Dear Dr. Tietjen,

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.

Methods
1. Figure with a schematic overview of the model and its processes would help, this figure could substitute Figure 2

We substitute the figure according to your suggestions.

2. Climate input: problematic to use mean values for climate input, since the few large pulses mostly drive the biotic system dynamics - this should be discussed.

The model used for the water budget assumes a soil-vegetation system in long term equilibrium with the climate. Although your argument is valid at the short time scales of the pulses you refer to, observed patterns (at least the ones we are interested in) do not appear and disappear within short time spans (although they may migrate or change in shape as a result of peculiar climatic forcings). That suggests that their presence is the result of a long term adaptation process involving both vegetation and the soil, which can only be explained if the long term climatic conditions (i.e. average climatic inputs) are themselves the drivers of the phenomenon.
In order to clarify this in the paper, we added a couple of sentences in paragraph 3.2.

3. Vegetation density plays a crucial role in the model, but there seems to be no equation that describes the dynamics. This is crucial to understand the model!

As we mention in the methodology section, the 1D water budget, along with the vegetation density, is captured by means of Eagleson's model. Given the complexity of the model, we do not believe we can include the equations in our paper. 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.

4. Eq 3: link from Y to Rsy is not clear

We rephrased the sentence to clarify what those two quantities are and how they are linked.

Simulation of the system
5. p 8750, 14-18: difficult to understand. Reasoning would be helpful, schematic figure would be helpful.

We rephrased part of the section. This, along with a new schematic figure we added (which replaced Fig. 2) should be able to clarify the section.

Results and discussion
6. Description of PDF difficult to understand for readers that are not familiar with this concept. Short explanation of how to interpret result would be helpful.

We added a section that clarifies the concept and discusses the implications of the PDF analysis.

7. Eq 13: shows only the percentage of cells that are clustered and not the size of the clusters, It would be good to have an additional measurement on this (i.e. many small clusters vs. few large)

The paragraph where the equation is contained is intended to analyze whether the patterns (both observed and simulated) are indeed the result of a spatially inhomogeneous set of mechanisms or not. In order to verify the above, we compare the observed (and simulated) patterns with the ones resulting from a random process. Given the definition of cluster we provided (i.e. a group of adjacent pixels characterized by having vegetation density larger than the domain average density), we compare the clusters with a homogeneous binomial process with the same probability of occurrence. Eq. 13 is used in that context to estimate that probability. Additional statistics (mean size of clusters, quantile of cluster size distribution, shape ratios, filling factors) are already provided in the analysis of the results. We did evaluate other statistics but decided not to include them in the analysis to avoid crowding the paper with less significant information.

8. Reproduction of patterns: Not clear if reproduced patterns are a result of model calibration or if there are ecological reason for the parameters. Please clarify.

Regarding the validation sites, most of the parameters we used were taken from literature. Some parameters (e.g. specific humidity, screen height temperature, etc.) were arbitrarily set to a value which ultimately led to potential evapotranspiration rates equal to the ones reported in literature With respect to this, 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). The parameters of equations 8-12 are arbitrarily chosen within ecologically sound boundaries (as specified in section 3.3.5). Some tuning of the latter parameters was used to calibrate the output and match the observation, and we added a remark to clarify that. However, as shown by our sensitivity analysis, the model simulates patterns regardless of the tuning of those parameters, as long as conducive conditions in both climate and soil properties are present.

9. Suggestion for organization of section 5.2: first describe both sites and then show results. This could prevent explaining things twice.

Your suggestion is sound and we made the attempt of following it. However, we found that it would disrupt the flow of the section more than it would help in structure. Therefore, we eventually opted for maintaining the current organization.

10. Labyrinths should be discussed: why does the model not reproduce these? Are the processes not well parameterized? Are processes missing?

The model does not reproduce labyrinths for the conditions under analysis, which in only one case showed a labyrinthine pattern (Fig 4F.) according to our definition. Nevertheless, the corresponding simulated pattern (Fig. 4I) is indeed very close to the threshold to be classified as labyrinthine. In addition, the model is able to produce labyrinths in many cases, although they were not shown in the paper. We added a few sentences in sections 5.3.3 and 5.3.4 to clarify the point.

