

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2022-396

Anonymous Referee #1

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.,
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The manuscript describes a very important dataset from an under-studied region for biosphere-atmosphere exchanges and surface atmosphere chemistry: the Alaskan Arctic. The presentation quality of the manuscript is exceptionally good and I only found one typo, which is quite unusual. The figures convey the results well and are clear and well captioned. All of the methodology is adequately explained or referenced appropriately. The writing throughout the manuscript is strong and impactful. The science is very important. The Arctic is ongoing rapid environmental change, driven by increasing air temperatures. This causes a cascade of effects, including shifts in ecosystem type and composition. These changes have the potential to greatly alter atmospheric chemistry in the Arctic, but we currently lack adequate observations of the current state of the system. These measurements are unique and the comparison to a global chemistry model is essential. This is especially true since satellite methods of testing global chemistry models do not provide reliable data over the Arctic.

One concern with the presentation of the data is that only the early part of the growing season was captured. The measurement period ends just after the solstice, which is essentially the start of the most productive period for the tundra ecosystem. For example, isoprene concentrations are relatively low until the last week of the sample period and I would expect them to increase further in July. While this dataset is still important, the authors need to include some more cautions and caveats. As I note in the detailed comments below, this is particularly true in the conclusion section of the manuscript. While the agreement between the measurements and the model, particularly for isoprene, is encouraging, the ability of the model to predict relatively increased emissions in July is not tested.

Also, the discrepancy with methanol is interesting, but some of the physiological aspects should be discussed. Methanol emissions are often linked to plant growth and expansion. So the sensitivity test of increasing MeOH emissions by 3x is interesting, but perhaps it's only appropriate for the early part of the growing season? This should be explored in the

discussion.

Detailed comments

Lines 140-142: Should compare your measured temperatures with long-term averages available from the Toolik weather station. Was your sampling period typical, warmer or cooler compared to the average?

Line 212: Fix "accounts for considers."

Line 228: "T is the 2m air temperature which is assumed to be equivalent to the leaf temperature." Note this is not a good assumption. This issue is discussed later in the manuscript, but some note should be made at this point about the problems with this assumption.

Line 270-277: The rapid increase in isoprene concentrations (and presumably emissions) is due to both changing temperature and also phenology. Of course, temperature is ultimately driving phenology in this ecosystem. You mention phenology, which is good, but it would also be helpful to explicitly reference the observation that leaf-level isoprene emission is delayed relative to photosynthetic capacity.

Line 376-377: Need to limit this conclusion and specifically note that this is only true during the relatively cool season during which the measurements were performed. Given the non-linearities involved, 20% during this period could increase during a warmer period: for example, July.

Lines 460 and 474-475: Again, should note that these values for isoprene might increase later in the growing season.

Line 532: Need to insert something to the effect of "during the early-season study period."