Review report: Wind speed measurements using distributed fiber optics: a wind tunnel study

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General Comments

The study of van Ramshorst et al. investigated the actively heated fiber-optic (AHFO) technique and estimated its accuracy and precision under controlled airflow conditions by comparing to a three-dimensional ultrasonic anemometer. A valuable error prediction equation for the wind speed measurements at different heating rates was developed, as the heating rate can be a limiting factor for long cables. This equation is also accounting for averaging over space or time which further increases precision. They conclude that AHFO measurements are reliable in outdoor deployments when correcting the measurements for directional sensitivity with a ultrasonic anemometer, choosing the right heating rate and spatial or temporal averaging.
Distributed temperature sensing (DTS) measures temperatures along a fiber-optic cable spatially continuously and can be used in various fields. Especially for atmospheric research this technique offers new insight into the temperature field and thus was implemented in many studies. By using the AHFO technique, wind speed measurements can be added to the system. As the community using the DTS and AHFO technique is growing, the study of van Ramshorst et al. is important for users to be aware of the accuracy, precision and limitation of this technique. Hence, the paper is valuable for our community.
The author did do a good job on the first revision. The reviewed manuscript nicely rearranged the sections and made the manuscript more reader friendly. Also the additional turbulence statistics of the wind tunnel makes it a stronger manuscript. However, there are still some issues regarding precise description of the setup, definition of parameter, presentation of some graphs, and the error prediction function. Details are given below for each section. I recommend to accept the submitted manuscript after major revisions.

Terminology

- advective heat loss vs. convective heat loss:
  the correct terminology as is also used in the referenced literature of this manuscript is "convective".

Abstract & Section 1

- p.1, l.2-3: "...better characterization of fine-scale processes." - this is a very specific comment, but I think sonic anemometers can determine fine-scale processes better in time (20 Hz resolution vs. 1 Hz of DTS), while AHFO technique does have spatially continuously distributed measurements. Hence, AHFO can give inside into the wind field even a network of sonic anemometer might not be capable of on time scales > 1s. But I argue that a general statement like "better characterization" might not justify if not pointing out that the focus is on the spatial scale.

- p.1, l.4: "In this work, ..." this sentence is redundant
- p.1, l.6: "wind speed, angles of attack, and temperature differences" → Oxford comma!
- p.1, l.7: 1-s time scale
- p.1, l.9: the correlation numbers can only be found in the abstract and conclusions, but not in the results section. Please refer to that correlation numbers in the result section, otherwise those high correlation numbers seem to come out of nowhere.
- p.1, l.11-12: "AHFO allows for characterization...complex terrain..." - AHFO can be deployed in any terrain, so I do not understand the focus on complex terrain. I would remove that sentence and only mention the last sentence which is way more important.

- p.2, l.3: "Goodberlet et al. (1989)" - aren't there also more recent paper commenting on that?
- p.2, l.16: 1-s and 0.3-m resolution
- p.2, l.21-22: I do not understand the meaning of that sentence
- p.2, l.25-27: the statements in those lines are very specific and belong into material and methods
- p.2, l.29: magnitude of convective heat loss
- p.2, l.31-25: This paragraph can be shortened : "The heat transfer model assumes a flow normal to the fiber. Hence, non-normal angles of attack need to be accounted for by using directional sensitivity equations. Following the recommendations of Sayde et al. (2015) we tested different directional sensitivity equations from hotwire anemometry in the controlled setting of our experiments" - or similar

- p.3, l.7: remove "in complex terrain" - AHFO can be deployed in any terrain
- p.3, l.13: "precision of future experiments" - precision of AHFO experiments

Section 2.1 & 2.2

Both sections can be shortened by mentioning important literature and specifically focus on the parts which were changed in this manuscript to improve AHFO measurements. Section 2.2.1 has no new content and Figure 1 is from the paper of Sayde et al. (2015) altered in a incorrect way. The presented equations are also more or less a copy of Sayde et al. (2015). The most important changes are mentioned in Section 2.2.2, hence I would argue to remove Section 2.2.1 or only mention the most important points (like the energy balance) instead of the complete derivation of the equation determining the wind speed from AHFO measurements as already done by Sayde et al. (2015).
I also have the following specific comments for those two sections:

- p.3, l.17-25: I think this paragraph is unnecessary for the manuscript focusing on AHFO. One sentence and pointing to the publication of Selker et al. 2006 is enough to explain the basic measurement principle of DTS. The last two sentences of the paragraph can be inserted somewhere else.

