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Atmospheric Measurement Techniques Discussions

Interactive comment on "HOVERCAT: A novel aerial system for evaluation of aerosol-cloud interactions" *by* Jessie M. Creamean et al.

Anonymous Referee #1

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The manuscript by Creamean et al. (2018) introduces a novel measurements platform by which vertical profiles of aerosol size distributions (OPC) and multiple filter samples for ice nucleating particle (INP) analyses can be obtained. Furthermore, the authors characterize a newly build droplet freezing assay (DFPC – Droplet Freezing Cold Plate) and examine the uncertainty in temperature, test for dependencies in cooling rate and droplet size, and evaluate several hydrophobic coatings for the substrate. The second part of the INP analysis featured a deposition ice nucleation experiment coupled with Raman microscopy.

INP filter samples were collected with the newly build TRAPS (Time-Resolved Aerosol Particle Sampler), which was attached to HOVERCAT (Honing On VERtical Cloud and Aerosol properTies). HOVERCAT is a retrievable balloon-based measurement plat-

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form, which theoretically should be able to hover at a constant altitude by the use of positive (sand ballaster) and negative (lift-gas) buoyancy adjustments. However, the manuscript presents only one successful test flight (out of only three existing flights). Even in this flight the altitude control has had its problems and could not perform the scheduled flight plan due to unfavorable weather conditions. As a result, during four out of six of the sampling periods the system was unable to maintain a constant altitude. This is the biggest problem with the manuscript. The publication of this manuscript seems a little hastened. First (?) test flights were conducted in late May 2017, then the collected samples were frozen for about six months, and the manuscript has been submitted in early February 2018. HOVERCAT (and the manuscript in turn) would have benefitted from some more extensive tests to really proof the principle (and name-giving signature ability) is reliably working. The authors know about this caveat as they emphasize that HOVERCAT in its current state is considered to be a prototype in the experimental development Phase I. Yet, I am not sure whether or not this classification as Phase I alone justifies the publication of the manuscript in this state.

The manuscript itself is well written and fits into the scope of the journal. The figures and scientific methods are of good quality. As the authors have correctly discerned there is a great need for more airborne INP measurements, since ice actually forms in clouds and not at the ground, where still most INP measurements are conducted. Here, the manuscript provides a valuable contribution to the community by introducing a promising alternative to “regular” aircraft-based missions. This leaves me with ambiguous feelings about a recommendation to publish the manuscript.

I encourage the authors to resubmit a revised manuscript that demonstrates the applicability of HOVERCAT under flight technical aspects more convincingly than it does at present. One more successful flight showing that the system is indeed able to sample the atmosphere at prescribed levels should be presented (maybe as a supplement without the INP analysis). In its present form the manuscript is a very interesting contribution to AMTD, yet of pre-print character.

General/Major Comments:

1) As mentioned above HOVERCAT's ability to hover at a constant altitude could not be reliably demonstrated (Figs. 2 and 7). Only 2 out of 6 samples show stable enough altitude conditions (one of which was collected close to the ground). Page 6, Line 3 states the original flight plan was a 5-step altitude profile with 500 m steps each. Yet, the actual flight profile looks nothing like that. Section 3.1 explains that the plan could not be fulfilled because of too windy conditions and a time delay between command and execution of buoyancy adjustments. The reader is then reassured that on a calmer day it might work. Yet, it leaves the reader wondering. How to achieve it in the future? Why didn't the authors do more flights until, the flight plan worked as planned? The rate of 1 out of 3 flights producing high enough sample volumes to be analyzed for INPs, seems also to be improvable. Have there been more test flights in the meantime?

In Section 3.4, where the future directions of HOVERCAT are listed, point 2) says: "operate successfully on a routine basis", however, I feel this should be a prerequisite for the platform, before thinking about publishing. It feels like step two is done before step one. In the Conclusion the platform is stated to have "the capability to hover at desired altitudes, making it an ideal system to collect sufficient aerosol loadings at a range of altitudes up to 2.6 km AMSL". The system may be capable of doing it (as sample 4 might suggest), yet I don't feel like it has proven without a doubt.

