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Atmospheric Measurement Techniques Discussions

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Interactive Comment

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

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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.

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. 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).

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.

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.

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

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.

I propose the revision of the manuscript and the inclusion of some additional investigation as discussed above. Probably other hypothetical conclusions, including some points highlighted by Johannes Laubach, can be deleted.

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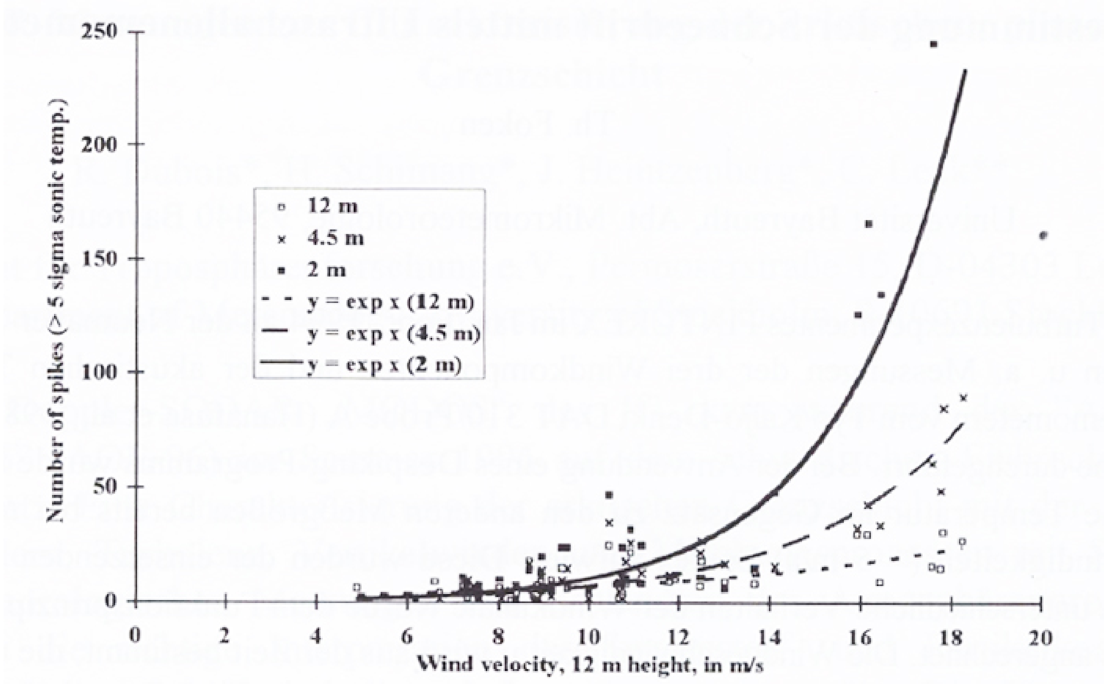

Fig. 1.

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