Response to reviewer comment RC1

The original reviewer comment are in black font and our detailed responses use blue font.

Summary: This paper is focused on the question whether or not the CSAT3 sonic by Campbell Scientific needs correction for flow distortion, i.e. correction for how the instrument structure itself disturbs the flow it measures. The answer, given towards the end of the paper is a somewhat vague no, and concerning friction velocity, the authors conclude that the CSAT3 sonic anemometer is "accurate enough for most applications". The big news in the study is, however, the introduction of the bistatic Doppler lidar as a new flow-distortion free reference instrument, which can measure 3d turbulence with a very small measurement volume. It is also mentioned that the bistatic lidar can serve as a replacement for commercially available wind lidars, which typically measures the wind speed at several heights, typically in the range 50 m to 150 m. When new instruments are introduced, it is necessary that their limitation and shortcomings are discussed, but in this paper, only selected signals from the bistatic lidar are presented and no limitations with regards to accuracy and measurement capability/signal quality are mentioned. Further, the effect of the extensive postprocessing of the data is not satisfactorily described. Whereas the study represents impressive works and achievements, I find that neither of the two main topics (#1 flow distortion in CSAT3, and #2presentation of the bi-static lidar for 3d turbulent measurements) is treated rigorously enough, and therefore recommend a major revision before being accepted for publication. The presented conclusion that the CSAT3 sonic is different from the other sonics on the market and measures sig_w perfectly without any flow distortion correction is controversial, given earlier evidence. It is hard to overlook that other carefully designed studies have come to a different conclusion, and a more balanced discussion of the presented results in the light of the earlier studies would be an improvement.

We thank the reviewer for this critical, yet very helpful, feedback and we will provide additional information where requested for further clarification. We would however respectfully disagree that this wind lidar is newly introduced in this paper at hand. As mentioned in the original manuscript version, the paper by Oertel et al. has already introduced and characterized the bistatic lidar itself. Moreover, the lidar has been presented at several instances before (see references below). In Oertel et al., the bistatic wind lidar was compared with an LDA as reference instrument in a specially designed wind tunnel. This allows the new wind lidar to be validated for wind vector measurements that are traceable to the SI units. All shortcomings and limitations of the bistatic Doppler lidar were already discussed by Oertel et al., and in the paper at hand we refer to it and repeat the main results in section 2.1.2.

Eggert, M., Müller, H. and Többen, H.: Konzeption eines Doppler-Lidar-Transfernormals zur Windgeschwindigkeitsmessung, Proceedings Lasermethoden in der Strömungsmesstechnik, 45/1 - 45/6, 2011

Eggert, M., Müller, H. and Többen, H.: Doppler-Lidar-Transfernormal zur ortsaufgelösten, vektoriellen Windgeschwindigkeitsmessung. Proceedings Lasermethoden in der Strömungsmesstechnik, 43/1 - 43/7, 2013

Eggert, M., Gutsmuths, C., Müller, H. and Többen, H.: A New Bistatic Wind LiDAR for Highly Resolved Wind Vector Measurements, Proceedings DEWEK 2015 - German Wind Energy Conference, 04.5/1 - 04.5/3, 2015

Major criticisms:

1. When applying the flow distortion correction by Horst et al (2015) (H15), the authors find that the sig_w and U are both increased (Table 2 compared to Table 1). Yet, when presenting the cospectra in Fig 8, the covariance is decreased when applying H15 for all the low and energy containing frequencies. This is an impossible result; since the u and w components are both increased when applying H15, the absolute of the covariance must also increase for the low-frequency range. I recommend the authors to double check their algorithm and make sure that the red and grey lines have not been accidentally swapped. Since the u* comparison is improved by applying H15, the uw co-spectrum using H15 should also be closer to that by the bistatic lidar and a mistake in labelling/accidental swap seems likely.

