species composition and productivity in an arid area (*c.* 100 mm yr⁻¹) under communal management over two growing seasons, suggests that drought in this very dry area has an ‘equalizing’ effect, such that, in a year of below-average rain, six dispersed sites with twenty-four enclosed and twenty-four unenclosed quadrats all experienced next to no herbaceous productivity with no statistical difference between them. Under these conditions, it is impossible for livestock to impact on herbaceous species because the lack of rain precludes their growth (also see Oba *et al.*, 2000a); it is

at these times that livestock switch to browse, particularly nutritious pods of leguminous woody species, and herders employ mobility in search of alternative forage resources. Conversely, in a year of above-average rain, species composition was highly variable between sites (reflecting persistent seed-banks) and was related more to site location (reflecting soil moisture) than to the different intensities of livestock pressure characterizing the sites. Similar observations have been made for areas at the more arid end of the scale by Lusigi *et al.* (1986 in Oba *et al.*, 2000a,b), Ellis & Swift (1988) and Ward *et al.* (1998).

Be that as it may, the main patterns regarding impacts of livestock on species composition noted by Illius and O'Connor are that grazing is associated with a change from a perennial to an annual sward, often with a high incidence of forbs (e.g. Kelly & Walker, 1976; Le Houérou, 1989; Tacheba & Mphinyane, 1993; O'Connor, 1995), and a decline in 'palatable' species (e.g. Abel, 1993). We do not deny that these patterns have been observed in these studies. However, we would add that assertions of simple (linear) trends frequently are problematic and beg reinterpretation, and that alternative study findings reveal complexity in species responses which seem to be masked in this section.

As an example, we draw here on a recent study by Parsons *et al.* (1997). This compared herbaceous layer condition under communal and commercial cattle grazing and commercial wildlife ranching in South African lowveld with a rainfall of 580–700 mm yr⁻¹. Contrary to the studies cited above, they found the highest *proportion* of palatable grasses under the very high stocking rates of the communal sites (p. 319, 325). This significant result did not even take into account the higher *frequency* of 'palatable' species, associated with extremely high tuft densities in these sites (see below). These sites, however, also had the highest proportions of annual and 'unpalatable' species, although it should be said that the *F*-ratio measuring the strength of this finding was relatively low ($F = 9.5$) (Parsons *et al.*, 1997, p. 324). The data indicate that this was mainly caused by a high incidence of the unpalatable annual *Perotis patens* Gand., a species which also was found in the commercial cattle and wildlife farming sites at levels within the range of those found for the communal grazing sites (Parsons *et al.*, 1997, p. 329). Also interesting is that the communal grazing sites had by far the highest proportion of the palatable perennial *Cynodon dactylon* (L.) Pers. (Parsons *et al.*, 1997, p. 329) which, as discussed above, is able to withstand high grazing pressure because the creeping habit of its underground rhizomes means that a high proportion of its biomass constitutes ungrazeable reserves. Conversely, the palatable perennial *Panicum maximum* Jacq. was present but with low cover values in these sites (Parsons *et al.*, 1997, p. 329). In other words, these are differences in species *dominance* which do not necessarily represent a loss in palatability or range quality. What we are trying to emphasize here is that simple trends rarely stand up to closer scrutiny, and that it can be problematic for findings such as these to be interpreted as a shift to less desirable pastures under the heavy stocking levels frequently associated, particularly in southern Africa, with communal tenure regimes.

Indeed, alternative studies highlight the following rather different findings. Dahlberg (1994, p. 1, 23) suggests that '...differences in land use (between communal and commercial grazing lands which were historically heavily and relatively lightly stocked, respectively), at 460 mm yr⁻¹ were not large enough to have caused any major differences' in the composition of either herbaceous or woody vegetation. There were, however, '...clear differences between the two characteristic soil types studied', and also in relation to temporal heterogeneity (again, albeit over only two growing seasons) caused by '...fluctuations in amount and intraseasonal distribution of rainfall'. Similarly, Ward *et al.* (1998), in comparisons of sites heavily stocked under a communal tenure regime with sites on freehold land lightly stocked for commercial beef production, found no evidence for differences in community structure (cover, diversity and richness) of perennial plants.

Given complexities in the interpretation of study findings, as well as the sometimes contradictory findings themselves, we wish to draw attention to three issues which we believe underlie and complicate observations of reduced forage quality resulting from the impact of herbivory by livestock on plant species composition.

First is the lingering spectre of classifications of range condition based on identifying the 'successional status' of species with reference to the perceived climax community or 'benchmark' for a given area. As pointed out by Hurt & Bosch (1991, p. 131), and as a result of the influences in the 1930 and 1940s of state officials well-versed in the orthodoxies of North American and European ecology (Beinart, 1996, pp. 57–59; Scoones, 1996, pp. 35–38), it is conventional within southern African rangeland ecology to define species as Decreasers and Increaseers of various classes based on their successional response to disturbance from a 'climax community' caused by grazing. Proportional species composition is then used '...as a basis for calculating some form of range conditional assessment' (Hurt & Bosch, 1991, p. 131; also see De Klerk, 1947, pp. 352–353; Foran *et al.*, 1978;

Tainton *et al.*, 1980; Vorster, 1982, p. 84; Bosch & Janse Van Rensburg, 1987; Danckwerts & Stuart-Hill, 1988; Barnes, 1990a,b; Trollope, 1990; Trollope *et al.*, 1990; Bosch & Gauch, 1991; Bosch & Theunissen, 1992; Strohbach, 1992; Joubert, 1997). These classifications incorporate the life history (e.g. annual or perennial) and palatability status of the plant. Given this tendency towards successional classification of species, and in the absence of long-term data for African rangelands under communal use, it is easy to interpret a dominance of the so-called 'subclimax' species as indicating a trend towards 'degradation'; an example being Illius & O'Connor's (1999, p. 804) endorsing of Snyman & Fouché (1991), who use conventional 'veld condition scores' of botanical composition to demonstrate, somewhat tautologically, that primary productivity is lowest in rangeland classified using these scores as 'poor' (discussed below).

