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

Reply to Thomas Foken

S. P. Burns et al.

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The thoughtful comments by Thomas Foken are greatly appreciated. In our response we have broken up the comments into sections to make our reply more readable.

Comment 1: *First of all, even if the problem was not able to be solved here, the paper is still very important for the discussion of the problem of unrealistically high sensible heat fluxes under high wind velocities.*

Reply to Comment 1: We fully agree this heat flux problem is an issue that should be discussed and (hopefully) fixed. Before receiving feedback about our manuscript we did not appreciate that this phenomena has been previously observed with sonic anemometers by many other researchers (we have added a discussion about these previous findings as a paragraph in the Introduction of the revised manuscript). In our revised manuscript we have presented a conceptual model of the Ts error with the CSAT3 and given an example of an empirical correction (more details in our replies below).

Comment 2: *I believe that the authors are right to see the problem as an unrealistic correlation between the sonic temperature and the vertical wind velocity. From my own experience, I do not believe that the problem lies with either a deformation of the instrument, or the firmware of the CSAT3 sonic anemometer. It is of importance that whereas in Eq. (2) the part (1/t1+1/t2) represents the sonic temperature, the relevant equation for the wind velocities has the part (1/t1-1/t2). If one of the times (t1 or/and t2) has an error, a self-correlation is generated between the sonic temperature and the wind velocity, and unrealistically high fluxes result.*

Reply to Comment 2: We appreciate the important distinction you point out in Eq. 2 related to measuring T_s compared to the wind velocities. We bring up this point at the end of section 3.5 in the revised manuscript. Also, in the revised manuscript, we now clearly show and discuss the differences between ver3 and ver4 of the firmware by a wind tunnel test (Fig. 7 in the revised manuscript) as well as independent tests by Campbell Scientific.

Comment 3: *This could arise from either a deformation of the sensor or, as I believe, particles in the measuring path. I found similar effects during an experiment in Antarctica under snow drift conditions. At this time I used a Kaijo Denki DAT 300, TR-61A probe (Hanafusa et al., 1982). The system measured the temperature in only one channel and the effect was probably much larger than for CSAT3, where the temperature and the wind velocity is measured by sound propagation in three measuring paths. The effect of snow particles could be found as spikes in the signals. The number of spikes at three different levels (2.0, 4.5, and 12.0 m) could be fitted well with typical snow particle profiles near the surface (Foken, 1998, see Fig. included, Neumayer-station Antarctica, Jan. 30, 1994). A similar effect was found by my colleagues (Lüers and Bareiss, 2011) for CSAT3 during measurements at Svalbard (Norway). I would propose the deactivation of spike detection software and the selection of even small spikes. This may be difficult because, due to the high wind velocities, the temperature fluctuations are small and at the same order as the resolution of the system. This factor corresponds with the findings that the effect is larger at night than in day time. It cannot be an effect of stratification, because for very high wind velocities the stratification is always neutral. But on hilly sites the wind maximum occurs—in most cases—during night, when the flow is less mixed and more stratified. Under such conditions the self-correlation could be higher. This can probably be controlled with the normalized standard deviations (integral turbulence characteristics). According to your paper the effect was probably found in winter time, and snow drift typically occurs under high wind velocities. Because the Niwot Ridge site is also often used for air chemistry research, perhaps data from counters for large particles are available. I propose that the authors should check this possibility, and—even when it cannot explain all cases—this discussion should be included in a revised version of the paper. Similar unrealistically high sensible heat fluxes occur in the case of gravity waves. This problem was discussed by Foken and Wichura (1996).*

Reply to Comment 3: We agree that the occurrence of spikes increases when it is humid and/or snowy/windy. As an example, we include Fig. C1 below that demonstrates how the number of spikes in windy conditions increases when the relative humidity is larger than 40-50%. The heat flux problem, however, is present during periods of high winds regardless of the humidity (e.g., the data shown in Fig. 2 in the manuscript include both dry and humid periods). In the revised manuscript, we have clearly described the qualitative reason for the CSAT3 heat flux error (section 3.4) and provided an empirical correction for this error (section 3.5) that agrees with our conceptual model of the error.

Comment 4: *The significant bias of the sonic temperature is known for some sonic anemometer types (Mauder et al., 2007) but less so for CSAT3. The temperature fluctuations are often not affected.*

Reply to Comment 4: We added the reference to Mauder et al., (2007) in section 3.3 where the CSAT mean bias is discussed.

Comment 5: *Furthermore, I want to address three problems which should be more carefully discussed in the paper (problems two and three are probably no longer relevant in the revised version).*

1)The thermocouple is very thick and has a high radiation error. The radiation error should be calculated (see e. g. Foken, 2008) for a more accurate interpretation of your daytime values.