2) On a more technical note: Why didn't you sync the profiles with the sampling times? Wouldn't it be better to have a flexible (e.g. command based) way to communicate with the sampling system to adjust for difficulties and only start the pump when the profile is stable? Alternately, why not include a buffer of some minutes between prescribed sampling periods to account for maneuvering to reach the desired altitude?

3) In the Introduction the authors describe why more airborne INP measurements are needed and that there is a lack of alternatives to aircraft-based missions to gain vertically resolved information about INP. In this regard the paper by Schrod et al. (2017)

should definitely be cited and summarized. Schrod et al. (2017) describe the first INP measurements made onboard of an unmanned aircraft system (UAS) and follow a similar train of thought. On a similar note, the paper by Ardon-Dryer et al. (2011) should be mentioned as well. In it INP samples were collected at the ground and from a tethered balloon in Antarctica.

4) Fig. 6: Why didn't you wait until all drops are frozen – is it due to instrument limitations? Also, Page 7 Line 28f says the measurements were continued, until all droplets were frozen. Which fraction is used for calculating the INP concentration by the Vali equation (Fig. 8)? The ones shown in Fig. 6 or the actual number of frozen droplets relative to the total number of droplets? If the frozen fraction as shown in Fig. 6 were used, you'd create a bias towards a more ice active sample. I would rather show the plots as done in Figs. 4 and 5 (with the frozen fraction not normalized to 100%) to avoid confusion about this matter.

5) Fig. 10: Does it show the onset conditions of the first observed ice nucleation? Or is it a constant activated fraction of X %? Is there any quantitative measure of this ice nucleation technique available at all? From what I understand from section 2.3.2 it is not (at least in this study). Therefore, I would advise to be careful in making too strong statements, when just the first ice activation was observed. Go through the paragraph with that in mind. E.g. rewrite the sentence on Page 12 Line 1ff to something like this: "Overall, ice activation onset conditions between the six samples were similar at all temperatures tested. However, at -40°C Samples 3 and 4 showed first ice nucleation activity at saturation ice ratios X to X % earlier than the other samples and may be characterized as more efficient deposition INPs at that temperature as compared to the remaining samples. However, it should be emphasized that the presented results are not of a quantitative measure."

6) My impression is that sections 3.2 and 3.3 are somewhat overanalyzed in terms of trying to explain the sources etc. After all, the result section of this manuscript depicts only six consecutive 30 minute samples of one day, most of which feature

a considerable variance in sampling altitude, which should make it difficult to make general statements about the different samples. Also only 3 – 5 INPs were analyzed with Raman spectroscopy from each sampling spot, again making it difficult to allow general statements about each sample. I will give some examples: a) Page 11, Line 10ff: I think this statement is somewhat misleading. Sample 6 had only a short period of approx. 5 minutes where it was hovering close to the ground. The corresponding reasoning is rather speculative. b) Page 11, Line 17f: 1) and 2) are identical, but only phrased differently? c) Page 11, Line 18f: Although it is a logical statement, is this really supported by the data at this point? d) Page 12, Line 5f: It should be avoided to speak too generally here, since only 3-5 particles per sample were analyzed. How this few particles be representative of a whole sample? e) Page 12, Line 15f & Page 12, Line 17ff: This is again rather speculative.

Specific/Minor Comments:

1. The terming of HOVERCATS ability to measure “time-resolved” INP concentrations (Abstract, Page 13 Line 11) seems a little misleading, since it averages over broader time periods instead of measuring in real-time.

2. Use meters above ground level throughout the manuscript (other than when discussion problems related to the flow of the pump). The Colorado plains has a rather high ground elevation, which may lead the reader to think the balloon was flying higher than it actually was.

