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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.

1) Mainly, the comparison of the wind-estimation methods among each other and against the reference systems (radio soundings, ground-based measurements and sodar) has to be performed more quantitatively.

More quantitative analysis has been performed and provided in the updated version of the manuscript, including computation of RMSE for several wind variables.

2) The data quality of the reference systems (radio soundings, ground-based measure-

ments and sodar) should be discussed quantitatively.

Statements about the quality of NWC Mesonet station, radiosonde, and sodar are added in lines (221), (206), and (275) respectively.

3) The mathematical and physical origin of the three methods has to be disclosed, as well as he applied simplifications and assumptions that lead to the three methods (currently eq. 1 and 2).

This information is now provided in the paper, following information and questions provided by the reviewer in the specific comments section.

*** specific comments (numbers indicate lines in the manuscript) a) It should be made clear from the beginning of the manuscript (and not only in half a sentence in section 2) that the discussed methods do not deliver vertical wind and turbulence.

It has been clarified in the abstract and in the beginning of the introduction (line 95) that only the horizontal wind vector is retrieved.

b) around line 75: More details on the SMART sonde would be helpful: size, weight, endurance, propulsion, ...

A more complete description is given in lines (79-84), with a reference to a paper with a more detailed description

c) 79: Briefly: what sensors are used for temperature, humidity, pressure and trace gases? What is their individual sensor inertia? At least temperature and humidity become important for fig. 7, and pressure for the altitude in all diagrams.

Specifications on the measurements of temperature, humidity, and pressure have been added in lines (86-88). Since trace gas measurements were not used in this study, the authors have decided to omit information about that sensor (although information about it can be found in the reference at the end of the previous paragraph).

d) What is the origin of eq.1 and 2, where do they come from? Of course the answer

should be 'The atmospheric wind vector is the sum of the ground speed vector and the airspeed vector, the first defined in the earth's coordinate system, the latter in the coordinate system of the aircraft. Its calculation requires a transformation tensor from the aerodynamic to the earth's coordinate system. See e.g. < Wind Measurements on a Maneuvering Twin-Engine Turboprop Aircraft Accounting for Flow Distortion by ALASTAIR WILLIAMS AND DAVE MARCOTTE, JOURNAL OF ATMOSPHERIC AND OCEANIC TECHNOLOGY, 2000 >'

The above has been added into the paper in lines (122-126) as a way of introducing the mathematical and theoretical basis of the algorithms.

dd) Then it should be explained what assumptions and simplifications were applied in order to obtain eq. 1 and 2. However, a less elegant and less comprehensive way can be to explain how eq. 1 and 2 consider the difference between heading and track angle.

The assumptions and simplifications are now explained in the paper after the mathematical and theoretical basis of the algorithms.

ddd) How did you obtain the aircraft heading (phi in eq. 1) without IMU?

This information has been provided in line (138).

e) 126/7: The airspeed can only be treated constant at constant throttle and pitch if the angle of attack remains constant. This is not true in turbulent flow where the angle of attack changes dynamically. However, averaged over a certain time or distance (how long?) there should be a mean angle of attack.

The authors agree with this comment, and have added a qualifying statement that the true airspeed will vary slightly over the course of a circle, but a mean airspeed can be considered (see lines 141-143).

f) Line 175ff. It is somewhat dissatisfactory to discuss an algorithm that is a complete secret. Can you please give an outline on the method that is used by Paparazzi to

calculate the mean horizontal wind? Any literature?

The authors have found no literature available that discusses how the algorithm retrieves the wind data. The authors have also spent considerable time looking through the open source code to find how it calculates the wind, and have only been able to determine the information that is already provided in the paper.

g) line 203: The mesonet data can show only whether the *surface* wind changes.

The statement has been qualified that it can only show if the 10 m wind changes.

h) How do you explain the (sometimes quite enormous) data differences in fig. 6d and all eight fig. 5 ? In general I do not agree with the authors, that the curves shown in fig.5 and 6 agree well or even very well. Thus, the differences and agreements between the three methods and the comparison with the reference data should be quantified!

The differences between the three methods and the reference data has now been quantified in Table 1. The differences can also be explained by the fact that the measurements often did not take place at the exact same time and were up to 70 minutes later (in the case of 5,b) than the rawinsonde observations. Additionally, rawinsonde observations below 300 m are often unreliable, which is why they have been excluded from the computation of error statistics. A statement has added in the text around lines (147-149) discussing these reasons.

hh) Also, some statements in the text about the quality of agreement that should be (statistically) quantified: line 118, 229, 254, etc.

