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Interactive comment on "Mapping the reduction in carbon uptake in subarctic birch forests due to insect outbreaks" *by* Per-Ola Olsson et al.

Anonymous Referee #1

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In "Mapping the reduction in carbon uptake in subarctic birch forests due to insect outbreaks", the authors combined data from an eddy covariance (EC) tower, a spectral tower, and the MODIS-based Normalized Difference Vegetation Index (NDVI) to develop light use efficiency (LUE) models of gross primary productivity as a function of NDVI in northern Sweden forests, for years without or with defoliation caused by two moth species. Using MODIS NDVI to identify defoliated pixels over the study area, they then compared the regional GPP for 5 no-outbreak years (based on the no-outbreak "GPP_lue" model) and 3 outbreak years (based on a GPP reduction factor derived from EC tower data (Method 1) or the outbreak "GPP_lue,defoliated" model (Method 2)). The manuscript is concise, presents many informative Figures, and addresses some uncertainties through Monte Carlo analysis. Overall, I think the manuscript fits within the scope of Biogeosciences. However, the use of the mean GPP over no-outbreak

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Discussion paper

years as the basis to estimate GPP reduction during outbreaks is questionable and the manuscript is not sufficiently well written to warrant publication in its present form. In addition to addressing the issues listed below, the authors should go through the manuscript to correct the many typos I did not highlight.

MAJOR COMMENTS

1. Using the mean GPP over no-outbreak years as the basis to estimate GPP reduction caused by outbreaks seems inadequate for two different reasons. First, differences in weather may affect both GPP and outbreak occurrence/severity. If the two variables are indeed correlated across years, the approach likely causes a bias. This should be checked; no need for fancy statistical tests, just compare, based on the GPP_lue model, the mean GPP and its standard deviation for outbreak vs. no-outbreak years over pixels that have never been defoliated. Second, GPP in pixels previously defoliated is unlikely equal to what it would have been if no outbreak had occurred. For example, canopy trees might have not fully recovered yet (hence underestimating the no-outbreak GPP) or total tree+understory productivity might increase for a few years due to the defoliation-caused growth release of the understory (hence overestimating the no-outbreak GPP). The authors should rather have estimated the no-outbreak GPP in pixels that have been defoliated up to X years before (with X to be defined; maybe 3-5 years?) based on the NDVI_DL values of neighbouring pixels that have never been defoliated. (More precisely: for each defoliated pixel, define a window large enough to include never-defoliated pixels, but small enough to have similar conditions. Then, compute the mean NDVI_DL difference between the defoliated pixel and the never-defoliated pixels over all years prior to the (first) defoliation event in the defoliated pixel; let's say NDVI_DL was on average 10% higher in the defoliated pixel. Finally, for the X years after the defoliation event (excluding the defoliation year itself), estimate the annual no-outbreak GPP in the defoliated pixel with the GPP_lue model, but with a value of NDVI_DL 10% higher than the mean annual NDVI_DL value over the neighbouring never-defoliated pixels [instead of using the annual MODIS NDVI_DL value in the defo-

liated pixel].) Ideally, the authors should re-do their analyses using this new approach. At a minimum, the authors must use this new approach for >100 randomly-selected defoliated pixels and see to which extent it affects their results.

2. In the Discussion, the authors must at least explicitly acknowledge four major methodological weaknesses; when possible, explain the likely impact of each weakness (i.e., under- or overestimating defoliation-caused GPP losses) and propose a way to address the weakness. (1) fAPAR was based on measurements for the upper canopy only, so it is unclear to which extent the fAPAR vs. NDVI_DL relationship applies to the entire forest. This is particularly critical due to the (possible) growth release of the understory highlighted by the authors. (2) The fAPAR vs. NDVI_DL relationship was derived for undefoliated years (2010-2011) only, yet was also used in the GPP_lue,defoliated model. Why not developing a fAPAR vs. NDVI_DL for defoliated conditions (no defoliation event at the spectral tower over the entire study)? (3) The defoliation detection algorithm missed 26% of defoliated areas and misclassified 39% of undefoliated areas. (4) How representative was the EC tower footprint of the entire study area, both during outbreak and no-outbreak years? This is critical, as EC tower data provided the basis for all GPP estimates through the values of \epsilon_max, \epsilon_max,def, and the GPP reduction factor.

