Interactive comment on “Using sonic anemometer temperature to measure sensible heat flux in strong winds” *by* S. P. Burns et al.

Reply to Reviewer 3

S. P. Burns et al.

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The thoughtful comments by Reviewer 3 are greatly appreciated. Our replies to the Reviewer 3 comments are below.

“General Comments”: *The authors report an interesting phenomenon concerning the Campbell CSAT3 sonic anemometer. During high wind situations sensible heat fluxes based on sonic temperature H_Ts are much higher than compared to H_Tc, which are based on thermocouple temperatures. The deviations start at 8 m s-1 and reach values of around 250 W m-2 at 18 m s-1. The authors demonstrate that in a well prepared manuscript. It is however not clear to me whether it is a peculiarity of the special set-up at the extreme site (vibrations or snow drift as other reviewer suggested) or whether it is a general deficiency of the CSATs, which would be alarming. Some additional work is needed to answer this question.*

Reply to “General Comments”: Additional independent experiments with the CSAT in a wind tunnel and by Campbell Scientific have been performed. These tests have added additional insight into the problem. The problem is related to an overestimation of the sonic path transit times. We feel that we have now clarified the source of the issue in the CSAT3 and provided an empirical correction for the sensible heat flux that is consistent with our conceptual model of the error (please see sections 3.4 and 3.5 in the revised manuscript).

“Specific Comments”: *Starting with the conclusions, the statement from Campbell Scientific, as reported in L243 to 248, is somewhat unsatisfying. Any hint from Campbell what could be the cause? Although an important finding it would be unneeded to initiate speculations and then it is a software problem which is not communicated. More information on what are the manufacturer’s comments is certainly necessary.*

Reply to “Specific Comments”: Larry Jacobsen from Campbell Scientific is now included as a co-author, and CSI has agreed that we should include more details from their independent experiments (see sections 3.4 and 3.5 in the revised manuscript).

Comment 1: *1. Positive offset depending on wind speed What you call "more positive" is a positive offset which adds to H_Ts both during day- and nighttime. During high wind speed situations nocturnal downward (negative) fluxes can change sign and are measured as upward fluxes, while daytime are measured too high. This means that the "artificial" contribution comes from* *the +w+T and -w-T quadrants, i.e. a positive correlation is introduced. As the w-component is claimed not to be the cause of the discrepancies how can high wind speeds cause the positively correlated overestimations of Ts fluctuations? I suggest to look at the w'Ts' distribution of half hourly runs of extreme cases which might help to better understand the nature of the error. (other reviewer's comment: I don't see how snow drift would introduce a positive correlation during night-time. I would rather expect a contribution to all quadrants and thus and increase or decrease of the up- or downward fluxes)*

Reply to Comment 1: We have shown an empirical correction to the CSAT "off-axis signal shape error" in section 3.5 of the revised manuscript that should satisfy the questions raised in this comment.

Comment 2: *2. Spikes*
The statement in the caption of Tab. 1 "but for higher winds around 2-4% of the samples were flagged" should be clarified. More information is needed. Were all CSAT3 affected by spikes? Was there a correlation to wind speed, higher wind speed - more spikes? (In the meantime I saw the answer of the first author to Laubach's comment. Well, it's quite a spiky record)

Reply to Comment 2: We have included a short discussion of the spikes in section 2.3 of the revised manuscript. The number of spikes detected are affected by both wind speed and humidity. Please see our reply to Thomas Foken on the AMTD webpage for more information about the spikes.

Comment 3: *3. Wind direction*
How is the wind direction during high wind speed situations? Are the CSATs all oriented in the same way? Flow distortion can affect Ts. I guess you had a look at that, but how are H_Ts-H_Tc related to wind direction for wind speed >10 ms-1?

