

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

Anonymous Referee #1

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The manuscript presents seasonal variation in half-hourly GPP estimates for five Scots pine stands from northern timberline to temperate central Europe. Eddy-covariance-based GPP estimates are compared with modeled fluxes. A leaf-level photosynthesis-stomatal conductance model, based on a theory of optimal stomatal behavior, is paired with a temperature- and light-driven ‘state of photosynthetic machinery’ model, which describes the seasonal changes in leaf physiology that drive those in their gas exchange. These (or similar) model structures have been tested in earlier publications. In the present study, the parameters of the leaf-level model are fitted to data collected from the northernmost stand. This single set of parameters is then used across the sites. The leaf-level flux is scaled up to stand using a site-specific scaling parameter that was derived using an independent dataset. The authors report on quite a remark-

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able predictive power of the model across the five ecosystems ranging >10C in the mean annual temperature.

The findings of this manuscript are both very interesting and important and will most certainly trigger further research, but to be of high impact, the paper needs more work.

General comments:

1) The upscaling procedure deserves to be discussed in more detail. Because the leaf-level model is tested at the ecosystem scale, explaining the range of values of the ‘ecosystem-specific scaling coefficient’ is an essential part of assessing the role of ‘common regularities in the behavior of photosynthesis’ in ecosystem-atmosphere CO₂ exchange. In other words, when the modeled leaf-level flux is scaled to match measured ecosystem flux, does the scaling coefficient incorporate among-sites differences in canopy structure (leaf area and/or shoot structure) only? One could conjure a scenario in which, for example, both leaf area and photosynthetic efficiency change when moving from one stand to another. Why not compare estimates of GPP per unit leaf area across sites?

2) The structure of the paper would benefit from further streamlining. Related to the previous point, there is a range in the level of detail given, or depth of discussion, among various sections, which interferes with the flow of the paper. For example, the lack of consistent descriptions of the methods makes it difficult to follow (and to replicate) what was done. Also, it would be very helpful if all the parameters and drivers in all the equations were described and their units and fitted values (including the scaling coefficient) were given.

Specific comments:

P1L20-22: What do you mean by stable regularities? The study by Duursma et al. (2009, *Tree Physiology* 29, 621–639) appears relevant here.

P3L15: Do you mean conifers from high latitudes?

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P4L22: What do you mean by 'differences in species' here?

P6L11-15: Please reduce repetition.

Fig.4: How do the residuals relate to soil moisture?

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-533>, 2017.