

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 reviewer #1 for taking the time to review this article. 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” in the revised manuscript. The fine-tuned name suits the content and the aim of the paper better than the earlier one.

The reviewer’s primary points of concern are that 1) the manuscript lacks a clear focus 2) no effort is made to link P_{max} to plant strategies 3) the potential causes of instability of P per leaf mass are not back-upped 4) The photosynthesis of the ground vegetation

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is large compared to published results of mature forests 5) and the manuscript needs to go through careful editing.

Our detailed responses to the concerns raised by the reviewer #1 can be found below

1) We have now worked on the sections of introduction, results/discussion and conclusions to get our objectives, key results and message more clear. The aim of the present work is to study the photosynthesis and respiration of most common early-successional species. The species-specific responses are important in future applications e.g. in estimating the production of young sites in changing climate and varying species composition. The starting point of our study was the need of detailed information on the different flux components for e.g. interpretation of EC results, for upscaling and for modeling.

We have clarified our aim in the introduction as well as the main results in the conclusions. In addition, we have rearranged the order of results and discussion concerning photosynthesis. Earlier, we first presented 1) the individual characteristics of P_{max} and then 2) 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 sub-chapters: 1) “The seasonal pattern of P_{max} ”, 2) “The level of P_{max} of different sized shoots” and 3) “Estimating photosynthetic activity”. We found that the new structure removes the problem of loose ends that the referee brought out.

2) Referee suggests that we should link our observations in the existing framework of plant ecology. This is a very well-grounded idea but not so easy to carry out. The timing of blooming, for example, did not give extra information because our shoots of *R. idaeus* were one year old so they did not flower at all, *E. angustifolium* flowered all the summer and *C. vulgaris* in August-September. However, we did not notice any change in the photosynthesis that could be connected to timing of blooming. We added this observation in the section 3.3.2. We tried to look for good literature references but we did not find anything relevant.

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The growth of leaves would be a very important measure to improve the model but unfortunately we do not have the information except built in Pmax values and therefore we need to leave the subject for other studies.

3) Due to laboured chamber measurements, the number of shoots in our work is too low to go deeper into the effect of potential self-shading or other reasons for more stable Pmax expressed as units per ground area. However, we agree with the referee that the lack of deeper discussion on the subject in the earlier version of MS was insufficient because the result of this obvious sign of complex canopy, even in relatively short ground vegetation, was one of our key results. We have sat down and read literature concerning self-shading and research on the species at the ground level and on trees as well. We ended up listing relevant literature references of potential causes for the low leaf mass based Pmax in dense vegetation (3.3.3 The level of Pmax of different sized shoots). We hope that we have satisfactorily addressed the referees concern in the revised MS even we do not give any numerical estimates as the referee suggested.

4) Referee 1: The photosynthesis of the ground vegetation is large. At Hyytiälä GPP varies between 850-1100 g C m⁻² y⁻¹. In the clear cuts under study, 760 g C m⁻² y⁻¹ is assigned to the ground vegetation, thus, leaving only 100-350 g C m⁻² y⁻¹ of GPP for the pine and birches on site.

Kulmala et al: The upscaled production of the fertile site (760 g C m⁻² s⁻¹) has its uncertainties as brought out both in section 3.5 and conclusions. However, we think the reason for the values to be in the same range is the enormous difference between the fertility of the young site and SMEARII. The fertility of SMEARII (VT+) falls just between the fertility of the young study sites (fertile MT-OMT and poor CT) according to Cajanderian forest site type classification. The difference is obvious also in vegetation. The fertile clear cut site used to be rich spruce stand while SMEAR II and the sites in the chronology studies from Hyytiälä are poorer Scots pine stands. Therefore these values are not truly comparable.

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Referee 1: Your finding also implies that GPP is almost constant during succession (confront with classic succession theory i.e. Odum 1969).

Kulmala et al: This is something very interesting but also a matter that we truly can not confirm with this data set because, as mentioned earlier, we do not think that the young sites and SMEARII are truly well-matching.

We thought of possibilities of making some estimations of the production of tree seedlings and missing species and interpolate the results roughly to attain the fertility of SMEARII. However, we found that this would include too many insecure assumptions (e.g. missing species and role of roots) and it would need changing the aim too far away from the current (species-specific responses of ground vegetation). Therefore we did not include the discussion of classic succession theory.

Referee 1: Last, GPP – Re equals NEP and this should be of the same magnitude as the standing biomass for perennial plants. Is this the case for your sites?

Kulmala et al: We have added now the comparison of NEP and biomass in the revised MS in section 3.5. There is a difference between GPP–Re and NEP but on the other hand we have not measured the roots that supposedly respire quite a deal due to their high biomass. We have added this discussion there too.

5) We have removed several typos and grammar errors that a native editor has unfortunately missed when he edited the paper before submission. In the introduction, we have shortened both sections that the referee found unnecessary but not removed totally because we still found these relevant: highlighting of climate change and the possible changes in vegetation motivates to the species-specific studies as well as the species-specific strategies in respiration and photosynthesis. Most of the text concerning of respiration and its temperature response has been moved to Methods (2.3.).

Specific comments

Referee 1: A separate results and discussion section typically help the authors to better

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structure their work and helps the reader to get the key results.

Kulmala et al: We agree with the referee but in our work, we found the joined results and discussion section to be justified because the text contains several single but still important results and separate sections would make the manuscript very fragmented. Therefore we have not detached these but we have worked hard with the introduction-results-conclusion and tried to highlight the key results for the reader, as discussed earlier, and to tie those loose ends that the referee pointed out.

Referee 1: Also §3.3.2 contains no text and sometimes the line of thought needs to be better explained i.e. page 4615 line 4 – and further you state that b is set constant and then engage in a discussion of the seasonal changes in b .

Kulmala et al: The chapters Individual characteristics and Environmental effects on P_{max} values were not supposed to have numbering but to be below the title Saturation level of the light response curve, P_{max} . The numbers have appeared in the editorial state and unfortunately missed during the first proofreading. We have now rearranged the chapter with new subtitles and proper numbering as explained earlier in detail. Due to low number of measurements and the tight connection between P_{max} and b we had to assume that b is constant. However, we had some observations that do not support this assumption. Therefore, we add comments about our experience on the seasonal development even we did not use them in our analysis. We agree that the outcome is rather confusing than informative and therefore we removed it.

Referee 1: One figures has Finnish text labels.

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

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