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

Reply to Johannes Laubach

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The thoughtful and insightful comments by Johannes Laubach are greatly appreciated. Our replies to his comments are below. We also thank Don Lenschow (NCAR) for discussions regarding the coherence squared and phase (in Comment 2 below).

Comment 1: *You state that your site is at 3050 m altitude and a "windy place in winter". Could you please clarify: Were all the data presented here collected at below-freezing temperatures? Were latent heat fluxes then practically negligible? Were some of the data collected while the sonic was surrounded by cloud/fog (on a mountain ridge this is possible even in high wind conditions), and if yes, do they show differences to dry/clear periods? I think such information would be necessary context for specifying the conditions when the observed spurious w-T-correlations occur.*

Reply 1: In our revised manuscript we will attempt to add a more details that clarify the environmental conditions at the site. Here is a short summary. Data used in our manuscript were collected at both above- and below-freezing temperatures. For example, the air temperature for November 2010 (used for Figs. 3–4 in the manuscript) ranged between $-20\,^{\circ}$C and 12 °C with a mean of $-4\,^{\circ}$C and standard deviation of 7 °C. Furthermore, we considered temperature as a factor in the heat flux error (e.g., on p.454, lines 2–3, we stated: "We did not observe any temperature-dependent H differences in our study."). Data used in our paper contain whatever temperatures and/or humidities occurred during the given period; though specific conditions might affect the "spread" of the $H_{\mathrm{CSAT}} - H_{T_{tc}}$ difference (e.g., Fig. 2 in the manuscript), it will not change the overall trend. For the months of Nov-Feb, daytime sensible heat flux is typically four times greater than the latent heat flux. We evaluated the w'q' term in Eq. 3 and it did appear not large enough to explain the observed H differences (see p.455, line 28). Our high-elevation continental site is west of the continental divide and generally dry (for example, riming of the instruments is very rare and might occur once or twice a year, usually in the spring). We will present further details comparing "dry" and "humid" periods in our reply to Thomas Foken.

Comment 2: *You show (Figs 3,4) power spectra of w and T with high-frequency noise (rather extreme for T), and I agree with the conclusion that some of this noise must be correlated to produce the spurious heat flux. Perhaps showing a w-T phase spectrum can elucidate this further.*

Reply 2: This is an excellent suggestion and we have plotted the coherence squared (Coh) and phase differences between the various w and T combinations in Fig. C1 as well as the T_{tc} (10-Hz) and T combinations in Fig. C2. (The data used in these figures are from the same period as Figs. 3–4 of the manuscript.) For comparison purposes we show the high- and low-wind speed cases.

The following observations are made from Fig. C1:

- The w, T Coh reaches a maximum of around 0.3 for most conditions and all w, T combinations, *except* for high winds at night when there is a dramatic loss of Coh between w and T_s which only reaches a maximum of $Coh = 0.1$. This is indicitive of the decorrelation between w and T_s that occurs at night with high winds (as mentioned by J. Laubach in a later AMTD post).
- As one would expect, w, Ttc (1-Hz) Coh drops off at a lower frequency than the other temperature sensors (also recall that the 1-Hz thermocouple is horizontally displaced from the CSAT3 by 1.3 m).
- For turbulent scales, the phase between w and T should be 180 deg at night and 0 deg during the day (e.g., Stull, 1998). In general, we find the w, T phase follows this pattern, except at high winds at night where there is an apparent shift in the phase angle between w and T_s toward 90 deg.

The following observations are made from Fig. C2:

- For high-wind conditions, Coh for $f < 0.3$ Hz between the 10-Hz and 1-Hz thermocouples is larger than 0.8 and greater than the coherence squared between T_{tc} (10-Hz) and T_s. In contrast, for low winds, T_{tc} (10-Hz), T_s Coh is generally greater than Coh between the two thermocouples (presumably because of the horizontal separation between the 1-Hz and 10-Hz thermocouples).
- The phase between T_{tc} (10-Hz) and T_s is affected by the slower time response of the thermocouple (which is why the T_{tc} (10-Hz), T_s phase angle is negative (e.g., T_{tc} 10-Hz is lagging T_s).
- We should also note that the T_{tc}, T_s phase analysis revealed that we had neglected to take into account the two sample "data pipeline delay" in the CSAT3 output. We have now corrected the CSAT data for this 0.2 second delay. This correction did not create any significant change to the figures or conclusions in the manuscript.

