Review of HOVERCAT (AMT-2018-47): A novel aerial system for evaluation of aerosol-cloud-interactions by Creamean et al.

The authors present HOVERCAT a platform for airborne sampling of aerosol and particulate matter which can later be used for offline INP analysis. In particular they propose vertical profiling is possible by doing stepwise ascents in their balloon platform. This is indeed very exciting, very promising and the right direction for the field instruments to progress in.

I have some issues with the scarcity of test flights conducted and the rush to publish something. I fully support a publication addressing HOVERCAT, but I can't help but wonder if the manuscript would be more complete and exciting if further development that is already being done would be included herein? I would recommend publication after major revisions and the concerns below have been addressed. I do not separate major from minor concerns and technical comments from general scientific comments; instead, I list all concerns by page.

In general I think many discussion points are missing and some of the data is not sfucciently addressed in the text. Also some of the instrumental development aspects need to be addressed. Overall I think this paper will make a very good contribution to the literature and I think it should be published, but more needs to be done.

Page 1

Line 11. Precipitation processes "can" be modulated by aerosol cloud, not "are" since other dynamics also influence radiative and precipitation processes.

Line 15-20: Upon reading the manuscript and seeing how only 1 successful test has been presented herein, I would soften the ideas about "hovering" at as given altitude. It was clear that the authors struggled to hover at a given altitude because of vertical winds and the balloon could not respond fast enough to maintain altitude. I think one test flight is too soon to claim this.

For realtime aerosol measurements, the vertical profile would still be achieveavle, but for INP analysis as the authors rightfully acknowledge more time is needed at each sampling altitude to get enough PM on their filter, and this is harder to achievbe without fluctuations (sudden rises, or falls). Perhaps it would help if the authors stated a range xx +/- yy m hovering capability. So that instead of being sample to sample at say 500 M agl.. HOVERCAT can sample at 500 +/-100 m as an example.

Page 2

line 10. An appropriate reference to add or even to replace Cziczo et al. (2017)(who talk about IN methods as a focus not the mechanisms) with would Vali et al. (2015) who have a nice discussion that included the IN community on the terminology and IN mechanisms.

Line 12: immersion freezing is the most relevant heterogeneous ice nucleation mechanism for MPCs but not for cloud ice formation. We don't know that, there is a fair bit of literature to suggest that majority of cloud ice may come from secondary ice multiplication. So I suggest re-phrase to make this more clear.

Page 3

Line 1: here the authors say 2 km AGL is a limitation but then the platform they present is only shown to go to 1.1 km AGL, this is contradictory. I suggest correct this or modify the way this is presented.

Line 4 -5: there may have not been INP measurements on a balloon system, but a similar idea of collecting aerosol on filters/wafers on a UAV was already done couple years ago and published (Schrod et al., 2017). This study should be discussed and acknowledged here.

Page 5

Line 8: Maybe state what density/shape factor relationships were used to convert OPC data into PM values

Line 10: typo should be 1.2 L not 1.2-L

Page 6

This is a nice description of the flight and the testing done. Indeed, it is exciting to read that such developments are taking place. My only concern about this is if two out of the three test flights were not successful and if the altitude of 2.5 km AMSL was determined to be the max altitude range of operation, why weren't more flights conducted for this study, once the operational parameters were clear? This should have been straightforward to do, no? Furthermore, if stronger pumps are being tested for higher altitude (lower pressure) sampling why not wait for those tests so that a test flight with higher altitude can also be conducted.

Line 22-28: The data given in this paragraph is all for ground level or at a given height? I guess if the goal is vertical profiling, shouldn't the meteorological data for the different height be useful or may be needed to understand the aerosol properties (e.g. phase state) during sampling. Does it help to have all the ground based data when all the sampling is occurring above ground?

Page 7

Line 1: make clear 47 mm is diameter.

Line 25: here the authors say control experiments were done with ultrapure water at various cooling rates. How was the water treated here. In order to have a true control, the water should also have been subjected to the same process as the samples i.e.

" (i.e., samples) were successfully collected before the battery died. Each spot was placed is a 29-mL sterile Whirlpak® bag with 2 mL of UPW to resuspend particles deposited on the filter"

The water should be treated as above to ensure there is no INP contamination coming from the whirlpak bag. Have such experiments been done? If not, I suggest these should be included in order to really be confident that the freezing is from the aerosol re-suspended into the sample.

Page 8

Line 4: I understand how the temperature calibration experiment was done, but I tried hard but do not follow how the data are plotted in Figure 3. Which data are for the probe at the center of the copper plate and which for the drop on top of the stage? The vertical lines just mean that the temperature difference doesn't change with cooling rate, but then why are there so many vertical lines, and what determined the position of the data on the x-axis, this figure is difficult to follow. I also don't understand why there is a cooling rate on the y-axis and then the data are also colored by cooling rate?

Line 27, why does it take more time to prepare drops with a pipette. Are you using a single tipped pipette, but you could use an 8-channel or 12 channel pipette for this too. It would speed things up.

Page 9

Line 4. It was not clear to me why all 100 drops could not be recorded? If you place 100 drops on a cold stage what is the hindrance in recording data until all 100 drops are frozen?