Indeed, sig_w, U, and u* are increased by the H15 correction. We double-checked our algorithms and the labels were displayed correctly but we actually found a bug in the averaging procedure of the CSAT3B + H15 (co-)spectra. Thanks very much to the reviewer for pointing this out! We corrected this mistake in the manuscript, and a copy of the revised Fig. 8 is presented here below for convenience:
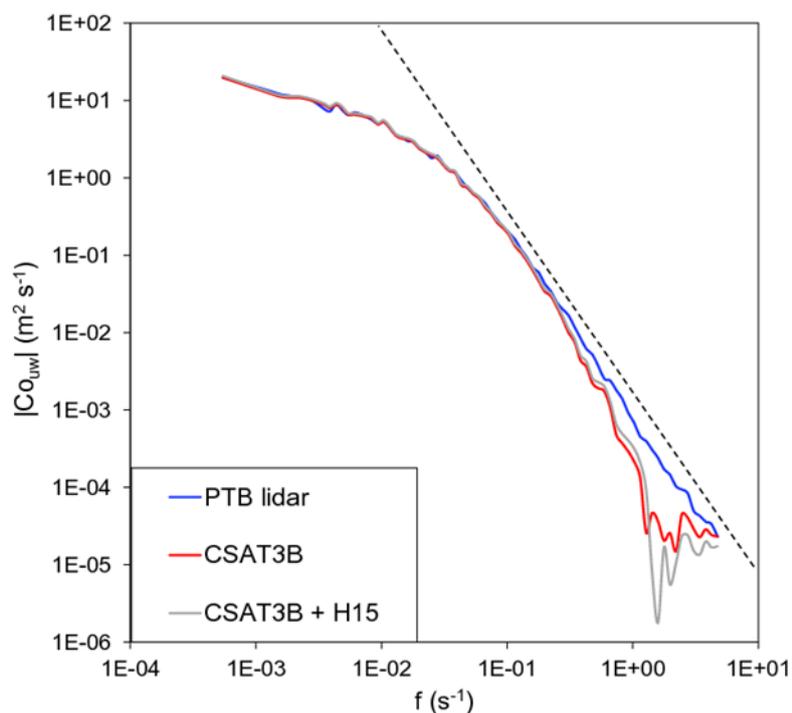
Now, the grey line (CSAT3B + H15) is very close to the red line (CSAT3B) or slightly above. We would like to stress that this mistake does not affect the results of the statistical comparison.

Further, the authors write: *Apparently, an artificial correlation between u and w is introduced at high frequencies, which can be explained by the interdependence between u and w introduced through this correction algorithm*. This argument is based on that the absolute of the cospectral density changes from around 0.00001 to around 0.0001-0.001 for high frequencies. Probably, if the absolute operator is removed, the authors would find that for the spectral range f > 1 Hz, where zero cospectral content can be expected for the investigated setup, the sum of the co-spectral content is

indeed zero and that deviations from zero are just noise. This very minor change to the spectrum is not an argument for not using H15.

We agree that the red and the grey line in Fig. 8 basically just show noise for f > 1 Hz. However, the blue line (PTB lidar) does not only show noise, it actually follows the expected -7/3 power law very well up to the Nyquist frequency of 5 Hz. For clarification, we added a corresponding sentence in the revised manuscript (Sect. 3.2):

*"An analysis of the cospectra shows that the H15 correction somewhat distorts the expected −7/3 power-law behavior at very high frequencies in the inertial subrange (**Fehler! Verweisquelle konnte nicht gefunden werden.**). However, the values for f > 1 s⁻¹ are very small anyways and represent mostly white noise, which appears as horizontal line in spectral space. It can also be seen that the H15 correction slightly increases the cospectral energy across the entire range of frequencies. However, the too steep drop-off of the CSAT3B ensemble cospectrum is not improved effectively."*

We also modified the last sentence of the abstract:

"We also found that an angle-of-attack dependent transducer-shadowing correction does not improve the already good agreement between the CSAT3B and the PTB lidar effectively."

And we modified the corresponding sentences in the conclusions section:

"We also evaluated whether the overall accuracy of the CSAT3B measurements can be improved by the H15 flow-distortion correction, and our results indicate that this method increases the spectral energy across the entire range of frequencies equally and does not appropriately correct the CSAT3B data in the inertial subrange. It leads to an overestimation of $\overline{(w'w')}^{\wedge}(1/2)$, and it does not correct for the wind-direction dependent error of $\overline{u}$."

We agree that the minor change in the cospectra is not really an argument for not using H15: However, it still does not improve the cospectra effectively, it leads to an overestimation of sigma_w, which is a valid argument for not using H15 if one is interested in scalar fluxes. Therefore, we modified the conclusions as follows:

"Based on these results, we conclude that the probe-induced flow-distortion issue of sonic anemometers warrants further investigations in the future to effectively correct general measurements of scalar fluxes."

