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 very valuable error prediction equation for the wind speed measurements at different heating rates were 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 introduction to the determination of wind speed is nicely done, however, I think it can be organized more reader friendly. The overall structure of the paper is logic, but could be reorganized and shortened. In my opinion some figures are redundant. The development of the error prediction equation needs clarification. I could not differentiate results from discussion. Further, I am missing some turbulence statistics of the wind tunnel (friction velocity, velocity aspect ratio, turbulence intensity in different directions,...) to give an estimate how representative the turbulence within the wind tunnel is to outdoor turbulence. I recommend to accept the submitted manuscript after major revisions.

Title and structure of the paper

Throughout the paper the abbreviation AHFO is used, however the title uses "distributed fiber optics". The title may also incorporate that is not the first paper using the AHFO technique. I propose "Revisiting wind speed measurements using actively heated fiber optics: a wind tunnel study" or similar.
I would propose another order of the sections. After the introduction, I would start with the introduction to the DTS technique and the signal-to-noise ratio (Section 2.4), then introduce the

energy balance of the fiber (Section 2.2), then introduce the experimental setup (Section 2.1), because then the reader already know why two fibers are needed, why spatial averaging is potentially important, etc. However, this order is a minor point and could also be chosen differently. Afterwards I would not differentiate between results and discussion. The discussion was insufficient, as I could find no references comparing the work to other studies nor testing or discussing the error prediction equation. I propose to have the following sections instead of Results and Discussion: Directional Sensitivity, Accuracy of AHFO, Precision of AHFO, Error Prediction Equation, Outdoor Deployment of AHFO. Then finally the conclusions.
I also have specific comments on the following sections:

- Determination of Wind speed (complete Section 2.2):
  The reader is barely able to follow. The equations are introduced in Section 2.2.1, but in the following section the introduced equations are altered or simplified. I suggest to define subsections each concerning one part of the energy balance of the fiber-optic cable, similar to the study of Sayde et al. (2015). Within each subsection all assumptions and simplifications should be noted. This should also shorten Section 2.2.
- DTS and Signal-to-Noise ratio analysis (Section 2.4):
  I don't see the motivation of this sections besides describing the measurement principle of the DTS technique and sources of noise. Also some sentences are not precise and remain unclear (p.10 l.11, l.17-18, l.20-21) or could be removed (p.10 l.18-19).
- Using AHFO outdoors (Section 4.3):
  The last paragraph belongs into the introduction

Questions on deriving the error prediction equation (Sect.4.1&4.2)

The main goal and strength of this paper is the development of the error prediction equation depending on the wind speed, the heating rate and accounting for averaging over space and time. However, in my opinion, the development of this equation have to be presented in a more reader friendly way and the assumptions and the validity of this equation has to be reconsidered.
I have some questions concerning the intermediate constants and Eq. 20:

- the parameter γ is introduced representing σ_p at 1 s temporal, and 10 measurement spatial, resolution, hence a specific, empirically derived σ_p.
  $\rightarrow$ Does this mean that the authors averaged over 10 u_{DTS} measurements spatially? Or did the authors average the temperature differences over 10 spatial measurements? Or did the authors average the temperature of the unheated and heated cable over 10 spatial measurements and from that computed the temperature difference and thus u_{DTS}?
  $\rightarrow$ in Eq. 16 γ stands for a specific, empirically derived σ_p, however, when included in Eq. 20 the same γ is representing σ_p depending on n, P_s and u_N, which does not seem logical to me. How can the authors defend this?
- $\sqrt{\frac{t_{avg}}{t_{sample}}}$ is $\sqrt{n}$, with n being the number of measurements over which u_{DTS} is averaged over time. This is not mentioned in the text. Following this, Eq. 16 can be rewritten:

  $$C_{int} = \gamma \frac{\Delta T}{T_{error}} \sqrt{n} \quad (1)$$

  C_{int} is then presented as an intermediate constant, which is also shown in Fig. 9b. However, is C_{int} the mean over all ΔT and attack angles? Fig. 9b shows a spread of C_{int}

from 1.3 up to 2. Further, did the authors use the constant ΔT or the actually measured ΔT? Please clarify or use different symbols.

- "By using the shown $\frac{1}{\sqrt{n}}$ dependency, we can easily convert C_{int} into C_{DTS}" (p.15 l.1-2):
  $\rightarrow$ what is C_{DTS} compared to C_{int} or what does it represent?
  $\rightarrow$ is n representing the space or time domain in this context?
- C_{DTS} is computed "by multiplying C_{int} by $\sqrt{\frac{10}{1}}$, as n is 10 times less" (p.15 l.2), what is a confusing statement. In my understanding $\sqrt{\frac{10}{1}}$ is $\sqrt{\frac{x_{avg}}{x_{sample}}}$ with x representing the space domain. This is $\sqrt{n_{space}}$ with n_{space} being the number of measurements over which u_{DTS} is averaged over space. Hence I derive the following equation for C_{DTS}:

$$C_{DTS} = C_{int}\sqrt{n_{space}} = \gamma \frac{\Delta T}{T_{error}} \sqrt{n \cdot n_{space}} \tag{2}$$

