Referee Comment Responses: The DataHawk2 Uncrewed Aircraft System for Atmospheric Research

Referee Comment #1: Sean Bailey

The authors present an overview of their updated version of uncrewed aerial system (UAS) designed to simultaneously measure different thermodynamic properties of the atmosphere including turbulencs. The paper does a good job of reviewing the different components of the systems on the aircraft and provides evidence of validation of the successful operation of these systems.

I found the paper to be well written and, as noted, each system is generally well described. This information is potentially useful for other researchers interested in developing their own UAS, or for researchers who are interested in the data produced by the DataHawk2 who may wish to know more details about the systems which produced the data. I therefore recommend the article for publication in Atmospheric Measurement Techniques.

I did feel, however, that although the authors described the aircraft systems in great detail, they did not provide much information about the sensor data acquisition process, particularly given the asynchronous mix of what I expect would be both analog and digital data streams. Given that the role of the UAS is to essentially be a platform to carry the sensors, it is important that the system used to acquire and store the sensors' signals be described in more detail. For example, presumably the output from the fine-wire array is analog, what is the resolution of the ADC? Does it support serial communications? if so, which ones? How many sensors of different types can it support? How does it handle the asynchronous messages and ensure their timing? etc.

We would like to thank Dr. Bailey for the time put into this thorough and detailed review of the paper. To address some of the questions in the paragraph above, additional text about the overall sensor data handling process has been added (lines 266 – 276). Other sensor-specific details have been added in the Scientific Payload section.

Other, minor comments that I also feel should be addressed before publication are as follows:

1. *[Line 84] Although the authors use the more contemporary description of uncrewed aircraft systems, they still refer to traditional aircraft as manned aircraft. Perhaps rephrase to refer to crewed aircraft instead for consistency.*

The authors appreciate Dr. Bailey's attention to detail, have updated "manned" to "crewed" for consistency (revised draft line 86).

2. *[footnote on page 2] I would also add unmanned/uncrewed aerial vehicles to the list of other designations used to describe UAS.*

The authors agree that this would be a good addition and have added unmanned/uncrewed aerial vehicles to footnote as requested (footnote on page 2).

3. *[Line 147] The DH2 acronym used for DataHawk2 is already defined on line 106. Also, once DH2 is defined, the authors occasionally seem to return to using the full name of DataHawk2 (e.g. lines 158 and 171)*

The authors originally made an attempt to have the full aircraft name mentioned at the start of each section, but agree that this is confusing and appears inconsistent. We have changed all instances of DataHawk2 in the text body and figure captions to DH2 following the definition on revised draft line 108.

4. *[Section 2.3] Is their a reason that solar shielding has not been used for the RSS-421?*

Solar shielding has not been used on the RSS-421 in this case as it is not present in some Vaisala-designed applications with a very similar sensor package, such as the RS-41 radiosonde. The silver solar reflective coating on the temperature sensor helps mitigate solar effects in both our application and the RS-41 radiosonde. The authors have added a sentence (lines 313 – 314) to clarify this design choice for future readers: "The RSS-421 is unshielded

on the DH2, similar to the RS-41 application of these sensors; the silver solar reflective coating on the temperature sensor helps mitigate solar effects."

5. *[Line 324] The authors mention a post-flight calibration process but do not provide the details until much later. Perhaps indicate to the reader that this process will be described in a later section.*

The authors agree that adding this detail will help the readability of the paper, and have added "as detailed in Section 3" to revised draft line 345 for clarification.

6. *[Line 382] As above, the wind comparison to the radiosonde is conducted prior to the description of how winds are obtained from the platform.*

The authors agree that this is confusing and have decided to re-arrange the third section to create a more logical flow of information in the manuscript. Now, the wind comparison is conducted after the description of how the winds are obtained from the platform. Additionally, a reminder pointing the reader to the wind speed estimate calculations in Section 3.3 has been added to revised draft line 528.