Van Rooyen *et al.* (1991, p. 63, 70, 1994, p. 358), however, suggest that for semi-arid Kalahari savanna conditions species such as *Eragrostis lehmanniana* Nees and *Chloris virgata* Sw., which are commonly viewed as pioneers or Increasers and thus as indicators of degradation in the moist eastern savanna areas of southern Africa (cf. Rethman *et al.*, 1971, p. 60), should not be viewed as such in environments characterized by greater aridity (also see Bosch & Janse Van Rensburg, 1987, pp. 143, 146–147). Similarly, and as observed in Kalahari duneveld vegetation by Fourie *et al.* (1987, p. 53), species classified as relatively undesirable pioneers may increase under successive wet years but *not* at the expense of the more desirable so-called Decreaser species and under '...relatively undisturbed conditions', despite their conventional association with degraded areas. This has been observed, for example, for *E. annulata* Rendle ex Scott-Elliot and *Enneapogon desvauxii* P. Beauv. in the Kalahari (Van Rooyen *et al.*, 1990, pp. 85–86). Assessment of veld condition based on the relative proportions of species classified conventionally as Increasers and Decreasers would, in this case, erroneously conclude that '...veld condition declines during wet periods, although the increase in the density of Increaser species is not accompanied by a decline in the density of Decreaser species' (Fourie *et al.*, 1987, p. 53). In more arid areas 'pioneer' species may in fact represent a healthy and palatable response to relatively marginal and variable conditions. The so-called South African 'lovegrasses' or *Eragrostis* spp., for example, are known to be persistent under a range of drought conditions (Cox, 1984, p. 160) and might be considered desirable species in arid areas. Even in relatively moist areas species normally classified as unpalatable may be consumed by livestock 'with relish' during particular times of the year, as noted for *E. plana* Ness in the high altitude 'sourveld' of north-eastern Transvaal during winter grazing by sheep (Rethman *et al.*, 1971, p. 57; also see Parsons *et al.*, 1997, pp. 325–326).

An alternative interpretation, therefore, is that irruptive grass species usually classified as pioneers or 'Increasers' should not be considered as an early stage of a classical successional progression held back by overgrazing. Instead, they can be viewed as a 'normal' component of a dynamic mosaic of species in an assemblage conferring resilience in arid and variable conditions (cf. Frost *et al.*, 1986, p. 57; Mace, 1991). Even for the wetter 'western grassland biome' of South Africa (670–700 mm yr⁻¹), Janse van Rensburg & Bosch (1990, p. 11), in a critique of their earlier work in this area, conclude that '[t]he ecological grouping ... of species on a grazing gradient differed not only for different topographical units but also for subhabitats within a certain unit. Broad standardized groupings of species into Decreaser and Increaser groups are therefore scientifically wrong and of little practical use'.

Additional complexity is bestowed by both autecological variations in the responses of species to similar environmental conditions (cf. Deshmukh, 1986) and ecotypic variation which may cause the same species to respond differently under various environmental conditions (cf. Bosch & Theunissen, 1992, p. 95, 100). For example, ephemeral species may behave as perennials under favourable conditions, as described by Seely & Louw (1980, p. 29) for two grass species of the Namib desert dunes (*Stipagrostis ciliata* (Desf.) De Winter and *S. gonatostachys* (Pilg.) De Winter. As pointed out by Shackleton (1991, p. 114) with reference to grass species in southern Africa, there is a clear need for detailed autecological information for so-called pioneer or 'undesirable' species in these 'extreme' environments before these species can be used as definitive indicators of degradation.

Secondly, we feel that some confusion surrounds the mechanisms by which livestock might bring about compositional changes in species. In particular, this relates to teasing apart changes caused 'indirectly' by livestock through impacting on substrate parameters (i.e. biotic–abiotic interactions), as opposed to those caused by the direct consumption of plants by herbivores (i.e. density-dependent biotic–biotic interactions) (cf. Sullivan, 1996, p. 4; Parsons *et al.*, 1997, p. 325). Our contention here is that the former processes often are used to justify the existence of the latter relationships, although the linkages between animals, plants and substrate conditions are qualitatively different in the two cases. Key examples here are observed changes in vegetation, normally from a herbaceous sward to a higher incidence of woody plants, around newly established waterpoints (cf. Molelele & Perkins, 1998). The

key feature of these situations is that livestock are spatially focused by their water, rather than their forage, requirements. Similarly, in the study in north-west Namibia cited above, one site was dominated by forbs as opposed to grass species, but this was the location of a former settlement and species composition thus is likely to be caused by the impacts of settlement and concentrations of livestock on soil parameters (Sullivan, 1998, p. 275). Further complicating the issue is that this site experienced the highest *diversity* of species following above-average rains (Sullivan, 1998, p. 234) which, from a biodiversity conservation perspective, might be considered a more desirable 'state' than other sites which would be classified as in better condition from a range management perspective.

This brings us to our final observation, which is that for the studies of species composition, commented on by Illius & O'Connor (1999, p. 803), no mention is made of the potential 'resilience' of semi-arid and arid rangeland sward composition following the 'release' of these areas from heavy grazing. Dahlberg (1994, p. 21), for example, found that sites 'rested' for 4 years from what seems to have been continuous use for grazing and cultivation, at least during this century, demonstrated high regenerative power in terms of the establishment of species considered indicators of good grazing condition as well as in herbaceous productivity. Similar observations have been made by Ellis & Swift (1988) and in a review of several southern African studies presented by Shackleton (1993). At the same time, and as demonstrated in a range of studies led by Kenyan ecologist Gufu Oba (e.g. Oba, 1998; Oba & Post, 1999; Oba *et al.*, 2000a,b, 2001), arid and semi-arid rangelands from which livestock are excluded over the long term (defined in these studies as >5 years) may exhibit declines in species richness and the productivity of graze- and browse-tolerant species (at least where herbivory occurs at 'moderate' levels, as defined in these studies).

