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Biogeosciences Discussions

Interactive comment on "Photosynthetic production of boreal ground vegetation after a forest clear-cut" *by* L. Kulmala et al.

Anonymous Referee #2

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Kulmala et al. present a study on photosynthesis of pioneer vegetation colonising boreal forest clear-cuts. The authors constructed light response curves, both on a leaf mass and ground area basis, for four non-tree species over one growing season on two sites with contrasting nutrition regimes. The authors described the seasonal variability of the light response curve parameters, especially the photosynthetic activity (Pmax) and aboveground plant part respiration, and concomitant change in environmental factors, basically air temperature history and soil moisture. Kulmala et al. also estimated the photosynthetic production and the respiration of ground vegetation for the entire growing season for both sites by upscaling spatially using species distribution and temporally using irradiance and temperature records.

The study addresses a relevant scientific question about biophysical factors affecting

CO2 exchange over young boreal forests which are underrepresented in the literature. The study can also provide valuable data on photosynthetic activity of pioneering, non-tree plant species colonising boreal forest clear-cuts. However, critical information is missing in Methods section regarding sampling and determination of leaf biomass and ground area, making some interpretations and conclusions hard to judge. Discussion on the eco-physiological relevance of the study is thin and more precise objectives would help to deliver a clear message. Site description also needs improvement. The number of samples per species is low and at times results are mostly a description of time series making some interpretations somewhat speculative. Discussion on photosynthetic production at the site level completely omits contribution of tree seedlings, mosses, and other plant species not include in the study. Finally, the text lacks fluency and is imprecise at times. Hence, the manuscript would need major improvements before being acceptable for publication.

Specific comments

The objectives of the study are not formulated from an eco-physiological stand point, hence the discussion lacks the structure to deliver a clear and straightforward message. The authors should aim at reaching interpretations and conclusions which can be applied more generally. In that sense, site description needs improvements.

Currently, it is hard to judge how similar or different are the fertile and infertile sites in terms of soil characteristics (horizon depth including organic soil, biomass), nutrient status, hydrology (e.g. water table regime), (micro-)topography. Quantitative measures would be best as proper characterisation of study sites is crucial for meta-analysis and modelling exercises that use published data. Also, did the sites receive any site preparation treatments (e.g. burning, plowing)? If so, how does it affect soil and vegetation spatial heterogeneity (e.g. compacted vs. undisturbed soil)?

It is not clear to me why the author choose to use the state of development model to estimate the seasonal variation of Pmax because (1) it takes advantage of only one

Pmax measurement per species while the seasonal variation of Pmax appears to be well characterised by the whole measurement sequence, hence linear interpolation appears to be suitable, and (2) its use is inconsistent because linear interpolation was used for two out of four species studied, the other two used the model. Is an implicit objective of the paper to further test this model? If so, it should be stated clearly and results for the model could be compared with the linear interpolation approach where applicable.

My understanding is that measurements were repeated on the same two shoots per species throughout the growing season. Then, how was determined leaf biomass, which involved destructive sampling, for each sampling dates? Using the value from the last sampling date for all sampling dates, as the actual text suggests, would render most of the interpretations invalid because those pioneer species are characterized by fast-growing tissues, hence rapidly changing biomass would be a major confounding factor. Also, how was plant "ground area" determined at all? Is it similar to leaf area index or how does it relates to this concept?

In addition, given the number of samples per species, the discussion on self-shading effect is highly speculative, more so without convincing references. Also, more quantitative relationships would be needed to make the results and interpretations applicable elsewhere. For example, a direct quantitative relationship between Pmax and any environmental factors or proxies to describe seasonal variability of Pmax would be useful.

Finally, the authors mentioned that concomitant chamber and eddy covariance measurements are advantageous, yet they fall a bit short to convince the reader their study is of great relevance without the EC measurements. The lack of discussion on photosynthetic production of ecosystems components not studied by the authors accentuate that the use of eddy covariance measurements would have been advantageous in this study.

Technical comments

p.4604 l.10-11: Replace "within individuals" with "between individuals" or "within species".

p.4604 l.15: Replace "during an entire growing season 2005" with "for the entire 2005 growing season".

p.4605 l.2-6: What is the importance of young forests in the terrestrial C cycle? To strengthen their point, the authors could add something about the C status of boreal forests that changes from high C source to high C sink in early successional stages and information on physiology of young sites is thus crucial.

p.4605 l.9: What do you mean by "well-dispersed"?

p.4605 l.17-19: Please precise what you mean by "produce benefit". How do you define "benefit"?

p.4605 l.19: Replace "effective assimilators" with "effective CO2 assimilators". A reference would be needed here.

p.4605 l.27: add "net" before "photosynthetic production".

p.4606 l.1: replace "chamber measures" with "chamber measurements are made".

p.4606 l.1: replace "and the role" with "hence the contribution".

p.4606 l.3-6: Please rephrase to something like "These studies face uncertainties in scaling point measurements to a larger area (e.g. at the ecosystem level) because the ground vegetation is usually spatially very heterogeneous, even at small spatial scales".

p.4606 l. 6: Delete "scaling". The EC technique produces direct, integrated measurements at the ecosystem level, no scaling involved.

p.4606 l.12: Replace "to detect exactly the role of different species and small changes" with "to distinguish the contribution of different species and detect small changes".

p.4606 l.12-13: What do you mean by “the EC measurements are unable to detect small changes in photosynthetic activity”? How do you define “small changes”? A short explanation and/or references would be needed.