7. *[Line 428] The authors mention their use of both ascent and descent portions of the profiles during calibration to cancel out lag-induced-offsets. This is the first mention of these offsets and no information is provided about their source. Are they due to sensor time response? Data acquisition system timing? More details are required.*

The authors agree that the cause of these lag-induced offsets should be clarified in the manuscript; these lag-induced offsets are due to sensor time response. The authors have added clarification about the source of the lag-induced offsets is added to lines 415 – 416, referring to the sensor time response differences discussed on line 413. The following phrase is added to the paper (lines 415 – 416): "… caused by these differing sensor time responses… ".

8. *[Line 458] The authors are using the pitot sensor for calibration of the hot-wire through spectral comparison. Frequency response of the pitot probe will play a role in this calibration and should be mentioned.*

The revision notes (lines 447 – 453) that the pitot frequency response is not a factor in the 800Hz measurements (400 Hz Nyquist frequency). Propeller vibration noise is the chief limitation, necessitating calibration of the hotwire at frequencies below 100 Hz.

9. *[Line 465] Which Kolmogorov constant is used by the authors in the inertial subrange model used to determine the dissipation rate? More details about this process would be beneficial.*

The details of the Kolmogorov model used are provided in the Frehlich 2003 reference cited in the original version. This citation is moved up in the revision (line 460) to make the source of the method clearer.

10. *[Figure 5] The authors present example spectrum to demonstrate the cold-wire measurement capability. A similar plot for the hot-wire should also be provided.*

The authors agree that a hotwire plot should also be provided and have updated Figure 5 (revised draft Figure 4) to include a hotwire plot.

11. *[Line 542] I would argue that the difference between the hybrid approach and radiosonde is as high as 2 m s−1 in the lowest 200 m of measurement. Do the authors have any insight as to why the disagreement of is approach with the traditional approach increases at lower altitude?*

The authors do not have a definitive reason for the disagreement in wind speed between the traditional and hybrid wind estimation approaches shown in the example flight (revised draft Figure 5). Fully determining the cause of this difference would require significant work that the authors feel falls outside the scope of the paper, but they can provide some insight. The hybrid approach is more heavily filtered,

which could explain some of the disagreement between the two estimation approaches, though it is unclear how this difference would affect the results seen in the example flight. Additionally, it should be noted that the disagreement between the DH2 and radiosonde's estimated wind speed at lower altitudes could be attributed to the temporal (up to an hour) and/or spatial difference (up to a kilometer) between the two flight trajectories.

We have changed "agree quite well" to "similar to one another" to better convey the differences between the wind estimation techniques (revised draft line 572).

12. *[Line 543] The authors mention confidence intervals for the wind estimate, but I could not find a description of how these intervals were determined.*

The authors have added a statement to line 576 "(detailed earlier in this section)" and changed "… calculated from" to "… calculated in" to clarify that these confidence intervals were described earlier and point the reader back to them if necessary. The re-organization of Section 3, placing the radiosonde comparison section at the end, puts the description of the confidence intervals closer to the wind estimate discussion, which the authors believe will help make the source of these calculations more clear. Additionally, some clarifications on the interval determination reasoning/methodology were made earlier in Section 3, shown below.

Replaced the following text:

"Therefore, a confidence interval was computed to determine a range for the actual difference between the radiosonde and DH2 sensors given the same 95 % significance level."

With a revised statement to clarify confidence interval methods:

"There is minimal usefulness in knowing that the two sensors are not absolutely the same; this is already assumed. However, knowing a range for the actual difference between the radiosonde and DH2 is of interest. Therefore, a confidence interval was computed to determine this actual difference between the sensors given the same 95 % significance level."

13. *[Line 563] The authors use the more-traditional capitalization of Pitot here, whereas in the rest of the manuscript the contemporary non-capitalized form is used. I believe AMT prefers the more contemporary form.*

The authors again thank Dr. Bailey for his attention to detail, and have corrected "Pitot" to the more contemporary "pitot" form.