

Interactive comment on “Characterisation of ecosystem water-use efficiency of european forests from eddy covariance measurements” by F. G. Kuglitsch et al.

Anonymous Referee #3

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General Comments

This manuscript attempts to synthesis information on different estimates of WUE from a broad spectrum of forests, an issue which is clearly relevant to the potential for increased drought stress in future climates. The use of many different sets of data is useful and should improve predictive understanding. However, the manuscript suffers from lack of clarity in several locations and inadequate analysis of data (soil moisture, diffuse light etc). Further the concept of WUE is fraught with problems due to the assumptions that the relationship between the loss of water and carbon gain is linear and has no intercept. The results clearly show this is not true for these forests; appro-

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ropriate analyses should be conducted to understand how such nonlinearities/intercepts challenge our calculation and understanding of WUE.

Specific Comments

-pg. 4483 lines 16-17 awkward wording with (since); used twice

-there are many instances where GPP is not subscript in WUEGPP (starting with line 11 pg. 4484)

-pg. 4484 lines 10-15 imprecise language is used here, meteorology is often short time scale weather phenomena-climate should be used instead, what does (hardly related) mean? What is meant by (10% high GPP rates)?;

-pg. 4484 lines 17-23 the first and last sentence of this paragraph are redundant

-pg. 4485 lines 1-3 suggesting that a topic has received a lot of interest and then citing a 1958 paper only does not provide much support, citing a key paper from the main disciplines would be better

-pg. 4485 lines 6-8 sometimes WUE is defined as assimilation to conductance (intrinsic see recent review by Seibt et al Oecol 2008), I donot like this type of WUE but it is used and is not just water loss

Figure 1 should show some overlap between the different disciplines, they do not have clear boundaries as implied in the figure.

-pg. 4485 line 12 epsilon is not defined

-pg. 4487 lines 19 need to define how GPP is calculated (especially the respiration component given the problems of nighttime turbulence). Given the number of sites and very different conditions, there are probably site related biases based on the methods. This needs to be analyzed or logically dealt with in the results and discussion.

-pg. 4487 lines 22-23 how is an active canopy defined? Is the criteria the same for

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all sites? Is it based on drivers, plant status or some calendar day? How do these assumptions change the calculations? If it is based on latent energy then the winter respiration season is completely ignored even though it should be included in GPP. Since GPP is used as the main WUE value in much of the paper, respiration is occurring outside of the time period of an (active canopy) so how might this winter respiration impact GPP calculations and assumptions?

-pg. 4488 lines 5-11 one of the major problems with using any of the potential WUE efficiency calculations is that it assumes there is no intercept in the relationship between carbon gain and water loss. Since the value is usually calculated as a ratio, the lower the WUE the more this intercept assumption plays a role (See Fig.2 of paper). Methods three and four deal with this issue, but I would like to see a more explicit comparison of how WUE calculations change at low values due to this intercept problem. Further the ratio calculation assumes the relationship is linear, which is rare in most ecosystems (see Fig. 3 of paper). This problem directly impacts the comparisons in this manuscript.

-pg. 4488 lines 23-25 something is wrong with the equation in my pdf file, which stats package was used to run these results?

-pg. 4490 line 1 Currently the last section of the manuscript is cumbersome to read because of the combination of results and discussion. I suggest splitting these apart; combining them only works for relatively short papers. After the split, the authors will be able to see that sometimes the discussion reads more like a rehash of the results and that synthetic discussion is missing from some key areas. For instance no literature is cited nor any synthetic discussion made of the different calculations of WUE (slope vs. ratio methods).

Fig. 5 needs to include the time units for WUE on each axis and something is wrong with the caption.

-pg. 4491 lines 9-13 In this section of the discussion, it would be nice to see a separa-

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tion of the impact of stomatal conductance on assimilation from biochemical limitations. Most models assume that most of the reduction is due to stomatal limitation but some is biochemical and the proportion of biochemical increases with soil drought (e.g. Grassi & Magnani PCE 2005).

-pg. 4491 lines 24-29 and 4492 lines 1-9 there is little analysis to support this portion of the discussion. The authors should actually include diffuse radiation in their analysis (e.g. Law et al Ag For Met 2002) the impact of VPD and soil moisture on WUEGPP or NPP should also be explicitly analyzed between sites(e.g. Tang et al JGR Bio 2006).

-pg. 4492 lines 20-21 does (set to) mean that the data were filtered to these values? Are the results the same if different regions of the climate parameter space are used to filter the data?

-pg. 4493 lines 1-2 could this also be due to lower respiration? Does the peak GPP occur when temperature peaks?

-pg. 4494 1-7 I donot follow what this sentence is trying to say, it appears that the first clause is incomplete.

-pg. 4494 lines 23-27 the lag could also be caused by capacitance in the plants so that ET is lagging its drivers (see work by Phillips et al 2002, 2003)

-pg. 4495 lines 2-5 vpd does not impact conductance directly but indirectly through transpiration and/or leaf water potential (see classic work by Mott and Parkhurst PCE 1991, recent papers by Franks et al in Tree Physiology and PCE)

-pg. 4495 lines 6-8 I don't follow that VPD impacts GPP more than ET. GPP is also a function of respiration which, while impacted by conductance, is not as directly linked as assimilation. I suggest that the authors try analyzing GPP and ET responses to VPD separately to resolve this issue. See work by Bowling et al Oecol. 2002 and the many papers since that look at the link between VPD, conductance and respiration on daily time scales. Also see work by Tang et al JGR Bio 2006 which used sap flux to

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investigate the role of VPD in ecosystem WUE.

-pg. 4495 lines 15-17 the discussion of these citations is weak and is not well connected to the results.

-pg. 4496 lines 4-5 I interpret this completely the opposite. The fact the ratios do not conform to the slope analysis indicates that the GPP to ET relationship is nonlinear with an intercept so a simple ratio is inappropriate.

-pg. 4496 lines 6-13 keep the conclusion focused on the findings of this paper. The use of sap flow and issues of evaporation should be in the discussion. This is also true of lines 26-29, these should be removed and incorporated into the discussion

-pg. 4497 lines 3-7 comparison to other literature should be in the discussion, (is no implicitness) does not make sense

Pg. 4497 lines 10-22 much of this is just redundant or rehashes statements already made elsewhere, it should be mostly trimmed out

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