These coherence/phase results emphasize how challenged the CSAT3 is to measure temperature at night with high winds. As has been mentioned by several reviewers (including comment 6 below and in the more recent post by Johannes Laubach), the noise in T_s is overwhelming the true temperature signal at night with high winds. Here we note that the peak of the T power spectra in nighttime windy conditions (e.g., Fig. 3b in the manuscript) is smaller than 10^{-2}. For all other conditions, the peak in the T spectrum is at or above 10^{-2}. The dramatic drop in Coh for high winds at night for any Coh pair that involves T_s (e.g., as shown in Figs. C1 and C2) corroborate with the conclusion that T_s is problematic.

The phase analysis also revealed that the sensible heat flux calculated with T_{tc} (10-Hz) has some potential drawbacks due to the thermal time lag and radiation effects on the thermocouple wire (this was also alluded to by Thomas Foken). We note that the radiation effect should be small or minimal because w will not be influenced by radiation as mentioned in a later comment by J. Laubach. For a thermal lag that follows a first-order system, the phase should approach $-\pi/2$ at higher frequencies (Tagawa, et al. 2003). We do not quite observe this in Fig. C2, which might be related to the fact that T_{tc} is being compared to an imperfect T_s. We will include a discussion of these issues in the revised manuscript.

Comment 3: *High-frequency noise can show up in the original time series as "spikes", and spikes in the wind and temperature signals of a sonic anemometer can be correlated (if they have a physical cause in the underlying speed-of-sound measurement). Was any algorithm to detect and remove spikes used? If so, was its effectiveness tested?*

Reply 3: This reply is in our AMTD comment, “Response to J. Laubach (comments 3–4)” posted on 16 Feb 2012. We will post additional information about the despiking in our reply to Thomas Foken.

Comment 4: *It is well-known that high-frequency noise can be corrected for by filtering. In order to obtain valid covariances, it suffices to filter one component only, either w or T. Why is this not attempted, and then checked if filtered data lead to plausible surface energy budgets?*

Reply 4: This reply is in our AMTD comment, “Response to J. Laubach (comments 3–4)”, posted on 16 Feb 2012.

Comment 5: *In p455, L20, you state "the problem appears to be with Ts' not w' because w'Ttc' ... produces reasonable heat fluxes". This is logically flawed. Mean w'Ttc' is OK because there is no correlation between the noise of w' and of Ttc', but w' may still be noisy, and in fact your Figs 3 and 4 show that it is. The u-w and v-w correlations, not investigated here, may also be affected, and hence sonic-based u* estimates, too.*

Reply 5: We agree that our statement needs to be more precise. The point we wanted to make is that you can calculate a “reasonable” flux using w', but not with T_s'. We have no other sensor to compare with the CSAT wind fluctuations so it is more difficult to make conclusions about them. Also, as has been noted by Thomas Foken, T_s is related to the transit times by a sum (e..g, $(\frac{1}{t_1} + \frac{1}{t_2})$, as shown in Eq. 2) whereas the wind components are related to the *differences* between these terms. So any transit time error will be magnified for temperature. Our comparisons of T_{tc}, T_s Coh in Fig. C2 show that T_s is problematic.

Comment 6: *I can offer a couple of hypotheses on the physical cause. Sound is detected as a pressure oscillation. As wind speed increases, firstly, the ratio of transducer-created sound amplitude ("signal") to turbulent pressure fluctuations ("noise") may decrease. Secondly, the shape of the sound signal may be distorted more than in calmer conditions. Both the reduced signal-to-noise ratio and the distorted shape would make it harder for the sonic anemometer's detection algorithm to identify the exact arrival time of the sound signal at the receiving transducer. Errors in the arrival time detection would manifest themselves as spikes.*

Reply 6: We agree with your excellent description of the problem. Based on our “Wind tunnel tests” comment posted to AMTD on 5 March 2012 that showed the sensitivity of T_s to ver3 and ver4 of the CSAT3 firmware it is clear that the technique used to identify the signal shape is a critical part of this process (note that ver3 and ver4 of the CSAT uses a different technique to determine the transit times). One other comment: it’s not obvious to us that the result of what you describe is a “spike”—perhaps a clearer definition of what constitutes a spike is necessary. For example, in Fig. 1 in our post “Response to J. Laubach (comments 3–4)” the raw data clearly have large spikes, but smaller “spikes” of a different nature may still be present after the removal of the large spikes.

