Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-595-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Volatile Organic Compound fluxes in a subarctic peatland and lake" *by* Roger Seco et al.

Anonymous Referee #2

Received and published: 19 August 2020

In their manuscript, "Volatile Organic Compound fluxes in a subarctic peatland and lake," Seco et al. present the results of flux measurements of volatile organic compounds (VOCs) at a subarctic fen and lake. The methods used are sound and are explained clearly and thoroughly. The results are important in that they provide one of the few measurements of VOC fluxes from these types of biomes in an understudied geographical region. The observations show that the fen is a source of many VOCs, particularly isoprene, and that the isoprene temperature response is stronger than is often assumed based on lower latitude data. Conversely, the lake appeared to be a sink of acetone and acetaldehyde. Overall, the study is of high quality and I recommend publication following minor revisions as described below.

As the authors point out, the results show that (1) there is a large difference between

Printer-friendly version

Discussion paper

the air temperature and vegetation surface temperature and (2) VOC emissions are extremely sensitive to temperature in this region. This is an important finding that can be used to improve model estimates of VOC emissions at high latitudes. It also suggests the importance of accurately measuring the temperature. I would suggest that the authors discuss in more detail the uncertainties associated with the temperature measurement method, how it compares with contact measurements of vegetation surfaces (or cite appropriate references). They should also discuss the uncertainty introduced by using a surface temperature measurement obtained some distance from the flux measurement site. Given these uncertainties, what is the uncertainty in the calculated Q10 values?

Lines 266-269: "Indeed, the response of our isoprene emissions to air temperature was even steeper (Q10 = 131; blue triangles in Fig. 5) than to surface temperature, which could translate into increased modelled isoprene emissions if implemented in models that do not calculate the vegetation temperature but instead use air temperature to drive biogenic VOC emissions." This sentence is overly long and while I understand what the authors are trying to say, it's not stated very clearly. I suggest separating into two sentences (replace the comma with a period), and rephrasing the second part of the existing sentence. In particular, the authors should more specifically state how the implementation of the Q10 result in models would lead to increased modelled isoprene emissions. It seems like an error would arise if there was a mismatch between the Q10 value and the temperatures used (i.e., using the high Q10 from air temp, but using leaf surface temps in the model, or vice versa) and the direction of the error would depend on the sense of the mismatch (Q10(air) + Tsurf vs. Q10(surf) + Tair). Please restate to improve clarity.

Lines 335-337: "Air temperature also influenced the flux of these two carbonyl VOCs. Acetone deposition was more intense at higher air temperatures, in July, when its mixing ratios were also higher (Fig. 6)." The authors state that air temperature "influenced the flux of these two carbonyl VOCs," but it seems like this could be simply correlation

rather than causation. The difference in flux between the two time periods (July and September) happens to coincide with a change in temperature, but also with a difference in mixing ratios. Also, the sense of the relationship is opposite in the two cases, with acetone deposition being higher at higher T, whereas acetaldehyde deposition is higher at lower T. Is there a mechanistic explanation for this? If not, and the relationship with temperature may not be causal, I would suggest rewording to clarify this.

Lines 357-358: "Instead, average methanol fluxes showed both net deposition and emission along the day during both seasons." How statistically significant is this conclusion? From Figure 3, it appears that the blue shaded region representing +/- 1 standard deviation overlaps 0 for most if not all data points (possibly excepting the last July data point, which I believe represents only a single measurement for that time window). Given the relatively sparse data and indicated standard deviations, wouldn't it be more accurate to say that the results indicate little to no flux (emission or uptake) of methanol to/from the lake?

Lines 395-396: "...similar to our July average of 4.7 $\pm$ 3.1 μmol m-2 day-1 (Table 1)." Referring back to lines 318-320, which state, "In particular, compounds such as DMS and monoterpenes had mean daily fluxes dominated by one or two hourly average data points that were not actually hourly averages, since they were based on only one measurement during that hour (i.e. data points without shading in Fig. 3)." along with the data shown in Figure 3, there appear to be two time periods with large positive fluxes representing single measurements. How were the daily averages calculated? Were they an average of all measurements equally weighted, or an average of the hourly averages? If the latter, that would give disproportionate weight to the high "hourly averages" that represent single data points. Please clarify in the text.

My last few comments regarding the lake fluxes suggest that the authors should consider alternate ways of analyzing and presenting the lake data to increase the statistical robustness of the results. For example, instead of hourly averages, they could consider averaging over 2 or 3 hour time periods to increase the number of data points in each

time period and improve the statistics, perhaps allowing for more definitive conclusions.

Minor grammatical changes:

Lines 270-273: “At the same Stordalen wetland as our study, Holst et al. (2010) found a steep temperature response to air temperatures above 15 °C, in agreement with our results (Fig. 5). Nevertheless, our high Q10 values corroborate that Arctic vegetation can have a stronger temperature sensitivity compared to plants from lower latitudes, which underpinned the most used biogenic emission models (Guenther et al., 2006), as already suggested from previous high-latitude studies (Holst et al., 2010; Kramshøj et al., 2016; Lindwall et al., 2016b, 2016a; Rinnan et al., 2014).” The second sentence is overly complicated. It obscures the important point that the high Q10 found in this study differs from model values based on lower latitudes. I would suggest rearranging the two thoughts into different sentences. Also, “underpinned” should be “underpin”. E.g., “At the same Stordalen wetland as our study, Holst et al. (2010) found a steep temperature response to air temperatures above 15 °C, in agreement with our results (Fig. 5) and other high-latitude studies (Holst et al., 2010; Kramshøj et al., 2016; Lindwall et al., 2016b, 2016a; Rinnan et al., 2014). Our high Q10 values corroborate that Arctic vegetation can have a stronger temperature sensitivity compared to plants from lower latitudes, which underpin the most used biogenic emission models (Guenther et al., 2006).”

Lines 364-365: “Nevertheless, our available data showed maximum hourly average net emissions of 1 nmol m-2 s-1, being the daily average net rate of 0.24 $\pm$ 0.12 nmol m-2 s-1 (equivalent to 20 $\pm$ 10 μmol m-2 day-1; Table 1).” The wording of this sentence is awkward and confusing. Assuming I’m interpreting the authors’ intent correctly, I would suggest replacing “being” with “and”, e.g., “Nevertheless, our available data showed maximum hourly average net emissions of 1 nmol m-2 s-1, and a daily average net rate of 0.24 $\pm$ 0.12 nmol m-2 s-1 (equivalent to 20 $\pm$ 10 μmol m-2 day-1; Table 1).”

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-595,

2020.

Printer-friendly version

Discussion paper