Biogeosciences Discuss., 11, C2026–C2028, 2014
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Biogeosciences
Discussions
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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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Received and published: 1 June 2014

I would like to thank Anonymous Referee #1 for his/her in depth review of our work and the useful suggestions made in the discussion. Here, I would like to address two issues raised in the review, namely the model choice and the use of LOESS in Figure 1.

The referee's main comments deal with the complexity of our model, in particular the assumption that stomatal opening in trees responds different to VPD than that in grasses. In selecting our model and the parameterizations that control ET, we applied Occam's razor: all parameterizations are complex enough to allow the study on the sensitivity of the process, but not more complex. The parameterizations that were used are robust and have been tested and optimised using results from several field experiments, and are at the heart of the operational model used by the ECMWF for their weather forecasts. The referee comments explicitly on the results produced by our model, stating that the simulated difference between forest and grassland ET is "To the best of my knowledge ... highly unrealistic". It seems that the referee is not familiar with the work of Teuling et al. (Nature Geoscience, 2010, dx.doi.org/10.1038/ngeo950), which forms the starting point of our current work. In the 2010 paper, large differences in ET (and H) between forest and grassland are reported for heatwave conditions, exactly the situation that we address in our current work. The simulated values are well within the range reported by Teuling et al. (2010) from observations, and in fact our model soil moisture was tuned to reproduce the results of Teuling et al. (2010) in the best way. Our goal is thus to produce results that are directly in line with observations, rather than unrealistic ones.

The response of stomata to VPD, as also pointed out by the referee, is a central element of our analysis. We however disagree with the referee that "It is therefore an example of circular logic" that trees with sensitivity to VPD respond to VPD whereas grasses do not. Our goal was a comparison of the magnitude of several effects in a coupled system, of which the VPD response is just one. It is not trivial that in a coupled system, the VPD response is strong enough to influence the temperature of the whole ABL and that this effect is stronger than effects of differences in albedo, roughness and surface resistance operating at the same time. This is the main finding of our work. Given the comments by the referee, I feel we should be clearer in stating our main conclusions and implications of our work. Clearly, we don't prove or even want to prove that the VPD response is different between forest and grassland. What we want to do is show that *given* a VPD response as is used in many (climate) models and which is consistent with other literature, we are able to *explain* the observed differences in

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fluxes during heatwaves resulting from coupling the land surface with the atmosphere, *even without* taking any possible differences in soil moisture into account. Since soil moisture is likely different between forests and grasslands (however this is difficult if not impossible to test with current observations), we will also investigate possible effects of soil moisture differences in our revision.

I would like to ask the referee if changing our manuscript along these lines would solve some of the issues brought forward in the initial review. Also, we would be interested to learn about studies that address the sensitivity of grass stomata to VPD independently of soil moisture (I agree that the sensitivity to soil moisture increases rapidly under dry conditions for grasses as indicated by the referee, but I believe this effect plays a minor role in the results of Teuling et al. (2010), since all grassland sites showed positive ET anomalies, thus higher rather than lower evaporation as would be the case under stronger soil moisture reduction). We have done an extensive literature review and did not find any studies other than the ones cited already, and that show a smaller sensitivity for grasses.

A second point I would like to address concerns the comments made on the use of LOESS interpolation in Figure 1. The referee disputes the use of LOESS as a regression method, and claims that any x,y-scatter plot should be fitted with a parametric curve. This is incorrect. Methods like LOESS, which can be categorised as locally weighted scatterplot smoothing techniques or local regression, have been developed for cases in which traditional (linear) regression is not applicable due to, for instance, changes in distribution/variability of the residuals or an unknown nonlinear model that is behind the data. In the case of Figure 1, there is no reason to assume a linear model, and the goal of the interpolation was to show the relationship in the raw data *without* assuming a particular model a priori. For this, LOESS is the default option.

Interactive comment on Biogeosciences Discuss., 11, 5969, 2014.