Plant productivity

In their section on plant productivity Illius & O'Connor (1999, p. 805) conclude that '...intensive utilization by herbivores may reduce primary production, while increasing its interannual variability'. Again, we would like to posit some reinterpretations of the studies they refer to in support of this position, particularly those by Le Hou  rou (1984), Snyman & Fouch   (1991) and Kelly & Walker (1976). We also offer some alternative examples.

Illius & O'Connor (1999, p. 804) draw on the studies by Le Hou  rou (1984) and Snyman & Fouch   (1991) because these seem to demonstrate that rain-use efficiency (RUE, i.e. annual primary productivity by annual rainfall) and plant production are compromised by rangeland degradation. A problem here is that it is by no means clear in these publications what relationship asserted 'degradation' has to land-use or to livestock grazing, and the corresponding argument that degraded sites show degradation again is somewhat tautological. For example, the statement by Le Hou  rou (1984, p. 241) that RUE is '...substantially lower in degraded ecosystems or considerably higher in pristine conditions' comes with little qualification as to how these 'states' are defined. Similarly, Snyman & Fouch   (1991) classify range condition as 'good', 'moderate' and 'poor' using predetermined 'veld condition scores' that, as discussed above, may categorize environments as degraded in a perhaps untenable manner.

Key 'evidence' in Le Hou  rou are high RUEs for the Serengeti plains presented in comparison with lower values from elsewhere in East Africa and from West Africa. Le Hou  rou (1984, pp. 233–234) asserts that '[t]he high values found in the Serengeti are explained by the fact that they were obtained in a National Park, in still unspoiled savannas which are among the most productive in Africa, growing as they do on the basalt derived soils of the Rift Valley'. Leaving aside the human rights issues associated with excluding pastoralists from using the Park, and the hoary issue of to what extent former pastoralist use of the plains have created the 'unspoiled savannas' which are so appealing to conservationists, it would seem from this statement that like is not being compared with like. To what extent, for example, do substrate factors account for observed higher RUEs in the Serengeti, given that the other areas mentioned do not have the luxury of its rich basaltic soils? This is particularly relevant given the clear relationships between substrate and RUEs demonstrated elsewhere in Le Hou  rou's paper (1984, pp. 235–240). Moreover, it seems to us that the relationship between RUE and consumption of biomass by herbivores is somewhat circular. RUE values rely on measures of plant biomass (primarily aerial phytomass) taken at particular moments in time and frequently are herbage yields at the end of the growing season (Le Hou  rou, 1984, p. 229). It stands to reason that these will be affected by their relation to both rainfall and grazing events: lower RUEs for grazed compared with ungrazed sites (cf. Le Hou  rou, 1984, Tables 6, 8 and 9) simply demonstrate that herbivores eat grass. Biomass thus has been reduced at the moment of measurement compared with ungrazed sites, but this

does not in itself indicate that the productive potential of plants has been reduced because of grazing. Elsewhere, Le Houérou's paper focuses predominantly on demonstration of the close positive relationship between rainfall and primary productivity: even Sims & Singh's (1978) work in North America, which is used to show that RUEs are lower in grazed sites, affirms that rainfall and actual evapotranspiration explain a higher proportion of variability in grazed sites (in Le Houérou, 1984, p. 230, 234).

Kelly & Walker's (1976) study over two growing seasons also is drawn on by Illius and O'Connor to demonstrate that herbivory acts both to lower overall primary productivity and to increase its interannual variability. Interpretations made in this study have been critiqued thoroughly by Homewood & Rodgers (1987, pp. 113–114). They emphasize the following: that the 400% increase in yield in the wetter second year of the study in the site which was the most heavily grazed (cf. Illius & O'Connor, 1999, p. 805) demonstrates high resilience in response to rain, and is well within the range of values for the 'most productive' plots; that this site produced forage of the highest quality in terms of high crude protein and low crude fibre; and that the plot which was the most productive in the first year of the study was the least productive in the second year. Apart from embodying classic type 1 pseudoreplication errors, and as Homewood & Rodgers (1987, p. 114) conclude, the field experiment does not represent '...different points along a temporal succession of overgrazing' and, given varying substrate conditions and grazing regimes, differences between sites '...cannot be asserted as irreversible'.

Nevertheless, Illius & O'Connor (1999, p. 805) maintain that the regression analyses of Kelly and Walker's data demonstrate that '...the variability of primary production, in response to annual variation in rainfall, will be greater at the heavily used sites'. A cursory glance at Illius and O'Connor's Fig. 2, however, suggests that this is far from the case. Instead, the best-fit regression lines for the heavily utilized sites vs. the others run parallel to each other, indicating comparable levels of variability between years. Moreover, values for the heavily grazed sites in the wetter second year of the study are comparable to those under moderate grazing, and on par with one site classified as experiencing no grazing, although this site received rainfall in the region of more than 150 mm (Illius & O'Connor, 1999, p. 804). Given the lower levels of standing biomass in the previous low-rain year on the heavily utilized sites, this would seem to indicate high resilience in productivity of these sites. Finally, we again come up against the problem of distinguishing plant production or growth from measures of plant biomass at particular moments in time, i.e. of asserting that livestock are compromising productivity simply because they eat grass and thereby contribute to reduced biomass at the end of the growing season.

Alternatively, a number of short-term and long-term studies suggest that primary productivity is both maintained (or even increased) under livestock grazing, even in situations of heavy stocking in communal areas, and is related more to rainfall variability. For example, Parsons *et al.* (1997, p. 322) found the highest tuft densities (by an order of magnitude) and basal areas in sites under high levels of stocking in communal grazing lands. While these also had a higher dominance of the smallest tuft size classes, the *F*-ratio measuring the strength of the differences between land under communal and commercial livestock grazing and under commercial game ranching was relatively small ($F = 8.47$). Similarly, Venter *et al.* (1989, in Shackleton, 1993, p. 68), found no difference in the productivity of heavily stocked communally held grasslands of Kwazulu compared with the neighbouring Umfolozi Game Reserve, and a range of studies demonstrate that ungulate grazing enhances productivity of graze-tolerant species (e.g. McNaughton, 1979, 1983; Oba *et al.*, 2000a, and studies cited therein).