3. Page 2, Line 10: When listing the ice nucleation modes, a reference to Vali et al. (2015) should be considered. Also, the concept of pore condensation freezing as introduced by Marcolli et al. (2014) could be mentioned, when describing deposition freezing.

4. Describe in more detail how the system adjusts the altitude, how it lands and how it is retrieved. Is the balloon still intact after landing, can it be reused?

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5. Why haven't you used a more powerful pump that can go above 1.2 lpm? I understand that payload restrictions are crucial for this approach, but I think the system would be much more flexible when the sample flow could go higher.

6. Page 10, Line 28ff: I think it would be better to give median (or average) particle concentrations instead of the maximum, since near ground values showed episodic peaks. Also, a plot showing the particle concentration vs. altitude could be worthwhile.

7. I wonder if you considered to calculate the correlation between OPC concentration larger 0.5 μm (as in DeMott et al., 2010, 2015) and the INP concentration at T = –X °C averaged over each sampling period (although N = 6 isn't very good statistics), and/or add a scatter plot.

8. Page 11, Line 12: Give the range of minutes (from X to X minutes) that HOVERCAT hovered near the ground. Following the reasoning on Page 11, Line 14ff it may be worthwhile to correlate the number of minutes close to the ground with the INP concentration of the corresponding sample.

9. In section 3.4 the authors should add a goal to improve on the ability to stay at a constant altitude with HOVERCAT.

10. Add a Figure that shows a detailed schematic of the TRAPS. I see that a similar Figure can be found in Ogren et al. (2017). Yet, I feel it is essential to this manuscript as well and should be added therefore (also the design probably is different to what is shown in Ogren et al. (2017)). A few sentences describing the filter collection with TRAPS in more detail could also added to the text.

11. Fig. 2: Indicate the exact sampling times (e.g. using a black sphere to mark the beginning of a new sample on the flight track, and a black line one the time color scale)

12. Fig. 3: Y-axis and color scale give the same information, one of those seems redundant. A stacked (color-coded by cooling rate) histogram might give more information (or as additional Fig. b). Something like my Figure 1 attached to this review

(not your data):

13. Fig. 7: Give the altitude in m AGL (starting from 0). Use more distinct colors for altitude and OPC (instead of light and dark grey). Coloring for Sample 2 is not the same orange as in the others figures. Maybe use logarithmic scale for the OPC concentration?

14. Fig. 10: Add information to the labeling describing what exactly is depicted on this figure. Is it the onset conditions of the first observed ice nucleation event (or of X % activated fraction)?

15. Fig. 11: What is meant by “most representative particle type per sample”? In section 2.3.2 it says that only 3 – 5 particles were analyzed with Raman spectrometry per sample. How do you know which particle type is representative for the whole sample when only 3 – 5 particles each were analyzed in total?

Technical Comments:

Page 2, Line 17: Replace “adroit” with “efficient”

Page 5, Line 10: Remove “-“ in “1.2-L min-1”

Page 5, Line 13: Add space between “12 V DC”

Page 10, Line 23ff: The term “profile” is a little confusing, since it does not correspond to the same samplings (e.g. profile 3 is sample 4). Maybe add a short sentence that the two terms are not the same.

Page 12, Line 16: Typo in reference “Möhler et al., 2008”

Fig. 1 Caption: b) Picture of the aerosol module.

Fig. 8: Labeling of “UPW in breaker” is covering part of the data. The last sentence of the caption does not make sense for this plot.

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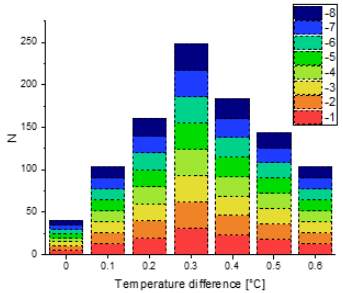
N
250
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0.3
0.4
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Temperature difference [°C]
-8
-7
-6
-5
-4
-3
-2
-1

Fig. 1.