

Interactive comment on “Response of water use efficiency to summer drought in boreal Scots pine forests in Finland” by Yao Gao et al.

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General comments :

In their paper **Response of water use efficiency to summer drought in boreal Scots pine forests in Finland** Gao et al. address a timely problem in biogeochemistry, the interaction of the carbon with the water cycles. Knowledge of this relationship is particularly uncertain during periods of water stress, for which the exact physiological mechanisms and their ecological variability are unknown.

Nevertheless, I have the following major comments that I think should be addressed:

1. The authors should discuss why IWUE was chosen as a metric in addition to WUE. A recent study (Zhou et al. 2015) demonstrated that a definition based on a square-

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root relationship with VPD is superior to the definition of Beer et al. (2009). Notably, the latter is already expected to be dependent on VPD, as stomata react to this variable and thus the surface conductance changes accordingly.

AR: In the revised paper, we added the underlying water use efficiency (uWUE) that introduced by Zhou et al. (2014) for a comparison with WUE and IWUE for studying their performances during the summer drought. IWUE is defined as WUE multiplied with mean daylight vapor pressure deficit (VPD), and it has been found to increase during short-term moderate drought. In the formulation of IWUE, ET/VPD is a hydrological measure of the surface conductance at the ecosystem level (Beer et al., 2009). The uWUE is proposed based on IWUE and a simple stomatal model of Lloyd and Farquhar (1994). Different to IWUE which is still affected by the nonlinear effect of VPD, the uWUE has been found to represent the best linear relationship among GPP, ET and VPD at the half-hourly time scale by Zhou et al. (2014). Later on, the appropriateness of uWUE at daily time scale has been demonstrated (Zhou et al., 2015). However, we were not clear how uWUE behaves during drought period. In our study, we found that uWUE does not show a change during the short-term summer drought at our site. As uWUE is more independent of VPD, it is considered that uWUE is more suitable as a plant functioning metric to evaluate the impact of global change on plant functioning at ecosystem level in the long term. Those contents have been introduced and discussed in the revised manuscript.

2. Lines 184-190 address the problem of soil evaporation. While it is true that model predictions of transpiration can be used as proxy variable, this comes at the cost of additional model uncertainties. The cited paper of Beer et al. (2009), which establishes the concept of IWUE, tries to circumvent this problem by excluding days following precipitation events. Most of the excluded days would lie outside dry spells, hence retaining sufficient sample size for these periods. The data presented in the current manuscript could be filtered according to such a criterion; then it would be important to see whether the observed patterns persist or change in magnitude. Generally this

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approach would be more robust than basing the IWUEt/EWUEt estimates on a model with known deficiencies.

AR: Yes, we filtered our data to exclude the rainy days and the certain amount of dry days after the rainy days in the revised manuscript. As suspected by the reviewer, sufficient sample size for the drought period retain after the data selection. We could observe a more significant pattern of the impacts on GPP and ET from the soil moisture drought.

3. I think it is questionable that daily averages were used for the analyses. Especially in light of problems such as dew-fall it would make sense to use day-night-time separated data for the analyses. At least, the absence of day-night-time separation should be mentioned in the text.

AR: The data has now been reprocessed for the analysis, and only daytime data without precipitation influence were selected. The data selection process is described in section 2.2. In the revised manuscript, only half-hourly data with shortwave radiation (R_s) larger than 100 W/m² were selected for the aim to select the effective time for plant photosynthesis. The rainy days and certain amount of dry days after the rainy days were also excluded. By doing this, data with negative GPP and ET were excluded.

4. Regarding the effect of atmospheric humidity the text states that "Our results indicate that the combined effects of soil moisture and atmospheric drought on stomatal conductance have to be taken into account." (ll.351-353) I think the current version of the text doesn't fully establish the interaction and correlation between the atmospheric and subsurface stress factors. The observed effects by themselves are not unexpected, as the model in its current form simply lacks the stomatal response to atmospheric humidity.

