

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 #2

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The authors present an interesting article combining remote sensing data with eddy covariance measurements and a simple model to estimate changes in the carbon exchange of a birch forest due to moth outbreaks. The article is generally well written (particularly at the beginning), but the treatment of results needs more detail. There are a few methodological issues that need to be resolved or, at the very least, acknowledged and discussed in detail. Providing the limitations of the approach are properly considered and addressed, I believe this article should be suitable for publication.

Specific comments:

In several places the approach seems to take too simplistic a view, without properly discussing the assumptions or their impact. For example, a key result presented is the difference between GPP derived from EC measurements in an outbreak year (2012)

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compared to five other years without insect outbreaks. Unfortunately, it seems EC data were only available for one outbreak year, but there is no analysis of differences between years due to factors other than insect damage. Inter-annual variability in meteorological conditions (rainfall/soil moisture, solar radiation and temperature in particular) can result in different annual total GPP. The values given in Table 1 should therefore be analysed with respect to meteorological conditions. This should also allow the 2012 value to be given some context – if the insect outbreak had not occurred, would the 2012 total be lower/higher/similar to the average based on meteorological conditions alone? Furthermore, is it possible that the outbreaks in 2012 and 2013 contributed to the lower GPP obtained in 2014, or is this attributable to meteorological conditions?

The comparison with the 2004 results of Heliasz et al. (2011) on page 20 is useful and indicates that closer analysis of the temporal evolution of the EC data may be beneficial. Currently data are separated into years with and without insect outbreaks. During those years with outbreaks, does the reduction in GPP over the course of the growing season agree with the timing of insect population growth/insect damage? Is this also supported by the NDVI data? In the years classified as being without insect outbreaks, are there any effects of (albeit smaller) insect populations on the GPP or NDVI values? Perhaps such analyses could offer further insight into the refoliation effect; it is currently hard to draw meaningful conclusions on this subject from the information given in the article. Some evidence to support the assumption that NDVIDL captures refoliation would also be useful; Fig 3 is not very convincing in this respect.

P15, L6-10: Section 2.2.3 states that EC data were available from 1 May to 30 Sep, covering most of the growing season. Do the EC observations and values given in Table 1 agree with this timeframe, or is it possible GPP in Table 1 is underestimated if the growing season extended beyond these dates? This study focuses on GPP, but could the authors comment on other potential impacts of the insect outbreak on the carbon balance? For example, how might respiration rates be affected, and might this impact the partitioning into Reco and GPP?

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On a similar note, many of the decisions taken in the presentation of results and development of the model rely on data collected during non-outbreak years. Is the gap-filling approach also suitable in defoliated years?

P8, L10-11: More detail is needed about the quality control. Under which 'bad' weather/measuring conditions were data removed? How much data remained after quality control and what proportion was gap-filled?

P10, L11-2: Here, it is not clear which years have been used or why. The years for which the EC data are available should be stated in Section 2.2.3. Why was 2012 the only year used to calculate ϵ_{\max} with insect defoliation? Why were data from 2008 and 2013 not included/not available?

P12, L13: It is not clear how these statistics were calculated and they don't seem to follow from Fig 4. Please provide more details/clarification

P14, Fig 6: The two green lines for NDVIDL show higher NDVI values early in the growing season for the defoliated year. Possible reasons for this should be investigated and the impact on the results commented on.

Minor comments:

P2, L22: Change 'difference' to 'differences'

P2, L26: Would be useful to give the land cover type for the southern France site

P3, L17: Change to read 'of the form'

P3, L18: Number this equation and update the others accordingly

P3, L20-1: Change to read 'and with variability in meteorology'

P4, L24-6: It is not clear that these recent outbreaks are for the study site – please state the area they apply to. Also give some information about the EC tower in that study (i.e. location, and mention the flux measurements were also for the birch forest)

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P5, L10: Change 'derived' to 'derive'

P6, L16-7: Change to read 'to be about'

P6, L18: Keep the order of 'east/west' the same as previously (L17). When the wind is from the west, the footprint is located to the west of the tower

P6, L24: Change 'wind speeds' to 'stability', as wind direction and stability tend to be the major controls on EC footprints

P7, Fig 3: Change y-axis of RH plot to 'NDVIDL'

P8, L16: Change 'measured the' to 'the measured'

P9, Eq3: Units on LHS do not equate to units on RHS

P11, L22-23 Suggest choosing alternative notation for GPP_{reduction}, as it is a ratio of GPPs, rather than GPP itself

P12, L6: Change 'accurate' to 'accurately'

P13, Fig 5: Add units for RMS

P14 L5-6 and Fig 7: The text mentions 'low GPP observations' but in Fig 7 it looks as though the modelled GPP values are lower than the EC observed values, with several zero values. Please clarify

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