Reply to Comment 3: This is an excellent point, and something we neglected to include in the original AMTD manuscript. High winds at the NWT site are almost exclusively from the West (i.e., downslope). The EOL and CU CSATs were oriented in the same direction (toward the SW), and we have added information about the winds and sonic orientation to section 2.3 and Table 1 of the revised manuscript.

Comment 4: *4. Energy balance*
This part is not supporting the search for the causes of the overestimation which is demonstrated by the comparison to H_Tc. Looking at nocturnal values of DOY 43 immediately shows that there is a serious problem and which estimate is likely wrong. This needs no support from very obvious EB considerations where the available energy is afflicted with uncertainties of estimated storage and net radiation measurements (see below).

Reply to Comment 4: We reduced the discussion of the EB in the revised manuscript. However, we feel that the EB section provides an independent perspective on the heat flux error. It also gives new insight into the results that were shown in Turnipseed et al (2002) so we have not eliminated the EB discussion from the manuscript.

Comment 5: *5. We have ongoing measurements in a desert environment. There we measured with three CSAT3 and had 300 cases with wind speed (30 min average) above 10 ms-1 up to*

15 ms-1. No unusual high fluxes are detected. In contrary the ratio to net radiation is rather smaller than at low wind speeds. Preliminary checks did also not show spiky records similar to the ones presented in the manuscript.

Reply to Comment 5: As we explain in the revised manuscript, the CSAT performance is related to the firmware version of the CSAT (ver3 performs better than ver4) as well as the amount of drift from a factory calibration (for ver3). In our reply to Thomas Foken, we show the number of spikes at NWT is a strong function of humidity which could explain why you experience less spikes in your desert site.

Comment 6: *L1 and L101*
The surface energy balance (SEB) is something different than the ratio of the sum of turbulent heat fluxes to the available energy. You may call it closure ratio CR. So define these terms clearly.

Reply to Comment 6: Good point. We modified the text in sections 2.3 and 3.6 to use the term closure fraction “CF” rather than ”SEB”.

Comment 7: *L19*
In brackets also downward fluxes should be included or better omit the explanation. Readers of this manuscript probably know what is meant with latent or sensible heat flux.

Reply to Comment 7: This explanation has been omitted in the revised manuscript.

Comment 8: *L29*
Desert environments are also affected by the limitations of the eddy covariance method, i.e. if there is a small average w-component (up or down) induced by larger scale influences then "advective" parts are missed. It is just that in dry conditions underestimating the latent heat flux is not that important.

Reply to Comment 8: We agree.

Comment 9: *L74*
"Equation" at begin of sentence.

Reply to Comment 9: This section has been re-written and this wording has been changed.

Comment 10: *L110*
That is strange. It is not only that the spurious correlation contributes to the covariance as you say, but additionally the sign of the flux is reversed. First I thought this is similar to what I experienced with our measurements in a desert. Often we observe upward night-time fluxes which are not plausible regarding the stable stratification. This happens, when there are strong temperature gradients and the wave-like motions causing apparent fluctuations which are not connected to mixing. The use of much smaller averaging intervals (1 to 4 min) results in reasonable fluxes, both in sign and magnitude. However this can be ruled out in your case, as H_Tc shows a reasonable behavior.

Reply to Comment 10: We show the reason for the CSAT error in the revised manuscript (see our Reply to “General Comments” above).

Comment 11: *L143*
Although it is clear what is meant I would not say "becomes more positive". A number is either positive or negative. The effect of this strange behavior is that H_Ts is increased by a certain amount depending on wind speed. The effect is similar during day- and night-time. This means, that either in the quadrants +w+T or -w-yT gain more weight. Especially during night-time this is strange and should show up as a prominent feature. Instead of looking at averages like in Fig. 3 and Fig. 4 it might be instructive to look at the distribution of w'Ts' for single runs and compare it to w'Tc'.

Reply to Comment 11: We agree with your point about the wording "becomes more positive". We have changed the wording in section 3.1 to "becomes greater than". For the rest of your comment, please see our Reply to “General Comments” above.

