

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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Response to comment by Anonymous referee #1:

1) We thank the reviewer for his positive comments. In particular because he explicitly recognizes in his review the value and novelty of the model we developed. We agree with the reviewer, that we still have not exploited the model in its full potential. We expect to have several follow up papers were we target different research questions, which go beyond the aim of this paper that focuses in banded systems widely found in Australia and other arid regions in the world.

2) ***The introduction leaves the impression that the only relevant process for vegetation pattern formation is the runoff-runoff process. In general these patterns***

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are recognized to emerge from both competition and facilitation feedbacks (such as increased infiltration under vegetation patches, but also through competition for the limiting resource through the root system). Both mechanisms alone have been found to be able to generate patterns in models.

We have included, in the revised paper, other views (and models) on pattern formation (pages 6 and 7). See also response to points 1 and 12 in the response to Dunkerley's comments.

3) Section 1.2 mentions field observations that incised rills and gullies lead to the disappearance of banded vegetation. The model presented would seem to be an ideal tool to explore this issue, but it is not further discussed in the paper.

Indeed. As explained in the introduction, in this paper we focus our attention in banded pattern where runoff occurs mostly as sheet flow. As soon as flow concentrates in gullies, the banded pattern tends to disappear giving rise to different vegetation patterns, these results have been reported in Saco and Willgoose (2006) and Saco and Willgoose (in preparation).

4) At the beginning of section 2: the vegetation model presented is more than only 'partially' based Rietkerk et al 2002. It is basically an extension of this model apart from the different form of the overland flow equation. The reasons for modifications could be discussed briefly (such as the introduction of a flux term in the overland flow equation, which is needed in order to have a flow to use for seed transport and erosion).

The word “partially” has been removed. Note however, that the overland flow dynamics included in this model is substantially different from that in the previous model. We have further clarified that these modifications allow us to model seed and sediment redistribution (page 9, first paragraph).

5) I do not expect any significant difference, but how sensitive are the results to

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the fact that the lateral soil moisture diffusion term was neglected?

Please refer to point 5 on the response to Dunkerley's comments.

6) Vegetation bands are found to be stable if an appropriate value for the parameter regulating seed transport, c_2 , is chosen. This is an interesting and novel result. But how fine-tuned is the particular choice presented? Would even higher values lead to downslope migrating bands? In general the particular choices of c_1 and c_2 should be justified.

The parameters c_1 and c_2 in Table 1 were selected to produce a stationary pattern. Migrating bands can be reproduced by taking $c_2 = 0$, in which case there is no redistribution of seeds by overland flow, or by taking c_2 small enough so that overland flow seed dispersal becomes negligible compared to seed dispersal by isotropic mechanisms. If, on the other hand, c_2 is large (and c_1 not limiting) overland flow seed dispersal becomes dominant (as compared to the isotropic seed dispersal) and the banded pattern disappears. That is, the vegetation moves downhill due to enhanced preferential colonization of the downslope portions of the grove and, as there is no source of seeds in the most uphill portion of the landscape the vegetation pattern is slowly "flushed away" from the hillslope. Though this is an interesting result, it does not lead to the banded landscapes that are the focus of this paper. This explanation has been added to the revised paper (page 22).

7) The results presented in section 5.3 show the organization of the hillslope profile into a series of steps, similar to profiles observed in the field. Is the profile presented at $t=500$ an asymptotic, almost stationary solution? (n.b. There is an obvious loss of elevation over time due to the absence of a tectonic uplift term and other source terms such as dust deposition, but if this is neglected are the steps stationary?) If so this should be said. If, as I suspect, it is not, why was this particular time chosen for the comparison, and what is the evolution of the hillslope on longer times? At least some discussion should be devoted to this

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issue.

The observation of the reviewer is correct; the profile shown corresponds to a “declining relief” profile. That is, unlike the dynamic equilibrium profile that occurs for the case of a hillslope undergoing tectonic uplift, the simulated profile shown in the paper undergoes a continuing loss of elevation with time and the stepped microtopography becomes more pronounced with time. This issue has been clarified in the revised paper (first line of page 23).