- in Section 4.2 the goal was to have an estimation for σ_p. Therefore, Eq. 16 and Eq. 17 are combined, σ_p is inserted for γ (which is a point of discussion for me as mentioned earlier), solved for ΔT, and inserted in Eq. 19 to derive Eq. 20 when solving for σ_p.
  $\rightarrow$ when I did the proposed evolution from Eq. 16 to Eq. 20, I got a factor of $\frac{1}{\sqrt{n}}$, not $\sqrt{\frac{1}{n}}$.
  $\rightarrow$ in Eq. 20 n "is the number of measurements over which the observed wind speed is averaged, in either space or time domain" (p.15 l.22). However, I interpret this n as $n + n_{space}$ and not $n \cdot n_{space}$ (as shown in my Eq. 2). Hence, I think C_{DTS} is computed incorrectly.
  $\rightarrow$ are the authors proposing that there is also a $\frac{1}{\sqrt{n_{space}}}$ dependency for σ_p? I am missing a figure showing this.
  $\rightarrow$ this changes Fig. 10 completely
- the authors should consider to use different symbols for the time and space domain like n_{time} and n_{space} or similar
- why did the authors choose γ representing the empirical derived σ_p when averaging over 10 measurements spatially? As the effect of spatial averaging is also under study in this paper, I would derive σ_p without spatial averaging, so the lowest possible precision, and then investigate the effects of spatial averaging. Also the derived C_{int} and C_{DTS} seem to be biased by this decision.
- in my understanding C_{DTS} is derived empirically when choosing P_s depending on u_N to have a constant ΔT and C_{DTS} is a mean over all experiments. This is not considered in Eq. 20 nor further discussed. Is C_{DTS} representing σ_p for a constant P_s during different wind speeds, even though the experimental design was different and C_{DTS} is a mean over all experiments? Was there an experiment done using a constant P_s verifying the error prediction function?
- do the authors suggest to use a different P_s depending on wind speed? In my opinion it might be not useful in field deployment with quickly varying wind speeds to change the heating rates constantly as the fiber-optic cable needs to reach steady state and there is also a response time between the changed heating rate and DTS measurements. This could potentially lower the precision instead of increasing it.

I propose to derive the error prediction function in a more clear way. As shown in Fig. 8 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}:

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

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. 8. When simplifying Eq. 18 of the submitted manuscript we can assume that ΔT is mainly depending on the following parameter:

$$\Delta T = \frac{AP_s}{Bu_n^m} \quad (5)$$

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

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

If I did not miss a point, no empirically derived intermediate constant has to be used for the error prediction equation.

Terminology

- p.2 l.28-29: "advection of cooler ambient air". I think convective heat loss is the correct phrase here. Please make sure the correct terms are used throughout the manuscript.
- p.6 l.14: There is a difference between the turbulent Prandtl number and the Prandtl number representing the ratio of momentum diffusivity (kinematic viscosity) and thermal diffusivity. Please clarify.
- p.10 l.20:
  "The precision is an indication of the variability of the wind speed (e.g. RMSD),..."
  → variability of the wind speed can also describe the deviation from the mean wind speed. However, I think in this context the authors refer to the precision of a measurement assuming a constant wind speed.
  → what is RMSD?
  "... as opposed to accuracy which describes a systematic error that can be removed through calibration (e.g., a bias)."
  → Accuracy it the combination of trueness (bias) and precision. The accuracy of a measurement can be low due to a poor trueness (high bias) or due to a poor precision. Please adjust throughout the manuscript.
  → DTS measurements need to be calibrated in post-processing to derive the actual temperatures from the ratio of intensities of Stokes and Anti-Stokes. Hence in this context I would avoid using the word calibration to not confuse the reader.
- p.12 l.13: If RMSD is the root-mean-squares deviation, how was Eq. 15 derived and how does this equation represent the precision? In Eq. 15 the precision of both instruments are combined, even though σ_p should represent the precision of only the AHFO technique.
- Throughout the manuscript σ_p is used as a synonym for or parameter representing precision. However, it is counter-intuitive that σ_p is decreasing for higher precisions.