Long-term studies also suggest that rainfall is the strongest determinant of plant productivity. For example, analyses of archival landscape photographs for thirty-eight sites in north-west Namibia matched with recent repeat images, and of matched aerial photographs between 1958 and 1981 corresponding to six of these ground photo sites, appear to tell the following story: that an increase in woody vegetation has occurred throughout the region since the first half of this century; that this increase includes species used intensively for browse, firewood and building material, and is independent of degree and type of land-use and of land tenure; and that, when analysed in conjunction with available rainfall data, this change in woody vegetation cover during the last 100 years is attributable to long-term climatic factors, primarily a gradual and sustained increase in rainfall averages between 1930 and 1980 (Rohde, 1997a,b, pp. 307–331, 341–375). With regard to herbaceous vegetation, they also illustrate '...that dry periods in the past resulted in denuded landscapes' without apparently hampering the potential for herbaceous productivity in years with above-average rainfall (Rohde, 1997b, p. 309).

Similarly, Nicholson *et al.* (1998) have tried to distinguish anthropogenic from climatic factors in determining productivity for the Sahel, a region which is now synonymous with images of drought, destitution and 'desertification'. As Nicholson *et al.* (1998, p. 818) state, the problem here has been that '...while desertification itself was defined as anthropogenic, the evidence used to assess it could equally have been a product of climatic variability'. So, for example, Lamprey's pioneering work in the 1970s on the advance of the Sahara (cf. Lamprey, 1983) also corresponded with a nearly 50% decline in rainfall in the Sahel (Nicholson *et al.*, 1998, p. 818). Nicholson *et al.* (1998, p. 827), who rigorously analyse the relationship since 1980 between satellite derived normalized difference vegetation index (NDVI) data and rainfall, instead suggest that '...the extent of the Sahara and the vegetation cover within the Sahelian zone fluctuate from year to year in accordance with interannual variability of rainfall. No progressive change in either the desert boundary or the vegetation cover in the Sahel is evident during the 1980–95 analysis period. Neither has there been a change in the "productivity" of the land, as assessed by the ratio of NDVI to rainfall'.

Secondary productivity

Illius & O'Connor (1999, p. 805) admit that '...there is little clear evidence showing whether or not system output declines over the long term as a result of repeated impacts of animals during successive droughts, or under continually high defoliation rates'. As Shackleton (1993, p. 65) notes for southern Africa, '...despite the excessive stocking rates (usually 200–400% of the recommended) most of the communal areas continue to support large numbers of cattle without there having been any catastrophic decline in total numbers over the last century'. We agree with Illius and O'Connor that the lack of conclusive evidence relating high stocking rates with secondary productivity declines over long time frames is surprising, given the mantra-like repetitions of degradation and immanent environmental collapse that have accompanied much environmental research and policy rhetoric in drylands during the twentieth century. We would like to suggest, however, that the apparent lack of decline in system output is a 'real' phenomenon, confirming high resilience, low determinism in terms of biotic-biotic interactions, and high abiotically sourced stochasticity. In other words, conferring dynamics based on non-equilibrium principles.

Illius & O'Connor (1999, p. 805) draw on a single study of fence-line contrasts in a commercial ranching area of northern Australia which shows that animal performance declines with increases in stocking rates in sites which have experienced changes in vegetation, attributed to more intensive grazing regimes (Ash *et al.*, 1995). For the purposes of debate, it should be pointed out that these findings are not unproblematic for several reasons. First, the two sites studied had average annual rainfalls of 535 and 950 mm, respectively. In other words, they both fall very much towards the wetter end of our arid–semi-arid continuum where, in terms of interannual variability, dynamics would be at the less variable and more predictable end of that continuum. Secondly, and as acknowledged by Ash *et al.* (1995) themselves, possible major errors in interpretation are introduced by assuming that land condition, i.e. their vegetation States I and II, is a direct artefact of different grazing regimes over the last decade. In other words, '...the inherently large variability within the landscapes in question' means that '[s]tatistically significant differences between vegetation states ... may represent inherent "site" differences rather than differences due to land condition *per se*' (Ash *et al.*, 1995, p. 81). Thirdly, the analysis is taken from a commercial management system with fundamentally different objectives to those employed by many pastoralists (cf. Sandford, 1983). In single-product commercial production systems the optimum stocking rate is geared towards maximizing beef production per hectare where it makes sense to have fewer but fatter animals. In many African pastoralist systems, where production is orientated towards both daily subsistence and wealth accumulation, the economic optimum might mean having higher stocking densities thus coming closer to ecological 'carrying capacity' (Barrett, 1992). Barrett's study of the economic role of cattle in communal farming systems in Zimbabwe does not analyse the effects of grazing on vegetation, but it does indicate that livestock production under these circumstances is significantly higher per hectare than commercial systems when taking the value of draught, milk, manure, local consumption as well as sales into account. It is likely that this situation has persisted for at least several decades, if not longer. Related to this, Ash *et al.*'s study does not represent the pastoralist management practices of African communal rangelands where mobility is employed as a means of both exploiting the differential opportunities offered by patchy dryland environments and to spreading risks. In other words, a study of one management 'system' is being used here to extrapolate perhaps inappropriate conclusions for very different production systems characterized by different management practices and objectives. Finally, it is worth pointing out a

summary by Ash *et al.* (1995, p. 89) of the implications of their material: that ‘...it is clear from our study, where different stocking rate–animal gain relationships were derived for the contrasting land condition treatments, that the two-dimensional nature of stocking rate models are not appropriate in dynamic and non-equilibrium pasture systems where changes in vegetation composition, quality and productivity can occur over a relatively short time’. In particular they acknowledge the significance of forage *quality* as well as quantity, of which the former might actually increase and maintain high levels of animal performance, even as conventional understanding maintains that pasture is degraded.

