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Interactive comment

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 #5

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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 carful editing. The manuscript contains some interesting results, and I recommend addressing some comments and suggestion raised below to make it clearer.

Specific comments

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 val-

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ues. 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.

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.

Lines 109–114: Could the authors provide information as what percentage of the data was observed and/or gap-filled for CO2 and H2O 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.

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.

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.

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

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(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?

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

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)?

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.

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

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?

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).

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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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.

Technical comments

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

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

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

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

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

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

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

Line 153: Delete the phrase 'to be able'

Line 157: "...] and θWILT is the ..."

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

Line 170: "... and θWILT= ...)"

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?

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

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

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

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

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 . . .’.

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

Line 344: ‘. . . when soil moisture was under . . .’.

Line 370: Delete ‘as a whole’

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