Authors' response to the comments of referee #2 on the article "Identification of a general light use efficiency model for gross primary production" by J.E. Horn and K. Schulz in Biogeosciences Discussions

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RC - Referee Comment; AR - Authors' Response

1 Response to the general comments of referee #2

RC: "The authors described their processes for the formulation of a light use efficiency (LUE) model, and quantified and analyzed model parameters across many flux tower sites in great detail. However, the paper has some major flaws, including:

(1) The model formulation might not be appropriate to address the issues intended. The author used the additive approach to represent the impacts of moisture and temperature on LUE. This is just one of the many possibilities. The multiplicative approach mentioned by the authors is another one. Yuan et al., 2007 used the the Liebig law (the most limiting factor controls LUE) in GPP model (The authors wrongfully referred Yuan's paper as multiplicative approach, see p7691 lines 5-6). The results on the importance of moisture and temperature may well be dependent on specific model formulations. For example, using the Liebig-law approach, Yuan et al (2007) found that temperature only controlled the start and end of the growing season, and LUE was solely controlled by moisture across all sites. More reliable approaches should be used to attribute the impacts of different environmental factors on LUE. Optimal mathematical solutions doe not always mean that the solutions really represent the underlying mechanisms. Therefore, the results generated from this study are very questionable.

(2) Most of the comparison and discussions about LUE values throughout the paper was invalid. It is meaningless and incorrect to compare LUE values derived from two different equations/models. For example, even if use the same f(T) and g(W), the additive approach (this study) would generate smaller LUE values that those generated from using a multiplicative approach (because f(T) and g(W) are usually smaller than 1)."

AR: (1) As mentioned by the referee, it is *one* possible approach to use such a model as presented in the discussion paper. The authors do *not* claim that it is the only one. But the authors think

that it is worth testing another approach than the usually applied models with a global or vegetation class specific maximum or potential light use efficiency and down-regulating scalar(s) for the reasons stated in section 2.2 in the author's response to the comments of the referee #1. It is also stated more clearly in the revised manuscript (p. 4, r. 24ff). The proposed approach has several advantages as pointed out in the section "Discussion" on p. 7691 in the discussion paper and p. 24/25 in the revised manuscript: maximum ε vs. potential ε; weighting of the subfunctions which also reduces prediction uncertainties due to parameter calibration uncertainties; the avoidance of universal assumptions on environmental optima across biomes; the weighting factor as important site-specific characteristic suitable for regionalization strategies. We apologize for wrongly quoting Yuan et al. (2007) on p. 7691 as example for a "multiplicative" approach instead of classifying it clearly as example for a model which uses a potential ε that is often not reached by vegetation stands in reality; we corrected that (p. 24, r. 25ff; p. 25, r. 10ff).

The referee states that "more reliable approaches should be used to attribute the impacts of different environmental factors on LUE." This is exactly our opinion. One solution to identify these factors is the analysis of measurement data at the scale at which the sought model is to be applied; we explore this approach in our study following Jarvis et al. (2004). A possible modeling approach to account for varying influences of environmental drivers is the comparison of these influences at each time step according to Liebig's law as applied in Yuan et al. (2007). Another one could be to assign a site-specific influence-factor to the functions describing the environmental drivers as proposed in this study; this weighting factor can be expected to be a site-specific characteristic feature and therefore suitable for a model regionalization approach.

Further, the referee states that "optimal mathematical solution do not always mean that the solutions really represent the underlying mechanisms". We are not sure if this refers to the data-based approach presented in the discussion paper or to the LUE models in general. That approaches founded on a sound scientific basis should be applied and alternatives to current light use efficiency models should be sought and tested is exactly our opinion. We approach this problem with a data-based method in which the functions representing the environmental factors are obtained directly from measured data. A strategy following the site-specific calibration of the parameters of course has to show that it is possible to extrapolate the optimized parameters. This is done by the authors in a follow-up paper.

The above mentioned statements by the referee would also hold exactly with the same argumentation e.g. for the mentioned model proposed by Yuan et al. (2007). So why does the mentioned model better represent the "underlying mechanisms" than the one proposed in our paper? Why does the mentioned model or any other model using biome-specific or even global constants/optima better describe the "underlying mechanisms" of fluxes that are representative for study sites which are such different as in our study or that of (Yuan et al., 2007) and which contain plenty of heterogeneity concerning plant/species distribution with e.g. different routing depths or soil hydraulic properties? Why has an weighted additive approach less physical explanation or reasoning behind it? Why are our results "questionable"? While not having intended to present a comprehensive LUE model comparison, we did compare our model structure to other LUE modeling approaches and found at least equally good fits to the measured/derived gross primary production.

(2) Concerning the "comparison and discussions about LUE values throughout the paper", we would like to stress that we compared only the maximum LUE and only in one chapter (the discussion). The maximum LUE (ε_{max}) in our model is reached when both subfunctions are maximal, thus their sum is 1; in the multiplicative approach, the maximum LUE is reached (theoretically) when the product of the subfunctions is one. We are aware of the fact that the global maximum such as used in the studies

of Yuan et al. (2007) and Yuan et al. (2010) is at most sites not reached by the vegetation stands and "realized" by the model, and thus represents a theoretical, *potential* light use efficiency. This is exactly our criticism of such an approach with a global maximum/potential LUE – or one fixed optimal temperature for all biomes. But we think it is justified to point out (p. 7693 in the Discussion Paper), that the highest ε_{max}-value calibrated in our study equals the globally optimized ε_{max}-value in Yuan et al. (2010). Comparing our ε_{max}-values with that of the modeling study of Mäkelä et al. (2008) we just noted that their calibration resulted in a (compared to other sites) relatively high ε_{max} at a site, at which the highest forest ε_{max}-value was reached in our study; this should be justified, too. We furthermore compared our calibrated ε_{max}-values to those stored in the look-up table of the MODIS GPP product and to those retrieved by (Yang et al., 2007) for the conterminous U.S. with a machine learning approach based on EC data. In most of the cases (comparisons made), we compared the calibrated ε_{max}-values to studies which analyzed *observed ε*-values (Goetz and Prince, 1999; Schwalm et al., 2006; Garbulsky et al., 2010; Lindroth et al., 2008). This is valid since our ε_{max} is a value actually realized by the model, hence, it is *not* a *potential* LUE. Yuan et al. (2007), too, compare their "realized" values to other studies, even to studies which related APAR to NPP instead of GPP (Yuan et al., 2007, p.203). In the revised manuscript, we compare the results of the ε_{max} calibration to other ε_{max} values reported, since we think this is an important issue to be discussed. However, we have condensed this discussion in the revised manuscript (p. 25, r. 22 – p.26, r. 8) – we agree that we devoted somewhat too much attention to it in the discussion paper.

Finally we would like to state that we are somewhat confused by the reviewers statement of the inclusion of "some major flaws" in our paper. He/she has pointed out two issues (the additive nature of our approach and the discussion of ε_{max}) that (we think) has been clarified and properly addressed in this revised version of the manuscript. Concerning other points it would have been very interesting and helpful if the referee would have provided more specific hints about the nature of those.

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