3. I object to providing the 3-year *total* GPP reduction caused by defoliation, as this inadequately inflate numbers. Please provide the 3-year *mean* reduction instead throughout the text, making it clear the reduction is for outbreak years only (not the mean values over all years since 2000): Abstract; P16, L15 to P17, L1; P17, L7-8; P19, L5-7; P19, L15; P21, L18-20.

4. P3, L15-26. I have various issues with the text from “Since near-linear” to “(Liljedahl et al. 2011)”. First, it should be in the Methods; I suggest merging at the beginning of Section 2.3. Second, units are not provided for the variables and the equation is not numbered; please number *all* equations and provide units for *all* variables throughout the text (even when unitless; e.g., NDVI). Third, the sentence starting on L20 is

cumbersome; if kept in Section 2.3, I suggest re-writing along the following lines: "The light use efficiency coefficient varies between vegetation types and the influence of meteorological conditions is accounted for through reduction factors for temperature and vapour pressure deficit [...]". Fourth, the sentence starting on L23 is an overstatement because: 1) temperature is not always the main limiting factor in cold climates (Nemani et al. 2003; Beer et al. 2010); and 2) neither Bergh et al. (1998) nor Lagergren et al. (2005) really tested for the impact of factors other than temperature in their studies that covered only two sites in Sweden. The authors should acknowledge that they assumed accounting for temperature only was sufficient in their study region, an assumption supported (but not demonstrated) by Bergh et al. (1998) and Lagergren et al. (2005). Fifth, delete the sentence starting on L25: water stress is a major limiting factor in some boreal and other forests, not just for "ecosystems dominated by non-vascular plants".

5. P6, L12. Why wasn't fAPAR_canopy also smoothed with TIMESAT before the regression?

6. P7, Figure 3. The TIMESAT smoothing removed a second NDVI 'peak' each year. Please explain what was the origin of this (wintertime?) second annual peak and why removing it was OK.

7. P12, L4-6. I do not understand why Method 1 should not also capture the effect of refoliation: the EC tower data should account for the post-refoliation increase in GPP, no? Unless no refoliation occurred within the EC tower footprint after the 2012 outbreak?

8. P14, Figure 6. Many readers will likely expect defoliation to substantially decrease NDVI (due to a much lower leaf area index (LAI)) and leave LUE barely affected, so they will question the defoliation results (small NDVI decrease, large LUE decrease). It would thus be helpful to explain that small reductions in NDVI are associated with large reductions in LAI (e.g., Wulder et al. 1998), while LUE can substantially decrease

for lower LAI because more leaves operate in the light-saturated portion of the photosynthesis curve (e.g., Medlyn 1998). Also, please indicate the weeks during which defoliation occurred.

9. P15, Figure 7 and P16, Figure 8. Give the equations for the regression lines on Figures 7 and 8. The line seems pretty close to 1:1 with zero intercept in Figure 7 (hence no bias for no-outbreak years), but the GPP_lue,defoliation model in Figure 8 seems to underestimate GPP over most of the range of actual values (i.e., up to ∼2 gC mˆ-2 dayˆ-1); this should be added to the list of weaknesses in the Discussion (see comment #2).

10. P18, L7-10. Unless I am mistaken, the authors do not correctly interpret these results. If Method 2 captured the effect of refoliation whereas Method 1 did not, then Table 3 results should have a higher absolute value for refoliated than non-refoliated pixels *regardless* of the actual sign. This is what was obtained, suggesting that Method 2 did better capture refoliation's effect; however, the differences between refoliated and non-refoliated pixels are always within the error margin, so this better performance is marginal. (The sign of Table 3 values (negative for 2004 and 2012, positive for 2013) is not directly related to the effect of refoliation, but to the bias between the two methods.)

