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Biogeosciences Discussions

Interactive comment on "Chemical composition of volatile and extractive compounds of pine and spruce leaf litter in the initial stages of decomposition" *by* V. A. Isidorov et al.

Anonymous Referee #1

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The authors report their findings on emissions of volatile organics and concentrations of extractive compounds of pine and spruce litter. They present quantitative data for terpenoid and tertiary metabolite emissions from litter and concentrations of water soluble compounds in the leaf litter. These topics are important, the paper address relevant scientific questions within the scope of BG and it presents novel data. The manuscript is easy to read, the description of the methodology is quite good and the experiments seem to have been conducted carefully using an established method. I'm impressed by the extent of the data set given in the supplement. Overall, I find this paper highly interesting and the content certainly merits publication. However, there is one point that should be written very clear and concise.

To demonstrate my major concern I just use the example of total terpene emissions during the first 77 days of the experiment.

Assuming that the numbers given in table 2 are in units of μg /g(dw), the loss of total monoterpene hydrocarbons within the first 77 days would be 58 μg /g(dw) for pine litter. But using the emission rate of about 1.9 μg / (g (dw)x h) (Table 1) would indicate that after 77 days about 3.5 mg terpenes per g(dw) have had evaporated; much more than existent as total content at the beginning of the experiment.

The same point but looking the other way around: Assuming validity of mass balance and supposing that the dead plant matter does not biosynthesize terpenes implies that the differences shown for terpene concentrations (Table 2) are upper limits for the mean emission rates between the respective measurements. If so, an upper limit for the mean total monoterpene emission rates for pine litter between day 0 and day 77 would have been in the range of 0.03 μg / (g (dw) x h). This may be due to low temperatures during the first 77 days. Nevertheless, this is a very low emission.

On the other hand it is stated that even 1.5 years after removal of needles the emissions are on the same order of magnitude as emissions from living plants. This is possible when the reserve of these compounds is very high (chapter conclusions) and this is indeed shown by the authors' results. However, one method to keep a high reserve is a negligible loss. If, as estimated above the emission rates from litter are very low, the emissions can stay for a long time. But these emissions cannot be both, high and long lasting. The capacity of the litter to emit such compounds is limited because dead plant matter does not synthesize these compounds. Again, using the number given in table 2 for the content of monoterpenes in litter (2.53 mg / g (dw)) and emission rates between 0.8 – 12.1 μg / (g(dw) x h) similar to those of living needles (p. 1737 line 27) the emissions would last between 9 days and about 4 months. This is much shorter than 1.5 years.

In summary, I'm a little bit confused when comparing numbers in the tables to some

statements. Indeed, a comparison between emissions from litter and emissions from living plants is necessary to assess the relative importance of litter emissions. The authors should discuss the importance of litter under consideration of basic processes underlying the emissions. I suggest to publish this manuscript in Biogeosciences after this comparison is written in a clear and precise way.

Some minor points:

Table 2 lists the fractional composition of extracts and mean concentrations of some selected compounds. In the superscript of the table units are μg/g but it is not clear what this means for compound concentrations. Does it mean μg / g(dw) or μg /g(hexane-extract)? Please clarify.

The formula showing the calculation of emission rates (p. 1734 line 23) does not include the initial gas phase terpene concentrations (Ciog, p. 1735 line3). Please correct either formula or sentence.

It is not easy for a reader to find the citation Isidorov et al., 2009. Checking this citation one can read the same statement as that in the conclusion but no numbers. Either delete the sentence written in the conclusion (p. 1742 lines 20 – 22) or exchange Isidorov et al., 2009 by a reference where the statement is proven, preferentially a peer reviewed journal.

Table 2: typo “3-Karene”

Interactive comment on Biogeosciences Discuss., 7, 1727, 2010.