Biogeosciences Discuss., 7, C4163–C4166, 2010
www.biogeosciences-discuss.net/7/C4163/2010/

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.

Anonymous Referee #2

Received and published: 1 December 2010

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

The discussion paper by Taipale et al. presents an estimate of the relative importance of de novo synthesis and storage-related release of monoterpenes in a Scots pine ecosystem. Ecosystem fluxes were measured with the disjunct eddy covariance method, and the measurements were analysed statistically to obtain estimates for the emission potentials and the fractions of de novo synthesis and pool release. Based on the analysis, the authors conclude that de novo synthesis contributes significantly to the ecosystem monoterpene fluxes from Scots pine.

The analysis is interesting, and attempts to quantify an assumption that has been posed qualitatively in previous studies: The generally assumed light independence of monoterpene emissions holds only for part of the emissions, and de novo synthesis plays a role as well. This makes it an interesting topic that is relevant to the audience of Biogeosciences, and that is worth a publication. However, in its attempt to quantify this, the study has some critical weaknesses that need to be resolved before the outcome of such an analysis can indeed be trusted as a quantitative estimate of the role of de novo biosynthesis. I highlight my main concerns below, and hope that the authors are willing to adjust their study.

Major remarks:

- page 8023, line 25: The statement that the formulation has two free parameters is misleading: The equations for C_T, C_L and γ have many more parameters, but the authors assume these not to change from the traditional formulations that assume either light-dependent or light-independent emissions. This assumption should be highlighted and discussed more than is done currently, because the original formulations (including the parameters used in there) are derived based on the assumption that there is either no pool-related emissions, or only pool-related emissions, assumptions which are not adopted by the authors.

- page 8024, line 25: The determination of the model parameters $E_{0,synth}$ and $E_{0,pool}$, which is most essential for the conclusion that de novo synthesis is a significant factor, needs to be explained in more detail than is currently done. The two algorithms combined in equation (1) are not independent, because both of them contain a temperature dependence. Moreover temperature and light show a clear correlation, as pointed out by the authors, both in the diurnal cycle and in the annual cycle. These dependencies need to be quantified, and the current description ("using non-linear regression in the least squares sense") does not provide the detail needed to judge the method. Least squares non-linear regression is not a trivial and unambiguous method, and the authors should provide a better description of the assumptions/a priori estimates/optimization technique for the non-linear regression.

- Most pronounced changes in the relative contribution of biosynthesis to the total emissions would be expected from the difference between daytime (with biosynthesis) and nighttime (without biosynthesis) emissions, with a second, but (presumably) less pronounced trend in the annual cycle. In the current method, these two timescales are merged into one dataset, and the regression has been performed on the whole set as such. In the analysis, the difference in the trend between the different months (Fig. 3) is highlighted, but the trend in the diurnal cycle can only be guessed from Figure 4. Has there been a correction in the regression for nighttime observations? Obviously, results for $E_{0,synth}$ (or f_{synth}) for nighttime are not to be trusted (because C_L=0). And would an analysis that focusses more on the diurnal differences yield similar results?

Minor comments:

- page 8023, line 25: The argumentation used for the choice of the algorithm here is that the formulation is "extremely suitable", which seems inane (one would not choose an unsuitable formulation).

- page 8025, line 7: Does this mean that there is no difference assumed in the relative importance of de novo synthesis and pool emissions between the top of the canopy and the shaded parts? And is this assumption realistic?

- page 8026, line 1: What is the word "now" in this sentence referring to? Any particular period in the measurements?

- page 8027, line 15: Would a reasoning based on the light-dependent or hybrid algorithm give different results? Obviously, the amount of PAR changes over the season as well.

- page 8028, line 15: What could be the causes for changes in the diffusivity? Are the authors thinking of a change in the monoterpene composition towards lower volatility, or a physical change of the diffusion within the leaves or from the leaves to the

atmosphere?

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