Final author response

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We are grateful for the very valuable and constructive comments by the reviewer. The comments are in black font, responses are presented in blue font and changes in the manuscript in red font.

Response to RC2 (anonymous)

General Comments "As the last comprehensive intercomparison experiments were conducted more than 10 years ago, …" is the motivation for the authors do carry out a new intercomparison for prevailing sonic anemometers. They present the analysis and the results in a well-prepared manuscript in a straight and standard way. The sonic-user community will be eager to see how the different instrument types perform. Insofar it is worthwhile to publish their results and it is perfectly within the scope of AMT. There are however some points the authors should address.

The explanation/discussion of the much better agreement is not convincing. I don't understand what the consistent digital data acquisition has to do with the better agreement. Give an example. And demonstrate how your quality tests improve the agreement.

The quality tests of Mauder et al. (2013) are the basis for the quality control applied for this intercomparison. The effect of these tests is presented in that paper as well, also for the same site. In addition, we applied a filter for rain and obstructed wind directions. Our wind sector filtering implies the exclusion of all obstructions, including the tripod and the neighboring systems at a distance of 9 m. In addition, the filtering for rainy periods was critical to exclude implausible measurements. In the discussion section of revised version, we have provided some examples of the comparison statistics for the dataset after only the tests of Mauder et al. (2013) had been applied and also for the dataset after exclusion of obstructed wind sectors before the filtering for rainy periods was applied.

On top of that, the filtering for obstructed wind direction sectors and for rain, as described in section 2.2, was crucial to remove poor quality data. Both additional steps improved the agreement between instruments considerably. For σ_w, regression slopes ranged between 1.00 and 1.24 and intercepts were between −0.05 and 0.00 m s^{-1} after processing according to Mauder et al. (2013). After filtering for obstructed wind direction, slopes ranged between 0.98 and 1.22 and intercepts remained between −0.05 and 0.00 m s^{-1}. As can be seen from the results (**Fehler! Verweisquelle konnte nicht gefunden werden.**), the overall agreement further improved after the filtering for rainy periods. Especially, some outliers of the CSAT3_2, which did not have the rain-guard meshes at the transducer heads, were rejected after this step. The effect of the data filtering on other quantities, such as Hs, was smaller. Here, the slopes ranged already only between 0.97 and 1.00 after processing according to Mauder et al. (2013), which did not change much further after filtering for obstructed wind directions and for rainy periods (**Fehler! Verweisquelle konnte nicht gefunden werden.**). This can be explained by the fact that the scheme of Mauder et al. (2013) is designed for quality control of fluxes and not necessarily standard deviations. It therefore much stricter for H_s than for σ_w.

You also indicate that contributions in changes of firmware might have an influence. Say more about that. And finally you say that five instruments apply “some sort” of correction. There is more information about the corrections, they should not be treated as blackbox. Add the information where it is existiong. E.g. the calibration files for R3 and HS are available and can be applied later (at least it used to be like that). One has the possibility to sample uncalibrated data and apply the calibration afterwards. Did you do that? The HS and the CSAT3: how would they compare then? The sonics from Young and Metek seem to be black boxes but they allow to switch on and off a wake or head correction. You probably used the sonics always with the corrections on. Any idea how strong the corrections are? It irritates me that an instrument like the Young 81000 with a magic wake correction is so close to the other instruments. Insofar I can understand that you find the good agreement “somewhat surprising” and I can follow your conclusion that this is rather a conservative estimate because of special conditions (small variation in angles-of-attac. I guess you investigated the differences on azimuthal dependencies).

It is possible to operate the Gill instruments in an uncalibrated mode, but our intention was to compare the anemometers in the configuration recommended by the manufacturer. We do not have information about a calibration of the Gill instruments that can be applied during post-processing. We also used the METEK with the head correction turned on. As far as we know, the CSAT3 is the only one of the tested instruments that does not apply any internal correction at the sensor level. We do not have all details about the corrections applied by the different manufacturers. Therefore, the scope of this comparison was limited to a characterization of a typical configuration as it is applied by most users. We have added text providing details about the settings and firmware versions.

All other settings were left at the factory-recommended values, including flow-distortion corrections. The differences due to different firmware versions are quite well documented for the CSAT3. Accordingly to Burns et al. (2012), discrepancies between firmware versions 3 and 4 occur mostly for the sonic temperature measurement and they become significant for wind speeds larger than 8 m s^{-1}. During our field campaign, wind speeds were mostly lower than 5 m s^{-1} (Figure 4). Therefore, we do not expect large errors. Nevertheless, we used the same firmware version (ver4) for both CSAT3.

We as authors share the reviewer’s irritation about missing or incomplete information on the sensor-based corrections in the respective manuals or firmware documents by all manufacturers. Campbell Scientific certainly has the more transparent policies with respect to the internal processing routines.

The angle-of-attack figure disappeared. It was surprising to see that the deviations from horizontal were that small (mostly within ±6°). Now there is a standard deviation of 15°. What happened?

