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

Reply to Reviewer 2

S. P. Burns et al.

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The thoughtful comments by Reviewer 2 are greatly appreciated. Our replies to the Reviewer 2 comments are below.

General Considerations: *This paper presents evidence for problems that arise when 'sonic temperatures', i.e. temperatures derived from measurements of transit times and the temperature dependence of speed of sound in a sonic anemometer are employed to derive sensible heat fluxes. The authors show that the derived heat fluxes are 'more positive' (i.e., a positive increment is added) than those obtained from combining the sonic anemometer's vertical velocity component fluctuation (w') with another, independent measurement of temperature fluctuations (from a thermocouple, tc) to yield a reference heat flux. The problem is clearly laid out and the author's approach to investigate it using different versions of the sonic, different arrangements with respect to sensor separation (for the tc-sonic system) is well thought and takes into account potential error sources. The analysis is clear and well described and the conclusions are derived in a straight-forward manner. The authors offer an explanation (which I doubt, see below), but basically conclude that the sonic-derived sensible heat fluxes are erroneous due to an error in the sonic temperature fluctuation and the manufacturer is working on a solution. The problem itself is certainly of relevance and has not received much attention in the past (although a number of comments during the open review process have pointed to similar findings by other authors). Therefore the paper, in one form or the other, certainly should be published. I have, however, a number of major comments that I feel should be addressed before it can be published in a final form.*

Reply to "General Considerations": 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).

Comment 1: *Experimental layout. One of the problems with this study is certainly that the authors use 'the possibly most difficult environment' to test a turbulence instrument, i.e. a site in truly complex terrain. Hence they can only hypothesize that (for example) the sensible heat flux during the night is more likely to be negative and thus the H_tc is more likely to be correct* *than the H_sonic. The complex site of course introduces all sorts of other problems that cannot be ruled out to be the source of the problem. One of them is the frame of reference for the sonic coordinate system (planar fit, l.97), which is i) possibly wind direction dependent (if the slope is not perfectly regular) and ii) through this possibly also dependent on wind speed (if strong winds prevail from a preferred direction). Therefore, the ’more likely negative heat fluxes’ (during the night) are equally challenged as the conclusion that the problem is dominantly attributable to the T’ (and not the w’). The authors argue that at least the high-frequency noise in the temperature spectra (as a potential source of the error in T’) is not site specific because it was observed at other sites (arguably neither perfectly ’ideal, undisturbed sites’ from the CHATS project). So as a conclusion from the problem of the difficult site, it would at least be more convincing if other observations (or indications) of the same problem could be added.*

Reply to Comment 1: We agree that NWT is a complex site and perhaps not ideal for testing an instrument such as a sonic anemometer. However, we feel that the CSAT should be able to operate in such an environment (i.e, the flow angles are not so severe that they are outside the measurement range of the CSAT). As described above in the Reply to “General Considerations” we have now performed additional tests that confirm the tower observations. These points are included in the revised manuscript and will hopefully convince you that the problem is not an artifact of the NWT site.

Comment 2: *The author’s explanation. On l. 194ff the authors offer an explanation for why the correlation between T’_s and w’ be larger than that between T’_tc and w’ (T_s being the ’(humidity adjusted) sonic temperature’, and T_tc the thermocouple temperature, respectively). This explanation is based on the observation that <T_s> error is negatively correlated to <u> (<..> referring to 30min averages). They state ’Since w’ is negatively correlated with u’ in the surface layer and the T’ error is also negatively correlated with u’, the <w’T_s’> error is positive, as observed’. However, what Fig. 5 shows is that the <T_s> error (not the T’_s error) is negatively correlated to <u> (and not u’), and hence the conclusion is by no means valid. Unless it can be shown that the T’_s error correlates with <T_s> (which is by no means clear) and u’ correlates with <u> (which is of course better founded) this ’explanation’ should be carefully reformulated or removed.*

Reply to Comment 2: In the revised manuscript we have shown how the mean temperature and heat flux are related to each other by our conceptual model of the Ts error and the empirical correction applied to the NWT CSAT data (described in sections 3.4 and 3.5).

Comment 3: *Enhanced correlation between T’ and w’. Figure 1 shows (as an example) that apparently during the night H_s can even show an opposite sign than H_tc. If we (as the authors do) assume that the downward heat flux as determined from the thermocouple is ’more likely’, this suggests that during these high wind speed situations, when positive w’ are occurring predominantly together with negative T’_tc (and vice versa), the sonic temperature reading (T’_s) during w’>0 events even becomes positive (and vice versa). In other words the flux partitioning can change from quadrants 2/4 into 1/3. On the other hand, the evidence of Figs 2 and 3 only supports a change in the magnitude of <w’T’_s> (the average covariance in Fig 3b is still negative, but much smaller than <w’T’_tc>). This could be effectuated by having the dominant quadrants unchanged but only the T’_s changed in magnitude. All this suggests* *that the reason for the <w'T'_s> error (if <w'T'_tc> is assumed to be correct or at least more likely) could be found in a combined effect of <u> on both w' and T'_s. Wind speed components from sonic anemometers are known to be subject to errors from 'flow distortion' (e.g., Wyngaard 1981) and transducer shadowing (e.g., Zhang et al 1986). Since these authors have published their results, great efforts were made by sonic manufacturers to i) reduce the errors (by optimizing the alignment of the transducers) and ii) offer correction algorithms built in the sonic software. The errors of the three wind components are known to be dependent on mean wind speed (as well as on the angle of attack) and so correction algorithms are based wind tunnel calibrations using different angles of attack and different wind speeds (e.g., Vogt et al. 1997). I am not quite sure of the technical details (of the calibration) for the CSAT3 sonic under consideration, but I would assume that the calibration range does not exceed 15 m/s. At the same time, the complex site will produce fluctuations that are distinctly nonhorizontal (depending on how the sonic is mounted—normal to the surface or strictly 'horizontal'— this will even with the oblique sensor design of CSAT3 produce instances of flow close to the sensor axes and hence potentially large distortion). So under high-speed conditions at this site the sonic is likely being used at least at the outer edge of the validity range of the calibration for wind speed. Temperature, on the other hand (which in principle suffers from the same flow distortion and transducer shadowing effects, but is—as far as I am aware—not corrected because it is the fluctuation and not the mean temperature that usually is of interest) does not exhibit a change in the 'calibration performance' under these conditions (is subject to flow distortion and transducer shadowing, but not corrected, at low wind speeds, and the same at high wind speeds). Now, wind speed components are linearly (1/t) dependent on transit times, while temperature depends on the square of it (eq. 2 in the manuscript), so that this different behaviour in connection with the different status with respect to calibration could indeed lead to the observed too positive heat fluxes. I am not in the position to substantiate all the ingredients of this hypothesis (in particular the details of the CSAT3 calibration range and applicability), but I think it would be extremely interesting to investigate—possibly in collaboration with the engineers of Campbell Sci.—whether this could be a possible reason.*

Reply to Comment 3: We are generally in agreement this comment. However, it appears that a well-calibrated CSAT running ver3 of the firmware can successfully operate at the NWT site. We did not evaluate the effect of transducer shadowing on the CSAT winds (or sonic temperature), but perhaps this could be considered in a future study.

Comment 4: *In Fig 4 the right column panels need to have the x-Axis labelled (as in Fig. 3, I guess).*

Reply to Comment 4: This has been corrected in the revised manuscript.