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Geoscientific Instrumentation Methods and Data Systems Discussions

Interactive comment on "Development and comparisons of wind retrieval algorithms for small unmanned aerial systems" *by* T. A. Bonin et al.

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Thank you for your comments. The changes have been made in the updated manuscript that will be posted shortly. Followed is a point by point response to your comments.

The following comments were taken into consideration in the initial round of comments before the GID paper was open. As such, the following responses are a reply to the comments before the GID paper was published online.

1. Given that the aim of this work is to compare the performance of 3 algorithms, the authors are obligated to provide more than just a qualitative comparison. Plots of wind fields derived from each method are included but no substantial analysis is conducted

to justify conclusions such as "all of the wind data produced using the SMARTSonde's algorithms agreed with the rawinsonde observation" and "overall, the three algorithms themselves were in good agreement with each other. No algorithm appeared to perform drastically better than any". Error statistics must be included. Their overall conclusions may indeed be correct, but until robust analysis of the results is documented, their conclusions are not supportable.

The authors agree with this suggestion. Error statistics have now been included in Table 1 and a discussion on them has been added in lines 230-244.

2. Since the comparison of the 3 algorithms is the purpose of this work, it is unclear why the SODAR section (3.2) and the "Example Application" section (4) completely neglect this comparison.

The comparison of the 3 algorithms has been added to the SODAR section (3.2), as the authors agree with the recommendation on that aspect. However, section (4) primarily provides an example of how a wind algorithm can be used in combination with the thermodynamic data for a more complete picture of the state of the planetary boundary layer. Additionally, since the no-flow-sensor and best curve fitting algorithms have been found to be accurate, relative to the rawinsonde, to the same extent, applying both algorithms would be redundant.

3. Given the very specific FAA regulations concerning UAS operation in the NAS, the authors are obliged to demonstrate that they were operating in a manner consistent with these regulations. As the authors are undoubtedly aware, the FAA has specifically stated that both civil and public entities "have mistakenly interpreted FAA advisory circular (AC) 91-57, Model Aircraft Operating Standards, for permission to operate small UAS for research or compensation or hire purposes" (FAA order 1110.150 pg. 1). According to the Acknowledgments, the development of the SMARTSonde was funded by the University of Oklahoma (OU) Advanced Radar Research Center (ARRC) and through a grant provided by the National Oceanic Atmospheric Administration (NOAA)

National Severe Storms Laboratory (NSSL). Thus, the SMARTSonde appears to be a public aircraft and, as such, authorization to fly must be granted via a Certificate of Authorization. The authors need to declare which COA they were using to conduct the flights presented here.

The reviewer was generally supportive of the material presented in the manuscript and its overall structure; however, he/she rightfully points out that a COA (Certificate of Authorization) is required when collecting research data using unmanned aircraft. We readily admit that the data presented in the study were collected without having obtained a COA. Having said that, we would like to mention a few points that we feel are relevant to this issue:

It was never our intention to circumvent FAA regulations when operating our UAS. In fact, we made several lines of inquiry with various individuals and with the Academy of Model Aeronautics (AMA) along these lines. Despite our best efforts to determine the requirements to operate an R/C aircraft with an autopilot system, we received conflicting accounts.

All members of the research team were members of AMA and the local hobby airplane club near the University of Oklahoma (OU) during the time of the study. We have diligently tried to maintain flight safety at every step during the project.

Our research team has was able to work with OU and helped in securing an account with the FAA, which allows us to apply for COAs.

We submitted an application to the FAA to operate the vehicle described in this paper at an OU research site – The Kessler Atmospheric and Ecological Field Station (http://kaefs.ou.edu/) up to an altitude of 3000 feet above ground level under jurisdiction of Oklahoma City Approach.

The FAA granted a COA (2012-CSA-57), which is valid starting September 22, 2012.

As part of the process of applying for the COA, the aircraft has undergone air-

worthiness certification from the OU Department of Aviation.

All future flights will be operated under this or similar COAs.

4. The authors do a decent job of describing the "best curve fitting" and "no-flow method" but assert that, because the Paparazzi algorithm is "not well documented", a comprehensive description is not possible. The Paparazzi algorithm is open-source so it's not clear why this algorithm should be treated as a "black box".

While it is true that the Paparazzi autopilot system uses open-source code, being able to interpret the code and fully understand the details of how the winds are retrieved in the algorithm is not trivial, as the code calls variables and functions from many other sources. The authors have spent considerable time looking through the code for the Paparazzi wind algorithm, and have only been able to determine the information that is provided in the paper.

Additional suggestions and corrections:

1. Line 37: The authors assert that remote sensing approaches to observing the PBL suffer from the drawback that "one must rely on retrieval algorithms to obtain profiles of meteorological variables." However, this same drawback applies to wind observations collected by UAS using GPS positions alone. As such, this statement needs to be qualified or omitted.

This statement has been omitted, replacing it with other challenges of using remote sensing equipment.

2. Line 54: The assertion that "the onboard instrumentation for the M2AV is relatively expensive" demands a reference or more substantial justification.

Reference has been added including the cost of the equipment for the wind measurements compared to the cost of a small UAS (particularly, the SMARTSonde).

3. Line 86: I understand that the purpose of the statement that begins with "the wind

speed is found by taking the difference" is to provide a simple summary of the essential principle of the best curve fitting method, but this summary is inadequate since it fails to explain how the ground-relative wind speed is derived. I strongly suggest omitting this entire paragraph and including the essential elements in the following paragraph. After all, the value of the paragraph that begins on line 84 is to provide context for the following paragraph that begins on line 99. Combining the two will improve the flow and obviate the cryptic summary statements like the one addressed in this comment.

The two paragraphs have been merged as suggested.

4. Line 91: The statement "a Chinese group has experimented with a UAS to obtain soundings of the atmosphere" is awkward and should either be revised or omitted.

The statement has been omitted.

5. Line 119: The method is independent of the platform so it is unnecessary to include the statement "from a SMARTSonde flight".

The statement has been omitted.

6. Line 148. The authors need to provide a description of the Nelder-Mead optimization scheme, which is referred to multiple times but never explained.

In line 148, a citation is given which a reader can refer to for details on how the Nelder-Mead optimization scheme operates.

7. Line 170. I may just be missing something obvious here, but why is constant airspeed required?

The airspeed, a, is defined in equation (2) as an average of the ground speed minus the wind over a series of measurements. If the true instantaneous airspeed changes significantly over the averaging time, then it will not agree with the computed airspeed. This causes problems and increases the error when minimizing σ.

8. Line 206: "Synoptic weather conditions were fairly weak." How can weather conditions be "weak"? Please revise this statement to accurately reflect the germane synoptic scale meteorological characteristics.

This statement has been revised to "when the synoptic-scale forcing was weak, absent of nearby frontal zones that could quickly change the PBL wind profile".

9. Line 238: There appears to be a pretty significant difference between the temporal granularity of the SODAR and UAS data. Was any attempt made to low-pass or average the UAS data to match the averaging of the SODAR data?

This would be a good idea if the UAS took multiple measurements at the same height over the 15 minute averaging period of the sodar. However, since the UAS only performed one helical ascent during the 15 minute period, only one set of wind measurements at any given height were made. Therefore, it was impossible to average the UAS data to match the SODAR data.

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