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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 #4

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The article presents an analysis of GPP, ET and water use efficiency metrics for two flux-towers in Finland in search for drought effects or more generally for soil moisture controls on the carbon and water fluxes. The climate and hydrological regime of these sites restrain appreciable effects of water limitations on GPP, ET and water use efficiency to few days during summer 2006 in the southern site of Hyytiälä (Line 210, Line 241-242, Fig.1 and 3) This is probably the most interesting result of the article but constrains quite significantly the scope of the analysis. The data analysis in Fig. 2 is interesting in certain aspects, but overall, the study leaves me quite doubtful about its novelty in the presented conclusions (see comments below). The link between soil moisture and plant physiologically meaningful thresholds is also very weak. A land-surface model, JSBACH, is also used to reproduce the water and carbon fluxes but serious model limitations, a relatively weak rationale for using the model, and a poor

model-data comparison make this part insufficient.

Major comments

1) I struggle to identify the novel conclusions of the manuscript (beyond the presentation of the data themselves). The main conclusions are: (i) There are only few days of water limitations in only one of the two analyzed sites despite the 11 and 8 years of analyzed data. Interesting result but it partially hampers the scope of the article. (ii) IWUE and EWUE are identifying two different aspects of ecosystem response, with the first more appropriate to capture changes in surface conductance. This is of course important but it is expected too because one depends explicitly on VPD and the other does not. (ii) Ecosystem models, in this case JSBACH, need to have a very good representation of stomatal functioning and its dependence with VPD or humidity, which is known since quite some time (Ball et al 1987, Leuning 1995) and widely debated in literature (e.g., Monteith 1995; Damour et al 2010) and actually included in most of the models.

2) In my opinion, the use of model simulations in the article lacks a clear rationale. The model is simply used to run the same period of observations and to reproduce the same variables which are observed (only transpiration and EUWEt, IWUEt are additionally analyzed). Therefore, there is not really a benefit or the idea to use the model for specific numerical experiments that go beyond observations. If the scope is confined to test the model performance only, also in this case there is not a direct comparison with data. No scatter plot to evaluate magnitude, seasonality, or other aspects of model performance is shown. Even the behavior with respect to the driving variables (Rsw, Ta, VPD and an index of soil moisture) is shown only qualitatively and not quantitatively, since the simulated and observed variables are never presented in the same plot (Fig. 2, 3, 4).

3) I also have doubts about the choice of the model. From the manuscript description, JSBACH has only few layers of soils, which do not allow a proper representation of

soil moisture vertical dynamics (Line 237-240) and most importantly, there is no representation of vegetation physiology with regards to water stress (thresholds for stomatal closure or plant vital functions), or at least this is not described in the article. Water content limits rather than more physiologically meaningful water potentials are used, which leaves the doubt if the selected thresholds have any meaning for the plant response to drought or not (e.g., Hsiao 1976).

4) The manuscript is generally decently written but there are parts, (e.g., abstract, introduction) which can be written much better (see minor comments below). Also the choice of the presented material is debatable. For instance, Sodankylä is one of the two presented case studies but nothing about Sodankylä is graphically presented in the main manuscript. Data uncertainty issues are discussed but not represented. Different figures share the same information; those can be better re-organized to highlight some of the main conclusion, which are not so evident from the current Figures (e.g., Line 325-326).

Specific comments

Page 1. Line 11. I respectfully disagree with this statement; we have a good knowledge and wide body of literature about the carbon and water coupling, from stomatal level to ecosystems (e.g., Katul et al 2012). What it is still problematic is the modeling of the response of vegetation to periods of water stress at different temporal scales (from hourly to multiannual) and at different spatial scale (from a single tree to a region).

Page 1. Line 18-20. This sentence is very badly phrased, what does exactly mean that the decrease in ET is alleviated by increased VPD? If a decrease in ET is observed, this is already implicitly account for changes in VPD. The authors are here referring to the difference between EWUE and IWUE, with the first affected by VPD, why the latter is independent and therefore more indicative of how surface conductance changes. This is, however, not clear from the text.

Page 1. Line 25. What do the authors mean with "deviated groups"? This is explained

only much later in the manuscript. I am not a native speaker but the use of the term "deviated groups" appear, at the very least, awkward to me.

Page 2. Line 42. It is not very clear what the authors mean with "physiological stress" but if they refer to impairment of vital functions and plant mortality, I think that physiological stress may occur much later (at much higher levels of water stress) than reduced carbon uptake.

Page 2. Line 46. I think this sentence could be written much better in English.

Page 2. Line 54-55. This sentence is overly approximate. Ecosystem functioning depends on many more factors than WUE (e.g., nutrient dynamics, species competition and forest demography, just to quote some) and WUE is not simply "closely related" to water and carbon cycles but it is the metric which summarizes how the two cycles are coupled, at least at the flux-level.

Page 2. Line 62-73. I think many of these contrasting results can be simply related to the fact that a water-stress, which is perceived by the plants, occurs or not.

