Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-533-RC2, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Prediction of photosynthesis in Scots pine ecosystems across Europe by needle-level theory" *by* Pertti Hari et al.

Anonymous Referee #2

Received and published: 25 September 2017

The manuscript reported GPP estimated from eddy-covariance systems in five Scots pine vegetation in Europe. A leaf-level gas exchange model was used to explain the behavior of GPP in response to the environmental factors across the five study sites. The novelty in this study may be the consideration associated with the adjustment in photosynthetic machinery due to the changes in temperature in the proposed modeling framework. However, further elaboration on model development and the interpretation of model-data comparison is required.

General comments:

1. I am not able to follow the modeling framework. A separate section for the model description is necessary. The model derivation in detail and a list of variables and units should be also provided in the Supplement. Is the adjustment in photosynthetic

machinery due to the changes in temperature common for Scots pine? To my knowledge, the photosynthetic machinery in some species (e.g., Pinus edulis, Juniperus monosperma and Pinus taeda) even did not acclimate after long-term manipulation of precipitation and atmospheric CO2 concentration. The authors also pointed out that acclimation is omitted in the proposed model. However, how acclimation occurred at a longer time-scale is differentiated from the short-term changes in photosynthetic machinery needs further explanation.

2. To predict GPP across the five Scots pine stands from leaf-level model, a scaling coefficient was used to bridge the two largely separated spatial scales. The scaling coefficient for current year was estimated by data from previous year. This suggests that the scaling coefficient is dynamic (i.e., yearly). What would be the information from this yearly scaling coefficient? When the dynamic of photosynthetic machinery is only estimated from one site and subsequently used for the other four sites, how would you interpret the differences in the scaling coefficients across the five sites?

3. I am not sure if the proposed model can accommodate the effects of water-stressed condition in the soil on stomatal conductance especially when the authors mainly focus on the responses to light and CO2. In fact, how the differences in the environmental factors impact the behavior of GPP across the five sites is not discussed in the manuscript. If water-stressed condition in the soil is not explicitly considered in the leaf-level model, do we expect that this information is embedded in the scaling coefficient?

Specific comments:

1. P2L22 Definition of stable regularities is needed.

2. The order of Fig. 1 and 2 should be corrected to match the main text.

3. Comparison between measured and modeled S (i.e., the state of photosynthetic machinery) as well as related discussions should be provided.

4. P4L25 Description of up-scaling processes and the calculated scaling coefficient should be reported. Interpretation for the scaling coefficient is also required.

5. P5L13-16 Is it possible that the photosynthetic parameters for the five sites are actually different but this information is embedded in scaling coefficient?

6. P5L27-29 There are many models that can be used to predict stomatal conductance and then photosynthetic CO2 flux in response to different environmental factors.

7. Discussion regarding different environmental conditions (e.g., temperature, precipitation, soil water status. . .. .) across the five sites should be included. To do so, time series of environmental factors for the five sites should be also provided when comparing the measured with predicted GPP (i.e., fig 1 or 2?).

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