We decided to apply the absolute operator to the cospectra Fig 8. because the uw cospectral densities are mostly negative (since the momentum flux is negative) and they would therefore not show up at all in a logarithmic plot. We chose the logarithmic plot in order to compare the results with the -7/3 power law graphically. The non-logarithmic version of the same plot without the absolute operator is shown below for illustration.
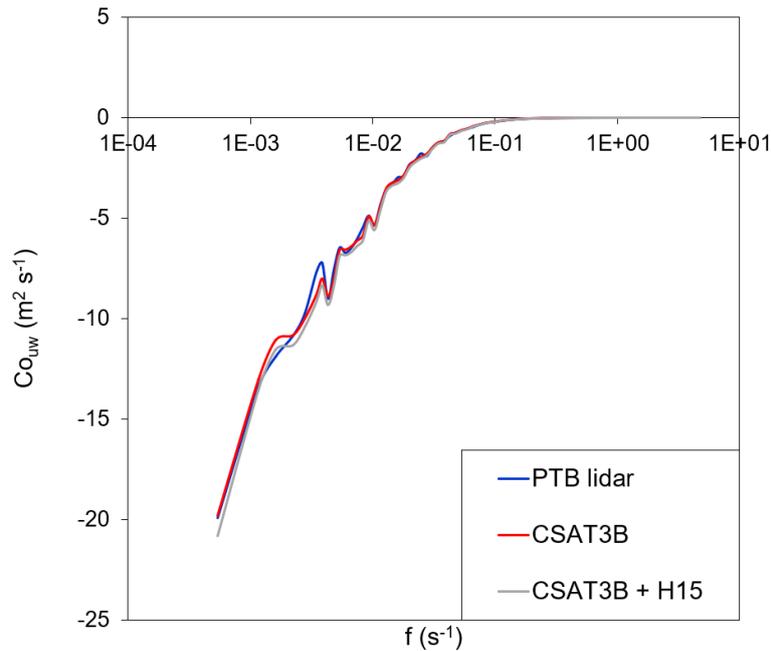
2. LL 16-18, P 14: Regarding how the H15 correction changed the spectral ratio. In H15, it is shown that the implemented transducer shadowing model has a stronger effect on w than on u. This is consistent with the difference of the results presented in Table 1 and 2. Hence, since the H15 correction is frequency independent, the correction should lead to that the spectral ratio in the inertial sublayer is increased, but here it is decreased. Again, a very strange result, please double check.

Due to the abovementioned bug in the averaging algorithm for the H15 (co-)spectra, the spectral ratios were recalculated for the revised version of the manuscript. They are now in agreement with expected behavior:

"It is interesting to note that after the application of the H15 correction, which is supposed to correct for flow distortion effects, the spectral ratio indeed agrees better with the theoretical value of 4/3 and with the PTB lidar values than without the correction (Table 3)."

3. When introducing a new instrument for measurement of 3D turbulence, measurements of all velocity components should be presented. Hence, the authors need to at least also show also comparisons of sig_v, sig_u and tilt angle. The latter should preferably be shown as a function of wind direction, since mismatches can result from imperfect alignment of the instruments to true vertical. Such misalignment will result in a cosine dependence of observed tilt angle to wind direction. These plots, if showing perfect agreement, can be put in an Appendix.