- p.4, l.1: "heat the fiber" - heat the FO cable
- p.4, l.2-3: "Also the creation of ..." - very confusing statement
- p.4, l.6: "which van be" - which can be
- p.4, l.10; p.5, l.5; p.5, l.13; p.5, l.14; ...: "advective" - "convective"

Section 2.3

The whole section needs to be revised. Some information is unnecessary and could be addressed in one paragraph instead of mentioning it in different paragraphs. For example p.7, l.17-23 define the heating rates P_s creating ΔT for different wind speeds, however, the actually used ΔT are mentioned p.8 l.8 and the corresponding P_s p.9, l.1. Also the total number of experiments could be mentioned at the end after defining/mentioning all setups instead of consecutively summing up the experiment setups (p.8 l.2, l.6, l.8). Further, in this section FOs is used as a synonym for FO cable or FO cores, however, was not defined as such.
The FO configuration as also introduced/proposed by Hausner et al. (2011) is introduced in p.8, l.10-11, then changed to a double-ended configuration (p.9, l.12), but actually a single-ended configuration (p.9, l.12) was used. This is more than confusing to the reader especially by mentioning it in different paragraphs. I would propose to mention the setup, only the actually chosen calibration method (as proposed by Hausner et al. (2011)), and step loss correction in one paragraph.
I also have the following comments and questions:

- p.7, l.17-23: This paragraph should be revised. It is confusing to the reader. The main point should be the definition of the heating rate P_s fixing ΔT for different wind speeds. Accordingly, ΔT is representing P_s.
- p.7, l.17: "The angle of the fiber..., wind speed, and heating rate were systematically changed" - This sentence is fine, however you start defining heating rate, then the angle of attack and then your wind speed settings... A reorganization makes it more reader friendly.
- p.7, l.26-28: I am not sure if this sentence is important unless you add that those parts were excluded from analysis to avoid artifacts of the fiber touching the mounting material.

- p.8, l.2: "The AHFO wind speed measurements can be calibrated..." - calibrated is not the correct use here, as DTS data itself is also calibrated. I suggest "adjusted" or "verified". Also removing that sentence would not affect the meaning of the paragraph.
- p.8, l.10: The manufacturer of the FO cable is missing.
- Figure 3: The figure is not necessary for understanding the described method, Figure 2 is already showing the needed information. Either remove or put into Appendix.

- p.9, l.1-6: this paragraph is describing the heating rate and how it can be estimated from the resistance of the FO cable. However, the heating rate is first mentioned p.7, l.22. I suggest to reorganize the section and to shorten this paragraph. The most important information is in the last sentence.

- p.9, l.16. "splice loss": was a step loss correction performed?

Section 2.4 & Section 3.1

I do not completely agree with p.10, l.8-10. Sayde et al. (2015) never adjusted DTS measurements, they adjusted the sonic anemometer measurements and compared them to the AHFO measurements. Hence, the statement is not completely correct. However, I agree that the equation can be used to adjust for different attack angles and adjust/correct AHFO measurements if a sonic anemometer is near the AHFO setup.
Figure 4 should be adjusted by showing violin plots or boxplots for each wind speed configuration. The shown dots can not show the distribution of the DTS measurements in a clear way. Are real measurements of the sonics shown or the proposed fixed wind speeds?

Section 3.2

The definition of σ_a and σ_p should be moved into material and methods. The result that σ_a and σ_p are dependent on wind speed and averaging over space and time and the corresponding discussion can remain in Section 3.2.
On page 11 line 1-6 present details on the duplex FO configuration and that only the middle of the FO cable was used to estimate u_{DTS}, because otherwise the accuracy is decreased (Table C1). I think that this important piece of information, especially about the 90° attack angle, needs to be mentioned in Section 2.3. Further, shouldn't a step loss correction cover the effects of a splice?
I think Figure A1 and A2 are unnecessary as Figure 5 is giving the same information and derive the same conclusion. If the author decide to include those figures, they need to be adjusted in the same way as Figure 4.
Eq. 13 and 14 are presented in a confusing fashion by introducing dependencies which do not affect the definition of the introduced parameter. Eq. 13 and 14 define σ_a and σ_p, however, the definition itself is not reflecting any dependency on n_{space} nor n_{time}. This dependency is shown in Fig. 5 and Fig. 6 and should not be included in Eq. 13 and 14. The original equation from the first manuscript were better. Besides, the parameter u_j remains unclear to me as it also doesn't define σ_a and σ_p. Both are defined by u_{DTS} and u_{sonic}. I think u_j is introducing an unnecessary parameter. Further, Eq. 15 is introduced, which is changed to Eq. 16 without justification and both are different.

