

Interactive
Comment

Interactive comment on “Needle age-related and seasonal photosynthetic capacity variation is negligible for modelling yearly gas exchange of a temperate Scots pine forest” by M. Op de Beeck et al.

M. Op de Beeck et al.

maarten.opdebeeck@ua.ac.be

Received and published: 15 December 2009

We would like to thank the referee for the thorough review of our manuscript and for her/his constructive and helpful critique. Below are given our answers (A) to the referee's comments (C). We copied the comments and answered each comment separately.

C: This study presents evidence for small but significant variations of key photosynthetic parameters with needle age in a Scots pine canopy and their relatively small

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



impact on the predictions of canopy gross primary productivity and transpiration using a process-based multilayer model. The paper is well written and the results are overall well presented, and I think the paper could be published provided small revisions are made, notably to put the paper in a broader context and discuss the generality of the results.

A: In the Conclusion, we highlight the fact that our stand is not fully representative for temperate Scots pine forests and to use caution on the generalization of our findings. In addition, we addressed the majority of specific comments below, related to this general comment.

C: In particular, some clarifications should be made on which variations of the photosynthetic parameters are considered, here or in other studies. Indeed, needle-age variations can include seasonal variations (each needle cohort age over the growing season) but can also create spatial variations (if the needle cohorts do not occupy the same part of the canopy). I think the apparent contradictions with or between other studies would vanish if such distinction was made. For example, if the total canopy leaf area is small (the case for this study), then spatial variations are less important because all canopy layers have nearly the same photosynthetic capacity and see almost the same amount of radiation. This is vaguely evoked in the paper (for example p. 9738 l. 29) but obviously not stated clearly enough (see for example comments by M. Letts). I would suggest for example to add “sparse” temperate Scots pine forest in the title

A: (1) We believe that we are quite clear on which variations of the photosynthetic parameters are considered in this study when describing the aims of the study at the end of the Introduction. (2) We already mentioned that, in general, the shape of the distributions of needle age classes in the canopy will affect the outcome of simulations. Yet, we agree with the referee that these spatial variations might possibly partially explain the apparent contradictions with or between the referenced studies. Therefore, we reformulated the relevant Discussion paragraph (4.4 Canopy gas exchange simulations)

C3555

BGD

6, C3554–C3560, 2009

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



so that this spatial variation is now also brought up as a possible explanation for the different results between the referenced studies and ours. (3) We inserted the adjective “sparse” in the title to give the reader a first indication of the peculiarity of the studied stand.

C: I would also suggest to pay more attention (and give more details) about the way light penetrations is computed, and especially how the grouping of needles into shoots or whorls is dealt with. Depending on the species, whorl structure (and thence radiation interception) changes dramatically with needle age and this could have a bigger effect on simulated photosynthetic or transpiration fluxes than age-related changes in photosynthetic parameters. For example, and although not stated explicitly in their study, I believe this is what Ogée et al. (2003), who accounted for both shoot structure and photosynthetic capacity changes with needle age, observed .

A: (1) We followed the referee’s suggestion. In the model description (Appendix A), we replaced the reference to the equation we use to take into account inter- and intra-crown clumping by the equation itself (actually, it is a modification of Eqn A1). (2) Ogée et al. (2003) did not observe a bigger effect of omitting radiation interception changes on simulated fluxes than of omitting the age-related photosynthetic capacity changes. It is to say, they did not discriminate between the two. (3) We reformulated the relevant Discussion paragraph (4.4 Canopy gas exchange simulations) so that the reader is informed about the fact that Ogée et al. (2003) and Bernier et al. (2001) also considered shoot structure, and that the averaging of this shoot structure over age might have contributed to the significant model differences found by Ogée et al. (2003).

Specific

C: p. 9738 l. 5 Comma missing after parameters

A: We rewrote this sentence in order to deal with this punctuation issue.

C: l. 6 Remove capital letters and replace ‘different-aged needles” by “needle cohorts”

BGD

6, C3554–C3560, 2009

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



A: We replaced “Leaf Area Index” by “leaf area index” in the entire text. We added “cohorts”.

C: l. 9 Capital letters

A: Done. We replaced “Gross Ecosystem Productivity” by “gross ecosystem productivity” in the entire text.

C: l. 10 Add “and/or spatial”

A: We did not test the effect of omitting spatial variation in photosynthetic capacity on simulated canopy gas exchange. (With the benefit of hindsight, we can say that this would have been irrelevant in our case, because no significant differences in photosynthetic capacity between different canopy positions were observed)

C: l. 15 Indicate by how much.

A: Done. We included the observed percentage-wise differences in Vm25 and Jm25 between current-year and one-year-old needles.

C: l. 20 Remove “Measured”. By how much?