Based on the reviewer’s comments in the Quick Review, we have double-checked our results and we found that there was a mistake in the calculation of the angles of attack. The spread in angles of attack during our experiment was actually much larger as previously thought, i.e. a standard deviation of 15° rather than ±6°. This standard deviation of 15° is at the upper end of values reported for previous intercomparison experiments. Therefore, we have also changed the line of arguments in the discussion in accordance with the new results. We now really believe that the different instruments all show the same biases despite their differences in geometry and internal corrections. Since there is strong evidence for a bias in σ_w of the CSAT3 from other studies (Horst et al. 2015, Frank et al. 2016, Huq et al. 2017), this seems to be the only logical conclusion.

I do not see the advantage of using the PCA load in the first place, for deciding on an etalon. Choosing rather one instrument for all comparison is much more stringent and makes it easier to compare the instruments.

We wanted to avoid any subjectivity in choosing the reference instrument. If we had chosen the CSAT3 as etalon, as was done for previous intercomparsions, the presentation of the results would have been biased, perhaps favoring the CSAT3. Moreover, such a priori decision would contradict one of our main conclusions, that they all are equally suitable for flux measurements. Therefore, we would like to stick with the PCA-based decision on the etalon, because it allows us to compare the all results with the "best" estimate of a certain quantity.

Comparison plots are a bit monotone and do not transmit much information. Plotting rather differences to reference than sonic value versus sonic value gives an immediate impression on statistics. For a direct connection to the scatter plots the regression results should be placed in the plots. Special features can be highlighted in the text.

Thanks for this suggestion. We agree that there are other ways to present the results of such an intercomparison. However, we followed the style of previous studies, which have also used similar scatter plots (Dyer et al., 1982; Fratini and Mauder, 2014; Mauder et al., 2007; Tsvang et al., 1985) in order to maintain comparability. The monotone nature of the plots in our study, in comparison to plots in previous studies with much more scatter, is an actual result.

Comment on the speed-related temperature of a CSAT3 (Firmware v3)? You don't mention whether you determined the zero offset of the two CSAT3 before the experiment. Did you? The serial numbers of the CSAT3s tell us that they are relatively old instruments. How long ago was their last calibration? Figure 5: why the CSAT3 deviate that much although they should be better comparable. Could it be related to zero offsets or old calibration?

The older of the two CSAT3 (SN 0771) had been sent for re-calibration to the manufacturer in 2014. This one has firmware version 4t. The other one (SN 1791) still has its original manufacturer calibration; it was purchased in 2009. It has firmware version 4. We did not determine the zero offset before the experiment.

Technical corrections

Abstract 2/16 (Wieser et al., 2001). Full stop

Thanks, full stop has been added.

3/33 indications of a

This has been corrected.

4/31 synchronized how? Please be more specific how this was done? Why it is that important if you compare just average quantities?

Here, we wanted to stress the importance of digital data acquisition with precise clocks in order to attribute the correct time stamp to each data line. Therefore, more information on these details are provided in the revised version:

Data from all instruments were digitally recorded on synchronized single-board computers (BeagleBone Black, BeagleBoard.org Foundation, Oakland Twp, MI, USA), equipped with temperature-compensated clocks (Chronodot, Macetech LLC, Vancouver, WA, USA), using an event-driven protocol for recording data lines, implemented in the Python programming language. The digital recording minimizes the influence of data cable properties on signal quality and minimizes the impact of loss of resolution by conversion between analog and digital signals outside the scope of the sensor. Issues stemming from cable properties usually have a more apparent effect on digital than on analog signal transmissions. In case of a signal deterioration by oxidation of contacts or loosening cable connections, digitally transmitted data lines will start to show up in a corrupted format, while loss of signal resolution in analog transmission may go unnoticed for some time. Therefore, the potential for added uncertainty to the observations recorded by analog data transmission can in part be avoided by digital communications.

5/5 DE-Fen ? 6/5 It looks shaky. Were there guy wires?

The tripods did not have guy wires. Nevertheless, they are more stable than they might look like on the photograph because the legs are partially hidden within the grass canopy. We also checked the spectra of the wind velocities and found no visible deviations from the typical inertial sub-range behavior, which might indicate vibrations of the masts at distinct frequencies.

7/2 All data were processed

This has been corrected.

7/15 DE-Fen

The missing dash has been added.

8/6 total wind velocity? You mean the magnitude of the 3d vector i.e. =(u2 +v2 +w2) 0.5 compared to the horizontal wind speed =(u2 +v2) 0.5, which is your mean wind speed?

Since we have applied double rotation, the mean vertical wind velocity is zero and the mean cross-wind velocity is also zero. For clarification, we have now consistently used the term “mean total wind velocity”.

12/5 was chosen as etalon for

This has been corrected.

12/15 CSAT3 is very good except for

This has been corrected.

14/5 Young.81000RE

This has been corrected.

15/5 The lower row is slightly too large so the y-axis is missing

Thanks, reviewer 1 has also noticed this, and we have recompiled the figure.

16/3 of this study for many

This has been corrected.

16/7 etalon for this quantity because (is a redundant, or omit “For this comparison”)

This has been corrected.

17/7 error of due

This has been corrected.

17/6 measurements systems (?)

This has been corrected.

20/2 243-251 instead of 363-372

This has been corrected

21/6 Frank and Massman listed twice

Thanks for the careful check of the reference list. This mistake has been corrected.