With the addition of the quantitative analysis, the authors see no need to make statements about the statistical agreement to be qualified at these lines since they refer to visual agreement. The quantitative analysis paragraphs and table provide a more in depth analysis of how the algorithms compared with the rawinsondes.

i) Also, both eq. 1 and 2 offer easy opportunities to calculate standard deviations / a measure for the statistical significance of the methods. So, error bars should be

included in fig. 4 to 7.

Error bars have been added to these figures.

j) 264: Please cite literature that quantifies the critical gradient Richardson number.

Literature has now been cited.

k) 270: Please quantify 'small': centimetres, metres, ...?

This has been clarified to be less than 10 m.

l) The sodar delivered data between 30 and 400 m altitude above ground (agl). Why does fig. 6 show a case where this range was reduced to a few metres? There should be days or hours with better sodar data sets for comparison, shouldn't it?

There was a short window of time when SMARTSonde flights occurred nearby at the same time that the sodar was operating, since it had been inoperable for a considerable amount of time before this study period. As such, only 4 flights in total on 2 separate days occurred with overlapping observations between the sodar and UAS. The two profiles shown in the figure are when the sodar had the best coverage for comparison on each of the 2 days. While the sodar can provide data up to 400 m, that is under ideal conditions. Typically the range is under 200 m, which has been clarified in the paper.

m) 311, 329, 333: Please quantify 'high-resolution'.

The resolution of the profiles has been quantified in the first two cases in lines (354) and (360). However, the authors chose not to quantify the last statement since the resolution is largely adjustable based on the ascent rate and a statement in the previous sentence states the highest possible vertical resolution.

n) 317: Please quantify 'accurately'

This is now quantified based on data from table 1.

o) 316: Conclusions: In the sections before it was not shown / discussed that the first two methods 'performed similarly'.

It now is discussed and shown that the two methods perform similarly in Table 1 and in the last 2 paragraphs of the comparison with rawinsonde section.

p) fig. 7 top left: Please explain why the remaining temperature inversion experienced a cooling in time as shown in the successive flights.

The negative heat flux cooling the air above the PBL is now explained in lines (327-330).

q) fig. 7 top right: Please explain why the flight at 7:50 shows a dryer profile than the flight at 7:18 local time.

By analyzing surface and upper air maps, it appears that weak cold and dry air advection may have been taking place that was cooling and slightly drying the air above the surface. It's tough to determine for sure, though. At the surface, the winds were southeasterly and to the southeast temperatures were cooler and drier. Maps at 925 mb are substantially above this layer and showed basically no advection. As such, a brief statement was added stating that advection may have modified the thermodynamic profile in lines (330-332).

r) fig. 7: Can you please attach error bars to the calculated Ri?

Error bars have been provided now with comments on how they are calculated and why they are quite large.

*** technical corrections (numbers indicate lines in the manuscript) 73 and others: Consequent use of the hyphen between two words that become an adjective in front of a noun, such as 'boundary-layer research'

This has been corrected throughout the paper.

93 and others: To avoid confusions, I'd prefer 'aircraft' or 'aeroplane' instead of 'plane'

‘Plane’ has been changed to ‘aircraft’ or ‘airplane’ throughout the paper.

116: ’input’ instead of ’inputs’ ?

This has been corrected.

118: In order to simplify the reading, please invent an acronym for ’best-curve fitting’, e.g. ’BCF’

This has been corrected and BCF has been used in the remainder of the paper after this point.

eq.1: insert space between v and cos, and v and sin

This has been corrected.

eq.1 and 2: please use a consistent variable for the ground-relative speed (not both Y and S, or is there a difference?). By the way, ’ground speed’ should be sufficient.

While there is a small difference in the variables, as one is a vector and one is simply the magnitude, this has been clarified in the paper and S has been used in both locations (although one is a vector to reflect that difference).

194 ’do’ should be ’did’

This has been corrected.

258 ff: ’Ri’ should not be italic

This has been corrected throughout the paper.

333: typo: ’high-resolution’

This has been corrected.

fig. 5: Due to the small size of the diagrams it is pretty difficult to identify the individual curves and lines (dotted, dashed etc)

Individual curves are now colored for easier identification by the reader.

Interactive comment on Geosci. Instrum. Method. Data Syst. Discuss., 2, 953, 2012.