

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

**Anonymous Referee #2**

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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-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

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and thus the surface conductance changes accordingly.

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

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.

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.

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?

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Aside from that, it could be worth commenting on whether differences in adaptation between the southern and northern site would be expected *a priori*, e.g. due to average recurrence times of droughts at these locations.

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.

#### # 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" (II.364-365). This would make it all the more important to report which exact criteria were used to exclude observations with insufficient data quality.

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

- II.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.

- The limitations of the EC method mentioned in II.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.

#### # Technical corrections

- "In addition, there may be systematic errors source from imperfect spectral corrections

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and gap-filling procedures or calibration problems" (II.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."

#### # Citations

Zhou et al. (2015). Daily underlying water use efficiency for AmeriFlux sites. *Journal of Geophysical Research: Biogeosciences*. DOI: 10.1002/2015JG002947.

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