

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

L. Kulmala et al.

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We thank the anonymous referee #2 for taking the time to review this article and for all the constructive and detailed comments on our work. We really appreciate it. The comments have been most helpful in strengthening the paper and the revised manuscript turned more solid. We have worked on the logic and highlighted the aim and key results that seemed to be unclear like referee #2 pointed out.

The manuscript was earlier called “Photosynthetic production of boreal ground vegetation after a forest clear-cut” but after reading the reviewer’s comments we decided to decrease the role of upscaled photosynthetic production and shorten the name to “Photosynthesis of boreal ground vegetation after a forest clear-cut”. The fine-tuned title suited the content and the aim of the paper better than the earlier one.

C1618

The reviewer’s primary points of concern are that 1) critical information is missing in Methods section 2) Discussion on the eco-physiological relevance of the study is thin and more precise objectives would help to deliver a clear message 3) The number of samples per species is low 4) more quantitative relationships would be needed to make the results and interpretations applicable elsewhere 5) Discussion on photosynthetic production at the site level completely omits contribution of tree seedlings and other plant species not include in the study 6) the text lacks fluency and is imprecise at times. The referee has made also several other specific and technical comments.

Our detailed responses to the concerns and comments raised by the referee #2 can be found below

1) According to referee’s suggestion, we have now added more site description such as the depths of organic layer and A-horizon as well as the Cajanderian type of site classification. Unfortunately we don’t have quantitative measures on soil nutrient status, mainly because traditionally the site type classification has dominated the description in Finland. In addition, the information on area, scarification and topography on both sites has been introduced in the revised MS.

How site preparation affects spatial heterogeneity? That is an interesting question and surely it increases the already notable diverseness even though scarification is rather light preparation compared to plowing, for example. However, we decided not to discuss it in the MS because there are already several loose ends and we don’t want to make the MS too complex. Nevertheless the site preparation is identical at our study sites.

According to referees comments, we have clarified the determination of leaf biomass and ground area. The ground area refers to the basal area of the used chamber. This is now brought out in the revised MS below the Eq. 1

2) We have now worked heavily on the sections of introduction, results/discussion and conclusions to get our objectives, key results and message clear. Therefore, we have

C1619

rearranged the order of results and discussion concerning photosynthesis. Earlier, we first presented the individual characteristics of P_{max} and then the environmental effects on P_{max} below a subtitle called "Saturation level of the light response curve, P_{max} ". In the revised MS, those are replaced by three titles 1) "The seasonal pattern of P_{max} ", 2) "The level of P_{max} of different sized shoots" and 3) "Estimating photosynthetic activity".

In the revised MS, we have tried to open up the reasons behind the use of state of development model because it was not presented clear enough in the earlier version. We think that the ability to model the P_{max} is useful in the future applications e.g. in estimating the production of young sites in changing climate and varying species composition whereas the linear interpolation without any connections to environmental factors is useless. The linear interpolation is used only in the upscaling exercise with species that did not fit into the model. Nevertheless, the referee is right that we aimed in testing the model. We have now highlighted this in the revised MS.

The idea of comparing the upscaled results for using both linear interpolation and the state of development would be very easy and worth of trying in the case of *C vulgaris* and *D flexuosa* but we found that reporting such a calculation would confuse our focus and therefore we did not perform it in the revised MS even though the referee suggested it.

The referee was concerned about the determination of leaf mass that was weighed as late as on the last sampling date. Our measurements were whole-plant measurements and, as a result, our P_{max} is not only about the photosynthetic activity on a leaf element but it includes also the increment of leaf area in spring and early summer. Therefore, the reporting of the results was inaccurate earlier in the way that they were not based on momentary leaf mass but full-grown leaf mass. We have now added this specification in the revised MS. The campaign ended before species-specific senescence but naturally after reaching the maximum leaf area. The inventory was made in the turn on July and August i.e. at the time when the leaf area of every species was in its maximum. Therefore, we can scale the results up using only one (full-grown) leaf

C1620

mass because the P_{max} includes the growth of leaves too.

3) The number per species is low as the referee points out. We have now added the concern of the low number of samples in the revised MS in the section 3.5.

We agree with the referee that the discussion on self-shading is pretty speculative because our number of experimental species is very low. However, the species that we measured have not been under particular interest of gas exchange researchers and therefore the claim for convincing references is hard to fulfil. We ended up listing relevant literature references of potential causes for the low leaf mass based P_{max} in dense vegetation (3.3.3 The level of P_{max} of different sized shoots). We hope that we have satisfactorily addressed the referees concern.