8) *The coupling of vegetation bands with soil erosion does not seem to be explored to its full potential: The authors report that migrating bands lead to flat topography. This is an interesting issue, is there a critical value of c_2 over which the steps appear? There is a clear separation between the vegetation timescales and the erosive timescales using the parameters considered by the authors: if these were closer, or if different values of c_2 were chosen, could coupled migrating modes appear?*

The stepped microtopography develops for stationary bands. We haven't found coupled migrating modes for the range of (erosion) parameters explored. Note that, as stated in the paper, we selected these parameters following values recommended in the literature.

9) The typos and remarks have been corrected.

10) *There is a strange mix of units in the paper, with both mm and m being used at the same time. Using only one of them would be advisable (maybe choosing always SI units for mass and length).*

We chose these units for consistency with the previous work of Rietkerk et al. (2002), note that (as noted in point 4) we are following their approach for the vegetation model. We also found that the use of mm for flow depths and m for distances is convenient given the differences in magnitude between these variables.

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Response to comments by reviewer # 2 (David Tongway):

We thank this reviewer for his positive and encouraging comments. We are currently analyzing the response of the vegetation patterns to climatic variability (partially reported in Saco and Willgoose, 2006). As the reviewer suggests, future work will look in more detail at band movement and the results will be reported in follow up papers. We have added the missing reference (Noy-Meir, I. 1981).

Response to comments by M. Boer (reviewer # 3):

1) ***When reporting the on the approach and results of simulation studies it is crucial to discuss the choices that have been made while building and running the model as detailed as possible.***

We have included further discussion on model assumptions and parameter choices where necessary (See points 1, 5, 6, 7,10, 11 and 13 in the answers to Dunkerley's comments, points 6 and 7 of reviewer #1, and the responses that follow below).

2) ***... the reasons for initialising the model (page 2574) with this particular vegetation pattern consisting of randomly placing "biomass peaks" in 1% of the grid cells? What processes could be responsible for generating such a pattern? What effects could this particular initial pattern have on the modelling results?***

The random initial pattern is used to avoid initialization with any predefined deterministic pattern.

Why combine this random vegetation pattern with a purely planar slope and not with one that has some noise added to the general gradient? What would be the impact of such surface irregularities on the vegetation patterns that evolve?

Initial surface irregularities lead to flow concentration which precludes the formation of the banded pattern. This is discussed in Saco and Willgoose (2006) and is mentioned in page 26 of the revised manuscript).

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Another modelling choice that has not been discussed in detail concerns the climate or weather data that drive the coupled models. Was the rainfall rate assumed to be constant or to vary in any particular way? To what extent has this choice of weather input affected the resulting patterns of vegetation and microtopography?

The rainfall input considered in these simulations is constant (as stated in page 2574 of the original manuscript). The effect of rainfall variability is not analyzed in this paper. Recent results on the effect of rainfall variability on banded systems have been reported in Saco and Willgoose (2006).

3)the positive effect of vegetation on infiltration and other soil hydrological properties may remain for considerable time after the vegetation cover has declined. I suspect that omitting this hysteresis effect could have potentially important implications for the resulting pattern of vegetation and surface topography. For example, if erosion rates on nearly bare soil surfaces are relatively high compared with the rate at which local soil hydrological properties improve with increasing vegetation density, the vegetation cover might never develop from the nearly bare initial condition to a banded form that retains all water on the slope

The effect of biomass density on infiltration was modelled using a simple expression already available in the literature. We agree with the reviewer that the dynamics can be more complex. However, the question is, how much complexity we need to include into the model to capture the essential dynamics of band formation and feedback effects. It is also very important to note that infiltration rates have been “observed” to change fast and dramatically with biomass density in field experiments (references in page 2567, and point 19 in the response to Dunkerley’s comments). We have included at the end of the revised paper a paragraph acknowledging the possible hysteresis effect that the reviewer is mentioning. However, further research will be needed to tackle these interesting questions, which we consider to be beyond the scope of this initial paper.

4) ***The authors assumed a constant and spatially uniform value for Manning's n.why would you ignore the effect on surface roughness?***

See answer to point 6 in the response to Dunkerley's comments.