Comment 7: *Both these hypotheses are based on observations that I made during my PhD work in 1994, using a different sonic type (Biral Solent 1012 R2, name later changed to "Gill"). Back then I observed that the probability of spike occurrence in the w time series was about constant for wind speed u $<$ 5 m/s, at 2 spikes per* `10^6` *data points, while for 7 m/s $<$ u $<$ 14 m/s the frequency of spikes increased with* `u^3`*. (For u $>$ 14 m/s I had no observations.) As the number of spikes increases, it becomes difficult to replace them in a meaningful way, and one should define a data validity limit for that.*

Reply 7: We agree that the contamination by spikes/noise can reach a point where the sonic temperature data may be unusable. We have relied on the CSAT3 diagnostic word to detect the spikes. We are going to discuss the despiking issues in our reply to Thomas Foken.

Comment 8: *In my view, the manuscript puts too much emphasis on the non-closure of the surface energy budget, which is really only the consequence of a measurement problem. Instead, there should be more emphasis on characterising at which conditions noticeable high-frequency noise occurs in the spectra. Ideally that might lead to identification of the physical causes of this noise; at the least, it should allow to pose a hypothesis what the cause might be, as suggested above.*

Reply 8: We felt that showing how the sensible heat flux problem affects the surface energy balance highlighted a relevant consequence of the H measurement error. We also wanted to include this information as a follow-up to the Turnipseed 2002 paper that first discussed this problem at the Niwot Ridge site. However, we (and the other reviewers) agree with your assessment—the discussion of the surface energy budget in the revised manuscript will be reduced.

References

[Stull(1988)] Stull, R. B.: ‘An Introduction to Boundary Layer Meteorology’, Kluwer Academic Publishers, Dordrecht, The Netherlands, 1988.

[Tagawa(2003)] Tagawa, M. and Kato, K. and Ohta, Y.: Response compensation of thermistors: Frequency response and identification of thermal time constant, Review of Scientific Instruments, 74, 1350–1358, 2003.