4) We have found our modelling results of *C vulgaris* and *D flexuosa* quantitative and tried to describe the changes in P_{max} of other species together with the environmental factors (light, temperature sum, ...) as well as possible. In the whole-plant measurements as here, the P_{max} includes also the increase of leaf as discussed earlier. Therefore, only mid-summer values would be rational to use if we are looking for relationships between P_{max} and environmental factors. However, our number on measurements is too low to perform such a comparison. In addition, the phenology and photosynthetic activity is not only an instant response to environmental factors but includes delays, addressed by e.g. our results of state of development. Therefore we have not explored any other direct quantitative relationship between P_{max} and environmental factors than introduced the model of state of development.

5) We have now rewritten the text so that it is clear that we do not aim for production of tree seedlings nor do our results include them or other missing species. We realize that our upscaled results are not comparable with the ecosystem scale studies as referee criticizes. However, we left our upscaled results in the earlier form and did not add any estimation for missing species. Instead, we moved the emphasis of the manuscript slightly more on the species-specific seasonal cycle and the modeling of P_{max} . We

C1621

have also highlighted that refereed studies have measured more species than we have but are still within the same range.

The starting point of our study was the need of detailed information on the different flux components for interpretation of EC results, for upscaling and for modeling. EC is best for solving NEE if we are interested in momentary ecosystem scale result. However, it is impossible to average EC results over larger space or time without any additional information on species-specific strategies because substrates, habitats and species compositions vary in space as well as the climate will do in the near future. The same upscaling problem faces chamber measurements that are usually containing somewhat unrepresentative vegetation too.

There were lot of differences in the species-specific responses and levels of photosynthesis according to our measurements. Due to labored measurements, the number of individuals was pretty low as the referee pointed out earlier. Therefore, we discussed that the exact average level of each species could be reached with simultaneous measurements with EC. Nevertheless, we still find our results very important even we have failed to formulate it in the earlier version of the MS as the referee observed. We have now clarified the conclusions so the results of this study do have value and relevance even without EC measurements.

6) We have worked with the fluency and corrected the imprecise spots that the referee has pointed out in detailed comments. We have removed several typos and grammar errors that a native editor has unfortunately missed when he edited the paper before submission.

Technical comments

Referee #2: p.4605 l.9: What do you mean by “well-dispersed”?

Kulmala et al: We meant that the opportunistic species are fast in colonising an open

C1622

habitat but due to our insufficient knowledge in English and the insignificance of the statement we took it of from the list to diminish confusion.

Referee #2: p.4605 l.17-19: Please precise what you mean by “produce benefit”. How do you define “benefit”?

Kulmala et al: Here, we thought that the leaf has produced benefit when it has fixed more carbon than it has cost to produce and maintain the leaf. We have replaced the confusing expression by carbon gain.

Referee #2: p.4605 l.19: Replace “effective assimilators” with “effective CO₂ assimilators”. A reference would be needed here.

Kulmala et al: Corrected in the revised MS. We added also the reference Chabot and Hicks, 1982 which summaries several studies.

Referee #2: 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.

Kulmala et al: We meant that changes in physical environmental factors can cause different kind of changes in different species and EC is unable to detect this. The understanding of species-specific responses is important in scaling up on large areas and crucial in the future climate when the species composition is probably changed as discussed earlier. We wanted to highlight the weaknesses of EC but the sentence was phrased in a wrong way and we corrected it in the revised MS: Again, the EC measurements are unable to distinguish the contribution of different species and detect changes in their species-specific photosynthetic activity.

Referee #2: 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

C1623

now, the last two sentences are somewhat pointless given the next paragraph.

Kulmala et al: We rewrote the last two sentences in the paragraph to make the text more fluent and clear the aim. We also precised and replaced according to referees advice.

Referee #2: 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."

Kulmala et al: Use of "CO₂ production" is surely misleading as well as the expression of "any young sites" is too wide-ranging. We have replaced the inaccurate sentence with the referees suggestion.

Referee #2: p.4607 l. 3-5: How far from each other are the sites? What is the surface area of the sites?

Kulmala et al: The sites are located circa one kilometre apart from each other and they both are somewhat small, circa one hectare. The information is now added to the main text.

Referee #2: p.4607 l.6-8: I presume climatic data are from the SMEAR II station, please state it clearly.

Kulmala et al: The data is not from the SMEAR II station but from the measurements next to the Hyytiälä field station organized by Finnish meteorological institute. We added to the text that the data is from Finnish climatological statistics.

Referee #2: p.4607 l.20: I doubt mosses are "fast-growing and opportunistic dominant species having rapidly reproducing new tissues". Please rephrase.

Kulmala et al: True. We removed the mosses from the list here due their appearance

C1624

is explained in detail later in the chapter Vegetation at the sites.

Referee #2: 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)?

Kulmala et al: True. The number of birches is substantial but we don't know how do they affect the microclimate. We have added the concern in the revised MS.

Referee #2: 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.

