Hydrology and Earth System Sciences Discussions

Interactive comment on "A mechanistic description of the formation and evolution of vegetation patterns" *by* R. Foti and J. A. Ramírez

Anonymous Referee #2

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A mechanistic description of the formation and evolution of vegetation patterns Authors: R. Foti and J. A. Ramirez

General Comments

This paper investigates effects of runoff redistribution, root density, surface albedo and soil moisture content in the evolution of patterned vegetation in arid areas. The paper is reasonably well written, with interesting results that are potentially useful for the understanding of processes leading to the emergence of these patterns in various geographic regions.

However as explained in detail below, the methodology is not well described, incom-

plete, and therefore unclear, to the extent that I strongly believe that the results cannot be correctly interpreted by the reader.

For these reasons I recommend a thorough revision of the paper before considering it for publication.

Specific Comments

1. Line 5 page 8741: misspelling "indluenced"

2. Line 15, page 8741: note that the first and last images in Fig. 1 show vegetation patches that are smaller than 10 m.

3. Line 15, page 8742: what are "the environmental forcings" that the authors are referring to in this sentence?

4. Line 16, page 8742: it is not clear from the methodology that the study is estimating "biomass density" as stated here. Usually, biomass density is defined mass per unit area of live or dead plant material, with units of g/m2 or multiples. Equation 1 mentions vegetation density (M) but it is unclear if M represents a biomass density or vegetation cover (fractional cover per unit area). In fact, the paper does not state anywhere how is vegetation "updated". What are the units of M in the paper?, how is M computed and updated?, is there a growth function?, a dispersal function?, a death rate?. This needs to be properly defined. Note also, that the paper uses "biomass density"," vegetation density", and "fractional vegetation cover" interchangeably and without a proper definition.

5. Page 8743: "Procedure schematization" This section is not clear as it stands. A more detailed schematic diagram, showing the interactions between the different processes and the "state variables" estimated and/or updated in each simulation step would be beneficial. The estimation of these "state variables", i.e., M, ks, etc, could then be linked to the equations that follow, making sure that there are no equations missing, for example for fractional vegetation cover, groundwater (note that this is absolutely

necessary, to ensure that results from this paper can be "reproduced" by others).

6. It seems that several equations have been omitted from the methodology. The paper mentions the estimation of "energy fluxes". However, the equations used for energy fluxes are not in the paper, though one of the climate variables mentioned throughout the paper is "net radiation". Once again, net radiation is not included in any equation. In fact Figure 14 corresponds to different "net radiations".

7. It is also necessary the clearly state the units for all the state variables, input, internal variables, and coefficients used in the analysis, in a table or immediately after they are defined.

8. Page 8746, equation (3): Y is not defined. Units of variables in this equation are not stated. For example, what are the units of runoff in equation (3)?

9. Page 8747, line 11: If nutrients are not modeled, then it is confusing to mention them in the methodology section. This line seems to indicate that plant "growth" in the model is a function of nutrient availability.

10. Page 8747, line 21: Please define all variables right after each equation, note that kv has not yet been defined (it is defined later in the next section).

11. Page 8748, Section 3.3.5, is entitled "Effect of vegetation on local soil nutrients and transpiration efficiency". However, as mentioned in line 9, there seems to be no estimation of soil nutrients in the model (unless the equation has been omitted from the paper). Therefore this title is inaccurate.

12. Page 8750, line 10 mentions that the system was simulated for various "spatial interaction functions". It is unclear what these spatial interaction functions are. Please refer to specific equations in the methodology. Are these functions related to the coefficients in equation 9, or are there any other spatial functions? Please explain.

13. Page 8750, lines 19-23 state: "Given the large number of combinations of feasible climatic, hydraulic and topographic conditions, several properties of the system were

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fixed. In particular, unless differently stated, simulations were carried out on a domain of constant slope whose hydraulic properties and climatic forcing are reported in the "base conditions" column of Table 1..." Table 1, shows a series of variables, MOST of which have not been described in the methodology, that is, they are NOT used in any of the equations included in the paper, or even mentioned as computed using an equation from previous literature (note, it is better to include all equations in the paper, possibly in an appendix). This is very confusing and prevents a proper interpretation of the results. The authors also need to include an explanation on the selection of the values for the parameters, both for the "base conditions" and the ones selected for the two sites (Niger and Somalia). One obvious explanation is mean storm duration and time between storms, which has been probably estimated from observed time series. But other parameters are more difficult to obtain from data, please explain the selection criteria (especially if any of them was used as a calibration parameter).

14. Page 8753, lines 22-24 mention that the parameters for Niger were obtained from Bromley et al., 1997. Further explanation is needed as this paper contains information to determine many but not all of these parameters.

15. Page 8755, lines 8-10 state: "In addition, the higher values of groundwater runoff observable in correspondence of the vegetated patches (shown in Fig. 5b) confirm that vegetation favors the infiltration of the hillslope run-on." How is the groundwater runoff computed? It is not part of the equations shown in the methodology section. Please note that "groundwater" is just mentioned in this line (and the figure caption), and its estimation is not described or mentioned anywhere else in the manuscript.

16. Page 8754, lines 22-28 explain: "simulations exhibited a noteworthy sensitivity of the emerging patterns to changes in the spatial interaction functions and in particular to the dependence of kv (Eq. 9) and hydraulic conductivity on vegetation (Eq. 1). Differences between patterns in Fig. 4g, f, for example, are due to changes of about 5% in the coefficients of the Eq. (9), while Fig. 4h was obtained by increasing the soil conductivity in the interval corresponding to a fractional cover only in the range of 0.3 to

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0.5 by about 10 %, while keeping the overall span of the range fixed between $3 \times 10-7$ to $9.5 \times 10-6ms-1$." This seems to explain that the simulations for the patterns of Niger (Figures 4G,H,I) were obtained by "calibrating" the parameters in equations 9 and 4 (shown in Table 2). Is this is the case? Please include an explanation in the paper, as well as, (if possible) a physical interpretation.

17. Pages 8759-61, section 5.3.1: Most of the analysis in this section is related to a portion of the methodology that has not been included in the paper. As explained above, it is unclear how net radiation, potential humidity, and the other variables in Table 1 are included in the model.

18. Page 8761, section 5.3.2: it is unclear how slope is included in the analysis.