

# ***Interactive comment on “Assessing the vertical structure of Arctic aerosols using tethered-balloon-borne measurements” by Jessie M. Creamean et al.***

## **Anonymous Referee #2**

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This paper describes many observations of vertical profiles size-segregated aerosol particle number concentrations and state parameters conducted from the US-DOE ARM site at Oliktok Point in Alaska using a tethered-balloon system (TBS). The main objective of the study is to address the question of how representative ground-based aerosol observations are of aerosol concentrations that feed into low-level Arctic cloud. The answer, based on 63 profiles (out of 282 in total) with particle concentrations measured at the ground and at cloud base, is that ground-based concentrations represent cloud-base concentrations only 14% of the time. The percentage is low and perhaps not surprising considering the relative stability of the Arctic atmosphere. Overall, the presentation of the paper is very good, the study is straightforward and the results

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are useful, in so much as they are for one location in the Arctic. Given reasonable responses to my few comments/questions, I would recommend publication.

Comments/questions:

1) On lines 44-46, the authors refer to higher particle mass concentrations in the Arctic in winter and spring (due to Arctic Haze) and relatively pristine concentrations in summer. It is a little difficult to extract from this paper whether the above statement applies to the Oliktok site. For example, if we think of Arctic Haze as being represented by the light-blue bars in Figure 6, we might derive the opposite conclusion for this site. Is this site impacted by the oil and gas industry, or perhaps by winds that lead to suspension of sea salt, enough that it does not fit into the above introductory statement the authors have made about the Arctic? There needs to be some discussion of this in the paper.

2) Line 113 – In what ways are Arctic clouds more sensitive to modulations of aerosol particles than clouds from more southern latitudes? Also, does the statement refer only to liquid-phase clouds or does it embrace the ice phase as well?

3) Lines 162-165 – I understand the need to simplify the TBS data. However, the implication here that the size range of 140 nm to 3000 nm is the only relevant size range for cloud activation is incorrect. In the pristine Arctic summer, the concentrations of larger particles (>100 nm) can be diminished so much that particles much smaller than 100 nm are activated in cloud. Under such conditions, particles as small as 50 nm often activate, and particles as small as 20-30 may activate (Leaitch et al., ACP, 2016). Related to comment 1 above, it may be that concentrations of particles in the 140-3000 nm range at Oliktok are sufficient to inhibit activation of smaller particles, but this point needs to be clearly discussed in the paper.

4) Lines 186-187 and line 254 – There is a statement on lines 218-219 defining ground-based concentrations, but it would help to clarify on line 251 that the comparisons were not done with the respective TBS and ground-based counters sitting side-by-side, and that the comparisons are between the TBS flights, constrained to 20-40 m-msl, and the

ground-based measurements. Were the counters ever compared while sitting side by side?

5) Line 265 – Here you say that number concentrations were higher when particles were smaller and vice versa, referring specifically to profiles 260-280. It is very difficult to assess this statement using just the colour scale plot in Figure 5. Would you add a panel showing the mean number concentrations and mean sizes that would clearly demonstrate this point?

6) On lines 296-299, you indicate possible summer sources as anthropogenic, biogenic and wildfires. One lines 304-308, the implication is that the higher POPS number concentrations in the summer were mostly due to biogenic. Would you make this discussion a little clearer? Underlying my concern, here and in comment 1, is that there are local oilfield emissions, but you don't give a good idea of how significant those emissions are to your measurements. Are there publications about this from the Pratt group that might help? Are biogenic emissions able to produce the POPS concentrations you have measured?

7) Lines 317-319 - The combined processes are complex and I don't see how they can be so clearly distinguished. For example, wet removal is not a constant with height, and therefore also plays a role in the vertical distribution. The atmospheric stability at an emissions location will play a significant role in the vertical distribution, and therefore the two are closely linked. I view your item 4 below as an example that this statement is not always true. Some revision of this sentence is needed.

8) Lines 341-343 - Could another explanation be that local/regional surface sources diluted as they mixed upward?

9) Lines 375-376 - Do you mean late summer, rather than late spring? As you state on lines 356-357 and show in 9c, there were no such spring cases.

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