

Interactive comment on “The importance of radiation for semi-empirical water-use efficiency models” by Sven Boese et al.

Anonymous Referee #3

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The authors introduced a new term into they called “water use efficiency model” which was from Zhou et al., (2014). The rationale is that by fitting ET with GPP using this model, they find bias in many sites and the seasonality of this bias is consistent with global radiation, and global radiation can directly affect transpiration independence of VPD. This added term can explain up to half of the predicted transpiration. This manuscript reads well and is within the scope of Biogeosciences. However, I have some comments to reduce its importance.

General comments:

Introduction: Ambiguity in concepts such as WUE models, physiological WUE models? Are the authors talking about stomatal conductance models? Please clarify them and provide details. Several statements are confusing and sometimes incorrect. A few

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others need reference. Please see my specific comments below.

Methods: I have concerns about the proposed models and selection criteria for the best model. The concerns include whether and how the authors test the collinearity between the variables such as R_g and $GPP \cdot VPD^{0.5}$ in the model fitting; how the authors deal with the interactive terms among those variables; In addition to MEF, index such as AIC or AICc are needed to account for possible Over-parameterization? The findings are likely to change due to different ways to construct and evaluate the models. Please also see my specific comments below.

Results: In results, in addition to the MEF, I would like to see the distribution of two other parameters ($uwue$ and r) of all the sites. Also see my specific comments below. It would be great if the authors could provide specific data used in this study for interested readers to reproduce these results.

Discussion: All the proposed models have their own assumptions and their possible violations. Please discuss them as well on how these violations could affect the results.

Specific comments:

p1, line 19: global change instead of Global Change

p2, lines 5-15: this paragraph needs to clarify the difference between existing WUE models. According to my understanding, Beer et al., 2009 assume the ratio of c_i/c_a is constant and therefore derives $WUE \propto r/VPD$; whereas in Zhou et al., (2014, 2015), they set up c_i/c_a depending on square-rooted VPD.

P2, line16: what is physiological WUE models? Did the authors mean stomatal conductance models?

P2, line 19: I may misunderstand this sentence. I thought the conductance term g_0 is in the stomatal conductance models in ecosystem models (de Kauwe et al., 2013).

P2, lines 22-29: there are several confusing/incorrect statements in this paragraph.

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“The models implicitly assume that, . . . , the ratio of both is constant with regard to this factor”: the ratio GPP/ET is never constant and is considered to be proportional to vpd or squared rooted vpd depending on assumptions (Zhou et al., 2014). Line 26: I did not find the result in Leuning et al., 2008 about the nonlimiting behavior of PET to radiation. “This is a second process. . . cannot accommodate”: again I do not think it is a true statement (see de Kauwe et al., 2013). Reference is needed to back up the statements in the first two paragraphs.

P2, line 10 Baldocchi et al., 2001 is not an appropriate reference for La Thuile database. Please cite this source: FLUXNET Synthesis Dataset (La Thuile 2007). Available at www.fluxdata.org

P4, line 6 the sentence was not complete.

P4, line 8: Not sure how water limitation can affect collinearity of parameters? Please clarify.

P4, lines 9-10: please provide reference for this statement.

P5 line 12-15: the authors tried to test the possible interactions between the two additional variables using equation (6). Would not it be an interactive term such as VPD*Rg in the equation? Please clarify.

P5, lines 24-28: More details are needed on the variance and covariance for each of the variables including GPP and ET, because this variance and covariance directly affect your L-M algorithm and likely results.

P5, line 30: an AIC or AICc index is more appropriate than the MEF, because there is likely over-parameterization of your models. Please provide these index in addition to MEF.

P7, line 15: STO? Typo?

P9, line 19-21. This is interesting finding. Could it be possible for the authors to provide

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this similar figure for each of the sites in the supplementary materials for the readers to eyeball the site difference or similarity?

P16 line 19: . . . degree on variables other?? Truncated sentence?

P17 line 2: the cited paper Keenan et al., 2013 is not listed in reference list.

âĀĀ Reference

Kauwe, Martin G., et al. "Forest water use and water use efficiency at elevated CO2: a modelâĀĀdata intercomparison at two contrasting temperate forest FACE sites." *Global Change Biology* 19.6 (2013): 1759-1779.

Keenan, Trevor F., et al. "Increase in forest water-use efficiency as atmospheric carbon dioxide concentrations rise." *Nature* 499.7458 (2013): 324-327.

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