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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.

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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

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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.

AR: (i) We have revised the manuscript to only focus on the severe drought in 2006 at Hyytiälä site, because no severe drought took place during the multi-year study period at the northern Sodankylä site. The summer drought in 2006 caused severe forest damages in southern Finland (Muukkonen et al., 2015). Using SMI calculated from regional soil moisture simulations over the past 30 years (1981-2010), such extreme drought affecting forest health has been illustrated to be rare and the summer drought in 2006 in southern Finland was the most severe one in the 30-year study period (Gao et al., 2016). Therefore, it is valuable to study the severe drought in 2006 and its impact on plant functioning.

(ii) We aimed to compare the behavior of different water use efficiency metrics under the soil moisture drought. From the literature presented in introduction, we have understood that there is no clear conclusion of the impact of drought on EWUE (increase or decrease), and therefore it is valuable to study the performance of EWUE in drought conditions, as it is widely used metric describing ecosystem level responses. IWUE is a quantity defined as EWUE multiplied with mean daylight vapor pressure deficit and has been found to increase during short-term moderate drought (Beer et al., 2009). In the revised manuscript, we also added uWUE which is developed based on IWUE and a simple stomatal model, to more explicitly assess the role of the stomatal conductance, and to see how uWUE behaves in comparison to the two other metrics.

(iii) We agree that it is ideal that ecosystem models can have a very good representation of stomatal functioning and its dependence on VPD. However, in global ecosystem models, simple representations of stomatal regulation have 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, and the results showed that Ball-Berry model (Ball et al., 1987) to be best in its response to atmospheric drought under non-limited soil moisture conditions. However, the performance of the default stomatal conductance model under limited soil moisture conditions has not been tested before this study. Our results indicate that the combined effects of soil moisture and atmospheric drought on stomatal conductance have to be both taken into account.

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 EWUEt, 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).

AR: The purpose of our study are twofold: one is to find how drought influences plant functioning using observational data; the other one is to see if the model can represent the drought and its impact on plant functioning in line with the observational data. In the revised manuscript (section 3.1), the observed SMI and simulated SMI were compared during the study period and drought year. We agree that the GPP and ET changes with respect to the driving variables show how the modelled GPP and ET perform qualitatively, but this (i.e. how modelled GPP and ET changes due to the drought) is what we are mostly interested in. We also think showing how the GPP and ET values differ under different environmental conditions, provide even more useful information for the model improvements than what we can get from correlations by plotting data in the same figure.

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).

AR: Most global land surface models have few layers of soil due to the limitations in computational costs. Nevertheless, drawn from Gao et al. (2016), we can conclude that regionally the model provides relatively robust estimate of SMI. In the revised manuscript section 3.1, good correlation coefficients were found between the simulated SMI and observed SMI over the study period and especially in the drought year. Moreover, there is parameterization of response of vegetation physiology on drought. We have added the formulations of stomatal conductance model in JSBACH as section 2.3.1. Furthermore, the thresholds are evenly set in terms of SMI. SMI is an indicator of soil moisture drought but not the physiological drought. We work more in the meteorological terms than in the plant physiological terms. The deviating responses among different SMI groups as functions of meteorological drivers reflect the response of vegetation. We agree that the original names for the SMI groups easily led to confusion about soil moisture drought or plant physiology drought. Thus, we renamed the SMI groups to describe the soil moisture conditions as very dry, moderate dry, mid-range, moderate wet and very wet.

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).

AR: We have tried to solve those minor comments below and rewrite parts of the paper. We revised the paper to focus on the severe drought at Hyytiälä site. The Sodankylä site is not included in the analysis anymore as there is no severe drought happened at Sodankylä in the study period. Figures were reselected and recomposed in the revised manuscript.

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).

AR: We tried to simplify this introductory sentence to be: "The influence of drought on plant functioning has received considerable attention in recent years, although our understanding of the response of carbon and water coupling to drought in terrestrial ecosystems still needs to be improved.".

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.

AR: We wanted to mean that the decrease in stomatal conductance can lead to decreased Transpiration, and low soil moisture can lead to decreased Evaporation. However, as the VPD also increases during the soil moisture drought, the increased VPD could stimulates ET to a certain degree. In general, the ET still decreased during soil moisture drought. We have reformulated this part in the revised manuscript.

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.

AR: The "deviated groups" there referred to the group of data under the severe soil moisture drought. We have deleted this term in the revised manuscript.

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.

AR: Yes, we agree the physiological stress may occur at higher levels of water stress when plant starves as a result of continued metabolic demand for carbohydrates. Therefore, we have revised the sentence as: "... which in turn leads to less carbon uptake (diffusion of CO2 into the leaf) and maybe also subsequent physiological stress . . .".

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

AR: We have reformulated the sentence as: "Even though the occurrence of drought is low in northern Europe, the summer of 2006 in Finland has been found to be extremely dry and 24.4 % of the 603 forest health observation sites over entire Finland showed drought damage symptoms in visual examination, in comparison to 2–4 % damaged sites in a normal year (Muukkonen et al. 2015)."

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.

AR: We agree with the reviewer. We have reformulated the sentence as: "WUE can be used to study ecosystem functioning which is closely related to the global cycles of water, energy and carbon."

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.

