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Hydrology and Earth System Sciences Discussions

Interactive comment on "Eco-geomorphology and vegetation patterns in arid and semi-arid regions" *by* P. M. Saco et al.

P. M. Saco et al.

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General Response to Reviewers' Comments:

We would like to thank all the reviewers for their helpful comments and suggestions; they allowed us to improve the manuscript as well as gave us some ideas for future research.

Reviewers #1, #2, and #3 agree that the paper presents a novel model/research as most of the previous models have only focused on either vegetation or erosion dynamics, and not on the co-evolution of the vegetation, runoff and erosion patterns. The comments of reviewer #2 are particularly encouraging. Some of the reviewers (M. Boer but particularly D. Dunkerley) express concern about some of the underlying assumptions and detailed mechanisms included in the model. We have added in the revised paper (where appropriate) more discussion on the underlying assumptions and included extra references to support them. However, it is important to note that we are trying to capture the key processes that are **common** to banded patterns across different geographic regions. We agree with the reviewers that the model can be more complex. However, the question is, how much complexity we need to include in the model to capture the essential dynamics that leads to banded vegetation formation, runoff redistribution and observed microtopography. We emphasize again that we are trying to build the simplest model that captures this dynamics.

Below we address the specific questions of the each of the reviewers. For clarity, we have included the reviewer's questions/comments in bold italic letters, and our responses as plain text.

Response to comments by D. Dunkerley:

We thank the reviewer for providing these comments. Though we don't agree with all the reviewer's comments, we have used most of them to improve the manuscript, by either modifying the paper to clarify some concepts or providing more detailed explanations on simplifications made. Answers to each comment are provided below.

1) ***The authors claim that their model accounts for the dynamics of runoff runon that controls the evolution of vegetation patterns. On the contrary, there are suggestions that these patterns evolve in independent ways, and that the runoff-runon system is at least in part a consequence of the evolution of a vegetation pattern, not its direct cause.***

This statement has been slightly modified in the revised version (abstract, line 11). We have included different views for the genesis of vegetation patterns (with the appropriate references, page 6 lines 4-12). However, we also explain that in most studies water is perceived as the primary causal agent for band formation (Tongway and Ludwig, 2001 and references therein). We are still emphasizing in the paper that our model

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accounts for observed and well accepted mechanisms (provided in the literature) for the function of these types of ecosystems as runoff-runon systems. Note that we have a system with CO-EVOLVING patterns, where some of the mechanisms involved (i.e., the runon-runoff system) are BOTH cause and consequence of the vegetation pattern. We also note that by including these observed mechanisms the model gives rise to the banded pattern and observed stepped microtopotography (as self-organizing phenomena).

2) ***The authors claim (page 5) that banded vegetation is effective in limiting hillslope erosion. I know of no published evidence that would support this claim***.

As stated in Valentin et al., 1999 (who give an extensive review on the literature on observations on banded vegetation patterns) and reported in our paper (page 5, lines 13-14): "The bands favor soil conservation by acting as natural bench structures in which a gently sloping runoff zone leads downslope onto an interception zone". A similar observation has been reported in Bochet et al., 2000 (reference added in page 5). Additional support is given in several papers that analyze the effect of disturbance of the bands, for example Wu et al. (2000) describe how the disturbance of the banded pattern gives rise to connected water paths, reduced water availability for remaining bands, and increased erosion through the appearance of rills and gullies.

3) ***The authors claim (page 6) that banded vegetation patterns occur on relatively steep slopes. I am not aware of any field occurrences on steep slopes.***

In southeastern Spain, banded vegetation has been observed on relatively steep slopes (Puigdefabregas and Sanchez, 1996; Bergkamp et al., 1999). References added to the paper (page 6, line 12).

4) ***The authors claim (page 7) that vegetation patterns occur on time scales from several years to several decades. To my knowledge there are simply no field data that establish the truth of this statement.***

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Note that this statement refers to “evolution of vegetation patterns” that is “change in time” not “complete development or genesis of the banded pattern”. More specifically, the statement is made in the context of effects of climate change and human pressure (We changed the wording in page 7 line 14 to avoid confusion). Modeling results by Gao and Reynolds (2003) on shrub-grass transitions due to changes in precipitation patterns in the northern Chihuahuan Desert show that important changes in vegetation patterns occur over a few decades (they modeled 75 years and show significant changes in the patterns over 15-year periods). Field observations also report important changes in vegetation patterns over a few decades (Brown et al. (1997) reported a three-fold increase in the shrub density since 1970 at 3 sites in southern Arizona, coinciding with a recent shift in regional climate). We also added these references.