AR: Yes, the model in its current form lacks the stomatal response to atmospheric humidity. In global ecosystem models, simple representations of stomatal regulation have

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often been applied to reduce computing costs. Because VPD and soil moisture are to certain degree correlated, inclusion of one of the either has often shown to be enough to account for drought effects. In the revised manuscript, the formulations of the default stomatal conductance model in JSBACH has been added. It can be found that the soil moisture condition is the only limiting factor in the default stomatal conductance model in JSBACH. Knauer et al. (2015) tested a few stomatal conductance models in the JSBACH model under non-limited soil moisture conditions, and the results showed that Ball-Berry model (Ball et al., 1987) to be best in its response to atmospheric drought. However, the performance of the default stomatal conductance model under limited soil moisture conditions has not been tested before this study. Our results showed that the model can successfully capture the turning point of GPP and ET when the SMI decreased to be lower than 0.2. However, the decreases of GPP and ET are not as strong as in the observations. Thus, our results indicate that the combined effects of soil moisture and atmospheric drought on stomatal conductance have to be both taken into account. Even though no such a correlation between the stress factors was established in our study, it was demonstrated that at certain point the correlation between GPP or ET and soil moisture or VPD vanishes. So it is insufficient to use only soil moisture or VPD to describe drought stress.

5. The paragraph in ll.325-329 is confusing. First, the statement that "This means that the intrinsic water use efficiency at the ecosystem level is enhanced during soil moisture drought." is merely restating the increase already mentioned in the preceding sentence. Further, wouldn't one expect that a better adaptation to drought leads to elevated IWUE, rather than interpreting a constant IWUE as the sign for this adaptation? Aside from that, it could be worth commenting on whether differences in adaption between the southern and northern site would be expected *a priori*, e.g. due to average recurrence times of droughts at these locations.

AR: Yes, we agree with the reviewer the sentence is redundant and we have revised this paragraph. However, we do not agree with reviewer on the assumption that elevated

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IWUE would necessarily be a sign of adaptation. The trees might also be opportunistic in their strategies. The different behaviors of IWUEs imply different strategies in the south than in the north.

6. Generally, the inclusion and evaluation of JSBACH simulations would profit from a more targeted motivation. What is the predicted behavior? What is already known? In what way could the presented analysis contribute to an improvement of the model? In addition to that, the formulation used for the effect of soil moisture on stomatal conductance should be stated.

AR: For our reply to the first part of this comment, please refer to our answer for the comment 4 above. Additionally, we have added a section 2.3.1 in the revised manuscript to describe the stomatal conductance model in JSBACH.

Specific & minor comments:

- The authors state correctly that "there may be systematic errors source from imperfect spectral corrections and gap-filling procedures or calibration problems" (ll.364-365). This would make it all the more important to report which exact criteria were used to exclude observations with insufficient data quality.

- l.109: Which partitioning method was used? Should be mentioned and cited in the text.

AR: For Hyytiälä, EC fluxes were calculated using standard methods as described in Mammarella et al (2016). Data quality of 30 min values of NEE and latent heat flux (LE) was ensured excluding records with low turbulent mixing (friction velocity below 0.25 m/s) as described in Markkanen et al. (2001), Mammarella et al (2007) and Ilvesniemi et al. (2010). The NEE was partitioned into Re and GPP according to Kolari et al. (2009). Shortly, Re was modelled using an exponential equation with temperature at a depth of 2 cm in the soil organic layer as the explanatory factor. The value of GPP was then directly derived as residual from the measured NEE. When NEE was missing,

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GPP was estimated according to Eq.7 in Kolari et al. (2009). LE was gap-filled using a linear regression against net radiation in a moving window of 5 days. Then ET is converted from LE. We have added these details and the missing references in the revised manuscript.

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- ll.379-381: "Also, in the relationships between ET/T and VPD at the two sites, both observed and simulated ET/T showed a small decrease under moderate soil moisture drought, compared to days with higher soil moisture conditions." It is not clear from this sentence, whether this relates to the sensitivity of ET to VPD or ET itself.

AR: We agree with the reviewer that the sentence is not clear enough. It was referred to the sensitivity of ET to VPD. We have revised this part also because the data was

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reprocessed for the revised manuscript.

- The limitations of the EC method mentioned in ll.361-365 are true by itself, however in the current text they appear very unconnected to any discrepancies or problems in the analyses. If specific problems of the method, such as the energy-balance-closure-gap can be made responsible for particular deviations, that should be connected in the discussion. Else, the part can be shortened and moved to the methods section.

AR: Yes, we will move this part to the methods section in the revised manuscript.

Technical corrections :

- "In addition, there may be systematic errors source from imperfect spectral corrections and gap-filling procedures or calibration problems" (ll.364-365) should be changed to "In addition, imperfect spectral corrections and gap-filling procedures as well as calibration problems may be sources of systematic errors."

AR: We did the change as reviewer suggested.

Citations Zhou et al. (2015). Daily underlying water use efficiency for AmeriFlux sites.

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