AR: We do not fully agree with the reviewer about this comment. We think the plants do perceive water stress if such conditions have been reported in those studies. Whether the plants react to water stress and how they react to it, depends on the severity of drought, and also other factors such as species (leaf properties, canopy architecture, rooting depth etc) and plants' adaptation to the local climate.

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.

AR: We have clarified this in the text.

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?

AR: We agree the original sentence was confusing and not totally correct. It has been deleted in the revised manuscript. Actually, the bad quality data was gap-filled, and the gap-filled data were used for averages. The gap filling method for GPP and ET was introduced in the manuscript.

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?

AR: We have added the descriptions of the stomatal conductance model in JSBACH, which is the most relevant part of the model to this work. We have no enough space in the manuscript to introduce details about soil hydrology and plant phenology of JSBACH model, however, please refer to the literatures cited in our manuscript. The five layer soil hydrology scheme has been introduced in Hagemann and Stacke (2015), and the plant phenology has been described in Böttcher et al. (2016).

Böttcher K., Markkanen, T., Thum, T., Aalto, T., Aurela, M., Reick, C. H., Kolari, P., Arslan, A. N., and Pulliainen. J.: Evaluating biosphere model estimates of the start of the vegetation active season in boreal forests by satellite observations, 8, 580, doi:10.3390/rs8070580, Remote Sens., 2016.

Hagemann, S. and Stacke, T.: Impact of the soil hydrology scheme on simulated soil moisture memory, Climate Dynamics, 44, 1731-1750, 2015.

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

AR: Prior to the actual simulation, a 30-year spin-up run was conducted by cycling meteorological forcing that was used for the actual simulation to obtain equilibrium for the soil water and soil heat balances.

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?

AR: We agree with the reviewer. This paragraph seems no need to be there anymore as the data processing method has been updated in section 2.2. We have deleted this paragraph.

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.

AR: We have renamed the groups of SMI to described soil moisture conditions (very dry: $0 \leq$ SMI < 0.2, moderate dry: $0.2 \leq$ SMI < 0.4, mid-range: $0.4 \leq$ SMI < 0.6, moderate wet: $0.6 \leq$ SMI < 0.8, very wet: $0.8 \leq$ SMI <1) rather than drought conditions (severe drought: $0 \leq$ SMI < 0.2, moderate drought: $0.2 \leq$ SMI < 0.4, mid-range: 0.4 $\leq$ SMI < 0.6, moderate wet: $0.6 \leq$ SMI < 0.8, very wet: $0.8 \leq$ SMI <1). In the revised manuscript, we used continuous color bar for SMI in the figures. However, we still need the SMI groups for fittings to describe the responses of GPP and ET to environmental variables under different soil moisture conditions.

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.

AR: This is a matter of introducing an offset. We were not meaning the two parameters are changeable. As θFC acts as a proxy for θSAT in the JSABCH model technically (Hagemann and Stacke, 2015), for consistency, the θSAT was used instead of θFC when calculating SMI based on the observed soil moisture data. By doing this, the SMI still indicates the soil moisture conditions and simulated SMI and observed SMI are comparable.

Page 5. Line 173-174. Also for Sodankylä, I 1do 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.

AR: This is a very good suggestion and worth trying. However, we have decided to leave Sodankylä out and mainly focus on Hyytiälä site, as there was no severe drought happened in our study period at Sodankylä.

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

AR: It is a measure of the surface conductance, but not inverse. Our original formulation was correct.

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

AR: It has been deleted.

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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".

AR: Yes, we agree with the referee. Because there is no severe drought in the study period, we decided to not include the Sondankylä site anymore.

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.

AR: We agree with the referee. In the revised manuscript, we have emphasized and gave detailed information about the severe soil moisture drought in 2006 at Hyytiälä in section 3.1: "The SMI in the summer of 2006 showed a period with SMI evidently lower (< 0.2) than in other years during the 11-year study period. According to the in situ observed SMI, in the summer of 2006, there were 37 consecutive days (23 July to 28 August) with SMI lower than 0.2, and 17 consecutive days (1 August to 17 August) with SMI lower than 0.15. The lowest SMI from observation was 0.115 on 16 August 2006. The simulated SMI was generally smaller than the observed SMI in the summer of 2006, showing 42 consecutive days (17 July to 27 August) with SMI lower than 0.2, and 33 consecutive days (26 July to 27 August) with SMI lower than 0.15. The lowest SMI from simulation was 0.052 on 15 August 2006."

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).

AR: Yes, we agree this could be the reason. We have added this in the discussion.

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

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in such a response? Is it really just the change in VPD?

AR: The low soil moisture leads to stomatal closure, and therefore decreased GPP and Transpiration. The larger decrease in GPP than in ET at the ecosystem level was because that the increased atmospheric demand of water (VPD) stimulated Evaporation from soil. The reason has been discussed in the discussion part.

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.

AR: It is difficult to provide any confidence bound in the figures. Instead, we could add some general discussion related to the uncertainty of EC flux data which is typically 20-30% for annual carbon budget Baldocchi (2003) and Aubinet et al. (2012).

Aubinet, M., Vesala, T., and Papale, D.: Eddy covariance: a practical guide to measurement and data analysis, Springer Science & Business Media, Netherlands, 2012.

Baldocchi, D. D.: Assessing the eddy covariance technique for evaluating carbon dioxide exchange rates of ecosystems: past, present and future. Global Change Biology, 9, 479–492, 2003.

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