Page 3. Line 96. I wonder if the choice of reporting LAI as "all-sided" rather than as "one-side/projected" as typically done in most of the literature (including for the very same sites, Lindroth et al 2008) is a good choice or not. At the very least, this should be clarified in the text and not only on the Table.

Page 4. Line 113-114. This sentence is not very clear to me. How do you distinguish between filled data of "good-quality" and "bad-quality"? Do you mean that that you discard days with observed low-quality data? Do you mean that you gap fill these data? Do you mean that you discard "half-hourly" periods and you average the others?

Section 2.3. I know that JSBACH is an established model, but the model description is extremely synthetic. I would invite the authors to add a bit more of information. For instance, there is no mention of how the hydrological budget is solved. How do JSBACH deal with transpiration, evaporation from ground, from interception, deep leakage? How root depth-distribution is considered? How vegetation phenology is considered?

Page 5. Line 146. Can the authors better characterize the spin-up? How long did you run? Which period did you use?

Page 5.Line 149. I would state that they were calculated from “observed data” rather than “model forcing”. Are there any differences between the two?

Page 5. Line 165-167. In my opinion, this classification of soil moisture conditions is very arbitrary, since there is no explicit link between the thresholds of SMI and plant physiologically meaningful variables such as “soil water potential” or better “leaf water potential”. While overall, it is clear that with decreasing SMI drought stress should increase, there is no reason to support that drought stress should start at SMI of 0.1 or 0.4. I would suggest avoiding such classification and just having a continuous variable SMI.

Page 5. Line 171. I do not fully understand the rationale of using θsat in place of θfc. Even leaving a part the problematic concept of θfc (e.g. Assouline and Or 2014), the two values may be quite different and they are not interchangeable.

Page 5. Line 173-174. Also for Sodankylä, I do not understand why the authors did not use a soil water retention curve to link soil water contents to soil water potentials and therefore define some plant-meaningful threshold rather than arbitrary values.

Page 6. Line 184. It is a measure of the inverse of the surface conductance.

Page 6. Line 208-209. “deviated group” is awkward wording.

Page 7. Line 235-236. As a follow up of my previous comment, SMI<0.2 does not necessarily imply a “drought” from the plant point of view and therefore the lack of deviation in GPP, ET, with respect to normal conditions should be expected as a consequence of the “lack of drought”.

Page 8. Line 250-251. How many days are those for which we see such a deviation? Are they continuous in time? This is probably one of the most important information to place in the article, which needs to be emphasized.

Page 8. Line 262-263. This is likely a consequence of the cross-correlation between high SMI and unfavorable meteorological conditions (e.g., cold, cloudy).

Page 9. Line 291-294. What is driving this proportionally larger decrease in GPP than ET? Looking from a leaf-level perspective this is hard to reconcile. What is keep sustaining ET if soil moisture is very low? How much is data uncertainty playing a role in such a response? Is it really just the change in VPD?

Page 11. Line 361-365. I agree that is important to highlight uncertainties of EC data, but how do you tackle this problem in the result presentation? There is not any confidence bound around the data, therefore we cannot really establish if some of data-driven result are very robust or not. At the very least, there should be a more quantitative discussion of the uncertainties.

References

Assouline, S., and D. Or (2014), The concept of field capacity revisited: Defining intrinsic static and dynamic criteria for soil internal drainage dynamics, Water Resour. Res., 50, doi:10.1002/2014WR015475.

Ball, J., Woodrow, I., Berry, J., (1987). A model predicting stomatal conductance andits contribution to the control of photosynthesis under differentenvironmental conditions. In: Progress in Photosynthesis Research: Volume 4Proceedings of the VIIth International Congress on Photosynthesis Providence,Springer Netherlands, pp. 221–224

Damour G, Simonneau T, Cochard H, Urban L. (2010). An overview of models of stomatal conductance at the leaf level. Plant Cell Environ 33:1419–1438. doi:10.1111/j.1365-3040.2010.02181.x.

Hsiao TC, Acevedo E, Fereres E, Henderson DW. (1976) Stress metabolism: water stress, growth and osmotic adjustment. Philos Trans R Soc Lond B Biol Sci, 273:479–500

Katul GG, Oren R, Manzoni S, Higgins C, Parlange MB (2012). Evapotranspiration: a process driving mass transport and energy exchange in the soil-plantatmosphere-climate system. Rev Geophys, 50: RG3002.

Leuning, R., (1995). A critical appraisal of a combined stomatal-photosynthesismodel for C3 plants. Plant. Cell Environ. 18 (4), 339–355.

Lindroth A, Lagergren F, Aurela M, Bjarnadottir B, Christensen T, Dellwik E, Grelle A, Ibrom A, Johansson T, Lankreijer H, Launiainen S, Laurila T, Molder M, Nikinmaa E, Pilegaard K, Sigurdsson BD, Vesala T (2008) Leaf area index is the principal scaling parameter for both gross photosynthesis and ecosystem respiration of Northern deciduous and coniferous forests. Tellus Ser B Chem Phys Meteorol 60:129–142

Monteith J.L. (1995) A reinterpretation of stomatal responses to humidity. Plant, Cell Environment 18, 357–364.

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