

Author response for

Prediction of photosynthesis in Scots pine ecosystems across Europe by needle-level theory

by Pertti Hari et al. in *Atmos. Chem. Phys. Discuss.*

Dear Dominick,

Thank you for the comments. The main comment of the reviewer 1, the scaling issue, is showing a difference at a conceptual level between a process-based model and EC measurement oriented research, and we have tried to clarify our view below in red.

Reviewer 1:

General comments:

The clarity and organization of the manuscript have been improved after the revision made by the authors. However, the authors did not take all the comments from both reviewers into account and explain their rationale and actions well. It is for this reason that I do not suggest publications in ACP unless the authors can clearly explain what is the information embedded in the ecosystem-specific scaling coefficient when up-scaling the leaf-level model to ecosystem-scale GPP.

Response: To make it easy, any measure on a specific scale reflects an integrated or averaged value at that certain scale. Any change in scale needs a “translating” coefficient and the direction, up or down, does not matter. It seems usually more “natural” to upscale from a more detailed to a more aggregated scale than to downscale. However, changing a scale implies always a loss of information, either on details or on the aggregated state. Therefore, it seems a bit awkward that the reviewer wants a “clear explanation” on the “information embedded in the ... scaling coefficient”. If there would be such a clear explanation, it would be sufficient to just sum up in the case of upscaling, no need for any coefficient that represents the “loss of information”. In the best case, we can assume that the scaling coefficient carries some information (=facts that we know about something) on the average noise gain (=loss of information) due to the scaling.

A note on the use of the term “upscaling”: We used this term in our original manuscript only once, in the introduction and referring to studies done by other researchers. We stated “The upscaling from leaf to ecosystem scale is done either using ‘big-leaf’ approaches (dePury and Farquhar, 1997; Wang and Leuning, 1998), or by incorporating the impacts of 20 vertical canopy structure on microclimatic drivers, solar radiation in particular, via multi-layer models of different complexity (Leuning, 1995; Baldocchi and Meyers, 1998)”. This does not imply that we used the exact approach as given in the literature cited.

In the latest manuscript, in the two paragraphs following the above citation we draw the attention of the reader to the GPP operating on the scale of the whole ecosystem. We acknowledge the processes in the underlying scales and finally point out: “This common functional basis generates common regularities in the behaviour of photosynthesis”. Therefore, our main assumption is, that the regularities are emergent and on a common basis, i.e. scale free. They have to be apparent on all levels of organization that make up the complex adaptive system we look at.

In the final sentence of the introduction, we express our aim: “The aim of our paper is to study the role of these regularities in the behaviour of the photosynthetic CO₂ flux, observed in the measurements at one site, Värriö, and use the above concepts to analyse the EC flux data in several Scots pine stands across Europe (Fig. 1)”.

These two statements tell, that we aim, in the ideal case, for a scale-free, abstract theory that explains photosynthetic behavior on ecosystem level (GPP). Comparison of the GPP across different sites of Scots pine “ecosystems” over Europe let us grade the prediction power of that theory. The novel finding in our theoretical

work is that the temperature (S parameter) explains on a longer temporal scale (annual) the largest portion of the seasonal change at a site and a minor effect measures the growth of noise (site specific scaling coefficient) according structural differences at different sites. However, the noise grows also consistently with the mean temperature at the different sites, lowest in northernmost Värriö, highest in southernmost Brasschaat.

1. The soil drying process can be considered by the leaf-level gas exchange model proposed elsewhere (Hari et al., 2017) through the model parameter λ (i.e., the cost of transpiration; a measure of water use efficiency). Based on the reply to previous review, however, only a “mean” optimal value (inferred from a large corpus of data) is used. This suggests that how leaf-level gas exchange is impacted by the soil water status is not explicitly taken into account.