Figures

- Figure 1: a and b are missing within the figure
- Figure 2: I would make this figure at least smaller. I don't think this figure is necessary as technical specification of the fiber-optic cable is given in the text.
- Figure 3: I like this figure very much, however I am missing the connection to Eq. 1. Breaking Eq. 1 into the relevant elements of the energy balance of the fiber and marking those elements with colors or boxes in Fig. 3 makes it even more powerful
- Figure 4: Fig. 4a and Fig. 5a as well as Fig. 4d and 5d are identical. Directional sensitivity is not corrected for an attack angle of 90° angle as this is considered the optimal attack angle, hence showing 4a in this context does not make sense. Why are the symbols differ between Fig. 4 and Fig. 5?
- Figure 5: comparing the subfigures is not easy because they are basically looking the same (also R^2 is relatively similar). It is already shown in Fig. 4b and c that the directional sensitivity can be corrected by Eg. 13 and Fig. 5 is not adding new content. I also did not see further description of Fig. 5 nor discussion of it. So I suggest to take this figure out, unless Fig. 5 is described and discussed.
- Figure 6: The effects of temporal averaging are shown and discussed in Fig. 8, however this is not done with Fig. 6. Take it out.
- Figure 7: The symbols are too small to see the filling you are using for different heating rates. Better use opaque colors like in $\Delta T = 2°C$
- Figure 8: see comments on Fig. 7
- Figure 9: I would rather add $\frac{1}{\sqrt{n}}$ for each heating rate to Fig. 8 than adding Fig. 9a. Each heating rate is following a $\frac{1}{\sqrt{n}}$-decay, so when normalizing by the heating rate the only logical outcome is Fig. 9a. Hence Fig. 9a provides no new content in my opinion. The same appears to me for Fig. 9b.
- Figure 10: I would show the same figure, but without spatial averaging. From this paper people should know that the precision can be further increased by averaging spatially or over time.

Specific comments

- p.1 l.5 : "operational conditions": I would rather name the conditions: heating rates and attack angles
- p.1 l.8-9: Under which conditions? For all conditions?
- p.1 l.9-12: "We conclude...": no new content. We already know this from Sayde et al. (2015). What is your new contribution?
- p.2 l.11: "High-resolution...": add spatially
- p.2 l.31-35 & p.3 l.1-5: this paragraphs is not well organized
- p.3 l.19-20: "...to create the temperature difference needed to determine wind speed..." a minimum needed ΔT is not determined in this study. I think it is rather: "... to create the wanted temperature difference of at least 2°C..." or similar.
- p.3 l.33: "steady state flow" → maybe add variability of the wind speed from ultrasonic anemometer measurements

- p.5 l.5: As you are mentioning the calibration set up here, which calibration set up did you use? single-ended, double-ended or duplexed?
- p.5 l.19-20: The captions are identical.
- p.6 l.11: Why is T_s used for the heated cable while T_f is used for the unheated cable/air temperature? f can be associated with fiber which can cause potential confusion. Maybe use T_a for air temperature and T_f for the temperature of the heated fiber-optic cable.
- p.8 l.13-15: I do not understand the meaning of this sentence.
- p.9 l.2-3: "...especially in outside atmospheric experiments." → I disagree. When using a fiber-harp as a set up like the study of Thomas et al. (2012) to measure wind speeds, attack angles are 90° and no directional sensitivity has to be taken into account.
- p.10 l.12-15: Why can the measurement error be assumed to be constant in the lab, but not in outdoor deployment? Further, here σ_T is the measurement error of the DTS, while later on p.13 l.13-14 the parameter T_{error} is introduced as the constant concerning the performance of DTS. What is the difference between those two constants?
- p.11 Eq. 14.: what does the overline indicate?
- p.12 l.1: what is the "uncalibrated data set"? Uncalibrated DTS measurements? Or were the directional sensitivity of the measurements not corrected?
- p.12 l.1-3: why should the energy loss due to free convection be different between different attack angles?
- p.12 l.5: even when comparing the other attack angles to the 90°, I can see no clear dependency of the bias. 45° has the smallest bias, while 30° and 15° have more or less the same bias. Hence, I would argue: In hotwire anemometry the bias for small attack angles is high and decreases with increasing attack angle. However, even if this effect is taken into account for AHFO measurements, there seems to be more sources of error causing different behaviour of the bias for different attack angles, but with no clear pattern.
- p.12 l.8-9: What does "extensive calibration" mean? What else could be done or for what else can AHFO be corrected?
- p.13 l.3: "The precision increases to a σ_p less than 5% by averaging over time" → where is this shown?
- p.15 l.6-7: "However, given the result that the increase in precision behaves independent of ΔT and the averaging time, it is possible to make a prediction for the precision of future work." → this contradicts Fig. 8 and the derived error prediction equation. The precision only behaves independent of ΔT and the averaging time, when the precision is normalized by both parameter.
- p.17 l.2-3: I do not understand the connection of Fig. 10 and the missing factor of free convection in the energy balance of the fiber. Please clarify.
- p.17 l.9-13: I would add, that either a set up like a fiber-harp has to be used, for which no reference device is needed as the attack angles are 90°, or a reference ultrasonic anemometer or even multiple ultrasonic anemometer have to be used to account for different angles of attack.
- p.18 l.1-3: Why is complex terrain specifically mentioned, while field experiments with the AHFO technique is valuable in any terrain?

References

Sayde, C., Thomas, C. K., Wagner, J., and Selker, J. (2015). High-resolution wind speed measurements using actively heated fiber optics. *Geophys. Res. Lett.*, 42(22):10064–10073.

Thomas, C. K., Kennedy, A. M., Selker, J. S., Moretti, A., Schroth, M. H., Smoot, A. R., Tufillaro, N. B., and Zeeman, M. J. (2012). High-Resolution Fibre-Optic Temperature Sensing: A New Tool to Study the Two-Dimensional Structure of Atmospheric Surface-Layer Flow. *Boundary-Layer Meteorol.*, 142(2):177–192.