A: Done. We removed “Measured” and we included the percentage-wise by which overestimation of GPP was overestimated by the model.

C: l. 22 “needle age-related”

A: Done.

C: p. 9739 l. 8 Add “and cohorts” after “clumps”

A: Done.

C: l. 9-10 Maybe add older references (e.g. Wilson et al. 2000, 2001, Meier et al. 2002)

A: Done. We inserted an older reference.

BGD

6, C3554–C3560, 2009

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



C: I. 18-20 Some global models include seasonal changes in V_{max}/J_{max} and, to some extent, age related changes too (i.e. maximum needle age is ca. 3 years and the V_{max}/J_{max} change with age but pooled together over the season a bit like in scenario S here).

A: We were aware of that. Therefore we wrote "... needle age-related and seasonal photosynthetic capacity variation is mostly not taken into account."

C: I. 21 "Here" is a bit ambiguous.

A: We replaced the word "Here" with "In those models" to avoid ambiguity.

C: I. 24 "canopy scheme reduction" is a bit vague. References cited did not discuss V_{max}/J_{max} variations or assumed it varied vertically in a similar fashion between the multi-layer and big-leaf approach.

A: We replaced "canopy scheme reduction" by "the use of one sun/shade layer and the spatial averaging of photosynthetic capacity", which is less vague. Dai et al. (2004) did indeed not compare a sun/shade model with a multi-layered model, so we removed this reference and replaced it with a more relevant one (de Pury and Farquhar, 1997). The fact that the referenced articles assume photosynthetic capacity to vary in a similar fashion between the multi-layer and the big-leaf approach is not an issue. The point is that they are able to represent the vertical profiles by one value without significant accuracy loss, like we investigate here whether age-related variation can be represented by one age-averaged value without significant accuracy loss.

C: I. 25 Add references. In general, I think the presentation of previous literature is a bit messy. See general comments above.

A: Done. References are added.

C: p. 9743 I. 15-7 It would have been good to give the reader some indications on how these vertical distributions look like.

BGD

6, C3554–C3560, 2009

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



A: These vertical distributions are given in Results (3.2 Leaf area index).

C: p. 9745 l. 16 What is a “biurnal”? I would replace each instance by “24-period”.

A: In “Materials and Methods”, we defined a biurnal as a day-night-day period (36 h). We agree with the referee that the word “biurnal” is not a proper English word and we replaced all instances.

C: l. 26 “validated against”.

A: Done.

C: p. 9746 l. 28 Maybe add “as in Ogée et al.” after “weighting factor”.

A: Done.

C: p. 9750 l. 20 Reformulate “more than satisfying degree”.

A: Done. We reformulated this to “satisfactorily”.

C: p. 9755 l. 7-14 If the footprint at night is such a problem why not using a different method to partition NEE into GEP and TER, based only on daytime measurements?

A: An alternative method to partition NEE into GEP and TER was applied to our site (Lasslop et al., <http://www3.interscience.wiley.com/journal/122525923/abstract>) and there are only very little differences between the different approaches. As the partitioning is obviously less of an issue than stated in the original manuscript, we reformulated the paragraph substantially.

C: l. 19-22 Rephrase (but see below). C: l. 19-24 A bit redundant with the results section...

A: We agree with the referee. We rephrased and reduced these 5 lines to one sentence in which we only reproduce the results relevant for the discussion part that follows.

C: p. 9756 l. 15-20: This is part of the explanation but the density of the canopy and the treatment of radiation interception contribute also largely to the apparent contradiction.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



A: We agree with the referee. We reformulated the relevant Discussion paragraph (4.4 Canopy gas exchange simulations) so that canopy density and the treatment of radiation interception is also brought up as a possible explanation.

C: p. 9757 l. 2-5 But this is the case at least in Ogée et al. no?

A: No. Ogée et al. (2003) also used needle-level measurements, but they didn't study the effect of seasonal photosynthetic capacity variation on simulated gas exchange, which is what this sentence is about.

C: p. 9758 l. 20 Replace "uniform" by "spherical"

A: Done.

C: p. 9759 l. 15 It is not clear how radiation at a given level is distributed between needle cohorts. Do they have the same interception efficiency?

A: We replaced the vague reference to the equation we use to take into account inter- and intra-crown clumping on light penetration by the equation itself (actually, it's a modification of Eqn A1). Needle cohorts have the same interception efficiency in our model. Shoot structures of current-year needles and one-year-old needles are very similar in our studied stand and, hence, the difference in interception efficiency is small.

Sincerely, M. Op de Beeck

Interactive comment on Biogeosciences Discuss., 6, 9737, 2009.

BGD

6, C3554–C3560, 2009

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

