**Author Response Revision3 - amt-2022-236**
Drone-based meteorological observations up to the tropopause
Konrad Benedikt Bärfuss et al., Atmos. Meas. Tech. Discuss.
https://doi.org/10.5194/amt-2022-236, 2022

Please note: Author's response in blue color

**Report #1:**

As I said in my previous review, I am a strong supporter of the utility and benefit of automated aircraft reports to fill the time and space gaps left between other in situ observing systems. Upon rereading the article "Drone-based meteorological observations up to the tropopause a concept study" by Barfüss, Schmithüsen and Lampert, I feel that it is now ready for publication after a few minor changes. Some of my reasoning for this decision for the suggested changes is summarized below. I apologize for some possible disjointedness in the review, but that may in places reflect the need for more organization in the paper's organization.

To start, I thank the authors for their continued efforts to improve the paper. The current version is much more informative and leave many fewer points unresolved.

The Authors are grateful for your extensive reviews, which helped a lot for improving the article and additionally led to productive internal discussions. We highly appreciate your effort and would like to thank you.

Please note, that the line numbers correspond to the plain revised document (not the one with tracked changes).

The introduction remains essentially unchanged and includes very little discussion of problems that other authors have documented with automated reports from commercial aircraft that should provide a far more stable platform for collecting data and providing representative measurements or the atmosphere without being affected by artifacts related to aircraft stability. For example, AMDAR wind observations from longer-range aircraft are much more accurate (by a factor of 2) than TAMDAR reports obtained from smaller/lighter regional jets. Possibly more distracting to is the statement in the first sentence of the Abstract reads "with large data gaps in the atmospheric boundary layer, above the oceans and in polar regions", with only a brief mention of that "the feasibility of reaching an altitude of 10 km with a small meteorologically equipped drone is shown." Through the remainder of the text, the utility of the drone observations, however, is judged by the ability to reach 10km in a polar environment. The Abstract should be revised accordingly.

We re-wrote the abstract, now emphasizing more the main intention of the article, which is demonstrating the feasibility of reaching an altitude of 10 km with a drone on own propulsion. This provides a new tool for the community to gather data for weather forecast.

We changed the abstract text to:

"The main in-situ database for numerical weather prediction currently relies on radiosonde and airliner observations, with large systematic data gaps, horizontally in certain countries, above the oceans and in polar regions, and vertically in the rapidly changing atmospheric boundary layer, but also up to the tropopause in areas with low air traffic.

These gaps might be patched by measurements with drones. They provide a significant improvement towards environment friendly additional data, avoiding waste and without the need for helium. So far such systems have not been regarded as a feasible alternative of performing measurements up to the upper troposphere.

In this article, the development of a drone system that is capable of sounding the atmosphere up to an altitude of 10\,km with own propulsion is presented, for which Antarctic and mid-European ambient conditions were taken into account: After an assessment of the environmental conditions at two exemplary radiosounding sites, the design of the system and the instrumentation are presented. Further, the process to get permissions for such flight tests even in the densely populated continent Europe is discussed, and methods to compare drone and radiosonde data for quality assessment are presented.

The main result is the technical achievement demonstrating the feasibility of reaching an altitude of 10\,km with a small meteorologically equipped drone using own propulsion. The first data are compared to radiosonde measurements, demonstrating an accuracy comparable to other aircraft based observations, despite the simplistic sensor package deployed. A detailed error discussion is performed.

The article closes with an outlook on the potential use of drones for filling data gaps in the troposphere."

Grammar and word choice continue to present problems throughout the paper. For example. In line 77, the term "Breakthrough requirements" is not defined. Are these "new' requirements or mesoscale requirements that were never intended to be fully met by GOS. Although lines 77-83 attempt to clarify this issue, inferring that 100 km horizontal and 1 km vertical resolution will not be sufficient to meet also local and regional needs, especially without a clear statement of temporal frequency.

Line 81: We clarified the term and added the missing information on observation cycle and timeliness.

In line 89, the term "back in the 60ies" should be "60s", or better should be simplified to "at that time". Also, in line 89, replacing "using binoculars" with "using theodolites" would suggest that the observations could be quantitative, not just qualitative.

Line 94: Switched to "at that time". We decided to stick with the word "binoculars" and clarified their use by adding "using binoculars to monitor the aircraft's attitude" which is not typically done with theodolites.

Lines 98-105 – The idea of using this application in the future is intriguing, but it must be noted that it will only have benefit if the full-resolution data can be downlinked and distributed in real time, which can't be done with the current system.

