Biogeosciences Discuss.,
doi:10.5194/bg-2016-198-AC5, 2017

Biogeosciences
Discussions

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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Received and published: 18 April 2017

General :

The general topic of the manuscript falls well within the scope of the journal. The manuscript presents an interesting study where the authors used eddy covariance (EC) method and a land surface model (JSBACH) to assess the response of daily water use efficiency (WUE; both ecosystem and intrinsic) to drought in two boreal forest ecosystems in Finland. The manuscript is well-written but may benefit from careful editing. The manuscript contains some interesting results, and I recommend addressing some comments and suggestion raised below to make it clearer.

Specific comments:

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Was nighttime data included in the analysis? Obviously, there is no assimilation during the night and the EC system may produce or the LSM may simulate negative ET values. This may not be easily detected if daily flux sums, as opposed to sub-daily fluxes, were used but the computed WUE values would be fundamentally flawed and have no meaning. Some of the simulated results still contain negative ET values but no corresponding WUE (IWUE) values are shown for such data in the graphs. I recommend that this issue be addressed in the manuscript.

AR: Agreed. The nighttime data were included in the analysis in the discussion manuscript. In the revised manuscript, according to reviewers' comments, only half-hourly data with shortwave radiation (Rs) larger than 100 W/m2 were selected for the aim to select the effective time for plant photosynthesis. By doing this, data with negative GPP and ET were also excluded. The data selection process is described in section 2.2.

It is not clear how the authors treated data during rainy days. Rainy days may underestimate WUE as evaporation from the canopy surface would be high during such times. It is vital that the conclusions drawn in this study do not include such low WUE values.

AR: Agreed. In the revised data processing, rainy days and certain amount of dry days after the rainy days were excluded. The data selection process is described in section 2.2.

Lines 109–114: Could the authors provide information as what percentage of the data was observed and/or gap-filled for CO_2 and H_2O fluxes. The authors stated that only good quality gap-filled data were used in the analysis. It is not clear whether data were discarded at half-hourly or daily time scale; or what percentage of 'good quality gap-filled data' was regarded good enough to be used in the analysis.

AR: 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. The gap filling method for GPP and ET was introduced in section 2.2

in the manuscript. The percentage of gap-filled half-hourly CO2 (for deriving GPP) is 35.8%, and the percentage of gap-filled half-hourly latent heat flux (for deriving ET) is 44.4%.

Lines 115–118 and lines 147–150: Were these (Ta, Rs and VPD) daily averages? If so please specify. Also in all the graphs where these weather variables are used. I would prefer total daily Rs to daily average Rs values though. Do observations and simulations use the same weather variables (I would assume so)? However, I see quite a number of 0 W m-2 Rs observations (at Hyytiälä which is south of Sodankylä!) but not so many in the simulations. These could be because the corresponding simulated y-axis variables were 0 as well. The question is how possible is it to have daily Rs of 0 W m-2 in the summer.

AR: Due to the new data processing for the revised manuscript, the Ta, Rs and VPD of the selected half hours in the selected days were averaged to represent the daytime mean Ta, Rs and VPD that effective for plant functioning. The daytime mean Ta, Rs and VPD have been specified in the text and figure captions. For consistency with Ta and VPD, having just Rs per day does not make sense. We realized that the 0 W/m2 Rs in the observational data is missing or bad quality data. However, those bad quality Rs has been gap-filled as the model forcing. In the revised manuscript, we selected the time periods with half-hourly Rs larger than 100 W/m2 according to observational data for both observed flux and simulated flux. Thus the problem is not exist anymore in the revised manuscript.

Lines 119–122. Time Domain Reflectometry (not Reflection). Also theta probe is not a technique, it is a sensor. It would be better if you mention the names (models) of the sensors and manufacturers, and readers could find out the techniques should they be interested.

AR: The soil water content at Hyytiälä was measured at 1-h intervals by the TDR-method (Tektronix 1502 C cable radar, Tektronix Inc., Redmond, USA) connected to a data logger (Campbell 21X, Campbell Scientific Ltd, Leics., UK) via multiplexers (SDMX50, Campbell Scientific Ltd, Leics., UK). We are not using Sodankylä site for our study in the revised manuscript.