P.4606 l.13: “studying the processes”, please precise which processes.

p.4606 l.14-15: Replace “all the processes” with “photosynthesis and respiration”.

p.4606 l.1-5: This paragraph misses something making clear which method you choose to use and how it is an improvement over the other methods described. As it is right now, the last two sentences are somewhat pointless given the next paragraph.

p.4606 l.22-24: What do you mean by CO2 production? Also, what do you mean by “at any young sites”, are your results applicable for any young sites? I would suggest rephrasing to something like: “At both sites, we (3) upscaled fixed and respired CO2 by ground vegetation at the ecosystem level for an entire growing to provide estimates of the C sequestration potential of young boreal forests.”

p.4607 l. 3-5: How far from each other are the sites? What is the surface area of the sites?

p.4607 l.6-8: I presume climatic data are from the SMEAR II station, please state it clearly.

p.4607 l.15: When were the sites clear-cut and sown? Did the sites receive any site preparation treatments?

p.4607 l.20: I doubt mosses are “fast-growing and opportunistic dominant species having rapidly reproducing new tissues”. Please rephrase.

p.4607 l.20-22: Quantitative measures would be needed here. If not available, can the authors provide a range of expected values given the presence of indicative species?

p.4608 l.2-3: 16 000 1.5 m tall birch trees per hectare seems like a lot. How does that affect microclimate (temperature, radiation, and wind regimes)?

p.4608 l.7-10: Where are the sampling locations to measure soil water suction and how were they chosen?

p.4608 l.4-10: This paragraph misses information about soil temperature measurements.

p.4608 l.11-19: The construction of temperature record as it is described in the text is not among the best scenarios, particularly because air temperature was not measured directly on either studied sites. This could mask potential site-specific micro-climatic effect on the physiology of studied vegetation. For example, vegetation at the fertile site might be exposed to a lighter wind regime because of the abundance of birch seedlings. This issue should be addressed in the discussion.

p.4610 l.2: How was irradiation level inside the chamber determined if PAR is measured outside the chamber?

p.4610 l. 8-14: Was the chamber placed over the sampled shoot for a whole set of measurements (4-6 measurements) at once or only one measurement at a time? In the former case, how did you account for heat build-up inside the chamber and CO2 depletion? In the latter case, how did you avoid damaging the shoots by inserting/removing them from the chamber multiple times?

p.4611 l.10: What was the time constant used and how was it determined?

p.4613 l.27: Please replace “much earlier” with approximate number of days/weeks.

p.4614 l.6-8: This is total aboveground biomass excluding tree seedlings. Please include this clarification.

p.4614 l.15: Delete “from different sample plots”.

p.4615 l.9-17: The b value is held constant over the season so this paragraph is irrelevant. p.4615 l.18: Please review section numbering.

p.4615 l.24: Pmax is defined as photosynthetic activity in the text, please use consis-

tent terminology. Replace all occurrences of “photosynthetic capacity” with “photosynthetic activity”.

p.4615 l.24: Do you have results (or reference) to support your statement about low photosynthetic activity “due to cold temperatures”?

p.4615 l.27: The first two sentences of this paragraph are vague and imprecise. Please rephrase and use actual numbers (e.g. min and max to describe amplitude of variation).

p.4616 l.15: I assume that “based on individual” means ground-area based. Please use consistent terminology.

p.4616 l.19: “shoots”, see above comment.

p.4617 l.7: How do you define/quantify “the amount of shoots”?

p.4618 l.16: My understanding is that only one measurement (maximum Pmax) is used to fit the model. This sentence suggests that more measurements are actually involved. Please clarify.

p.4620 l.6-4: Please give more details.

p.4621 l.25-p.4622 l. 8: The numbers from the cited references include photosynthesis from the whole ecosystem, i.e. tree seedlings, shrubs, herbs, and mosses. Yet, the authors do not put these numbers in context and fail to provide a relevant basis for comparison.

p.4622 l.21-24: This paragraph is out of place and should be moved to previous section or edit out.

Table 1. I would suggest including estimated biomass for tree seedlings.

Table 2. Please include r2 and uncertainty values for significant relationships.

Fig 1. Please translate x-axis tick labels.

Fig 3g. Y-axis ticks are missing.

Fig 8. Figure caption is incomplete or imprecise. Estimates include only few species at either site and can hardly represent the whole site photosynthesis activity.

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