Line 111: We added “It must be noted here, that the deployment of drone systems for operational meteorology only has benefits, if data can be transferred and distributed in near real-time, which has not been demonstrated within most of the above-mentioned studies.” to address this issue.

Line 122 – Add “extremely low specific humidities” to the list of challenges after “temperatures”

Line 131 – Replace “to play . . . and” with “as a future alternative to”

Line 156 – Replace “for a time period of” with “episodically over”

Line 167 – Be more direct at beginning of sentence by eliminating “trade-off” notion with “The LUCA system was designed . . .”

Line 171 – Replace ‘Applying wind speed condition” with “Restricting wind speed conditions”

We changed the wording, thank you for your explicit suggestions!

Line 178 – Are you saying that LUCA will operate at times when radiosondes will fail, e.g., in conditions of “rainfall, snow, heavy turbulence and within clouds”. No proof for that statement is shown, unless drones were launched alongside every radiosonde launch. This needs to be clarified.

Line 189:We added “, but these capabilities have yet to be proven in upcoming measurement campaigns.” to clarify the current state of testing.

Line 190 – The fact that 45% could severely reduce the number of drone profiles that are available, making the system appear to not be “all weather”. Can you estimate how many LUCA flights would be missed due to the potential for icing?

Unfortunately not, as we did not find a theoretical estimation for the location of Neumayer III in Antarctica, and no in-situ data. In addition, UAS behave differently from manned aircraft in icing conditions, and trajectories may vary substantially, which could interact with the extent of the altitude band in which aircraft icing will occur.

Line 198 – Add “shown here” after “flights” and be clear whether an icing sensor was in fact installed, not just “prepared to be installed.”

Line 211: We clarified that we did not install the icing sensor (but a camera instead) as the risk of icing was negligible during the demonstration flights presented in the article.

Line 220 – It would be good to include the typical ascent rate values along with the other performance specifications here so the reader doesn’t have to hunt through the paper to find them

Line 233: Yes, we totally agree and added the missing specification.

Line 236 – What is the meaning of the first sentence? It could be read to imply that multiple sensors were used for each atmospheric parameter being measured. Eliminating it removes nothing of value from the text.

True – sentence eliminated.

Line 241 – Mention should be made both that the radiosonde sensors were designed to have a low ventilation rate as the balloon drifts with the wind and that a drone travelling at constant air speed of 28 m/s will have much greater ventilation rate. How is these accounted for? If this is discussed later, say so.

Line 262:We revised the beginning of the section and included the statement: “The ventilation rate, which is assumed to be still higher than the ventilation rate of radiosonde sensors, minimises the response time of the sensors as the sensor is exposed to an increased amount of air per time compared to radiosondes.”

I will stop with individual comments here but suggest that the authors review the text thoroughly for other wording errors or inconsistency, if any. Overall, the details of the presentation have improved significantly since the first version.

Lines 290-564 – The authors have provided a very good description of the data processing and error estimation procedures. While the information is extremely helpful, I found that the amount of detail presented distracted the reader from the flow of the paper’s primary messages. As such, I highly recommend that this portion of the paper be moved to an appendix dedicated to the subject.

This fits well to our initial plan of the structure of our article, we highly appreciate the suggestion and moved the according sections to the appendix. The existence of the (from our point of view also very helpful) sections is indicated at the end of the introduction.

Line 334 – Are the coefficients the same during ascent and descent? Are these results consistent with documented hysteresis issues using radiosonde-like sensors for TAMDAR aircraft? Table 2 seems to imply that there is no hysteresis during ascent with a warm bias during descent, which is opposite of the TAMDAR results. How much is this a function of the choice of M ~ 0.07 in Equation 5?

Line 567: Yes, the coefficient m is the same for all flights – to clarify this, we included the phrase: “The coefficient $m$ which expresses the heat transfer rate from the fuselage to the sensor is regarded as independent of environmental conditions and universal for all temperature measurements with the drone \emph{LUCA}.”

Hysteresis and observed bias should ideally be fully resolved (zero bias, no hysteresis) by the signal reconstruction and correction methods presented in the Appendix (besides the phase neutral averaging). This is not achieved with the coefficients and time constants applied to the data, which indicates the need for more measurements to calibrate/tune our post-processing parameters.

The influence of these coefficients and time constants fold in a complex way with the actual vertical profiles of temperature/humidity, so no simple function of hysteresis/bias and the coefficients/time-constants can be named here.

