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

**Author of the paper: van Ramshorst et al.**
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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 manuscript improved substantially. It is well organized and leads the reader through the whole manuscript. I still have one major point: The manuscript propose to develop an error prediction function being valid for any kind of setup. However, the error prediction function is not tested or validated with the existing data set nor is the last point discussed accordingly. I did get a table in the authors' response to compare different approaches of the error prediction function, however, this table is not well explained. Further, no values derived from the prediction function is compared to actually measured quantities neither in the table nor in the manuscript. As the error prediction function is one main goal of the manuscript, either the authors need to explicitly state that the error prediction function needs to be validated in another experiment or another section validating the error prediction function is added to the manuscript. I recommend to accept the submitted manuscript after major revisions. More detailed comments are given below.

## Detailed comments

- p3 l23: $T_{error}$: measurement error? how determined?
- p3 l27: choose dominant or important or be more specific

- p8 l18-31: nicely done!
- p9 l4: "duplexing" → duplexed FO core (I would stick with the earlier already mentioned phrase)
- p9 l6-8: the argumentation to treat the 90° angle different than the others is a bit thin in my eyes: maybe speculate why the 90° angle had lower precision (sharper bending of the FO cable maybe?) and thus justify your decision. Or treat all attack angles the same and shorten all data down. Why should the splice only affect the 90° angle? What if the others were also affected just a bit less?
- p9 l12-14: "indicating and..." → "indicating an"; How is the actual spatial resolution defined? Nyquist-frequency?
- p9 l14-17: I think mentioning the goal of an error prediction function is more useful than already mentioning the unique constant which is used in the error prediction function later. This will make it easier for the reader to follow your manuscript. Especially the last sentence is confusing to me. Is one constant more representative if I am averaging, but if I am not averaging, it isn't?
- Figure 3, B1, B2: a 1:1-line would be very helpful
- Figure B2 and p11 l1-5: It would be easier if B2 has two figures a) 1-s data and b) 30-s data. It is impossible for the reader to combine all four plots in B1 into one plot and compare it to Figure B2.
- Figure 4: Why is accuracy of 90° & $\Delta T = 2$K getting worse for higher averaging time? I would suspect the opposite as also proposed by the mansucript (p11 l16-17). I would like that this is at least mentioned/discussed.
- p11 l4: coefficients of determination: I guess that is a linear regression and you show the R-values? What is the derived slope and offset? Please also add this information or at least add the 1:1-lines to the graphs if slope is close to unity anyway.
- p11 l16-17: You need to discuss this statement further and use another phrase for "extensive calibration" which is not accurate as different calibration methods can be applied to the FO measurements before even computing wind speeds. I would also argue that maybe the temperature signal needs to be averaged over time before computing the FO wind speed. Would that also increase the accuracy? If not, why? Was this also tested?
- p11 l18: I would argue that the dependence between accuracy and averaging time is less pronounced than the one between the precision and averaging time scale, not that the accuracy is constant over time. Besides, it is confusing that $\sigma_a$ is given in percent while $\sigma_p$ is given in decimal numbers, but percentage values are given in the text. Please make uniform for both parameter.
- p11 l20: The last sentence is redundant. Either comment in further detail what Eq. 13 is stating and what dependencies can be determined from Figure 5 or remove the sentence.
- p12 l1-3: The meaning of those sentences for the analysis is hard to understand. Further, is $j$ used instead of the measured $u_{sonic}$?

- p12 l6: rephrase: "the precision was averaged over all wind speeds which is justified, because $\sigma_p$ is normalized by the mean wind speed, hence any linear dependency should be removed" or similar; "... for all $\Delta T$..." → "... for each $\Delta T$..."
- p12 l7: "..., with ..." The sentence is redundant to some degree. The colors and symbols are already showing why there are 12 different points for each $n_{time}$.
- The following is only a suggestion/thought: What about dropping the attack angles for $\sigma_p$? They are not further discussed as you already account for them in the earlier section. So should that maybe not be considered moving on? It is kind of distracting from the main object of different heating rates and averaging time/spatial scales.
- p13 l10-15ff: If this statement is true, then it needs to be further discussed and why it can be applied to different settings. The statement is also refering to an Equation which is introduced later in the manuscript. So I suggest to insert this paragraph at the corresponding location to Eq. 21 and into Section 3.4, respectively. Also $C_{int}$ has a wide spread and needs to be further discussed. Again I suggest to insert a plot using the prediction function for $\sigma_p$ and the actual derived $\sigma_p$ for all averaging scales to show the strength and accuracy of the prediction function.
- Figure 6a: y-axis label is not representing what is actually plotted: $\sigma_p \cdot \frac{\Delta T}{T_{error}}$
- Figure 6b: same y-axis problem as Fig. 6a. Further, how can you justify that your proposed constant has a spread from 1.1 to 2.2? This needs to be mentioned and discussed.
- p14 l6-7: This is a contradiction as Figure 5 is showing the exact opposite: Higher $\sigma_p$ for lower $\Delta T$, lower $\sigma_p$ with increasing $n_{time}$.$\sigma_p$ can be estimated from those variables, but $\sigma_p$ is not independent of those.
- Section 3.5: Maybe dew fall on the fiber needs to be considered? Water droplet on the fiber will for sure affect the measurements altering the heat loss of the unheated fiber (assuming the water droplets quickly evaporate from the heated fiber).
- p17 l10: directional sensitivity compensation can only be applied if the angle of attack is known demanding ancillary measurement devices. Please add.