

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

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Reviewers comments: PM Saco, GR Willgoose & GR Hancock - Eco-geomorphology and vegetation patterns in arid and semi-arid regions.

(Hydrology and Earth System Science)

This paper forms one in a large series of papers by modellers who have been attracted by the questions posed by the diverse vegetation patterns that have been highlighted by ecologists and geomorphologists working in drylands. The paper, like others before it, presents a stimulating perspective on the phenomenon of vegetation mosaics. The paper is clear and well-written on the whole, and the topic is one of continuing interest

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to workers in a range of discipline areas.

I felt a number of concerns as I read the paper. A major problem is that various modellers tend simply to assert their own view of what “accounts” for banded or other vegetation patterns, and proceed to build a model that reflects their view. We now have a sizeable body of models all purporting to do this. What is really required in order to make this valuable scientifically is not more models, but rather more solid field data with which to parameterise them and evaluate them. I think that every modeller would be well aware that it is possible to erect many models that can generate plausible-looking outputs, but these alone establish little. I accept that there is a real place for models as exploratory tools, but without some solid field data, that is about where their value stops.

The paper as it stands seems to be too heavily reliant on bald assertion by the authors. 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. (For example, refer to the models involving cooperativity and competition among the above-ground and below-ground parts of the plant community). 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. 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. 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. 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?

In terms of model formulation, 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

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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? 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. 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. 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.

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.

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-

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world situation, and the authors ought to comment on this.

Apart from the impressive set of assumptions and presumptions, a few of which are listed above, there are other issues of concern in this paper. 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).

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 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 are patches of poor expression, as well as spelling errors. “Walekin-King” (page 6) should be Wakelin-King. This incorrect spelling is repeated in the reference list. Expressions like “These type of models.” (page 7; should be ‘this type of model’), “This type of feedback effects ..” (page 7; should be ‘this type of feedback effect’), “.. capture this dynamics ..” (page 23; should be ‘capture these dynamics’), “..of this type of patterns.” (page 24; should be ‘of this kind of pattern’), “this dynamics” (page 25; should be ‘these dynamics’). On page 4 para 1 ‘their role on determining’ should read ‘their role in determining’. On page 4 para 2, ‘vegetation patches reducing its density’

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should read 'vegetation patches reducing their density'. On page 8 para 3, "The model is partially based in .." should read "The model is partially based on..".

My suggestion in relation to this manuscript is that it needs significant re-working. Assumptions driving the performance of the coupled models need to be justified much more soundly than is the case at present. In particular, where the authors feel the need to adopt a presumption that is at odds with field evidence (e.g. a spatially uniform surface roughness in a system that exhibits contrasting surface roughness; an absence of lateral water competition in a water-scarce system) they should make it clear (a) why they do so, and (b) what the implications are for the interpretation of the model results. A straightforward and conventional sensitivity analysis could of course be used in this situation.

The structure and expression of the paper could be improved during this revision. 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. As noted earlier, other authors have offered quite different interpretations. In the formal model of a scientific paper, the authors speculations and hypothesising (which presents unproven material and ideas) should be kept visibly and clearly distinct from the presentation of results which they can demonstrate.

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 their claim, noting only that the model results appear consistent in some ways with field data. Whether this is more than an example of equifinality will have to await formal testing of the modelled processes.

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