5) ***The authors assume (page 12) that lateral soil moisture fluxes are negligible; on the contrary, I would argue that they are significant and vitally important. What is the authors evidence?***

We performed further simulations that include the effect of soil moisture re-distribution by adding a simple term to the soil moisture equation following the approach of Rietkerk et al. (2002). The inclusion of lateral redistribution of soil moisture did not make a significant impact on the results presented in this paper. This statement has been added to the revised paper (Page 13, lines 12 to 16).

6) ***I can see no justification for the adoption of assumptions like a spatially uniform surface roughness (fixed at 0.05) or a spatially constant diffusion coefficient for splash. Rather, I would argue that splash is highly non-uniform spatially (because of its dependence on ponding depth, which is highly non-uniform spatially), and that this is critically important. And given that the components of a mosaic have highly contrasting surface properties, how can it be reasonable to argue for spatially constant surface roughness?***

The diffusion coefficient does not only account for rainsplash but also for the effect of

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soil creep that is not related to overland flow depth. There is mixed information in the literature on the effect of vegetation on the diffusion coefficient (discussion in Instanbulluoglu and Bras, 2005, and references therein). Note however that most observations and modeling studies tend to agree on the important role of vegetation in reducing diffusive processes. We decided to include uniform coefficients in the present analysis (we will analyze the impact of spatial variations in future research).

Regarding the surface roughness, we took it as spatially uniform to simplify the analysis. A sensitivity analysis on changes in the (uniform) value of surface roughness did not lead to any significant changes in vegetation and erosion-deposition patterns. The case of spatially varying manning coefficient will be analyzed in future research. We should note however, that as the surface roughness increases with increasing vegetation cover, we expect that its effect will be to reinforce depth differences between vegetated and bare patches, thus slightly reinforcing the effect of differential infiltration rates and modeled microtopography.

7) ***I would dispute the assumption (page 20) that there is no lateral competition for water. This claim could only conceivably apply were there always excess water present; however, we are addressing water-limited ecosystems, and lateral competition for water (via the root systems) is immensely strong in drylands, and is known to influence the spacing of individual plants.***

Indeed. Lateral competition for water via the root system has not been included in the model for simplicity. This has been has been clarified in the revised paper (page 20, second paragraph). Note however, that the heterogeneity within the patches (i.e., the dynamics of individual plants) is not resolved in this model.

8) ***In terms of model formulation, I also question the use of a framework of free flow equations for a system which in the field is dominated by ponding and backwater effects.***

Note that the effect on “ponding” is captured in the model by computation of a variable

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flow depth which, in the modeled results, reaches a maximum in the lower interband area (this coincides with published field observations of maximum ponding depths). Backwater effects are clearly not included in the model.

9) ***I would question the presumption (page 16 and elsewhere) that erodibility is greatest for bare soil and decreases with biomass. Often, the bare soils are extremely dense and are anchored by very stable microphytic crusts. Vegetated soils on the other hand are commonly overturned by burrowing organisms, and are left friable and vulnerable to erosion by both water and wind. Furthermore, it cannot be forgotten that other processes, such as dust accession, may be significant components of soil development in dryland environments.***

Observations published in the literature disagree with the reviewer's comment. Though soil crusting reduces erodibility of bare soils as compared to bare non crusted soils or soils with disturbed crusts, there is abundant evidence in the literature that vegetation enhances aggregate stability, thereby decreasing erodibility (even for the case of areas with crusted soils) as described below. For example, Puigdefábregas (2005) reports: "Compared to bare ground, the soil beneath vegetated patches receives much larger organic matter inputs in the form of plant debris. . . . This enhanced biological activity and its products contribute to building stable soil aggregates (Imeson and Vis, 1982; Imeson and Verstraten, 1989), which dramatically influence soil structure and its implications for soil hydrology, erodibility and fertility (Cammeraat and Imeson, 1998; Cerda, 1998; Puigdefabregas et al., 1999; Barthes and Roose, 2002) that lead to increased water storage capacity, saturated hydraulic conductivity and to decreased soil erodibility." Similar evidence has been also reported for banded patterns in Australia (Greene, 1992). We believe that adequate references are already provided in the paper (page 16, last paragraph).

With respect to burrowing animals, Cammerat and Imeson (1998) reported that burrowing by earthworms produce very stable excremental pellets that improve soil aggregation, and found a clear increase in aggregate stability with increasing cover of the

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soil surface, from a bare crusted soil surface to full vegetated cover.