It is surprising, perhaps, that Dean and Macdonald’s (1994) study of changes in stocking rates in the Karoo, South Africa, has been cited in this context given the problems already identified with their methodology and conclusions (Dean *et al.*, 1995; Hoffman *et al.*, 1995, 1999). As Illius and O’Connor state, this study in itself provides insufficient evidence for Karoo degradation because it fails to take account of the importance of state interventions, economic factors and conservation policies. The Karoo, unlike many pastoral areas in Africa, predominantly is privately owned farm land with limited possibilities for migration and opportunistic management at a landscape level. Management practices here increasingly have become modelled on equilibrium concepts promoting conservative stocking rates and orientated towards single-product commercial production practices. In contrast, where communal livestock production systems have remained, and although these have been restricted to small ‘reserve’ areas, high stocking levels have been maintained and even increased during this century.

Thus, according to Dean & Macdonald (1994, p. 295), stocking rates in Namaqualand fell from 4.27 LSU (livestock units) km⁻² between 1911 and 1931 to 1.41 LSU km⁻² between 1971 and 1981. However, livestock data from the communal rangeland in Namaqualand (Leliefontein) show just the opposite: here stocking rates have risen steadily from *c.* 2.3 LSU km⁻² in 1890 to 3.8 LSU km⁻² between 1972 and 1987 (Rohde *et al.*, 1999). Fence-line studies between Leliefontein and adjacent commercial farms using evidence derived from repeat aerial photography (1958–97) and local oral histories indicate that both sides of this divide were even more ‘degraded’ (less cover, fewer palatable species) in the recent past than at present. During the last 40 years, conservative stocking and dispersed water points on the commercial farm have resulted in a massive increase of palatable perennials, implying rapid resilience of these species. Stocking rates in the communal area on the other hand have consistently remained several times higher than those on neighbouring commercial farms (R. F. Rohde, unpublished data), implying no loss of system output in terms of secondary productivity.

In Namibia’s commercial farming sector cattle numbers also have declined since 1960, following increases from 1914 to 1960 (Lange *et al.*, 1998, p. 555). Despite this, the number of cattle marketed has gone up by nearly 7% and the tonnage of beef by 2% (Lange *et al.*, 1998, p. 567). Apart from raising questions about extrapolating from declining numbers of animals that degradation has occurred, this clearly indicates that numbers of animals, as used by Dean and MacDonald, are only one measure of secondary productivity, especially given the particular economic aims of the people managing the land. Lange *et al.* (1998, p. 567) conclude that ‘...the decline in cattle numbers between the late 1950s and 1994 has been largely compensated for by an increase in herd productivity ... The doubling of turnover mirrors almost precisely the halving of herd size’. Recent research into the dynamics of livestock fluctuations in western Namibia (Rohde, 1997b) shows that even within small communal ‘reserve’ areas, livestock numbers have consistently tracked rainfall variations throughout the present century, without showing any signs of slower recovery rates or lowered ‘ceiling’ levels during peak stocking periods. Here, comparisons between adjacent commercial and communal rangelands show that resource ‘patchiness’ relates to ‘heavy’ communal stocking regimes without an overall reduction in primary productivity. Ward *et al.* (1998, pp. 358–360) also demonstrate for the communally managed reserve of Otjimbingwe in western Namibia that numbers of livestock have been maintained at consistently high, albeit fluctuating, levels relative to neighbouring commercial farms, for at least 150 years. Similarly, Homewood’s (1994) analysis of herds on the communally managed rangelands of the apparently severely degraded Baringo area of Kenya suggests that herd numbers and other demographic and production indices were maintained at levels consistent with, or higher than, herds managed under similar conditions elsewhere, despite repeated assertions of degradation for this area.

Soil erosion

As Illius & O’Connor (1999, p. 807) state, ‘...soil parameters are judged to be appropriate indicators of degradation because removal of soil and nutrients by erosion may lead to an irreversible decline in primary production and, in turn, of livestock production’. Soil parameters are considered important

because it is hard, if not impossible, to draw conclusions of degradation from primary and secondary productivity dynamics which might be reversible given different abiotic and management conditions. They assert that soil erosion caused by 'overgrazing' causes further declines in productivity through changes in both soil quantity and quality, i.e. through absolute soil loss and through the loss of specific nutrients. Again, we wish to present alternative analyses and interpretations that raise questions concerning the deterministic negative relationship between vegetation removal and soil loss, the postulated loss of nutrients under heavy grazing, and the validity of extrapolating to landscapes the results of erosion studies based on point or plot samples.

It is considered axiomatic that reduced vegetation cover leads to erosion of topsoils. Many studies indeed suggest this to be the case, although Stocking (1996, pp. 145–147) draws attention to '...the rather ambivalent effects of vegetation' in preventing movement of soil. Leaving this to one side, the next step in the argument is that it is livestock that are causing reduced vegetation cover, and that this is particularly true during droughts, thus '...leading to rates of soil loss much greater than under "normal" vegetation cover' (Illius & O'Connor, 1999, p. 807). It seems we are back to the chicken and egg scenario mentioned earlier. From a non-equilibrium perspective the linkages, to varying extents, are reversed. Thus, drought is a time of no or little rainfall; herbaceous productivity is linked extremely closely to moisture availability, particularly in environments at the more arid end of the spectrum; therefore, at times of drought there is little herbaceous forage for animals to graze; and therefore, it is illogical that droughts are the time when vegetation cover is '...seriously depleted by grazing' thereby increasing the susceptibility of soils to agents of erosion (water and wind). Elsewhere, Tapson (1991, *in* Shackleton, 1993, p. 74) maintains that there is '...a marked disproportional relationship between stocking rate and accelerated erosion'. The significant thing about the dramatic movements of soil seen in arid environments during rainfall events is that, by definition, the rain season begins after long periods of minimal primary productivity and/or vegetation cover as a result of dry seasons and drought periods. When rain falls it often is in torrential rainfall events and, in arid areas, onto soils with no herbaceous cover at all. One would expect dramatic movements of soil under these circumstances, and it is precisely this which lends many arid environments their rugged and incised landscapes. But the lack of herbaceous cover at the onset of the rain season is largely due to lack of soil moisture, *not* livestock grazing.