We welcome this suggestion by the reviewer, and present the comparison plots for sigma_v and sigma_u, and the tilt angles as a function of wind direction in the Appendix of the revised version.
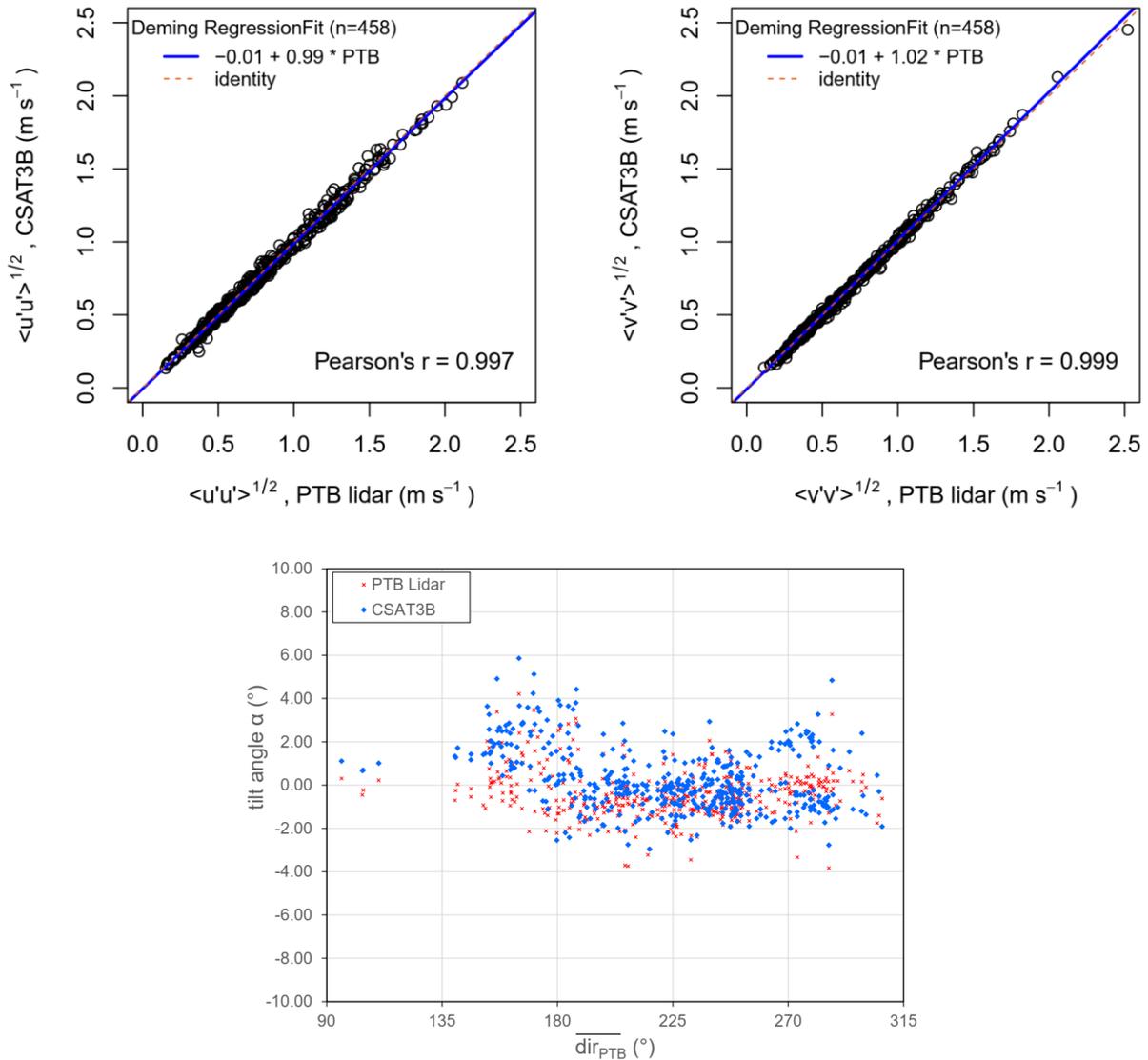
4. It is truly surprising - and point to near-fantastic signal quality - that it is possible to measure the horizontal wind speed from a near vertical path. In the direction of the wind vector, the half - angle to the emitter from the receiver in the presented setup is 0.95 deg for the receiver parallel to the wind direction and less than 0.95 deg. for the two other receivers. This means that the observed Doppler shifts are very close to zero and observations near-zero have been documented to be difficult to measure accurately using Doppler lidars (Abari et al. 2015). Further, any noise mistaken for a true Doppler change should introduce a strong signal, since it results from $U = V/sin(theta/2)$, where V is the velocity along the direction defined by the receivers and theta is the angle between the emitter and receiver. At 100 m height, as sketched in Fig. 1, the angle between the emitter and the receivers would be 0.5729 deg. When measuring a 10 m/s wind speed at 100 m height, the velocity recorded by a receiver will be lower than 5 cm/s. For 100 m, a receiver misalignment of as little as 0.01 deg. will in the system lead to a systematic error of about 1.5% in the mean wind speed estimation. What is the limitation to measurement height/measured wind speed range, in the given setup? – please explain! And how is it possible that the horizontal velocities show such little effect of noise?

Due to the bistatic design, with no optical component reflecting light with zero Doppler frequency from the transmitter to the receivers, there is absolutely no noticeable noise increase near the carrier frequency and therefore no problem measuring low or zero Doppler shift. For sure, as in every

coherent Doppler lidar, particle signals have to be distinguished from random noise peaks. To achieve this, about 20 single spectra per receiver were sampled for each 10 Hz velocity data point and the small bandwidth of true particle signals, coming from the small measuring volume, helps to detect outliers.

It is correct that the receivers have to be precisely aligned. As they are focused to the measuring volume, an alignment error as small as 0.0034 deg in azimuth will cause the receiver to completely miss the transmitting beam while the same error in elevation will cause a measurement height offset equal to the length of the measurement volume. In order to precisely control the elevation, the optical path length is observed by some modulation of the transmitted light and a time-of-flight measurement. The air condition of the system reduces temperature induced variations of the optics alignment, allowing the measured path length to be averaged over minutes. So even at measurement heights above 100 m, with less SNR and an increased height offset to angle misalignment ratio, the uncertainty of the measurement height is in the range of a few centimeters.