11. P19, L26 to P20, L3. I do not think these explanations for Table 3 results (i.e, the difference in GPP loss between Method 1 and Method 2) are appropriate. The refoliation argument does not hold because, as noted by the authors, both 2004 and 2013 had high refoliation yet had opposite signs for Method 1 minus Method 2. Furthermore, the lower GPP losses in 2013 for Method 2 were basically the same for non-refoliated and refoliated pixels (Table 3). The argument of "uncertainties in \epsilon_max,def" does not seem appropriate either, because the same value was used for all years (so why a sign change in 2013 for Method 1 minus Method 2?). The argument of higher NDVI due to understory growth would work if this growth was stronger in 2013 compared to 2004 and 2012; are there reasons to believe this was the case? Here is another hypothesis that could account for the much lower mean GPP reduction in 2013 un-

der Method 2 compared to all other values (and hence account for the sign change in 2013): could it be that growing conditions were better in 2013, thereby leading to higher f_8day and/or PAR_8day used in GPP_lue,defoliated compared to the mean f_8day/PAR_8day no-outbreak values used in GPP_lue?

MINOR COMMENTS

12. Throughout the text: when possible replace the vague “carbon uptake” expression by the more accurate term applicable (GPP, NEE, etc.). Therefore, the title should be: “Mapping the reduction in gross primary productivity due to insect defoliation in subarctic birch forests”. In the Discussion, the authors should stress that their results are not for the net carbon balance, but for GPP only; based on Heliasz et al. (2011), would it be possible to speculate whether, on a percentage basis, NEP losses should be higher or lower than GPP losses?

13. Throughout the text: please use the “regional GPP” expression when applicable, to make it clearer the values are for the total GPP over the study region.

14. P1, L18-20. I have various issues with this sentence; I suggest re-writing along the following lines: “In the study area of 100 kmˆ2, the results suggested a mean regional GPP decrease of XX +/- YY Gg C yrˆ-1 for the three outbreak years (2004, 2012, and 2013), compared to a mean regional GPP of 41.1 +/- 12 Gg C yrˆ-1 for the five years without defoliation”.

15. P2, L1. Reference(s) should support the “a warmer climate is likely to increase forest productivity” part of the sentence. Here are some suggestions: Pastor and Post (1988), Nemani et al. (2003), or Boisvenue and Running (2006).

16. P2, L21. Since Brown et al. (2012) is already cited later on, I suggest mentioning it here too.

17. P3, L3. Please give the temporal coverage of Landsat.

18. P3, L13. It is Monteith and Moss (1977).

19. P3, L27-28. Bright et al. (2013) already used remote sensing (Landsat to identify trees killed by the mountain pine beetle) and a LUE model (MODIS GPP results, which are based on LUE) to quantify the impact of an insect outbreak on carbon uptake. To my knowledge, the authors can still claim being the first ones to do it for defoliators.

20. P3, L30-34. Delete the "This combination of [...]" sentence or merge it with the previous one (it is repetitive). Delete the "The method was developed [...]" sentence (it is repetitive with "Our main study objective [...]").

21. P3, L34 to P4, L2. Delete or merge with the Methods.

22. P4, L2-4. The sentence is confusing, as it seems to imply that results will be provided for every year "between 2000 and 2015". I suggest re-writing along the following lines: "Our main study objective was to compare GPP for years with (2004, 2012, and 2013) and without (2007, 2009, 2010, 2011, and 2014) insect outbreak in the birch forest of a subarctic valley of northern Sweden".

23. P5, L18 to P6, L1. The purpose of "quality classes based on QA data" is not clear: how where these classes used in the analysis?

24. P6, L4. Replace "fraction canopy absorbed PAR" by "fraction of absorbed PAR by the canopy". Similar comment for "canopy absorbed PAR" in Figure 4 caption.

25. P6, L8. Is the "pure canopy absorbed PAR" different from fAPAR_canopy? If yes, quickly explain what is the difference and why it matters. If not, replace the expression by "fAPAR_canopy".