As the soil water content forms the core of the simulation results, it would be helpful if the authors could provide more information on sensor types, how they were installed (vertically, horizontally or make use of access tube), if they were static or portable, measurement (time) interval, replications at a site, etc. Besides, was the soil water content for each sensor and depth calibrated against standard gravimetric measurements?

AR: At Hyytiälä site, instrumentation for soil moisture was installed horizontally in the vertical face of five soil pits for each soil horizon (humus at 3 cm depth, the eluvial horizon at 8 cm depth, the illuvial horizon at 19 cm depth and parent material at 55 cm depth) 2 years before measurements were taken. The instruments were installed in the middle of each soil horizon, in the undisturbed soil at 20±30 cm distance from the face of the pit. Samples for measuring soil physical and chemical properties were collected from the walls of the pits during the excavation. The pits were filled with the original soil keeping the soil layers in the original order of excavation. We study the soil water content in mineral soil layers with the humus layer excluded. In the soil moisture data, the depth of mineral soil layers starts from 0 at the mineral soil surface.

Why was the 0 to -5-cm soil water content at Hyytiälä disregarded in the observed vs simulated comparisons?

AR: Because the soil moisture at the top mineral soil (i.e., layer-1) is too sensitive to climate varibility, which is not representative to show the soil moisture dynamics in the root zone. This is explained in the text in the revised manuscript.

Lines 161–167: Again, it is not clear why observed and simulated SMI for layer 1 were not used, at least for Hyytiälä. And, how could the observed and simulated SMI be compared when the depth of the third layer is different for the two (at Hyytiälä)?

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AR: The 1st layer of the measured and the simulated soil moisture were not adopted because there is too much variability of the surface soil moisture in response to climate variability. This has been explained in the text in the revised manuscript. We selected the second and the third observed layers to cover the root depth. This has been correlated with simulations to select the best correspondence. The difference remains to some degree. In the revised manuscript section 3.1, the simulated SMI that calculated with simulated soil moisture and soil parameters in JSBACH has been compared with the observed SMI that based on observed soil moisture and measured soil parameters. The simulated SMI agreed well with in situ observed SMI over the 11-year study period, with a correlation coefficient (0.625) and a root-mean-square error (RMSE) of 0.225. Moreover, a very good time correlation (0.965) between simulated and observed SMIs were found for year 2006, despite the simulated SMI is systematically lower than the observed SMI (RMSE = 0.12).

165–179: Lack of measured soil parameters was the reason behind adopting different soil moisture classification units for the two locations. If there was continuous long-term soil water content data for so many years, it would not have been so difficult to derive these soil parameters (at Sodankylä). Saturated and field capacity soil water content may be estimated in spring following snowmelt, and wilting point from extended dry periods later in summer.

AR: We agree with the reviewer's opinion. It would be good to estimate the soil parameters from long-time series data. However, the whole data series which is 8-year (2001-2008) data for Sodankylä site were used. Nevertheless, since there was no severe drought in the 8-year study period at Sodankylä, we decided to not include this site in the revised manuscript but mainly focus on the severe drought and its impacts at Hyytiälä site.

Assuming the soils are similar at the two sites, it seems unlikely that the very wet conditions at Sodankylä would only have a soil water content of 13 to 16 .

AR: The soil type is podzol at both sites. However, the mineral soil in Hyytiälä (the composition of mineral soil in 0-22 cm: clay 6%, silt 29%, sand 38%, stones 27%) is much finer than Sodankylä (the composition of mineral soil in 0-20 cm: clay 0.4%, silt 5.1%, sand 90.8%, stones 3.8%). This explains why the volumetric soil water content is much lower in Sodankylä.

Lines 194–199: Could the authors provide R2 and P values for the graphs? What was the improvement in R2 and P values due to adopting exponential rather than linear relationship between GPP and ET or T?

AR: The reason for adopting exponential rather than linear relationship is that more physiological relationships than just linear were used when existing. The R2 and P values for the relationships between GPP, ET or T and environmental variables (Rs, Ta and VPD) are provided in the table in the supplementary of revised manuscript.

Lines 205–208: In general, there are more points in zone (b) than in zone (a); and zone (b) includes a larger range of the environmental variables. Both zones share more common than different environmental variables. I wonder if the environmental variables could be used at all to tease out differences in drivers between the two zones (apart from SMI).