10) ***I would also question the notional time-frame embodied in the model. I am not sure what kinds of plant the authors had in mind in their modelling (grasses? shrubs? trees?), but I know of few apart from grasses that could in any sense emerge and develop in a few hundred days, and much less, in such a short time, modify soil hydraulic properties in the ways that the authors assume. Pedogenic processes take from centuries to millennia, and are especially slow in drylands owing to the rarity of leaching, the small fluxes or organic detritus, etc.***

As mentioned in the paper, to model the vegetation dynamics, we adopted similar parameters to those reported by Rietkerk et al. (2002) and HilleRisLambers et al. (2001). These parameters correspond to grasslands (clarified in the revised paper). These previous vegetation models are predecessors of the one we developed, and have been extensively cited in the literature. For this set of parameters, we found that the complete development of the vegetation bands takes 15 years (the intermediate stage in the pattern formation shown in Fig. 3 corresponds to approximately 7 years (2560 days, we found a typo in the caption and text that has been corrected in the revised manuscript). We are currently investigating the use of different parameters to obtain the response of different plant functional types for observed and/or modeling studies in arid Australia (i.e, Sparrow et al., 1997). We have presented some of these results with slower vegetation dynamics in Saco and Willgoose (2006).

It is important to note that we did not include in this model long term pedogenic processes that take centuries to millennia. We model the modification of soil hydraulic properties that affect infiltration due to the presence of vegetation and bare soil crusting. Note that Eldridge et al. (2000) report that simulation experiments from Zaady et al., unpublished data) involving the destruction of shrub patches at Sayeret Shaked demonstrate that cyanobacterial crusts can develop and cover the mounds in 5–10 years. Moreover, they report a short-term decrease in infiltration rates induced by the death of perennial shrubs.

11) ***The authors claim some value in their model showing migrating bands, but this only occurs when downslope seed transport is restricted arbitrarily. This authors say very little about this, and offer no biological explanation, merely presenting their result in passing. The imposed condition seems to have little correspondence with any real world situation, and the authors ought to comment on this.***

On the contrary, we claim that the greatest value of our model is to reproduce the results for **stationary bands**, as these have not been obtained before. We mention in the paper that the model is also able to reproduce migrating bands (as previous models) for c_2=0 or small. Note also that we have included a more thorough explanation to answer point 6 of reviewer #1 (page 22, lines 4 to 14)

12) ***The literature review is very selective, and many of the models addressing the evolution of banded vegetation are not cited at all. (Examples include the models of Jean Thiery, and of Olivier Lejeune, Lefever, and others).***

These references and others have been included in the revised paper.

13) ***The authors appear to have a changeable view of soil properties. On the one hand, they accept that vegetation groves have high infiltration rates (this is generally true, though the soils are not spatially uniform, and infiltration rates are only higher than those of intergroves when averaged over large spatial scales). But this does not stop them arguing (e.g. see page 11) that there would be significant ponding depths within groves, inundating areas of higher hydraulic conductivity. In the field, water often trickles (as laminar flow) from intergroves into groves. There is commonly no ponding in the groves on the scale that is seen in intergroves, and thus no inundation of higher areas (which are often lacking anyway). The notion of inundation of higher areas leading to greater apparent infiltration rates comes from very particular environments where plants are associated with a hummock-and-swale topography, and this is rarely present***

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in banded ecosystems, in my experience. I think that the authors are relying on a line of argument here that is scarcely justifiable, and doing so without any basis of field evidence.

There is no “changeable view of soil properties” in the paper. The “flow depth” estimated by the model accounts for BOTH the effect of variable infiltration (higher in the groves) and flow accumulation. The higher flow depths are indeed found in the lower interband areas, while within the groves the flow depths are much smaller (low ponding) due to the much higher infiltration rates found in the groves. By inundation of higher areas we refer to the total or partial inundation of the small mounds were vegetation is located in arid ecosystems (including banded systems) as reported by the reviewer and others in many field studies (i.e., Eldridge and Rosentreter, 2004; Bochet et al., 2000; Parsons et al., 1992; Gile et al., 1998; Dunkerley, 1997; and Dunkerley and Brown, 1995; and Dunkerley, 2000, and references therein). We have modified the text to specifically mention these mounds and added more references to support our assumption (last paragraph of page 11 continuing in page 12).

14) The spelling errors identified by the reviewer were corrected in the revised version of the paper.

15) ***I would delete much of page 19, for example. This is presented as model results, but in reality is simply an assertion of the authors’ views on how banded vegetation systems might operate.***

We disagree with the reviewer. These paragraphs explain how the model works and gives rise to the simulated patterns.

16) ***I cannot accept the claim made both in the Abstract and in the Conclusions to the effect that the nature of the model outputs confirms that the ‘essential processes driving these ecosystems’ have been correctly captured in the model. The presumptions listed above are by themselves sufficient justification to question the operation of the model. I would suggest that the authors should re-state***

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their claim, noting only that the model results appear consistent in some ways with field data.

This statement has been changed in the abstract and conclusions. However, we still emphasize the ability of model to generate multiple patterns (vegetation, erosion-deposition, runoff-runon) that are in agreement with field data. See also response to point 1.

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