Conversely, situations where soil erosion can be linked explicitly to livestock usually are when livestock are concentrated spatially to an extreme extent, i.e. by their water, and not their forage, requirements or when 'kraaled'. In these cases it seems to be their *direct* impact on soil factors through trampling, and *not* their consumption of vegetation, that creates conditions whereby localized erosion effects are observed. Perkins & Thomas (1993, p. 190), for example, report increased wind erosion effects in close proximity to boreholes in the eastern Kalahari, Botswana ($400\text{--}500\text{ mm a}^{-1}$). Away from this immediate 'sacrificial zone' around boreholes, evidence of water erosion was '...totally absent, even during intense convectional storms, suggesting that runoff is entirely negated by the low relief and high infiltration capacities of the Kalahari sand' (Perkins & Thomas, 1993, p. 190). As suggested earlier, the theoretical implications of these types of biotic–abiotic impacts are significant because they stem not from density-dependent relationships between herbivores and vegetation but from the focusing effects of water (primarily) which tends to be distributed at discrete points in the landscape. Increasing the number of such 'points', e.g. by drilling boreholes, inevitably will increase the likelihood of such localized biotic–abiotic changes.

Secondly, Illius & O'Connor (1999, p. 807) attribute losses in soil nutrients directly to livestock grazing. Their evidence is drawn from two studies of the same location at two different points in time which suggested that carbon and nitrogen were significantly lower in the later study on heavily grazed areas (under communal tenure) compared with lightly grazed areas (Kelly & Walker, 1976; De Jager, 1988). We would like to draw attention to a number of studies that indicate that levels of soil nutrients are maintained or even increased under the heavy grazing regimes which seem to be characteristic of African pastoralist production systems. Ward *et al.* (1998, p. 369), for example, clearly demonstrate that nitrogen, phosphorus and bioassay measures were not significantly different for samples taken from communal and neighbouring commercial land in western Namibia (165 mm a^{-1}), and that levels of carbon were greater for the communally managed sites. They concluded that '[e]ven though the communal area has twenty times more stock than the commercial farms, soil quality is similar' (Ward *et al.*, 1998, p. 369; also see Perkins & Thomas, 1993, p. 188). Rethman *et al.* (1971, p. 57) even suggest that an increase in soil fertility due to manuring by grazing may enhance the nutritional value of herbaceous species. Elsewhere (northern Australia), Ash *et al.* (1995) recorded higher nitrogen levels for soils in sites that apparently had experienced greater vegetation degradation, in analyses of both herbage and of faecal material from cattle.

Finally, and as detailed by Stocking (1996), major methodological problems exist for two reasons in the extrapolation of rates of erosion from point or plot samples to larger spatial units of analyses. First, the typical use of bounded field soil-loss plots to measure erosion results in high levels of experimental inference ‘...whereby measurement itself intrudes on the process being measured’ (Stocking, 1996, p. 149). Secondly, discontinuous scale effects usually lead to gross overestimates of soil loss when extrapolating the findings from field-plots upwards to landscape levels or wider spatial units (Stocking, 1992, p. 4, 1996, pp. 149–150).

DISCUSSION: NON-EQUILIBRIUM THEORY AND SIGNIFICANCE FOR POLICY

Non-equilibrium terminology has become a form of shorthand across a variety of disciplines. Illius and O’Connor are right to point out that technical terms used in community and population ecology have become loosely applied in contexts that deserve a more critical understanding of the theoretical origins of this vocabulary. One of the problems is that natural and social scientists tend to view people–environment interactions in different ways. For instance, the term ‘non-equilibrium dynamics’ might be used in a social science context to describe patterns of behaviour and decision making that arise in contexts of high environmental uncertainty: as in African pastoralist societies where opportunism, flexible movement and responsive livelihood adaptation are central components of human–environmental interactions. In contrast, ecologists and environmentalists tend to interpret rangelands from the perspective that natural systems can be understood as if they were somehow separate from human intervention and perception. Ecologically, non-equilibrium concepts are relevant to dryland grazing systems in part because they provide insight into the evolution and practice of African pastoralism and its relative significance in shaping these environments over centuries and millennia. Many of the dryland landscapes now considered ‘desirable’ from an ecological perspective are inextricable from this historical interaction.

Our response attempts to bridge some of the theoretical divides which increasingly have become polarized in debates about appropriate rangeland management. We agree that the reification of the equilibrium/non-equilibrium dichotomy becomes a blunt instrument when applied to the multiplicity of ecological, social and economic conditions confronting African pastoralists. We also agree that uncritical use of non-equilibrium concepts as the ‘new orthodoxy’ in pastoralist and rangeland ecology may dilute its theoretical underpinnings. In promoting a generalized theory of environmental dynamics, therefore, there is a danger that proponents of the new paradigm will fall into the same trap as their predecessors, i.e. losing sight of the specific outcomes of herbivore/vegetation interactions in particular ecological and socio-political settings. It seems equally unhelpful, however, to see all dryland grazing systems through the prism of density-dependent dynamics. Of course livestock consume vegetation. What is in question is the nature and mechanism of context-specific consumer/resource relations, and the consequent validity of concepts derived from equilibrium models which support potentially inappropriate conservation or development initiatives based on fixed carrying capacities and clearly demarcated land tenure units. We reiterate here that if particular situations are viewed spatially and temporally as being ‘...arrayed along a gradient of states ranging from non-equilibrium to equilibrium’ (Wiens, 1984, p. 451) then it becomes possible to fine-tune our understanding of consumer/resource dynamics from a theoretical perspective incorporating both equilibrium and non-equilibrium behaviours.

Illius & O’Connor (1999, p. 799) are right when they state that ‘...the term non-equilibrium means many things to many people’. At the start of their paper (p. 799) they pose three questions which attempt to clarify its meaning. Although they do not answer them directly we attempt to do so here, lodging our responses primarily in the context of African pastoralist–rangeland dynamics.

1. ‘Are non-equilibrium systems fundamentally different from systems where density-dependent mechanisms tend towards equilibrium?’