Page 10

Line 10: based on your filter sampling, what was the efficiency of smapling particles larger than 10um? From the deposition mode experiments the authors say they see particles between 1 – 50 um. This could be a strong indication of coagulation of the aerosol based on the suspension and nebulization methods used. Does it then make sense to assume that one particle lead to each ice crystal observed? And also try and analyses that particle? It is likely a particle that is composed of multiple particles. With more samples perhaps the authors could do a particle count by feeding the nebulized spray though an SMPS or even into a CPC to get a particle count. And then compare this to a particle count taken from image analysis of the deposition state to see if numbers are similar. This will provide some correction factor (if needed) for how much coagulation maybe taking place when the particles are being deposited onto the stage for deposition nucleation.

Line 25-30: regarding sudden drops and hovering – see comment above that I made in abstract section. I think it is too soon to claim the ability to hover at a fixed altitude. Perhaps consider putting a range to that, given that the balloon cannot respond fast enough to the updrafts experienced. I still think this method is super valuable even for example you say, each INP filter sample is conducted within a range of 100 m or so. This is a step in the right direction for vertical profiling.

Page 11.

Section 3.2. Results are indeed interesting, but it would have been nice to see that each sample presented did not have overlapping altitudes. Is there no way to automatically shut off filter sampling when a certain threshold of a sudden rise or fall is crossed and then only resume sampling when the balloon is within this threshold again? Would this be too demanding, or is there not enough time in the flight for this. This way you ensure that your INP filter samples are restricted to sampling at a certain altitude and you can resolve better the vertical profiling of the INPS. For example in Figure 9, sample 2, if the sampling was shut off for the periods when the instrument was below 600 m AGL, that would allow sample 2 to be representative of only INPs above 600m AGL.

Section 3.3: should be deposition nucleation – not freezing, freezing implies liquid to solid transition, I think the authors' intent is to imply vapour to ice mechanism here, therefore I would take freezing out of the phrase.

Page 12

Line 1: "…above homogenous freezing" are you referring to temperature or Si? Could clarify here already. Based on Fig. 10 it looks like you refer to both. However, some of your data points are quite close to water saturation (-25 for example), so it maybe a tough sell to claim those as be depositional ice nucleation.

Section 3.3: I like the discussion in this section. But here one must acknowledge that there could be artifacts of having your INPs as agglomerates of particles. So maybe the aerosol was externally mixed, but with the experimental method used, they could become or appear as internally mixed since you suspend and nebulize and then allow for evaporation to retain the residual particles on the cold stage. This should allow for some coagulation, and perhaps the Raman is investigating a single particle that is a result of multiple smaller coagulated particles.

Section 3.4: Again while reading this section, I can't help but feel that the paper is a little premature. Some of the plans and developments described herein could be already part of this manuscript, like the stronger pumps, a few more test flights, certainly more than 1, and the practice to control the balloon to stay at desired altitudes. i.e., the above should be part of Phase I.

I understand regulatory approval work like compliance with FAA can be phase II as well as launching on other airborne platforms (UAS, reverse parachute). i.e. demonstrating their instruments are versatile enough for other platforms, can all be phase II.

Page 13

Line 5. Certainly if the plans to operate at Jungfraujoch in Spring 2018 are on track (that is now), then stronger pumps have been implemented already. Since ground level at Jungfraujoch is already ~ 650 mb pressure.

Line 14: maybe put in also the altitude AGL to give context to your starting point.

Line 19-20: Soften the vertical profiling statement here because the authors showed overlapping vertical profiles, so it is not yet achievable in the strictest of sense.

Given that this is an instrument development paper, I would like to see a section on Benefits and limitations of the instrument. Where the authors state this clearly. I believe some of this is interspersed through the manuscript, but I think this should be brought together in one section to make it clear what are the benefits of HOVERCAT (and its current limitations).

Figure 4. For clarity that the control experiments are done with pure water and also it would be good to indicate the volume of the drops in the caption.

Figure 5. I was a little confused – the "by hand" drops, are those not the same as the syringe drops? Or how were the 2.5 ul drops by hand produced? There must have been some sort of tool for these.

Figure 7: Could you make it clear in the caption that the grey line plot is the one corresponding to the altitude and the scatter plot corresponds to the concentration from the OPC? Is it mentioned what densities or shape factors have been assumed to come up with the PM values from the OPC data?

Were the particles losses of 10% accounted for in the results? i.e. when calculating the concentrations in Fig. 8. Also, how were the control experiments for the ultra pure water in beaker and bag converted to INP concentrations? These fraction curves are not addressed sufficiently in the text I think.

References

Cziczo, D. J., Ladino, L., Boose, Y., Kanji, Z. A., Kupiszewski, P., Lance, S., Mertes, S., and Wex, H.: Measurements of Ice Nucleating Particles and Ice Residuals, 58, 8.1-8.13, doi:10.1175/AMSMONOGRAPHS-D-16-0008.1, 2017.

Schrod, J., Weber, D., Drücke, J., Keleshis, C., Pikridas, M., Ebert, M., CvetkoviÄ‡, B., Nickovic, S., Marinou, E., Baars, H., Ansmann, A., Vrekoussis, M., Mihalopoulos, N., Sciare, J., Curtius, J., and Bingemer, H. G.: Ice nucleating particles over the Eastern Mediterranean measured by unmanned aircraft systems, 17, 4817-4835, doi:10.5194/acp-17-4817-2017, 2017.

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