AR: This is exactly what we wanted to present that the zone (a) and zone (b) share more common environmental variables but have different soil moisture conditions. Moreover, in the revised manuscript, due to the new process of data, a group of data under low soil moisture content (encircled with a dashed line in Fig. 2(a)) showing GPP values lower than other days. The ET values of this group are also located in the lower end, but just partly lower than ET values on other days. It is found that the days in this group are all with very low soil moisture condition (SMI < 0.15). The old group (a) is not exist anymore with the exclusion of rainy days.

Line 222: There are negative simulated ET values (Figs. S1, S2, S5, etc.). Could the authors explain how they treated such data when determining WUE?

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AR: Those negative simulated ET values were dampened by daily averaging due to nighttime condensation. Through the reprocessed of the data for the analysis, 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 (Rs) 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. Therefore, there is no problem of the influence from negative ET on WUE anymore.

Lines 294–297: It would be better if the y-axis of Figs. 4 and S5 were IWUE rather than GPP$\times$VPD. As it is, it is difficult to tell whether IWUE increases or decreases with ET.

AR: The increase or decrease of IWUE can be seen from the ratio of GPP$\times$VPD to ET, as the definition of IWUE.

Be consistent in using either soil water content or soil moisture throughout.

AR: We will use soil moisture throughout the text.

Line 18 to 20: This is not clear at all. Rewrite please.

AR: We have revised this sentence.

Line 58: Change ". . . the ratio of GPP and ET." to ". . . the ratio of GPP to ET."

AR: Agreed. It has been changed.

Lines 80 to 83: Does not read well. Consider rewording–for example to ". . . the various land ecosystem model simulations highlight the current uncertainty with regard to plant physiology (water use) in response to drought."?

AR: We have revised the sentence as: "The various land ecosystem model simulations highlight the current uncertainty about plant physiology (water use) in response to drought in models (Huang et al., 2015; Jung et al., 2007)."

Lines 91 and 92: Consider removing this sentence and start with the actual experimental sites.

AR: Agreed. This part has been rewritten for the Hyytiälä site.

Line 93: 'annual temperature' should read 'annual air temperature'. Also in Table 1 (Lines 600–605).

AR: Agreed. It has been changed.

Line 116: Humidity is ambiguous term to use. Be specific which measure of atmospheric humidity was used.

AR: Agreed. It has been changed.

Line 153: Delete the phrase 'to be able'

AR: We have deleted the "to be able".

Line 157: ". . .] and θWILT is the . . ."

AR: This has been changed according to the comment.

Line 170: ". . . (i.e., soil water content . . ."

AR: This has been changed according to the comment.

Line 170: ". . . and θWILT= . . .)"

AR: This has been changed according to the comment.

Line 171–172: Confusing statement. This changes the definition of SMI and probably the boundaries of SMI set-up above. And, if θFC acts as a proxy for θSAT, then should not θFC be used instead of θSAT?

AR: Because θFC plays the role as 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. Using θFC or θSAT is a matter of introducing an offset.

Line 192 onwards: ET/T is ambiguous. Use ET or T instead.

AR: This has been changed according to the reviewer's suggestion.

Line 248: Replace 'slop' with 'slope' in Tables S1 and S2.

AR: Indeed. It has been changed.

Line 300: Simulated EWUE is not presented in Fig. S5.

AR: The simulated EWUE is now shown in the supplementary figure.

Lines 304–305: This sentence is not a result. Move it to the Discussion Section.

AR: We agree with the reviewer. However, this part about Sodankylä results has been deleted in the revised manuscript.

Lines 313, 320 and 337: Consider changing the word 'disturbance'. Some suggestions: Line 313: 'Moreover, GPP and ET were decoupled and EWUE decreased . . .', Line 320: '. . . there was no deviation in GPP, . . .', and Line 337: 'The simulated daily ET data contained frequent . . .'.

AR: We have revised the sentence in Line 313. The other two sentences are not exit in the revised manuscript due to the exclusion of Sodankylä site in the revision.

Line 341: '. . . limitations on GPP and ET or T relationships under . . .'.

AR: We do not agree to add "relationships" in the sentence as we referred to the impacts of drought on GPP and ET.

Line 344: '. . . when soil moisture was under . . .'.

AR: We agree with the reviewer about this grammar mistake. The sentence has been modified.

Line 370: Delete 'as a whole'

AR: Yes, this has been deleted.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-198, 2016.