26. P6, L11. I would provide here (after "NDVI_DL") — instead of on P3, L20 — the reference to Myneni & Williams (1994) that supports the linear relationship.

27. P7. I suggest combining Figure 2 with Figure 1. For all multi-panel Figures, please add letters to each panel and refer to the appropriate panel in the text (e.g., Fig. 5a).

28. P8, L28. Please put here (instead of on P11, L8-10) the sentence about the value

of the temperature lapse rate.

29. P9, L2. The units for PAR_8day should be MJ mˆ-2 day-1.

30. P9, L5. Add something like "(see Section 2.3.3)" so that readers know the value of \epsilon_max will be discussed later. Similar comments for GDD_thresh (P9, L16) and T_thresh (P10, L2).

31. P9, Equation (7). Replace the current condition on the middle line by "-8 $\leq$ T_min8 < -3".

32. P10, L3. T_mean8 was never negative for such a northern site at the end of September? If negative values occurred, explain how they were dealt with (Equation (8) suggests a negative value for f_8day, which would make no sense).

33. P10, L6. Here and in Figure 5, replace "RMS" by "RMSE".

34. P10, Equation (9). Replace "\epsilon" by "\epsilon_max".

35. P11, L8. Delete the first sentence (repetitive with the previous paragraph).

36. P11, L15-16. The text should be expanded, because at this point the reader is still unaware that an outbreak occurred in 2012 within the EC tower footprint: please state this explicitly here and add what was the percentage of defoliation within the tower footprint.

37. P12, L1-2. The sentence is cumbersome; I suggest re-writing along the following lines: "For each year with insect outbreak, the regional reduction in GPP was computed by summing, over all pixels identified as defoliated, the difference between the mean GPP for no-outbreak years and the GPP for this specific outbreak year".

38. P13, L6. For consistency, replace "GDD threshold" by "GDD_thresh", and "temperature factor" by "T_thresh".

39. P14, Figure 6 legend. For consistency, replace "f_MOD8" by "f_8day".

40. P15, L8. Here and throughout the text (including Figures): provide full units for local, mean, and regional GPP (i.e., with yrˆ-1) even when writing "annual".

41. P16, L14-15. This sentence is repetitive with P11, L4-6.

42. P17, L1-2. Make it clearer that the 41.1 value was computed as the mean over no-outbreak years, based on the GPP_lue equation (add a number) given on P14. Please also add this information as a footnote to Table 2.

43. P18, Figure 9 caption. Replace "birch moth outbreaks" by "outbreaks of autumnal moth and winter moth".

44. P19, L2. The word "demonstrated" seems too strong; similar comment for P21, L17.

45. P19, L3. Replace "decreased with 260 g C mˆ-2" by "decreased by 261 g C mˆ-2 yrˆ-1" (based on Table 1, the difference is 261). Similar comment for P20, L25.

46. P19, L5. According to Table 2, the highest value is 265 +/- 93 (not 244 +/- 73).

47. P20, L31. Replace "turned a forest into a carbon sink" by "turned a forest into a carbon source". Stands were carbon sinks only during the growing season; the main point here is that insect-caused mortality led to negative annual NEP... although these stands seem to be recovering quickly, as correctly noted by the authors in the second part of the sentence!

48. P21, L1-2. For (partially) contrasting modeling results about the effect of insect outbreaks on the carbon balance, see Seidl et al. (2008), Albani et al. (2010), and Landry et al. (2016) (the last one is not the same reference as already cited in the manuscript).

49. P21, L10-11. Delete the whole "since it has been suggested [...] (Medvigy et al. 2012)" part of the sentence. The "spatial distribution of defoliation" issue addressed by Medvigy et al. (2012) dealt with tree-level defoliation within a stand (e.g., 100%

defoliation of 40% of trees vs. 40% defoliation of 100% of trees). This is not something addressed here, nor is MODIS-like remote sensing appropriate for this.

50. P21, L13-14. Delete the sentence: there is no firm basis for such an extrapolation, and the number would then likely end up being cited by future studies...

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