

## ***Interactive comment on “Disentangling the response of forest and grassland energy exchange to heatwaves under idealized land–atmosphere coupling” by C. C. van Heerwaarden and A. J. Teuling***

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Hereby, we would like to react to the first comments of the reviewer. we will address each of the three major points separately.

1) First, we would like to state that we approach this problem from an atmospheric modeling perspective, where we have tried to demonstrate the first-order, rather than the exact behavior of the coupled land-atmosphere system, using a very commonly used model. The response of stomatal resistance to vapor pressure deficit (VPD)

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that we prescribe is exactly that of the ECMWF IFS model and can be found in their documentation. This surface model has been validated thoroughly and has been tuned to perform well in the European weather forecasts.

We are aware of the fact that the stomatal response to VPD is a topic of strong disagreement among many studies, both in its magnitude as in the underlying mechanisms (Monteith, 1995, Plant, Cell and Environment; Bunce, 1996, Plant, Cell and Environment; Streck, 2003, Current Agricultural Science and Technology). We have bypassed this discussion in the paper, and we have chosen to use a model formulation that has proven itself in weather forecasting. In a revised version of the paper, we will introduce this discussion and explain that we have followed a pragmatic approach.

We would appreciate if the reviewer can point us out literature that shows that well watered crops do not respond to VPD, whereas natural grasslands do. In case the reviewer is right, our results could potentially be explained by the fact that very few grasslands in Western Europe are natural grasslands, but instead are used for agricultural purposes. A discussion on this could be added to the paper as well.

2) In the paper we explain that the soil moisture has been tuned to reproduce the surface energy balance measurements, since the soil properties are strongly spatially variable and therefore the direct transfer from soil moisture measurements to the atmospheric model is a nearly impossible task. Since the surfaces are fully vegetated, and we do not model beyond the time scales of a single day, we find little sensitivity to variations in the first soil layer.

3) This point is based on a misunderstanding that is the result of our chosen soil moisture coordinate. The relative soil moisture is not the soil water content in  $\text{m}^3/\text{m}^3$ , but instead the relative saturation in the range from wilting point to field capacity:  $(sm - sm_{wp}) / (sm_{fc} - sm_{wp})$ , where  $sm$  is soil moisture, and subscripts  $wp$  and  $fc$  mean wilting point and field capacity. The actual soil water content is thus much lower than  $0.5 \text{ m}^3/\text{m}^3$ .

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The values that we compare Figure 3 against are the ones from the paper of Teuling et al. (2010, Nature Geoscience). In a revised version of the paper, we can add the values in order to facilitate the comparison.

The last point that we would like to comment on is the reviewers statement that we have circular logic by not introducing a VPD response for low vegetation and then claim that that this is an outcome of our study. I would like to state here that the main aim of this paper was to quantify the first order behavior that is already known from previous studies. Even though the importance of the VPD response stands out in our model results, it still requires the other differences, and in particular the albedo difference, to explain the measurements of Teuling et al. (2010). In a revised version of the paper, we will make this more clear.

We hope to have addressed the reviewer's comments adequately and we hope that by clarifying the reviewer's points, we get the opportunity to revise the paper.

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