Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2022-396

Anonymous Referee #2

Referee comment on "Atmospheric biogenic volatile organic compounds in the Alaskan Arctic tundra: constraints from measurements at Toolik Field Station" by Vanessa Selimovic et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-396-RC2, 2022

The manuscript by Selimovic et al. presents a comparison between VOC mixing ratios measured at the Toolik Field Station (TFS) in Alaska and mixing ratios obtained from the GEOS-Chem chemical transport model coupled with the MEGANv2.1 biogenic emission model. The total amount of reactive organic carbon (ROC) and the OH reactivity (OHr) are also compared between observations and model results.
There is a subtantial degree of uncertainty regarding the biogenic VOC emissions and the mixing ratios predicted by models, in particular in the Arctic region where there are few observations available. In this sense, the dataset and modeling exercise presented here are a first step towards filling this knowledge gap.
I must first mention that many –a lot!– of the literature references mentioned in the manuscript are missing from the reference list, which makes reviewing this manuscript very difficult. This is the first issue that the authors must address before working on a new version of the manuscript. Then, there are several other, scientific issues that need to be worked out before the manuscript can be accepted for publication, in addition to the caveats already raised by referee #1.

MAIN COMMENTS

Equation 1: I suspect that the excess of "expexp" is a typo. But I would like to know why the authors use a value of 200 for the coefficient named "CT2" in MEGANv2.1, while the value used in MEGANv2.1 is 230.
Line 232: MEGANv2.1 (Guenther et al 2012) assigns a light-dependent fraction of 80% to methanol. The authors here assign only 20%. Is this a mix-up or do the authors take these new values from some unknown source, for some unknown reasons?

Line 228: Authors can say that they used the air temperature instead of the leaf temperature due to the lack of leaf temperature measurements. However, in Arctic vegetation the discrepancy between those two temperatures can be huge. See for

example Lindwall et al (2016) and Seco et al (2020). Some comment about this should be included. Also in Line 243, I suggest taking with caution the stated difference of only 0.4 degrees C between the observed ambient temperature and the modeled surface temperature. Of course, the fact that the presented measurements only span the early part of the season may be the reason why the air and surface temperatures might not differ too much (the sun may not heat the surface as much as in July-August).

Figure 6: Isoprene temperature response is typically thought to approximately follow Equation 1 (100% light-dependent), and the authors also follow this line of though (see line 216). Why is then Equation 2 used instead in Figure 6 for the isoprene temperature response (methanol could be somewhat similar if we follow MEGAN's 80% light-dependence for methanol)? I understand that it is easy to simply derive a beta value that can easily be compared. I would suggest to, at least, comment on this. Also, have the authors checked what is the reason that the model predicts low isoprene emissions at 17-18 degreesC while the observations are much higher in that temperature range? Is it because the real PAR conditions were much different during those timest than in the meteorological data driving the model?

Line 373: I cannot see how the 20% discrepancy in PAR (the authors do not say the sign of the discrepancy but in Fig. 1c the modeled PAR is clearly underestimated by the model, at least on average) can make up for an underestimation of the temperature activity factor, because the light activity factor will be understimated as well. The rest of the paragraph needs some redoing since I found it confusing. Why is MCAR+MVK a more robust tracer to avaluate model isoprene emission?

Line 419: do the authors have any information about methane mixing ratios at TFS to be able to confindently exclude that methane oxidation is an important source of formaldehyde?

MINOR POINTS

Lines 26-27: I found the sentence about isoprene unclear. Please rephrase.
Line 212: "accounts for considers" should be either "accounts for" or "considers"
Line 253: I suggest replacing "masses" with "compounds"
Line 349: "only" is duplicated
Line 365: "9:00" should be "21:00"
Line 377: is the sentence ending in this line referring to Fig. 5? If so, please state it.
Line 380: The "comparisons of measured versus simulated" are shown in Fig. 3? Or Fig. 5? Or both? Please clarify this in this sentence to guide the reader.
Line 474: should the last word be "modeled" instead of "measured"?
Line 501: I suggest using past tense
Figure S5: This is not mentioned in the text, but I am curious about the last (right-most) measured datapoints of the graph. Shouldn't the air be well mixed at that point of the day (as the model suggests and the authors point out for the left side of the graph)? Is the high red point (0m) due to emission by vegetation at the surface?

References

Guenther, A., Karl, T., Harley, P., Wiedinmyer, C., Palmer, P.I., and Geron, C.: Estimates of global terrestrial isoprene emissions using MEGAN (Model of Emissions of Gases and Aerosols from Nature), Atmos. Chem. Phys., 6, 3181–3210, https://doi.org/10.5194/acp-6-3181-2006, 2006

Lindwall, F., Schollert, M., Michelsen, A., Blok, D., and Rinnan, R.: Fourfold higher tundra volatile emissions due to arctic summer warming, J. Geophys. Res.-Biogeo., 121, 895–902, https://doi.org/10.1002/2015JG003295, 2016

Seco R, Holst T, Matzen MS, Westergaard-Nielsen A, Li T, Simin T, Jansen J, Crill P, Friborg T, Rinne J, Rinnan R: Volatile organic compound fluxes in a subarctic peatland and lake. Atmospheric Chemistry and Physics, 20: 13399–13416, https://doi.org/10.5194/acp-20-13399-2020, 2020