Reviewer comments and author responses for the IDEAL overview paper – AMT Journal

Reviewer 1: Comments

Overview:
Doddi et al. provide a manuscript about the IDEAL measurement campaign. The IDEAL program and the associated campaign target a very relevant and interesting topic of atmospheric research, which is the structure of the lower troposphere in strongly stable conditions. New ways of sampling stability and turbulence with multiple UAS were explored in the campaign which could contribute significantly to a better understanding of the dynamics under such conditions.
The authors have revised the manuscript according to reviewer comments. It would have been easier to review the revised manuscript if a marked-up manuscript or a list of changes would have been provided as typically requested by AMT. It has not been clearly stated in the authors responses which changes were made, so a full review of the revised manuscript had to be made. Given the substantial changes that were made this was probably necessary in any case.
From my perspective, the manuscript has been improved significantly from the previous version. Nevertheless there are three major points which I think should be addressed before acceptance for publication in AMT and some minor comments:

Major points:
- Table 2: The authors should carefully revise the accuracy and resolution values that are given in this table and distinguish between laboratory or theoretical values and validated uncertainties in field experiments (incl. references).
- Methods: The authors refer to the doctoral thesis Doddi 2021 for many of the methods. The reference that is given is not traceable, no DOI, no book reference. It might be my fault that i cannot access the full text, but at this point I cannot judge if the presented results are fully traceable.
- Section 4: A series of plots are provided that show processed data from the UAS flights (Figs.10-19). Not all of the plots are discussed and it is questionable if they are relevant to the goals of the manuscript. In any case the authors should make clear, what the message of Section 4 is and clearly describe the findings with observations that are shown in the figures. This has improved and is maybe acceptable for Figs. 10-13, but especially for the new figures with regards to the spatial sampling, the description is very vague.

Minor comments:
- p.1, l.4f: I think that references should be omitted in the abstract

Noted. References are no longer cited in the abstract.

- p.1, l.16: "Atmospheric modeling motivated by IDEAL observations is reported elsewhere" That is a rather odd statement. Is it relevant in the abstract, if it is reported "elsewhere"?

This statement is omitted in the revised manuscript.

- p. 7, l. 154f: Although a time constant of 0.5 miliseconds and 800 Hz sampling rate might be technically correct for the coldwire sensors, how realistic are these characteristics? Is there a reference that shows that noise-free measurements are possible with a given resolution of 0.003 K in flight? Same applies to the hot wire. Spectra in Fig. 9 show significant noise from 100 Hz.

The time constant is a characteristic of the sensor wire at the mean airspeed and the electronics sampling rate is fixed. This determines the bandwidth that fluctuations can be measured, with roll off due to time constant at 320 Hz and Nyquist frequency at 400 Hz. Noise level is an independent issue, affected by signal quantization, electronics noise, and vibration disturbances.

- p.8, Table 2: 0.01% is not a realistic noise-free resolution for the SHT sensor.

This is the value quoted on the manufacturer's spec sheet.

- p.13, l.239f: How did the changing atmospheric features result in aircraft sorties? Maybe rephrase?

The authors meant to state that the rapidly changing atmospheric conditions around the Granite mountain influenced the choice of number of aircraft deployed in each sortie.
This statement in its current phrasing is unclear and will be revised for clarity.

- p.13, l.240: In the author response it is written that wind speeds that exceed 20 m/s are frequently observed above 3000 m and are the limit for operation and here 15 m/s are given. Please just clarify what are the operational limits and why the 3000 m are set.

The mean wind speed bound for operation of the UAS is 15 m/s as stated in the manuscript. This allows for operation in the presence of gusts up to 20 m/s (the top airspeed in level flight). The mean wind speeds above 3000 m frequently exceeded 20 m/s during IDEAL campaign. Therefore, 3000 m was chosen as the ceiling for UAS operations, and operations near this limit were carefully supervised to prevent the aircraft from being blown downwind.

- p. 18, Table 3: I think the table is very helpful and informative, but maybe the full table could go to the appendix.

The authors recognize this concern and revise the manuscript to contain a few notable UAS sorties in the main document and migrate the full table to the appendix as recommended.

- p.19, l.265: It is uncommon to state whose doctoral thesis a reference is in the text. Just give the reference. However, the reference that is given links to a preview-website, which does not give the full text. I cannot access it.

The links embedded in this reference were found to be temporary. The authors were not aware of this problem. This reference will be revised to contain an accessible (full document) version of the doctoral thesis document.

- p.19, l.265: "was ?? to the DH2"
- p.19, l.266f: "accurate up to 0.05 m/s": Table 2 claims a resolution of 0.05 m/s and accuracy of 0.5 m/s. I think an accuracy of 0.05 m/s is quite unrealistic, considering the uncertainty of all the involved measurements.

The accuracy mentioned in Table 2 is correct. The value stated here i.e., 0.05 m/s is a typographical error and will be changed to 0.5 m/s in the revised manuscript.

- p.19, Fig. 9 and l.275ff: To my understanding, these are spectra of single time series with a length of 5 seconds. Averaged spectra would be interesting to see the characteristics of the sensor with less noise. Are these so-called artifacts systematic or random?

The raw spectrum computed using the 5s time intervals (solid blue lines in Figure 9) depict the instrument noise (at 100Hz in the top panel of Figure 9). This artifact is also captured in the bin averaged spectrum (red dots in Figure 9). This is due to the DH2 motor vibration and is systematic.

The artifacts due to DH2 motor vibrations are observed in the pitot data predominantly during ascent flight legs due to increased throttle setting; descent legs are typically free of these artifacts. Hotwire velocity is free of these vibrational artifacts in ascent as well as descent.

- p.24, l.288f: There is a verb missing in this sentence.

This will be corrected in the revised manuscript.

Figures 11 & 13: What do the thick and thin lines represent in the dissipation rate and Ct2 plots? It should be mentioned in the caption.

The thin lines represent the baseline offset for each ascent (red) or descent (blue) legs from the flight. The thick lines are the data. The figure captions will be updated to reflect this information in the revised manuscript.

- p.31, ll. 336ff: These statements should go into a "Data availability" section.

This paragraph will be moved to a new section describing Data access and availability.

Reviewer 2: Comments

Overview
The authors have answered most of my questions and objections satisfactorily. The structuring of the article is much improved, as is the quality of the figures. Useful clarifications have been added. The list of references has also been completed.

However, I still regret the absence of an evaluation of the impact of instrumental noise. Except for the position of the fitting line on the PSDs, no error bar is indicated, either on the measurements (T, velocities), or on the deduced quantities (theta, N2, Ri, epsilon, CT2). Taking into account the instrumental noise would certainly be a plus for such a paper presenting data and analysis methods. About the only error bar shown, on the fit of the -5/3 slope line on the PSDs of T and v, how is it estimated? (it is not an uncertainty on the slope of the line since it is fixed a priori).

Conclusion: in view of the improvements made to the manuscript, I consider that it can be published in AMT. I recommend, however, to include a consideration of instrumental noise, either by specifying the uncertainties on the measurements and the inferred quantities (could be done in the text), or by explaining why it is not possible to estimate them.

Reviewer 2 raises similar concerns to reviewer 1's Major Point #1 – regarding instrument noise and the role of sensor uncertainty in determining the accuracy of measured and estimated quantities. These issues have been addressed in the revised manuscript.