5) ***I therefore concur with David Dunkerley's review that the author's claim of their model correctly capturing the essential processes driving these ecosystems is premature.***

This statement has been modified in the revised paper. See answers to points 1 and 15 in the response to Dunkerley's comments

Answer to the specific comments: (Note that the page numbers in the list sent by the reviewer are incorrect)

6) **Page 2561, line 15-20: *"The redistribution of water... that may be disrupted if the vegetation structure is disturbed"*** "Disrupted" has been changed by "altered" (page 3, line 24 of revised manuscript).

7) **Page 2562, line 0-5: *Effects of vegetation in controlling runoff flow paths is likely affected by overall slope gradient, being relatively important on gentle slopes (such as common in interior Australia) and relatively less important on steep slopes (common in drylands of the Mediterranean Region).*** The effect of slope gradient is accounted for in the model, and it has important consequences for the type of vegetation pattern, water and sediment re-distribution. So the interaction between vegetation and slope, and their relative importance in controlling runoff flow paths is included in the model.

8) **Page 2562, line 5-10: *Although, I agree that in principle vegetation patterns may provide information on hillslope redistribution processes, and therefore on resource retention capacity and land condition, I don't believe this is straightforward at all.*** We are not implying anywhere in the paper that this is "straightforward". We are giving throughout the paper a number of literature references to describe the com-

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plex interactions between vegetation patterns and hillslope redistribution processes.

9) **Page 2562, line 20-25:** “Most common” has been changed to “A common” (page 5, line 5 of revised manuscript)

10) **Page 2563, line 15-20:** *Rather than overland flow being “lost” redistribution distances increase. Even if this involves concentration in channels the water may still contribute to increased water availability (and biomass production) downstream.* It may, but this still leads to the well identified process of degradation of the upland (hillslope) areas, with the associated problem of losses of valuable topsoil resources (as identified in the references cited in lines 17 and 25 of that page).

11) **Page 2571, line 0-5:** *The model appears to be sensitive to the relative importance of isotropic versus anisotropic (runoff-driven) seed dispersal. What is the basis for the parameter values that have been used to control these processes and how common are these in nature?*

The choice of isotropic dispersal parameters was based on those published in the literature (Klausmeier, 1999; Rietkerk et al., 2002). The seed dispersal parameters by overland flow were selected to produce stationary bands. Please, see also point 6 in the response to reviewer 1, and new paragraph in revised paper (page 22, line 4).

12) **Page 2568, line 5-10:** *I doubt that for the prevailing conditions under which runoff-runon processes operate there would be a hillslope-scale gradient of flow depths. Please, provide support.* As reported in numerous papers, there is a clear distinction between runoff and runon areas (and numerous papers with field support). As the flow accumulates in the runoff areas (source zones) it will experience changes (an increase, even if very small) in flow depths. When the flow reaches the runon areas (sink zones), flow depths start to decrease as water infiltrates in the more permeable soils (with higher macroporosity, etc). See also point 8 in the response to Dunkerley's comments.

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13) **Page 2568, line 15-20:** *The trend of decreasing infiltration rates with distance from trees is usually explained by the greater porosity associated with high root densities, organic matter content, soil faunal activity, etc. near trees or large shrubs. I doubt that these trends in infiltration are due to micro-topography alone.* Indeed, infiltration rates increase because of the greater porosity associated with high root densities, organic matter content, soil faunal activity, etc., in the vegetated areas that are frequently located in slightly elevated mounds. 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) **Page 2576, line 5-10:** *The choice to not allow any lateral competition for water by vegetation in adjacent grid cells requires better support. To what extent does this choice affect resulting vegetation patterns?*

This statement has been rephrased to avoid confusion. Competition for water “is” allowed in the sense that the amount of total biomass in a given cell is prescribed by the amount of soil moisture in that cell. Lateral competition through the root system is not included in the model (Included in page 20 of revised manuscript).

15) **Page 2579, line 20-25:** *Earlier modelling work by Sánchez & Puigdefábregas reproduced similar terraced slope profiles for dry Mediterranean hillslopes dominated by the tussock grass *Stipa tenacissima*. See: Sánchez G, Puigdefábregas J (1994) Puigdefábregas J, Sánchez G (1996)* These previous results and references have been included in the revised paper.

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