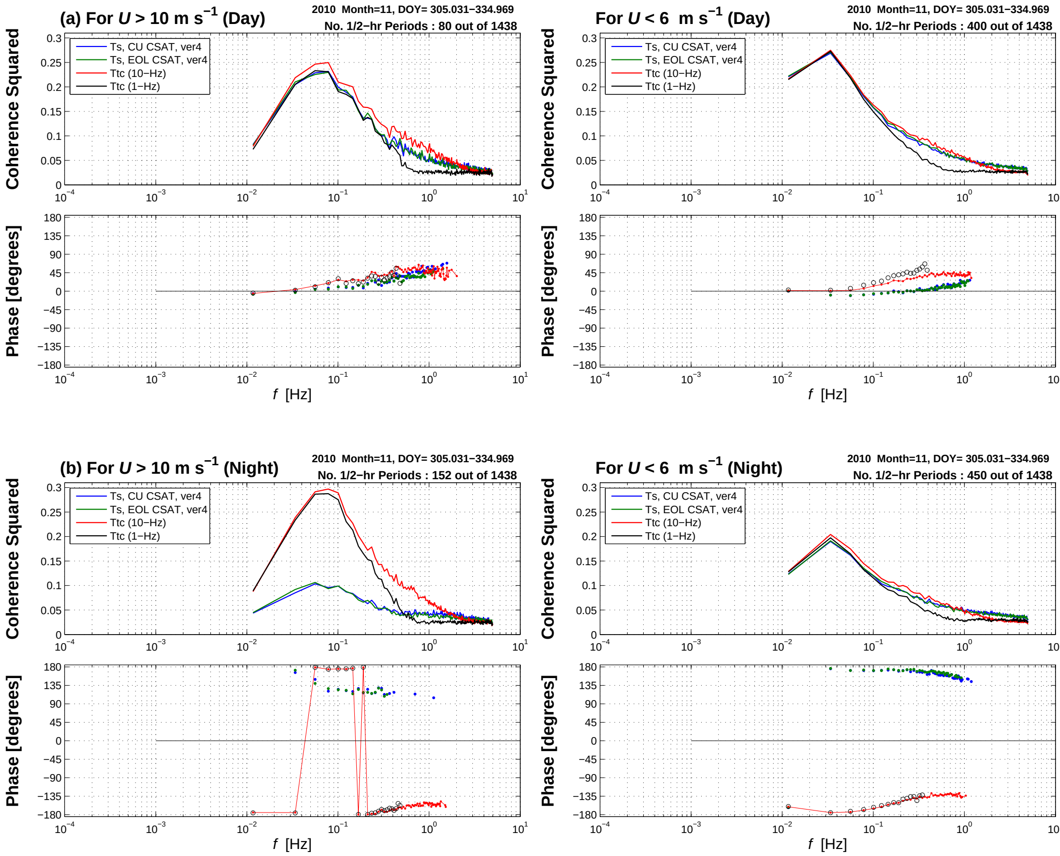

Figure C1: The coherence squared (Coh) and phase between w and T shown for (upper panels) daytime and (lower panels) nighttime conditions from November 2010. The phase has been masked out for $Coh < 0.05$. The left-side panels are for high-winds while the right-side panels are for low winds. Different sensors used for T are described in the legend. To calculate these statistics, 224 equi-sized (N=40 pts) linear frequency bins were used. Each line represents the average from any 30-min periods that satisfy the criteria listed in the title (above the Coh panel).

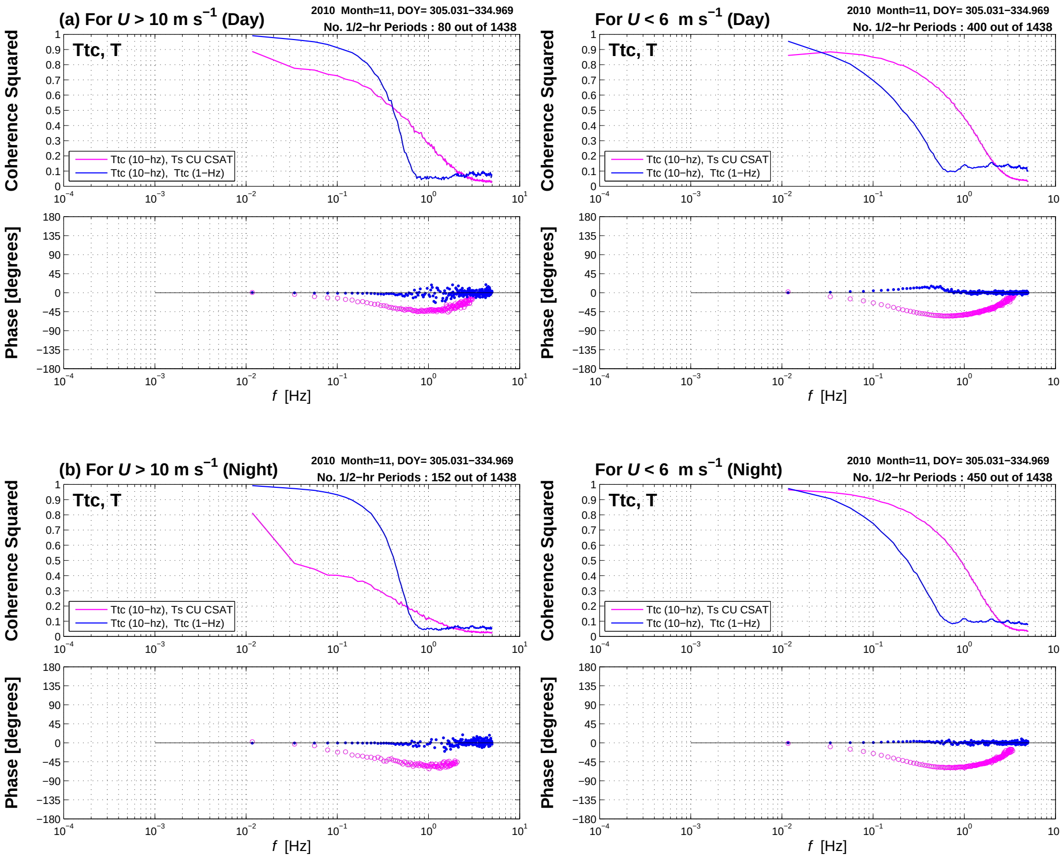

Figure C2: As in Fig. C1, except comparing the 10-Hz thermocouple (T_{tc}) to the 1-Hz thermocouple and sonic temperature (T_s) as specified in the legend. The 1-Hz thermocouple data has been linearly interpolated to 10-Hz to do the comparison.