Hamburg, 21 July 2014,

Dear sir/madam,

Hereby, we present a revised version of our paper: *Disentangling the response of forest and grassland energy exchange to heatwaves under idealized land-atmosphere coupling*. We have made a series of major edits to our paper, which we describe hereafter, and we have added a new figure (Figure 1).

The main critic of both reviewers was the fact that we have been overemphasizing the role of the response of the stomatal resistance to vapor pressure deficit, because we impose that role ourselves with the chosen parametrization for stomatal resistance. After doing more literature research on the plant physiological aspects of our topic, we agree with the reviewers and we have corrected our text accordingly. We realize that with our used parametrization, we are not able to separate the effects of temperature on the stomatal resistance from those of vapor pressure deficit. Therefore, in the current version of the manuscript, we consider only the total vegetation response and compare its importance to other properties of the system.

To illustrate this issue better, we have added a short overview of the current state of the knowledge on the response of plants to atmospheric temperature and humidity to the introduction of the paper, in order to explain that the issue is far from settled, thereby including the suggested papers by the first reviewer.

With respect to the previous points, we have modified the main conclusion of the paper. We are able to show the relative importance of the fast physiological processes compared to other differences in the properties between grass in forest, but we are not able to explain the driving mechanisms. Our main conclusion is therefore that there is a urgent need for a better mechanical understanding of those vegetation related processes that play a role in controlling the evapotranspiration rate of plants, especially in the light of assessing the impact of climate change.

The first reviewer has asked for more information about the soil moisture. In the revised version of the paper, we carefully explain that we use the soil moisture as a tuning variable, since specifying the exact moisture contents is infeasible in this study, due to the large variation in soil types and exact land use among the FLUXNET sites. Furthermore, we stress now that we are only studying short heat waves in which soil depletion, and therefore the exact properties of the soil, does not play a role and we have added references to studies that do study the role of soil moisture in the coupled system.

At the request of the reviewers, we have added a new figure to the paper that shows the relationship between maximum temperature during the day and the measured latent heat flux. In our results section we show that our model shows the same behavior as the data found in the new figure.

We hope that by taking the reviewers' comments into account we have improved the quality of the paper to such an extent that it fulfills the requirements of Biogeosciences. We have added the detailed responses from the interactive discussion as an attachment.

Kind regards,

Chiel van Heerwaarden

Biogeosciences Discuss., 11, C1438–C1440, 2014
www.biogeosciences-discuss.net/11/C1438/2014/

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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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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)

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 m3/m3, 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 m3/m3.

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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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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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Hereby, I would like to reply to the points raised by the second reviewer. Similar as the first reviewer, the reviewer mentions that he/she does not agree with the treatment of the vegetation in our paper.

1. The reviewer mentions that our parameters for grass are incorrect. We would like to state here that the parameters that we are using are the common practice for many atmospheric models, and that they are heavily tuned together in order to achieve an

optimal match between model results and measurement data.

The problem for stomatal resistance models such as Jarvis' model, which we are using, is that they are purely empirical and that their parameters are not purely independent. The fact that we use no VPD correction for grass does not mean that it does not respond at all. The fourth parameter in our Jarvis model, the temperature sensitivity, leads to an enhanced resistance under conditions in which the 2$\sim$m temperature exceeds 298 K. This parameter could as well be removed and instead a sensitivity to VPD could be introduced for grass leading to the same results.

We propose to this reviewer and the previous one, to rewrite our manuscript and taking the entire stomatal resistance dynamics as one phenomenon. Judging from the statements of both reviewers, we agree that might have been too ambitious in stating the VPD response as the one-and-only relevant biological factor. Instead, we shall consider the biological responses as a whole.

2. The roughness length of scalars is largely an unresolved issue in turbulent flows over rough surfaces, and its determination is again a purely empirical one. The values that we have chosen are solely the results of extensive tuning against measurement data.

3. The sensitivity study contains a wide range and the difference between grass and forest in the lower left area of the graph is only marginal. The exact difference in the cold, low-radiation state depends on the chosen values of the minimal resistance. As we state in the paper, our focus was not on making an exact prediction of the behavior of the system over the entire parameter range, but instead to show that we can reproduce and explain the behavior of the coupled system that Teuling (2010) has found in the measurement data.

4. Here, the reviewer makes an incorrect statement. Even though the real temperature decreases with height, the potential temperature increases with height in the free atmosphere. This means that air from above the atmospheric boundary layer that is

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Biogeosciences
Discussions

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We believe that by means of our previous comments, we have responded to the reviewers' concerns.

In order to address their shared concern, the model used to describe the vegetation response, we have created a new figure that shows the total stomatal resistance response to the heating. Within our model, also the grass responds to the heat waves, with a resistance increase between 10 and 20 percent.

After creating this figure, we agree with the reviewers that we might have overemphasized the role of VPD, because in the typical parameterizations that are being used in atmospheric models, the set of functions that in the end lead to the stomatal resistance cannot distinguish the temperature effects from the VPD effects. These two variables tend to be strongly correlated, with high VPD values mostly occuring during spells of high temperatures. As our figure shows, the VPD response that the reviewers expected are mainly taken into account through the response of the stomatal resistance to the temperature. Therefore, the typical parameterizations that atmospheric models use might be mechanically wrong, but still leading to the correct atmospheric temperatures and humidities.

We propose the following. Rather than stressing the role of the VPD, we suggest to rewrite our discussion focusing on the role of biology in general. We believe that one of the main findings of our paper, namely that the active behaviour of the vegetation is the crucial factor in explaining the data, still holds. We will introduce a deeper discussion on the role of biological processes and link it better with existing literature on the response of the stomatal resistance to the VPD. In addition, we will explain in more detail when the runaway feedback that we discuss can occur and why forest crosses the threshold during the heat waves and why grass does not.

With this, we hope to have addressed all the reviewers' concerns.

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