- p.12, l.12-14: those lines are redundant as they are already introduced on p.11, l.12-17
- p. 13, l.1-3: I do not see the justification of excluding the variation of sonic anemometer measurements by assuming it is small. If it was tested and did not change the results, I can see the justification of not considering that variation.

Section 3.3 and Section 3.4

The title of Section 3.3 is not representing the content. The use of an intermediate constant is still not justified. The authors response to my comment did not include a justification nor the manuscript. The intermediate constant is defined as:

$$C_{int} = \overline{f(\overline{u_j}, n_{space}, n_{time}) \cdot \frac{\Delta T}{T_{error}} \cdot \sqrt{n_{time}}} \tag{1}$$

Hence, C_{int} (or $C_{DTS} = C_{int} * 10$) is the mean of the precision over a variation of setup:

$$C_{int} = \overline{\sigma_p \cdot \frac{\Delta T}{T_{error}} \cdot \sqrt{n_{time}}} \tag{2}$$

Those constants are then included in the estimation of σ_p again which is not justified.
Besides, how was the factor T_{error} determined? Or is this number from literature?
The revised manuscript is describing the derivation of the intermediate constant very quickly without further explanation. This should be presented in further detail.
I still propose to derive the error prediction function in a more clear way. As shown in Fig. 6 for each ΔT the precision σ_p is following a $\frac{1}{\sqrt{n}}$-line. Hence, we assume the following dependency:

$$\sigma_p(n) = \frac{\alpha}{\sqrt{n}} \tag{3}$$

with α being a constant different for experiment set up. We found that α depends on ΔT and T_{error}:

$$\alpha = \left(\frac{\Delta T}{T_{error}}\right)^{-1} \tag{4}$$

with $T_{error} = 0.25$ K being the performance of the DTS dependent constant and ΔT being the measured temperature difference between the cables. Hence, α is representing the quality factor for the wind speed measurements. The lines derived from α could also be added in Fig. 6 or 7. When simplifying Eq. 20 of the submitted manuscript we can assume that ΔT is mainly depending on the following parameter:

$$\Delta T = \frac{AP_s}{Bu_n^m} \tag{5}$$

Combining my Eq. 4 and 5 and inserting that in Eq. 3, I derive the following error prediction equation:

$$\sigma_p(n, u_n, P_s) = \frac{BT_{error}u_n^m}{AP_s}\frac{1}{\sqrt{n}} \tag{6}$$

If I did not miss a point, no empirically derived intermediate constant has to be used for the error prediction equation.
I think the error prediction function could be tested with the existing data set by inserting P_s in the error prediction function and plotting that against the 'real' σ_p using Eq. 14 of the revised manuscript. This should show if C_{DTS} is needed or not.

Section 3.5 & conclusions & Appendix

Section 3.5 is not introducing new content. It should be incorporated in the corresponding sections as a paragraph of discussion or incorporated into the conclusions if the statement is more an outreach than a finding.

- p.17, l.19-20: how can turbulence be fully captured by the AHFO technique? Which turbulence scales are you talking about? How should the setup look like?
- p.18, l.6-7: "Due to the way this design tool is constructed, it can be generalized for all kinds of fibers, DTS precisions, and user preferred spatial and temporal resolution."
  I do not agree with this statement, because the accuracy and precision of the DTS measurements change with the use of FO cable, DTS performance, and the used calibration method. Further, the turbulence of the wind tunnel setup does not represent outdoor turbulence as also stated in this manuscript. Another point is the response time of the FO cable. The thicker the cable, the longer it takes to reach the FO core and measure the temperature change. Also the measurement location of the wind speed is important, as the noise of measurement increases with distance from the measurement device, hence with the location along the FO cable. So how should C_{DTS} and the error prediction function be representative for outdoor deployments, different choice of FO cable, different setup of FO cable, or a different DTS machine? I think the statement is too strong and not justified.
- p.18, l.11: "...applications in complex terrain, allowing for..." -...applications, for example allowing for... - AHFO can be deployed in any terrain.
- Figure B2: turbulence intensity should be defined in the caption, as well as the location of the x-, y- and z-coordinate.