We added a few words about the optics control to the revised manuscript:

*"The receivers are positioned at a radius of 1 m around the transmitter to ensure both sufficient particle-scattering light intensity (quasi-backward direction) and sufficient resolution for the determination of the horizontal velocity component. Each of the three heterodyne receivers converts the particle scattering light of its respective receiving beam into an optical beat signal, which is then converted into an electrical signal by a differential photodetector. The measurement volume calculated according to Gaussian beam optics has a diameter of 2 mm and a length of 50 mm for a measurement height of 30 m above ground. <u>A time-of-flight measurement of the overall optical path length is used to actively control the receiver optics in order to maintain the measurement volume at the desired, well known height.</u> To ensure a mobile operation with stable working conditions in the field, especially with respect to requirements on the mechanical setup and the optoelectronics, the bistatic lidar system has been enclosed in a temperature-controlled housing unit mounted on a trailer (**Fehler! Verweisquelle konnte nicht gefunden werden.**). …"*

5. Data treatment: Before presentation, the data is post-processed in several steps and it is unclear what these steps do to the investigated signals. Please state/answer:

a- how many spikes were removed in each instrument. In case there are many spikes in either of the instruments, please explain/discuss the reason for these spikes. How many removed spikes were maximum allowed in a 10 min. run?

We used 30 min averaging time, as it is usually done for eddy-covariance measurements, and we allowed a maximum number of 10% missing values, including those rejected by the spike test. This has been clarified in the revised version, and we also provide information about the number of spikes detected:

"After this preparation of the raw data, we discarded any 30-min statistics if more than 10 % of the high-frequency data were missing, including those data rejected by the spike test. These are the standard settings for eddy-covariance measurements (Mauder et al. 2013, Fratini and Mauder 2014). For the CSAT3B, no spikes at all were detected for 92 % of the 618 30-min intervals, and for the PTB lidar, 73 % of the 618 30-min intervals were spike-free. This means, application of the spike detection algorithm is important to ensure high data quality, but its impact on the comparison is limited."

b- spectral treatment. The correction by Moore (1986) only corrects for path averaging on the vertical velocity component. In 2006, Horst and Oncley published exact algorithms for compensating

for path-averaging for the CSAT3 geometry for all velocity components. For the spectral ratios in focus, the Horst and Oncley correction reduces the ratio (see Pena et al. Table 2) and it is therefore highly relevant for this study to implement the correct path – averaging correction. Please change/remove the Moore correction step.

The Moore correction was not applied on the presented spectra but only on the standard deviations and u*. Therefore, it did not affect the calculation of the spectral ratio for this study. Moreover, the implementation of the Moore correction in the TK3 software is applied to all three velocity components and the effect on the resulting standard deviations is very small as a result of the large ratio between the measurement height and the path lengths of the two instruments (>250). Pena et al. also only applied the Horst and Oncley correction to the CSAT3 data for the lower measurement height (6.4 m) and not to ones with a larger measurement height (76 m)

c- "After this preparation of the raw data, we discarded any 3-min statistics if more than 10 % of the highfrequency data were missing."How many samples were removed using this step? I assume that most of the removed samples stem from the lidar?

Please excuse the typo here, it should read 30-min statistics as in the rest of the manuscript. We report the requested information in the revised manuscript:

"As a result of the data preparation described above, 615 30-min intervals remained for the CSAT3B and 458 for the PTB lidar."

d- It is unclear how the model spectra were used (fits to Højstrup or Moore)? Are variances and co-variances calculated from fits to model spectra?

These model spectra were used to determine the cut-off frequency of the PTB lidar empirically. This has been clarified in the revised manuscript:

"The model spectra were calculated for each 30-minute interval and then averaged to one ensemble spectrum in order to determine the cut-off frequency. Please note, that this does not apply to the ensemble spectra presented to determine the spectral ratios. These are purely based on measured spectra."

e- Oertel et al. showed poorer agreement with reference observations for low wind speeds, but here very low wind speeds are included. What is the explanation for this improvement according to the authors?

In Oertel et al. we showed a direct comparison of 1 Hz velocity data measured with two instruments about three meters apart. Even at 135 m over flat terrain, this comparison obviously leads to systematic errors in the extreme values of the observed wind velocities: every time a local (temporal and spatial) minimum approaches the reference instrument, the device under test measures a higher velocity – independent of which of both instruments is the reference or the DUT and even with ideal, identical instruments. Thus, as this is an artefact of that way of a direct velocity comparison, we better analyze averages and turbulence statistics instead.

f- The two instruments are compared in the instrument specific coordinate systems resulting from rotations V = W = 0; hence the lidar and sonic coordinate systems could be slightly different. This is from a technical perspective reasonable; it is hard to measure the exact position, yaw and tilt of each instrument. The lidar should however be easy to level, such that its w observations should indeed represent true vertical. Was leveling attempted? How different do the authors estimate the two coordinate systems to be? The latter question could be answered by plotting tilt angle versus wind direction for the two systems.