11. Why did the authors not perform simulations with real topographic data? Was this not available?

Although that would have been possible, we chose not to do so at this stage of the research. While specific topography can have a strong impact on the peculiar shape of a vegetation clump, it would not affect dramatically whether or not vegetation is indeed self-organized at the slope scale, which was the main focus of this research emphasizing climatic, vegetation and slope scale soil controls. Clearly, examining the effects of topographic variations at point scales is a very interesting topic to explore and it will the subject of future research by the authors.

12. Section 5.3. which site was simulated? Please state in text.

The section deals with simulations that are not site-specific. The purpose of the section is to perform a sensitivity analysis and investigate the impact of each of the modeled dynamics on the pattern formation. In order to avoid confusion, we added a line that clearly states it.

13. 5.3.2 Title not well chosen. Authors didn't analyze the impact of slope, but the phenomenon of pattern migration (or temporal dynamics).

In order to avoid confusion, we renamed the section as "Temporal patterns dynamics".

14. Impact of slope would be very interesting to analyze, since patterns have been found for gentle slopes only, does the model reproduce this?

No. Unfortunately our model is currently only able to route the water directionally but does not implement a dynamic wave analysis (as we now underline with a remark to make sure the reader is reminded about the way slope is accounted for in the model). As you suggest, together with small-scale topographic variations, this is a very interesting topic to explore, but it necessitates of a level of sophistication that the current implementation of the model does not handle.

15. Section 5.3.4. Reference to Table 5 given in the text, but Table itself is missing.

We had failed to update the cross reference, as the correct reference would be to Table 2. We now fixed the problem.

Tables and Figures
16. Table 1 and 2: data source not given, reasoning for values missing

We clarify the point in section 5.2.1.

17. Table 3: add row "Cluster type".

We added the row as suggested.

18. Figure 6/9: Provide short intros into figures. Caption too long to grasp immediately. Highly difference between "higher than" and "lower than".

We adjusted the figure caption and added a paragraph to section 5.1.1 (see also response to comment 6) in order to address your suggestion.

Specific comments
1. p 8746, 10: definition of ds is missing

We added the definition.

2. p 8747, 7-9 authors should say clearly that they do not account for dependence of albedo on vegetation!

Lines 6-9 state: "*This formulation accounts explicitly for the dependence of albedo on soil moisture and implicitly for its dependence on vegetation through the dependence of soil moisture on vegetation density*". This clearly states that albedo depends directly on soil moisture, which in turns, depends on vegetation (and vice versa). We do not believe that the statement is deceiving in any way.

3. p 8747, eq. 5: kv has not been defined.

We added a definition of kv.

4. eq 5: the 4 refer to number of nieghboring cells?

Yes. We now specify that directly in the text.

5. eq. 6: can ζ be<1?

ζ cannot be less than one because Eagleson's model assumes that vegetation has access to the vertical soil column directly below the control surface.

6. p 8748, 14: in addition, soil porosity, slowdown of runoff

As suggested, we made those remarks in the text.

7. p 8749. eq 10-12: definition of mPA?

The definition was given above, at 8746, 7.

8. p 8752, 20-21: "corresponding uniform binomial process" Does this mean: cluster size distribution of a random vegetation distribution? Please clarify

We added a parenthetical remark to clarify it. As explained also in the response to comment 7, each cluster field has a binomial process associated with it through the rate *p* found by means of equation 13.

9. p 8755, 8: how was groundwater runoff calculated?

As for all the other water fluxes, it is calculated through Eagleson's water balance model. In order to avoid confusion, 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.

10. p 8760, 2: Add "Nearly": "Nearly all patterned fields..."

The sentence was modified according to your suggestion.

11. p 8760, 7. Which value for precipitation was chosen here?

That would be the value corresponding to the base condition (that is, 40cm). Each section evaluates the sensitivity of the system to only one of the drivers.

12. p 8764, 18. Table ref is not correct (Tab. 4 and not Tab. 1).

The reference was fixed.

13. p 8764, 19: A=-0.3 (and not +0.3)

The typo was fixed.

14. p 8771: Reference Jeltsch, Zehe, et al is not correct (Tietjen, Jeltsch, Zehe et al.)

We fixed the reference.