Response: We acknowledge that the impact of dry soil conditions on GPP is large. However, this paper describes the leaf level model applied at ecosystem scale, and does not aim at specifically addressing the drought conditions. The use of EC data also includes ultimately all the factors the ecosystem is facing, including potential drought conditions. Therefore, any drought condition is “implicitly” taken into account. We do not see the value for an explicit expression without explicit data on the ecosystem scale. Soil water potential is in EC data implicit included, not explicit.

2. The up-scaling is mainly done through LAI of the dominant species (i.e., Scot pine). However, the ecosystem-level GPP can be also impacted by the activities of understory species and the soil. When adding an extra parameter (i.e., ecosystem-specific scaling coefficient) to upscale leaf-level processes to ecosystem level, this scaling coefficient then contains information associated with all the possible contributions other than leaf-level processes from single species. This explains why both reviewers suggested that the ecosystem-specific scaling coefficient should be reported and further discussed especially when the soil-drying effects are not included in the leaf-level model.

Response: We agree with the reviewer that the site-specific “scaling” coefficient is aggregating many structural and physical features. But, where does the reviewer come to the claim of the upscaling by LAI of the dominant species? We did not write that in either version of the manuscript. In the answers to the reviewers of the first version, we discussed also the link between temperature LAI and photosynthetic efficiency. Furthermore, the residual analysis showed a large independency from LAI.

Again, the use of EC data includes also all these site-specific features, including understory species, soil features, stand density etc. We have therefore assumed that we do not need to explicitly introduce them here. Please revisit the text written before on “information” and “scaling coefficients”, there can be no other information in a scaling coefficient than a statistical measure of information loss, anything else is assumption. We discussed from the beginning that the “noise” on ecosystem level measured GPP originates from “omitting structural details”.

Specific comments:

1. p2, Line 24-30: Duursma et al. (2009) also did not consider drying soil effects in their model.

Response: We do not say they did. By adding the Duursma et al reference we wish to show that they also suggest a similar seasonality as in our model.

2. Eqs. (1) and (3): Is E the transpiration rate or efficiency of photosynthetic light and carbon? According to Hari et al. (2017), it seems like that E is the same as b. What is difference between Eq (1) and (3)? Is $b=a4S=E$ or there is time up-scaling from daily p (i.e., Eq (1)) to annual p (i.e., Eq. (3))?

Response: The difference between Eq (1) and Eq (3) is that the first one describes the daily and the second one the annual GPP. E is transpiration rate and b is the efficiency of photosynthesis. On annual scale $b=a_4*S$.

3. p5, Line 14-20: This is why the ecosystem-specific scaling coefficient should be reported and carefully discussed.

Response: Our theoretical approach in this paper is based on the Hari et al (2017) model, developed and parameterized at leaf level, and, based on our analysis, it seems to be rather well scalable to a longer time scale (annual) as well as from leaf to stand scales. This is a novel and surprising result and to us it clearly shows the power of a seemingly very simple model. We do not here extend the analysis into a more detailed analysis of the scaling coefficient, but we recognize that it is possible to do that with more detailed site-specific data on conditions and structure of the stand, if available.

However, it should be once more noted that we aimed to show differences between Scots pine sites over Europe on ecosystem scale and an annual temporal scale, not to fit some local parameterization of photosynthesis or GPP according drought.

4. Fig. 3 A): Should it be the onset dates of photosynthesis not the onset and cessation dates of

Response: Yes, the reviewer is correct. This was a typo remaining from a previous version of the manuscript, thanks for noticing!

5. p6, Line 22-29: Is the residual calculated as the difference between the measured and calculated GPP? If so, the same order of residual as measured GPP suggests significant deviation between predicted and measured GPP. This may not be surprised especially when the variation of GPP is highly non-linear with respect to the environmental factors (e.g., T , CO_2 , PAR and soil water status). That is, the residual for a specific environmental category can be still impacted by other environmental factors.

Response: The residuals are calculated as the difference between measured and modeled GPP. Figure 4 shows that there is very little any systematic behavior when plotted as a function of environmental factors. It is clear that some of them (e.g., air temperature and PAR) are co-varying, especially over longer time scales, and thus the impact of a single factor is difficult to separate.