Kulmala et al: As the referee suggest, we have now added this discussion below the section 3.3.4. We agree that there truly might exist some micro-site variation but unfortunately we do not have more exact data. On the other hand, the very similar temperature record at SMEAR II station suggests that there might not be very big differences.

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

Kulmala et al: The irradiation in the chamber was tested and found to be very similar to the radiation outside the chamber. This information has now been added to the MS.

Referee #2: 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?

Kulmala et al: Between the measurements the chamber was carefully raised up from the experimental shoot and let to ventilate. This insertion is now added to the text.

C1625

Inserting and removing of the chamber did not cause any trouble to the experimental shoots because it was not attached to the chamber. In most of the cases, the chamber laid on the cellular plastic.

Referee #2: p.4611 l.10: What was the time constant used and how was it determined?

Kulmala et al: The value of the used time constant (150 h) used to be in the result/discussion section but has now been removed here because, as the referee questions, we just tested different values and found the 150 h to follow the measured Pmax values closely. The time constant would need more detailed research and we have added the concern into the revised MS.

Referee #2: p.4613 l.27: Please replace "much earlier" with approximate number of days/weeks.

Kulmala et al: Unfortunately, we do not have information on the exact starting date of the growing season at our experimental sites. We have now rephrased the sentence to be more scientific: "Therefore we can assume that the growing season was in initial phase when we started our measurements".

Referee #2: p.4615 l.18: Please review section numbering.

Kulmala et al: The chapters "Individual characteristics" and "Environmental effects on Pmax" values were not supposed to have numbering but to be below the title "Saturation level of the light response curve, Pmax". The numbers have disappeared in the editorial state and unfortunately missed during the first proofreading. Now, we have renumbered and partly renamed the headings as described earlier.

Referee #2: p.4615 l.24: Pmax is defined as photosynthetic activity in the text, please use consistent terminology. Replace all occurrences of "photosynthetic capacity" with "photosynthetic activity".

Kulmala et al: The terminology is corrected to be consistent in the revised MS.

C1626

Referee #2: p.4615 l.24: Do you have results (or reference) to support your statement about low photosynthetic activity "due to cold temperatures"?

Kulmala et al: We have now edited the sentence and added a kind of question mark: "The photosynthetic activity of *Calluna vulgaris* was still quite low in early June (Fig. 3A) possibly due to low temperatures (Fig. 1A)." We have no results to support this nor have we added any reference because we thought that the temperature response of photosynthesis is such a self-evident matter that we don't want to go deeper into this herein.

Referee #2: p.4617 l.7: How do you define/quantify "the amount of shoots"?

Kulmala et al: We have now rephrased the confusing expression "the amount of shoots" to "the number of shoots per ground area".

Referee #2: 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.

Kulmala et al: True, we fitted the model only using one measurement. Here, we changed the fitting date because we thought the result would change due to the drought. We have now clarified the procedure.

Referee #2: 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.

Kulmala et al: We have now rewritten these sections so that it is clear that the cited references except for the first one are from whole ecosystem unlike our results.

Referee #2: Table 1. I would suggest including estimated biomass for tree seedlings.

Kulmala et al: We have not added the estimated biomass but a new table (table 1) that

C1627

collects together all available information on tree seedlings (height and number) as well as the soil profile heights. Earlier, the tree data existing in the body text has now been removed. We believe that it is more straightforward for a reader to compare the sites after the rearrangement.

We considered the estimation of biomass of tree seedlings and found it somewhat unnecessary because the users of the results can easily calculate the biomass estimates, leaf area index etc themselves if they need those measures. In that case, it is easier to them to take into consideration the uncertainties that the used biomass models include. The theoretical biomass information of tree seedlings is not crucial to adopt the results of this study. We believe the given measurements on tree seedlings are informative enough.

Referee #2: Table 2. Please include r^2 and uncertainty values for significant relationships.

Kulmala et al: The r^2 values are introduced now in the table but we decided not to introduce the uncertainty values. We think that the poor r^2 values as well as the obvious noise in the scatter plot (6) are informative enough.

Referee #2: Fig 1. Please translate x-axis tick labels.

Kulmala et al: The axis label is translated in the revised MS.

Referee #2: Fig 3g. Y-axis ticks are missing.

Kulmala et al: The ticks are inserted in the revised MS.

Referee #2: 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.

Kulmala et al: The caption is corrected to be precise in the revised MS.

C1628

The required information in these comments is added in the revised MS according to the referees' suggestion

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

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

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

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).

These sections are removed from the revised MS according to the referees' suggestion

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.4615 l.9-17: The b value is held constant over the season so this paragraph is irrelevant.

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

C1629

These suggested corrections are made in the revised MS

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.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.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.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.

Interactive comment on Biogeosciences Discuss., 6, 4603, 2009.

C1630