Biogeosciences Discuss., 7, C5502–C5505, 2011
www.biogeosciences-discuss.net/7/C5502/2011/

Biogeosciences Discussions

Interactive comment on "Role of de novo biosynthesis in ecosystem scale monoterpene emissions from a boreal Scots pine forest" *by* R. Taipale et al.

R. Taipale et al.

risto.taipale@helsinki.fi

Received and published: 7 July 2011

We thank the referee for the review and the good suggestions.

The referee had two main issues. First, the referee was concerned about the quality of the statistical analysis. We improved the analysis by inserting emission–temperature, emission–PAR, and temperature–PAR correlations calculated from all, daytime, and night-time measurements. As suggested by the referee, we also performed a restricted range analysis to determine whether the ecosystem scale monoterpene emissions had a light dependent component.

Second, the referee was doubtful about the reliability of the results derived using the

hybrid emission algorithm of Ghirardo et al. (2010). We chose the algorithm due to its simple formulation based on the widely used Guenther et al. (1991, 1993) algorithms for pool and de novo emissions. In this case, the formulation had only two free parameters: the total emission potential (E_0) and the ratio of the de novo emission potential to the total emission potential (f_{synth}). Large uncertainties (95% confidence intervals) made the results useless when more free parameters, such as the temperature dependence coefficient for pool emissions (β), were included. The values of f_{synth} were also sensitive to the amount of data. Due to the large uncertainties, they did not show a significant contribution from de novo emissions when the number of observations was reduced by applying flux quality criteria or using only daytime measurements. Thus we added information on the reliability of the determination of f_{synth} to Sect. 3.2. We also toned down our conclusions on the significance of de novo biosynthesis in the ecosystem scale emissions.

Response to the specific comments

P8023, L14–16: As stated in the discussion paper, the flux quality criteria eliminated many near-zero observations. Despite their low confidence, these observations were included in the analysis as they represented low emissions near or below the detection limit. Their exclusion seemed physically unjustified and, as mentioned above, affected the values of f_{synth}. We added the following note to the text: "In total, these quality criteria would have eliminated 23–42% of the data, which also would have decreased the statistical significance of the analysis (see Sect. 3.2)."

P8025, L7: Right. The revised statement reads: "Differences in PAR within the canopy were not considered when fitting the algorithms and bark biomass was not used as a proxy for storage pools."

P8025, L18–19: This slightly irrelevant sentence was removed.

P8025, L20: We improved the statistical analysis as described above.

P8026, L10: We changed ”from the canopy” for ”from the aboveground biomass”.

P8026, L11: To better illustrate the diurnal and seasonal variations, we calculated the monthly medians and correlations from all, daytime (12:00–16:00), and night-time (00:00–04:00) measurements. We also replaced Fig. 2 with a table which contains the medians and correlations.

P8026, L17: The diurnal distribution of the observations was rather uniform for each month. In addition, the daytime and night-time medians were calculated separately. Thus corrections for data gaps were deemed unnecessary.

P8026, L24: The paragraph was removed.

P8027, L18–24: We amended the statistical analysis as explained above and added a note on the correlation between PAR and the needle temperature to Sect. 3.1. The higher PAR in June was probably due to reduced cloudiness since PAR was measured well above the canopy.

P8028, L20: Right. We changed ”in Scots pine needles and bark” for ”in Scots pine”.

P8028, L27: The 95% confidence intervals were inserted in the text.

P8029, L19–21: We replaced the whole paragraph with a new one which discusses the reliability of the determination of f_{synth}.

Section 3.3: The new Fig. 4 shows that the hybrid and pool algorithm performed equally well when estimated from the monthly correlations between the measured and modelled emissions. We corrected the statement about the algorithm performance. Now we state that ”hybrid algorithms seem biologically more realistic than pool algorithms” as previous $^{13}CO_2$ labelling experiments (Shao et al., 2001; Ghirardo et al., 2010) have clearly demonstrated the substantial contribution of both pool and de novo emissions from Scots pine. We did not find mistakes in the non-linear regression calculations which would have explained the differences between the measured and modelled daytime medians in June. However, these differences were not significant at the

95% confidence level.

Conclusion 1: We used also midday values in the revised manuscript but came to the same conclusion as in the discussion paper.

P8031, L1–2: As mentioned above, we tried applying flux quality criteria and using only daytime measurements, but then the uncertainties in f_{synth} were even higher. This was due to the reduced amount of data. The new suggestion is: ”However, more reliable estimates of the contribution of de novo emissions are needed for improving monoterpene emission algorithms.”

All figures: The font size of the axis labels was increased.

Figure 1: The data gaps and the 30-year reference temperatures were removed.

Figure 4: A new figure showing the monthly correlations between the measured and modelled emissions was inserted.

Interactive comment on Biogeosciences Discuss., 7, 8019, 2010.