

Comments to the editor

We have taken the three major points of the editor into account in the following way:

- First, the editor concluded that we too easily classify our two chosen vegetation types as grass and forest, whereas those in fact cover a large part of a continuous parameter space. We have clearly stated now in the paper that our model is only a crude approximation of plants that has been tuned to work well on the landscape scales for, for instance, numerical weather prediction. We state in the introduction (lines 126-142):

In our model, we parametrize the response of the vegetation to the atmosphere in an empirical way following Jarvis (1976), similar to the parametrization of transpiration in the majority of the numerical weather prediction models (e.g. Noilhan and Mahfouf, 1996; Ek et al., 2003). In such parametrizations, individual vegetation species are combined in classes, such as grassland, cropland, deciduous forest, needleleaf forests, etc. This means that this type of parametrizations is assumed to only perform well on the landscape scale, where the variations among different species within the same class average out. In this paper, we deal with grassland and forest only for simplicity. This is justified by the observation of Teuling et al. (2010), who showed the clustering of grassland and forest sites in their respective response to heatwave conditions. Wherever we discuss model results, grassland and forest refer thus to their respective classes in the parametrization.

Furthermore, we state in the methods (lines 308-316):

As we already mentioned in the introduction, the results of our study are strongly dependent on the chosen properties for forest and grassland. Since both vegetation types encompass a wide range of subspecies that all have their own unique properties and responses to the atmospheric properties, we have chosen pure grassland and broadleaf deciduous forest as our vegetation classes, as those provide a good match with the data of T10 and a broad parameter range for our sensitivity study.

- Second, we have removed our suggestions that the VPD sensitivity of grassland is taken into account in the temperature. Instead, we state that this is a side-effect of the model that might be mechanically inconsistent (lines 277-294).
- Third, we agree with the editor and with the reviewer that we did not show well enough the importance of the boundary layer feedbacks. Therefore, we have added a new Section 4.3, including a new Figure 6. In this section, we clearly show the stomatal resistance enhancement due to the drying and warming of the air. In order to be able to show this, we have performed the experiment that the reviewer suggested: forcing forest with the atmosphere of grassland. This was a very good suggestion that, in our view, has improved the quality of the paper (lines 470-499).
- Fourth, we have incorporated the minor comments suggested by the reviewer (see next pages).

With these modifications, we hope to have addressed all concerns. On the next pages, we reply to the comments of the reviewer.

Comments to the reviewer

The reviewer is still not convinced about the modeling approach. In the points hereafter we respond to his critics and explain how we have addressed those. Furthermore, we have incorporated the very good suggestion of the reviewer to investigate the role of the boundary-layer feedbacks: We have used the atmospheric properties produced by our simulation of grassland as the forcing for the forest land-use type and have rerun the model to find out the exact importance of the atmospheric boundary layer. With the results of this experiment, we have created a new Figure 6 and a new Section 4.3. We hope that these results convince the reviewer.

We did not imply to say that the VPD response of plants is partially covered by the temperature response. This is only the case in the used model and therefore does not reflect the proper mechanisms. We have made this more clear in our text now (see our comments to the editor).

Next, we respond to each of the minor comments:

- *eq 3 has an error should be f^{-1} and not f (for all the functions. I check the original in Noilhan and Mahfouf 1996). Or, you need to invert the formulation of F_f and f_4 (eqs. 4,5).*

The reviewer is correct, we have corrected our mistake.

- *gD cannot be unitless (Table 1) – the product $gD*VPD$ must be unitless, and VPD has units. Please provide the units to ALL the variables you use, not only the parameters. For example- the units of VPD (hPa?), T_a (K?), LE , S_{in} ... should all be listed near the equation that first uses them and should be consistent throughout the paper (some units appear in the discussion section when you describe the conditions over grass and forest, make sure they are the same as the ones in the formulation).*

The reviewer is again correct, gD has units of Pa^{-1} . We have made a thorough check of all the units and defined them wherever they are used first.

- *Fig 3 – what is the meaning of the different colors of the bars? Are these grass/forest?*

These are indeed grassland and forest. We have added a legend to the figure.

- *Fig 6 – the titles of the subsections b-f should be more accurate or removed, and somewhere in the caption you must explain exactly what is the meaning of each panel. For example – f) "residual" - of what? c) roughness – is it only roughness length or also displacement height? d) stomata resistance – this is wrong. r_s (see your eq 3) is not what you tested here but only $f_1*f_2*f_3*f_4$ (As I stated above, I wish you would have at least isolated f_3 and f_4).*

We have edited the description. We believe that the displacement height is only relevant whenever one compares with observational towers that have a fixed height above ground level and not for a model as ours. At the reviewers request, we have isolated the effects of function f_3 now and edited the labels accordingly. In our revised paper, the caption of Figure 7 has been improved.