Our answer must be a qualified ‘yes and no’. Non-equilibrium environments have been defined as ‘...characterized by a general “de-coupling” of close biotic interactions’ where ‘species should respond to environmental variations largely independently of one another’ (Wiens, 1984, p. 451). Strong abiotic agents (such as climate and substrate) and stochastic environmental events (such as drought) continually ‘disturb’ populations in a way that is independent of density-dependent factors. Equilibrium is defined primarily by stability in community components (and by rapid resilience – the ability of the system to return to a previous state following perturbation) (Wiens, 1984, p. 439). Such systems are characterized by biotic coupling, density dependence, saturation and tight patterns of community organization. When applied to pastoralist economies the two extremes are qualitatively distinct: opportunistic decision making is required to make the most of unpredictable

non-equilibrium environments whereas conservative optimization is appropriate for predictable equilibrium contexts. In these senses (ecological and economic) there are fundamental differences between the two 'systems'.

But this is only part of the story. Following Wiens (1984, 1989) we maintain that non-equilibrium and equilibrium dynamics are not mutually exclusive, either/or states of communities but opposite poles of a spectrum of system states: conceptually, ecosystems exist across a gradient between these extremes. Furthermore, the non-equilibrium or equilibrium 'state' of an environment is defined by the scale of observation. Over the long term, *all* phenomena exhibit non-equilibrium dynamics as a result of unpredictable events that effectively decouple system attributes and instigate system change. Of broader theoretical significance, therefore, predictable equilibrium dynamics are an explanatory ideal, useful only at clearly defined scales and for problems far removed from broader temporal contexts. In this sense, Illius and O'Connor are asking an inappropriate question. The dynamics of *all* living systems intrinsically are non-equilibrium (e.g. Jantsch, 1980; Cilliers, 1998), although predictable and tightly coupled (i.e. equilibrium) interactions and dynamics might be exhibited at certain scales of observation. We feel that the question should be reframed as 'do semi-arid and arid rangelands exhibit equilibrium (i.e. density-dependent) dynamics over the temporal and spatial scales of relevance to human management systems?' We maintain that they do not, and that the evidence put forward by Illius and O'Connor to suggest that they do may hinder support for appropriate management practice and policy in these unpredictably varying environments.

2. 'Are there special properties in non-equilibrium systems which give rise to important community phenomena?'

Yes, if we are talking about environmental dynamics over the time-scales of importance to human management decisions. Where populations are subject to periodic 'environmental crunches' (e.g. droughts) and ensuing resource limitation, interspersed with periods when resources are superabundant and communities are under-saturated, a tracking inertia exists leading to 'density-vague' dynamics such that 'the lack of a close fit between populations and resources produces apparent non-equilibrium in community patterns' (Wiens, 1989, p. 145).

For example, Turner's (1990) study of long-term vegetation change in a fully protected desert site clearly illustrates the unpredictable (non-successional) change in vegetation communities that characterize arid environments where the mortality and recruitment of plants depends on particular sequences of events, combined with autecological factors. In unprotected sites, selective grazing by herbivores influences this equation. Succession theory, based on equilibrium dynamics and notions of a climax vegetation state, is not realistically explanatory or predictive (Hoffman & Cowling, 1990), because defoliation and dispersal caused by herbivores is governed by large fluctuations in livestock numbers due to recurrent and unpredictable drought. The strategies that pastoralists employ to survive in such event-driven environments are quite different from those of livestock farmers in temperate environments, where density-dependent mechanisms are more predictive and stable. The choice of whether to pursue a conservative stocking rate or to employ an opportunistic strategy based on mobility is essentially determined by climate. Therefore, specific plant community phenomena are observable because of the special abiotic conditions exhibited by drylands, taking into consideration the implications of scale noted previously.

3. 'Do communities in non-equilibrium environments form a continuum according to the strength and frequency of perturbation, but without any characteristically different form of consumer-resource relations (from equilibrium environments)?'

No, if we take this to refer to communities at the non-equilibrium end of a continuum across a gradient that spans the theoretical extremes of equilibrium and non-equilibrium as defined above. In wetter environments at the equilibrium end of the gradient, consumer-resource relations are a primary factor influencing vegetation change in the short to medium terms. At the non-equilibrium end of the gradient, however, consumer-resource relations *are* qualitatively different. Thus, in times of abundance, the range is typically under-utilized by livestock; during multiyear droughts, there is little or no primary productivity upon which herbivory might have a significant impact; and pastoralists effectively de-stock by moving their surviving livestock to areas of more abundant resources. Consumer-resource relations are fundamentally different insofar as lack of rain, rather than density dependence, is the primary cause of reduced primary productivity.

We feel that Illius and O'Connor have polarized the debate around definitions of the terms 'equilibrium' and 'non-equilibrium' as if they were objective, identifiable states in nature rather than conceptual ideals that help us to understand ecological processes. A fuller reference to Wiens' (1989, p.

261) definition of these concepts might have led to a more constructive critique: '[r]esource conditions vary in nature, and individuals, populations, guilds and communities also vary in the degree to which their status is closely linked to those resource states. The result is that the patterns and processes of assemblages may approach either end of the equilibrium–non-equilibrium spectrum at some times and be some distance away at others. ... Surely it is more important to attempt to understand the nature of these dynamics than to pigeonhole them into “equilibrium” or “non-equilibrium” categories’.

Illius and O'Connor state that they find no convincing 'mechanistic arguments' that semi-arid systems function in a fundamentally different way to density-dependent equilibrium systems. Their claim that non-equilibrium grazing systems exhibit strong consumer–resource coupling and consequent degradation is based on 'evidence' of 'density-dependent birth and mortality [...], directional change in vegetation composition and increased soil erosion caused by grazing, and lower animal production that ensues in some cases, at least' (Illius & O'Connor, 1999, p. 809). They find that 'animal numbers are regulated in a density-dependent manner by the limited forage available in key resource areas, especially during drought', and where 'spatial and temporal variation in primary production serves to localize and intensify herbivore impacts' (p. 809). Further, '[t]his model asserts that strong equilibrium forces exist over a limited part of the system, with the animal population being virtually uncoupled from resources elsewhere in the system' (p. 798).