Comment 12: *L170*
"that enhances (Co)wTs during the day and degrades it at night". What is meant with "degrades it at night"? If it changes from -50 to +150 W m-2 then degrading is not the right description (compared to "enhances"). What is remarkable, is that H_Ts is larger. And this is only possible if...see above (L143).

Reply to Comment 12: This wording has been removed because we now have a better understanding of the error (please see our Reply to “General Comments” above for details).

Comment 13: *L175*
One cannot rule out vibration in that way. If transit times are affected, then the temperature error is much larger than the speed error. A rough estimate: a 1% error in axis speed corresponds to 1.7 K error in temperature. Transit time offset 1 μs, path length 0.15 m, speed of sound 340 m s-1.

Reply to Comment 13: We agree.

Comment 14: *L205 (net radiation)*
*A side note in this context, but is shows how unproven statements stay alive: "Also similar to Turnipseed et al. âĂę.". To my experience the Q*7.1 underestimates the magnitude of net radiation during nighttime, therefor the closure is better (see also L221-229). In Turnipseed et al. (2002) it is written: "However, further testing is advised to determine the source of this discrepancy." Has that been done? Then refer to it, otherwise it is repeating a most likely wrong statement.*

Reply to Comment 14: No additional intensive testing of the radiation at the site has been performed (though a short side-by-side comparison with a second CNR-1 has shown them to both agree well). Following your suggestion, in the revised manuscript we refer to Turnipseed et al. rather than discussing the radiation measurements in detail.

Comment 15: *L221-229*
*I recommend to omit the discussion concerning the radiation sensors. The discussion is general and not very precise and does not draw the right conclusions. It is well known that the Q*7.1 has different responsivities in the long and short-wave range. Normally they are used with only one responsivity which leads to an underestimation of the nocturnal net radiation. More information on that can be found e.g. in Halldin and Lindroth (1992, J.Atmos.Oceanic Technol., 9, 762) or in Kohsiek et al. (2007, BLM, 127,55) or in Vogt et al. (1996, Theor. Appl. Climatol., 53, 23). In the latter it is described, how the short- and long-wave responsivities can be derived if independent measurements of the net short- and long-wave balances are available. These relatively old findings were confirmed with comparisons to better reference sensors like the CG4 from Kipp and Zonen (partly done in Michel et al. 2008). But again, this part is not relevant for the problem discussed here so it can be omitted.*

Reply to Comment 15: We removed the discussion of the radiation sensors.

Comment 16: *L240*
"At night H is typically...HCSAT smaller than it should be". This is a confusing statement. Even if you say magnitude it is not true e.g. for day 43 in Fig. 1.

Reply to Comment 16: We have re-written the conclusions so this wording is no longer included.

Comment 17: *References Be consistent with using abbreviations of journals. E.g. L269 and L282 and some more*

Reply to Comment 17: These have been double-checked.

Comment 18: *Fig. 1*
Legend is above curves.

Reply to Comment 18: This is fixed.

Comment 19: *Fig. 2, 3, 4, 5*
Minor point, but what is the benefit of DOY = 271.24 - 365.99, especially .24 and .99? Many characters can be omitted without missing relevant information (Nov 2010, 67 out of 1438 30-min periods). Or in the caption: "30-min" and "30-minute" in the same caption. Where you refer to differences or fluctuations K should be used instead of degC.

Reply to Comment 19: The DOY range in these figures shows the exact time periods of the plotted data. For Fig.2 (and others) the period is not always an exact month, so these have been left as-is. But for Figures 3–6 (in the revised manuscript) the DOY range has been removed as you suggest. The "30-minute" description has been changed to "30-min". Several published spectra and cospectra use degC rather than K (for example, Kaimal and Finnigan (1994), Kaimal and Gaynor (1991), etc). However, we agree with your comment, and changed the units to K to be consistent with the SI unit for temperature.