The lidar system was indeed levelled, and we mostly decided to apply the double rotation method because such exact levelling is almost impossible for the sonic anemometer (Wilczak et al. 2001). Nevertheless, we applied the method also to the lidar data, because we wanted to treat both data streams in the same way. The requested plot of the tilt angle is now included in the Appendix, see also response to comment 3.

6. LL 24-27: Unlike previous sonic anemometers with orthogonal sonic paths, where the horizontal velocity components are measured from a pair of axes located in the 25 horizontal plane and the vertical velocity is measured by a single vertical pair of transducers, the flow-distortion effects in the CSAT3B are minimized by positioning all six transducers and their supporting structures out of the horizontal plane This is an oversimplified statement. All sonic anemometer designs suffer from flow distortion and the extent is highly dependent on the wind direction and attack angle relative to horizontal. The arrangement of transducers in the CSAT3B sonic is to my knowledge designed for a low flow distortion effect on the horizontal wind components and a minimized effect of white noise on the vertical velocity component. In a flux measurement, the greatest contribution of the measured flux comes from incidents of high angle of attack on the instrument (Gash and Dolman, 2003), and for high angle-of attacks, the effects of transducer shadowing increases in the CSAT3.

We agree that no sonic anemometer is free of flow distortion and we did not intent to give this impression. Nevertheless, we believe it is fair to state that the CSAT3 design reduces flow distortion to a large extend, e.g. by having a very large ratio between the path length and the transducer diameter compared to other commercially available instruments. Comparatively to other anemometers with 45 degrees tilt angels the smaller tilt angle of the CSAT3 further reduces the transducers' wake effects. To address the reviewers concern, we modified the corresponding paragraph in the revised version:

"In comparison to previous sonic anemometers with orthogonal sonic paths, where the horizontal velocity components are measured from a pair of axes located in the horizontal plane and the vertical velocity is measured by a single vertical pair of transducers, the flow-distortion effects in the CSAT3B are reduced by positioning all six transducers and their supporting structures out of the horizontal plane. This is important because horizontal wind velocities are usually much larger than vertical wind velocities, and a distorted measurement of the horizontal wind speed directly affects the vertical wind speed measurement. In this non-orthogonal arrangement, each sonic path is tilted 30° from the vertical axis and spaced 120° apart in the horizontal plane. The length of the sonic path is 0.1154 m and the diameter of the ultrasonic transducers is 0.00635 m, giving a path length to diameter ratio of 18, which is larger than other commercially available instruments (Mauder and Zeeman, 2018). The higher this ratio and the steeper the angle between the sonic path and the vertical axis the less self-shadowing effects are expected on the wind measurement, because a smaller portion of the path is affected by the transducer wake (Kaimal, 1979; Wyngaard and Zhang, 1985)."

7. Fig. 6: This is an interesting result, possibly indicating that the flow accelerates through the measurements volume of the CSAT3, leading to an overestimation of the horizontal wind speed. H15 hypothesized that transducer shadowing was the major cause of flow distortion in the CSAT3, which can only lead to an underestimation of the velocity, so here we are potentially looking at another major cause of flow distortion. But the presented result also leads to more questions: what kind of fluid dynamics process describes a wind acceleration without affecting all the velocity components? In other words, can an instrument that measures the vertical component perfectly (as it is claimed in this study) measure the horizontal velocity imperfectly? There could be several reasons behind the mismatch in the results; small inaccuracies in the lidar measurement height or a larger focus volume than anticipated, effects from post-processing of the data, or other inaccuracies in the optics of the lidar.

This could be explained by the horizontally symmetrical design of the CSAT3 structure as recommended by Wyngaard and Zhang (1985). In the original manuscript, we refer to the studies of Grare et al. and Huq et al. which also found a wind-direction dependent error or u_bar for the CSAT3. Moreover, Horst et al. (2016) (Boundary-Layer Meteorol DOI 10.1007/s10546-015-0123-8) observed similar behavior when they measured the flow distortion within the IRGASON integrated sonic anemometer and CO_2/H_2O gas analyzer. They found good agreement for w, but not for U and u*. The other potential reasons for this behavior mentioned by the reviewer had already been ruled out beforehand, and they are however not discussed in the original manuscript. For example, we created the same plot based on planar-fit transformed data (instead of double rotation) and the result was quite similar. For clarification, we have modified this paragraph accordingly in the revised version:

"This could be explained by the horizontally symmetrical design of the CSAT3 structure as recommended by Wyngaard and Zhang (1985). A very similar wind-direction dependence of the error in $\bar{u}$ has also been reported by Grare et al. (2016), when comparing a CSAT3 sonic anemometer against a Gill R3-50 sonic anemometer. Moreover, Horst et al. (2016) observed similar behavior when they measured the flow distortion within the IRGASON integrated sonic anemometer and CO_2/H_2O gas analyzer. They found good agreement for w, but not for $\bar{u}$ and u_*."