Line 451 – If the time constant for temperature reports is 21 seconds, please show how that translate into spatial and vertical averaging distances. E.g., does 21 seconds for a drone travelling at 28 m/s (and 10 m/s vertically) in still winds conditions equate to 0.588 km horizontal averaging distance and a 210 m vertical average (~21 hPa near the surface). How would this change with tailwinds and headwinds?

Tailwinds and headwinds do not affect measurements of temperature and humidity.

As a time constant of a transfer function of type PT1 (control theory) has no representation in the time space without an explicit input series, the translation of the time constant value into spatial distances is not valid. In addition, the assumed time constant of the sensor is recovered during the post-processing (signal reconstruction/inverse filtering), but indeed the applied phase neutral filter to smooth the results after signal reconstruction can be approximated to the width of a phase neutral mean filter (centred averaging).

Line 685: We added “Assuming a constant lapse rate of $-0.0065\,K\,m^{-1}$ and following the fundamentals of control theory, the signal reconstruction would correct for a hypothetical hysteresis of $1.38\,K$ during the ascent.” and “[..]which is roughly comparable to a central average over a vertical extent of $120\,m$.”

The horizontal averaging/smoothing is not relevant, as we climb in circles around an earth fixed point.

Line 460 – For RH, the question becomes more complicated. For the same still conditions, do3s this mean that the spatial and temporal smoothing at the surface are 0.420 km and 150 m (~15 hPa) respectively, which at 3 km elevation become1.68 km and 600 m (~40 hPa) and at 10 km are longer than the entire flight length (2000 s = 33.33 minutes), therefore making the RH values at high levels reflections of the average RH throughout the entire flight.

For raw measurements, this assumption is close to be right – but as we apply signal reconstruction, the effect is fully corrected in theory (assuming a perfect sensor model and zero noise). The simplified sensor model leads to errors, and noise in raw sensor measurements require applying a filter after the signal reconstruction (which itself relies on quasi-gradients of the measurement values and therefore amplifies measurement noise). Regarding this phase neutral filter to smooth out artefacts introduced by sensor noise as a central average with a variable window size is not any more intuitive, as the maximum theoretical vertical extent to be averaged exceeds 10 km.

---Please include vertical profiles of the horizontal and vertical averaging distances that have been applied to each type of observation somewhere in the text. Figure 11 might be a candidate location.

Figure 8: As stated before, we regard the horizontal averaging as not applicable for our trajectory. The vertical averaging is approximated for the temperature signal processing to a constant value of 120 m (stated in the caption to Figure 8), but we did not found a representative value for the humidity processing which supports readers in interpreting the results. We simply included the time constant for the phase neutral smoothing filter over altitude in the Figure (now Figure 8) and hope, this indicates the variable smoothing well for the audience.

Figure 5 and text that goes with it. – I like this example. Thanks for creating it.

Line 664 – Clearly state the data set details. As it stands, I don’t know whether the statistics were obtained from the profiles in Fig. 9 or from all profiles in Fig. 8 until the second paragraph into the section.

Done in Line 383

Line 666 – The word “treats” is clearly not correct. Should this have been “threats” or better yet, “factors”?

Line 386: Changed to “factors”.

Figure 10, center top panel (RH) – A shift of the apex of a probability density plot away from zero (in this case toward -2.5%) usually reflects a bias in an observing system. Please include a statement of how the variable time averaging that was applied in line 460 affected this.

Line 404: We mentioned the possibly not finally tuned calibration parameters which would require a broader database with the phrase: “For the humidity measurement, the average of the differences between radiosonde and \emph{LUCA} measurements differs significantly between ascent and descent, indicating the possible need for further calibration and tuning of the post-processing parameters using a broader database in the future.”

Table 2 – Please ad the number “6” before “ascent” and “descent” in the last sentence of the caption. It makes it easier for the reader to understand the sample size.

Numbers added.

End of paper – I think that it is still important not to ignore the issue of system and operations costs. That could be incorporated in the future, more extensive testing that was suggested. Because much of the thrust of the paper was addressed at substituting for radiosondes, lack of mention of the question weakens what has become a very good paper.

While we rather aim for supplementing radiosondes than substituting them, we added a few words in Line 510, but did not feel able to include explicit statements which address the financial aspect.

As I said in earlier reviews, I support concept presented here. With the enhancements that the authors have provided, the paper is now essentially ready for publication, after the small number comments are addressed. Again, my intent here has been to provide constructive suggestions for improving the paper. I do not need to see the final revised version.

I look forward to seeing the article in print – Ralph Petersen