Dear Reviewer #2,

we thank you very much for your detailed and careful examination of our paper. We found your comments and observations very interesting and helpful in producing an improved version of the manuscript.

Below you will find the detailed responses to all of your comments.

1. Line 5 page 8741: misspelling "indluenced"

The typo was fixed.

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

Lines 14-16 of the manuscript state: "*We are interested in analyzing vegetation agglomerates emerging at the hillslope scale and whose typical dimensions are of the order of magnitude of* 10^0 *to almost* 10^2*m (Fig. 1)"*. The figure, therefore, is consistent with the statement.

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

We refer to climatic conditions (e.g. precipitation, temperature, solar radiation). A parenthetical remark has been added in the text.

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.

This source of misunderstanding was corrected throughout the manuscript and biomass density was changed to vegetation density.

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).

We have partly rewritten this section and now it should be clearer to follow. An improved figure was substituted for figure 2. Concerning the "*missing*" equations, we clearly state that the 1D water budget was modeled by means of Eagleson's model. The equations that the reviewer suggests that we provide are reported in Eagleson's work, which is cited throughout our paper. These equations are extremely complex and numerous. Given the complexity of the model, we do not believe we can include the equations in our paper (nor as an appendix) effectively. However, we rephrased several sections in order to make sure to refer to the model throughout the manuscript and clearly list both the input parameters and variables and the outputs of the model.

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".

We used the net radiation to compute the potential evapotranspiration rate. We added a remark in section 5.3.1 that should eliminate ambiguity.

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.

See response to comment 8 below.

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)?

We added the definition of variable Y. Units are not stated because the model is outlined coherently. For example, units of runoff in equation 3 are the same as units of precipitation in the same equation. The choice of the units is arbitrary and the formulation works as long as units are chosen coherently throughout the model.

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.

In order to avoid confusion, we eliminated the reference to nutrients in the paragraph.

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).

We added the missing definition and made sure variables were declared for each equation throughout the manuscript.

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.

We renamed the section "Effect of vegetation on transpiration efficiency".

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.

The sentence was modified in "*lateral interaction functions (Equations 1 through 12)"* in order to specify what functions we are referring to.

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 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).

As mentioned above (reply to comment 5), the variables listed in the table are all part of the water balance model used to characterize water fluxes and vegetation density. References to the model have been added throughout the paper to avoid confusion.

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.

We added a remark specifying that some of the climatic parameters (namely: temperature, specific humidity and cloud coverage) were assigned arbitrarily in order to match the observed value of potential evapotranspiration. It has to be underlined that the only energy flux needed to estimate the vertical water budget through Eagleson's model is the latent heat (through the value of potential evapotranspiration). Our model incorporates Eagleson's model, but allows a general input of parameters related to the energy fluxes (screen height temperature, cloud coverage, albedo, et cetera). Ultimately, those are used simply to estimate potential evapotranspiration. We

chose to implement our modeling in this fashion in order to allow for flexibility and be able to perform a detailed sensitivity analysis.

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.

Please see response to comments 5 and 13.

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 0.5 by about 10 %, while keeping the overall span of the range fixed between $3\times10-7$ to $9.5\times10-6ms-1$." 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.

As suggested, we included an explanation in the paper as well as the physical interpretation requested.

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.

Please see response to comments 5, 13 and 15.

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

In order to avoid confusion, we renamed the section as "Temporal patterns dynamics" and added a remark to remind the reader about the way slope is accounted for in the model.