8. P. 15, LL 27-30, "In our case, the H15 correction even results in improved $uu*$ values, but the ensemble cospectrum shows that this improvement occurred for the wrong reasons. In consequence, the observed behaviour of this correction for $uu*$ may very well be site-specific and not universally transferable. Moreover, as stated by Wyngaard (1981), such corrections are problematic because they violate conservation of vorticity and can therefore not generally be recommended.". As stated above, I doubt that the very small change in the inertial subrange will lead to a significant contribution of the u*. Moreover, the citation to Wyngaard is very strange. The mentioned tilt correction in Wyngaard (1981) has to do with the double rotation (which is used in this study) to V = W = 0. In my understanding, Wyngaard in 1981 stated that flow distortion effects cannot be avoided regardless of coordinate system. Please point to the exact place in the paper, where Wyngaard stated that flow distortion correction cannot be safely applied or remove the citation.

As mentioned above, there was a bug in the averaging of the H15 (co-)spectra. Therefore, this paragraph is modified in the revised version, see response to comment 1. We agree that Wyngaard (1981) referred to the double rotation method when criticizing tilt corrections, and we removed this reference to Wyngaard (1981) in the revised version.

9. Concerning isotropy and spectral treatment. The authors should apply stricter criteria to ensure isotropy. It is not enough to select an interval where the premultiplied spectra are flat. In Pena et al (2019), a stricter selection is suggested (for example, co-spectral density should be close to zero). Moreover, it is more correct to ensemble average the spectra based on wave number instead of frequency, since the spectral content changes as a function of wind speed. The cited Stipersky and Calaf (2018) does not dispute that the spectra in the inertial subrange shows isotropy, but they show that this cannot be assumed for all spectra. Hence, their result is well aligned with the methods chosen in Pena et al. (2019), but not well aligned with the method for calculating spectral ratios in this paper.

We agree that it is more correct to ensemble wavenumber spectra than frequency spectra, when one is interested in characterizing turbulence. However, the main aim of this study is characterizing the instruments and therefore we used frequency spectra in this manuscript, e.g. also to determine the cut-off frequency empirically. To test the sensitivity of the results on this choice, we calculated ensemble averaged wavenumber spectra for the same range as Pena et al., Fig.3, 0.6 m-1 < k < 2 m-1.

The resulting spectral ratios are as follows. (those for the frequency spectra are in brackets for comparison):

PTB: $kSv/kSu = 1.30$ (1.30) and $kSw/kSu = 1.20$ (1.20)

CSAT3B: $kSv/kSu = 1.26$ (1.26) and $kSw/kSu = 1.14$ (1.16).

CSAT3B + H15: $kSv/kSu = 1.30$ (1.29) and $kSw/kSu = 1.23$ (1.23).

Four of these ratios are identical and two of them are slightly different. This shows that the results presented in the manuscript indeed depend on thresholds that are somewhat arbitrary. Please note that also the "sharpened criteria" of Pena et al. (2019) have somewhat arbitrary thresholds; they are just differently defined using additional criteria. Nevertheless, we find that the resulting ratio is sufficiently robust, which also shows that the inertial subrange is well-represented in the data, so that the conclusions drawn from these results are not affected, i.e. whether or not a certain ratio is close to the theoretical value 1.33 or not.

We cited Stiperski and Calaf (2018) to support our statement that spectra in the inertial subrange may not always be isotropic. If these partially anisotropic spectra are averaged, the resulting ensemble spectra must also deviate from isotropic behavior, and this is what we see in these results. For clarification, we rephrase this statement more precisely in the revised version:

"Hence, we suspect that this theoretical value was probably not fulfilled in reality for the ensemble spectrum, presumably because the turbulence was not quite isotropic under all atmospheric conditions during the measurement period, which can happen due to different reasons (Brugger et al. 2018; Stiperski and Calaf 2018)."

Minor comments:

• L. 14, P. 6: Regarding synchronization: It is of no importance that both instruments were sampled in UTC, what matters in a 1:1 comparison is that the signals are simultaneous. Were the two instruments logged on the same data acquisition system or were both systems synchronized to GPS time? Or was synchronization attempted via the measured time series?