We hope to have indicated that their interpretation of much of the material presented to support this conclusion is open to reinterpretation; that some of their case study material does not compare like with like; that the evidence presented is highly selective; and that many of the cases that are central to their argument derive from semi-arid environments at the wetter end of the scale which, within the same scales of observation, are likely to exhibit somewhat different dynamics to environments at the drier end of the spectrum. Nevertheless, they successfully draw attention to the dynamics that obtain in semi-arid systems where key resources are crucial to system functioning, although we feel they have failed to substantiate the strong density-dependent function of key resource areas in these environments. They state that '...droughts may impose intense and localized defoliation on vegetation' resulting in 'altered species composition, reduced RUE, soil erosion and loss of productive potential' (Illius & O'Connor, 1999, p. 798). But these processes take place during drought even in the absence of herbivores. Livestock may exacerbate these effects in certain circumstances, primarily where they are concentrated by their water as opposed to their forage requirements, but this is not necessarily inevitable or irreversible. The lack of evidence pointing to long-term secondary productivity decline as a result of density-dependent factors supports this view. Moreover, while we welcome their analysis of key resource and wet season grazing relationships, we feel that this material does not justify extrapolating from a hypothetical model based on livestock impacts on environments at the wetter end of the scale, to a generalized theoretical position relevant to all semi-arid and arid environments.

Cases of degradation (as conventionally defined) related to overstocking may exist, but the ultimate causes of this often are due to political factors that confine livestock owners to inappropriately small areas of rangeland, or destroy traditional institutional arrangements for managing land and livestock (and, in some cases, by the long-term absence of livestock). A problem here may be that Illius and O'Connor seem to use the term 'communal rangeland' as a shorthand for situations where these latter conditions pertain, i.e. as in many of the crowded former 'homelands' of southern Africa, without exploring the validity of communal rangeland management and tenure practices under a variety of alternative political and historical circumstances. 'Communal rangeland' is not a standardized form of tenure, characterized by readily identifiable economic and social imperatives. Moreover, rangelands seldom are divided up into neat territorial packages owned by distinct groups or individuals, except in cases exemplified by the South Africa's labour reserve and 'homeland' entities. Instead, pastoral landscapes are likely to be used by different ownership groups of variable size and composition, with overlapping claims to territory derived from particular claims to different categories of resources within it (e.g. Birley, 1982; Berry, 1989; Barrow, 1990; Igoe & Brockington, 1998). The grazing landscape and the social processes governing its management generally comprise a complex pattern of tenure institutions, with some resources used and managed exclusively, while others are managed more loosely. Property regimes thus overlap in both time and space, and are governed by multiple institutions, operating at different scales and with different degrees of intensity (Mehta *et al.*, 2000; Sullivan & Homewood, in press). Focusing on livestock as the primary factor driving consumer–resource relations ignores the part played by people and manifest as complex land management and tenure practices. Viewing dryland environments from a human ecology perspective, it is evident that indigenous pastoral consumer–resource relations tend to be governed by flexible, often informal institutions, expressed in ongoing *ad hoc* arrangements that evolve as a practical means of capitalizing

on environmental uncertainty (Roe *et al.*, 1998). Non-equilibrium theory provides an apt lens through which to view the social response of pastoralists to uncertainty and risk, where pastoralist households rarely can be in phase with spatially and temporally variable resources and more or less 'opportunistic' and proactive strategies of resource use are necessary to secure livelihoods.

Illius and O'Connor conclude their paper by asking 'what really is the long-term future for communal rangelands?' They suggest that it may be that livestock farmers are 'paying for sustainability through the repeated destruction of their wealth', the only alternative being to create technical interventions that might fatally compromise the sustainability of local management practices (Illius & O'Connor, 1999, p. 810). We would suggest, however, that a more nuanced analysis of the situation confronting pastoralists in African dryland environments surely must confront the politics of inequality that in many instances correlate with observations of apparent environmental degradation today. For example, a non-equilibrium perspective on the understanding of drylands, in both their social and ecological dimensions, would find it unsurprising that degradation and poverty in these areas have been linked with a number of policies associated with the intention to create political stability and control, and to encourage the growth of formally defined economic measures – we are thinking here of, for example, the locating of socially and ecologically inappropriate political boundaries; the privatization of communal land; the promotion of agricultural intensification (frequently under internationally led Structural Adjustment Policies); the banning of fire-use and other methods of local environmental management; and shifts in market pressures and opportunities (e.g. Oba, 2000; Oba *et al.*, 2000a,c; Homewood *et al.*, 2001; Reenberg, 2001; Walker & Janssen, 2002).

We contend that non-equilibrium theory provides a powerful explanatory model of pastoral eco- and social-system dynamics that can indicate reasons for failures in these initiatives in terms of improving livelihoods. It cannot be discounted simply because density-dependent factors are perhaps discernible in wetter semi-arid environments with key resource areas, or where pastoralists have been confined to inappropriately small 'reserves' under repressive policies that have resulted in poverty and social fracture. Pastoral social adaptations tell us much about dryland ecosystems and provide important insights into the causes of environmental change as well as into processes of environmental persistence. Pastoral societies operate continually in a 'non-equilibrium' mode (Thébaud, 1998) where flexibility and mobility remain vital for optimal and efficient use of scattered and unpredictable resources. For both natural and social scientists the challenge is to refine and expand our knowledge of specific, historically contextualized, rangeland environments in the interests of social equity as well as environmental sustainability. Pastoralists cannot be put on hold until science provides a definitive solution to the multiplicity of complex environmental and socio-economic problems that confront them. Judging by past management and policy mistakes, we cannot afford to rely on over-simplistic models or to extrapolate local findings to inappropriate spatial and temporal scales. We thereby maintain that a conceptual approach embracing non-equilibrium phenomena, as well as being theoretically and empirically sound, will make possible a more people-orientated ecology: concerned with a greater sensitivity to the dynamic interplay between the social, political and natural processes that have both shaped Africa's drylands in the past, and that will guide their future prospects.

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