Reply to Comment 5: We appreciate that there are radiation errors in the thermocouple mean temperature. For our study, we have assumed that the radiation errors do not significantly increase the thermocouple sensible heat flux, because they are uncorrelated with the vertical wind fluctuations (Johannes Laubach makes this same point in his comment). For the mean comparisons, we use an aspirated T/RH sensor so the radiation errors are much reduced. However, we agree that the response time of this relatively thick thermocouple could be an issue. One of the recommendations/conclusions of our study is that the time response of the thermocouple should be evaluated by adding a fast(er)-response temperature sensor to the tower setup.

Comment 6: *2)The storage term in the canopy is probably small, but is this the case in the soil?*

Reply to Comment 6: We have soil heat flux sensors that show the soil heat flux is only a fraction (less than 5%) of the sensible heat flux. We have greatly reduced the discussion of the surface energy budget as recommended by several of the reviewers.

Comment 7: *3)The findings of your radiation measurements are not surprising, because Q7 underestimates the net radiation (Kohsiek et al., 2007) and therefore the energy balance closure is better.*

Reply to Comment 7: As mentioned above in Reply 6, we have greatly reduced the discussion of the surface energy budget. Perhaps a future study will re-visit the topic of the surface energy budget for the Niwot Ridge site.

References

[Mauder et al.(2007)] Mauder, M., Oncley, S. P., Vogt, R., Weidinger, T., Ribeiro, L., Bernhofer, C., Foken, T., Kohsiek, W., De Bruin, H. A. R., and Liu, H.: The energy balance experiment EBEX-2000. Part II: Intercomparison of eddy-covariance sensors and post-field data processing methods, Boundary-Layer Meteorol, 123, 29–54, 2007.

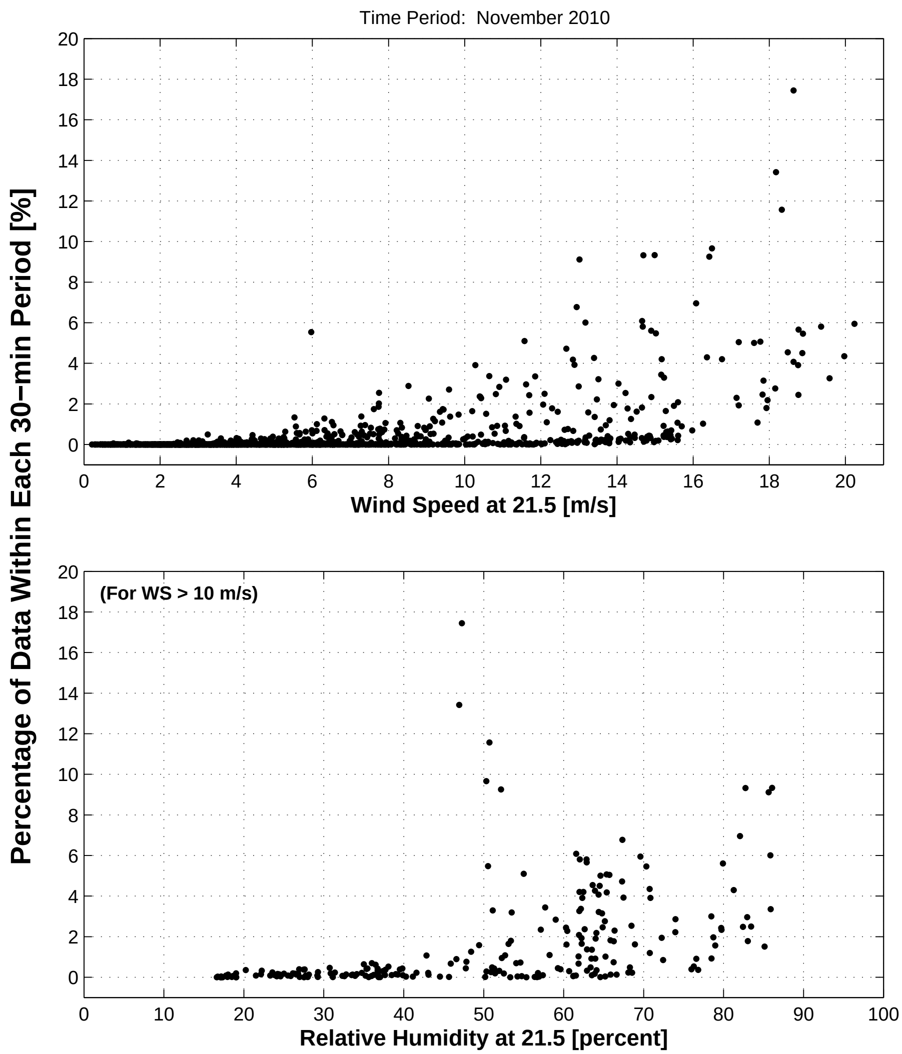

Figure C1: The percentage of data detected as spikes (or otherwise unusable) by the CSAT diagnostic word versus (upper) wind speed and (lower) relative humidity (for periods when wind speed is greater than 10 m/s).