The synchronization of both time series was ensured by synchronizing both data acquisition systems with a time server via internet. We clarified this in the revised version:

"Measurement times were logged in UTC, and data acquisition systems were synchronized with a time server via internet."

• Since the path-averaging is compensated for (at least for the vertical component), why do the authors expect low-pass filtering? And how should a time constant be interpreted in relation to the source of the low-pass filtering effect (path length!)?

Any path averaging will lead to a low-pass filtering effect. It is correct, path-averaging was compensated for. However, the corresponding correction is based on analytical transfer functions and the assumption that the path-length is correctly defined. This exercise of determining the cut-off frequency empirically was conducted to assess the sensitivity to these assumptions.

• Boom length and mast diameter?

This information has been provided in the revised version:

"Its measuring volume was 0.85 m from the center of the mast; the mast's diameter at mounting height was 0.05 m."

• Please merge Table 1 and 2, for an easier overview of the results.

Both tables are merged in the revised version.

• I am surprised that the CSAT spectra show no sign of noise in the high frequencies. Is it because we are looking at the ensemble model spectra rather than the observed spectra? Please show the original spectra.

These are ensemble averages of observed spectra, no model spectra are fitted here.

• LL 18-19: Perhaps the numerical simulations were not turbulent enough, so that wake effects are exaggerated, as it has also been found for wind-tunnel calibrations (Högström and Smedman, 2004). It is not shown in Hogstrom and Smedman (2004) that wake effects are exaggerated in the wind tunnel. This is a speculation on the side of the authors. The early Gill sonics were not as good as the sonics of today. For example, three consecutive runs in a wind tunnel showed considerable scatter for the same sonic anemometer (Fig. 3 in Mortensen and Hojstrup, 1994), possibly due to changes in temperature. This could also have been a reason for why the Hogstrom and Smedman (2004) observations differed in wind tunnel and atmosphere. In any case, please correct the citation to be more precise.

Here is what is stated in the abstract of Högström and Smedman (2004): "It is concluded that the correction for the effect of the vertical supporting rods of the R2 and R3 instruments, which gives nearly perfect agreement for laminar flow, does not work entirely satisfactory in the natural turbulent flow. This, in turn, is likely to be so because of high sensitivity of the wake behind the cylindrical supporting rods to the character of the approach flow."

This is in line with our statement in the original manuscript. Let us explain this reasoning further: It is well known from fluid dynamics that wake effects severely depend on the Reynolds number Re. As long as the flow is laminar, an increase in Re initially causes a growth in the size of the wake, and after transition to turbulence, a sudden reduction occurs. Wind tunnel studies are generally conducted under laminar conditions because Re is very much limited by the diameter of the wind tunnel. This means that wind-tunnel corrections are determined under conditions before this sudden reduction in wake extent at the transition from laminar to turbulent flow. Hence, they are not per se transferable to real-world turbulence, where wake effects are likely to be smaller. We explained this in the revised manuscript:

"Perhaps the numerical simulations were not turbulent enough, so that wake effects are stronger than under fully-developed turbulent conditions in the field. Generally, wake effects depend on the Reynolds number and the wake extent is reduced suddenly at the transition from laminar to turbulent flow (e.g. Williamson 1996). This is also the reason why it is problematic to transfer quasi-laminar wind-tunnel calibrations to real-world turbulence (Högström and Smedman 2004)."

• Last sentence in Abstract: We also found that an angle-of-attack dependent 25 transducer-shadowing correction does not improve this agreement effectivel because it leads to an artificial correlation between the three wind components and therefore severely distorts the shape of the cospectra. Only two velocity components are shown.

We removed this sentence in the revised version.

• LL 32, p 4: Please provide a reference for proof of the statement that the lidar measures the back scatter from each single aerosol (and not all aerosols in the measurement volume).

Thanks a lot for this comment, that sentence is really not precise. We actually see single particle signals at very low measurement heights, meaning with a very small measurement volume, with dimensions comparable to an LDV. In most conditions, with a larger measurement volume, the lidar receives signals from a lot of particles in the measurement volume. Anyway, the important contrast to monostatic systems is the fact that all three receiving optics receive light scattered from <u>the same</u> particles, so we changed that sentence:

*"The basic idea of this system relies on utilizing a bistatic measurement setup (Harris et al., 2001), i.e. on the use of one transmitting laser beam and three detection beams (spatial separation), in order to determine <u>all</u> three components of the wind vector simultaneously in a small measurement volume by means of <u>the same</u> aerosols (**Fehler! Verweisquelle konnte nicht gefunden werden.**)."*

REFERENCES:

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